# **Bienvenue** au **6e congrès** de la **SPS**

*Ce que vous trouvez dans ce livre …*


| | |
|---|---|
| **Remerciements** | **2** |
| **Programme** du congrès & **survol** des résumés | ***3-6*** |
| **Arriver** à **Lausanne** | ***7*** |
| **Lausanne** & arriver à l'**université** | ***8*** |
| **Carte** pour trouver le **congrès** | ***9*** |
| **Informations importantes** | ***10*** |
| **« Visiter Lausanne »** (*suggestions*) | ***11*** |
| **Restaurants & drinks** en ville (*suggestions*) | ***12-13*** |
| **« Social Dinner »** le vendredi 1 juillet | ***14*** |
| **Résumés du congrès** | ***15-136*** |
| **Index** de tout-e-s les **intervenant-e-s** | ***137-140*** |


# **Welcome** to the **6th meeting** of the **SPS**

*What you find in this book …*


| | |
|---|---|
| **Acknowledgments** | **2** |
| **Program** of the meeting & abstract **overview** | ***3-6*** |
| **Arriving** in **Lausanne** | ***7*** |
| **Lausanne** & arriving at the **university** | ***8*** |
| **Plan** for finding the meeting | ***9*** |
| **Crucial information** | ***10*** |
| **"Visiter" Lausanne** (*suggestions*) | ***11*** |
| **Restaurants & drinks** in town (*suggestions*) | ***12-13*** |
| **"Social Dinner"** on Friday 1st July | ***14*** |
| **Abstracts of the meeting** | ***15-136*** |
| **Index** of all **speakers** | ***137-140*** |

# Remerciements / Acknowledgments

*Nous tenons à remercier toutes les personnes et institutions qui nous ont aidés lors de la préparation et du déroulement de ce congrès, en particulier :*

Le comité administratif de la Société de philosophie des sciences (SPS) pour leur confiance et leur aide tout au long de la préparation du congrès, l'Université de Lausanne (UNIL) en tant que hôte du congrès et pour ses diverses structures de soutien à l'organisation, le Fonds National Suisse (FNS) de la recherche scientifique pour son soutien financier, la section de philosophie de l'UNIL pour son soutien financier, le centre informatique de l'UNIL, notamment Paulo Monteiro, pour leur soutien pour la création du site web du congrès et leur collaboration lors des phases de soumission des résumés, d'inscriptions et de paiement des inscriptions, toutes les personnes qui ont soumis des résumés ainsi que les conférenciers des plénières, le comité scientifique du congrès pour l'évaluation des résumés, le service financier de l'UNIL pour leurs services, le Helpdesk de l'UNIL pour la gestion du matériel informatique, la reprographie pour l'impression de ce « livre », l'Unibat, notamment Mme Enza Arena Mueller, pour la gestion des infrastructures et les conseils par rapport au déroulement du congrès, Frederic Seiler du Secrétariat de philosophie, la cafétéria « Nino » pour les pauses de café et *last but not least* les étudiantes et étudiants du « staff » – Camille De Boer, Virgile Delmas, Marie-Gabriele Mansour, Edith Salgado, Aliekber Tutam et Yvan Uhlmann – pour leur aide précieuse.

Lausanne, juin 2016   *Christian Sachse & Vincent Lam*

Sponsors:

*We would like to thank all the persons and institutions that helped us in preparing and realizing the meeting, notably*:

The administration committee of the Société de philosophie des sciences (SPS) for their trust and continuous help during the preparation period, the university of Lausanne (UNIL) for being host and its diverse support structures for this meeting, the Swiss National Foundation (SNF) for its financial support, the philosophy department of UNIL for its financial support, the IT centre of UNIL, notably Paulo Monteiro (for the help with the web site creation and in particular for the submission, registration and payment process), everybody who submitted a paper or symposium as well as the plenary speakers, the scientific committee for evaluating the submissions, the finance departement of UNIL for their service, the Helpdesk of UNIL for the computer material, the reprography for this "book", Unibat (notably Enza Arena Mueller) for the logistics & general advises, the cafeteria "Nino" for the coffee breaks, Frederic Seiler of the philosophy secretariat, and last but not least the students of our "staff" group – Camille De Boer, Virgile Delmas, Marie-Gabriele Mansour, Edith Salgado, Aliekber Tutam, and Yvan Uhlmann – for their precious help.

Lausanne, June 2016   *Christian Sachse & Vincent Lam*

SPS

| 29 JUIN / JUNE 29 | Amphipôle **348** (*auditoire/auditory*) | Amphipôle **315** | Amphipôle **315.1** | Amphipôle **319** | Amphipôle **321** |
|---|---|---|---|---|---|
| 08h45 – 09h00 | ***Opening*** | | | | |
| 09h00 – 10h30 | **Andler, Daniel**: *La double visée des chercheurs et le changement dans les sciences* *(The scientists' dual target and scientific change)* | **Séance plénière** **Plenary session** | | | |
| *Pause café / Coffee break* | | | | | |
| 11h00 – 11h40 | Symposium: **Bary, Sophie, Barberousse, Anouk & Faugère, Elsa**: *Evolution de la vision sur la diversité du vivant* | **Basilico, Brenda**: *La transformation de la notion de subalternation dans la théorie musicale de Marin Mersenne* | **Kirman, Alan**: *Will the economic crisis produce a paradigm shift in economics?* | **Weirich, Paul**: *Change in the decision sciences* | **Egg, Matthias**: *Real patterns without underlying stuff* |
| 11h40 – 12h20 | | **Pradeu, Thomas**: *Ch-ch-changes: diachronic identity and the immune system as a sensor of change (A tribute to David Bowie)* | **Cozic, Mikael**: *On the confirmational relevance of neuroeconomics* | **Métioui, Abdeljalil & Baulu M. W., Mireille & Trudel, Louis**: *Change in science: the case of the development of the atomic theory from Democritus to Dalton* | **Allori, Valia**: *Scientific realism, theory change and primitive ontology* |
| 12h20 – 13h00 | | **Ghica, Felicia**: *Ignace Meyerson et la psychologie historique ou la psychologie comme zone de sables mouvants* | **Henschen, Tobias**: *The logic of scientific discovery in macroeconomics* | **Sartenaer, Olivier**: Change in science and changing science | **Cordovil, João L. & Santos, Gil**: *OSR: Emergence or micro-physicalism?* |
| *Pause midi / Lunch break* | | | | | |
| 14h40 – 15h20 | **Dupin, Aurore & Gayon, Jean**: *Les fonctions en chimie* | **Gerville-Réache, Léo**: *Choix rationnel: définir et estimer son "intérêt"* | **Marasoiu, Andrei**: *Human minds don't extend* | **Onelli, Corinna**: *Change and tradition. For a new interpretation of Francesco Redi's experiments on the generation of insects (1668)* | **Esfeld, Michael**: *Quantum Humeanism* |
| 15h20 – 16h00 | **Yapi, Ignace**: *Le bricolage évolutif: un modèle pertinent des mutations dans les sciences* | **Giroux, Elodie**: *La relativité individuelle de la santé et de la maladie: Canguilhem et la médecine personnalisée* | **Theurer, Kari**: *What a brain state is not* | **El Skaf, Rawad**: *How thought experiments cause change in science* | |
| *Pause café / Coffee break* | | | | | |
| 16h30 – 17h10 | **Ferreira, Anthony**: *L'addiction est-elle une maladie ? Un débat théorique et ses enjeux pratiques* | **Bedessem, Baptiste**: *La sérendipité: un argument pour la liberté de recherche?* | **Schmitt, Eglantine**: *Decisions and choices in computational linguistics systems: a social epistemology of automated codification* | **Arcangeli, Margherita**: *Real, thought and numerical experiments: the experimental triangle* | **Matarese, Vera**: *For an approximate continuity of structure between Newtonian and Bohmian mechanics* |
| 17h10 – 17h50 | **Ferry, Juliette**: *Explorer les limites de la phénoménologie de la médecine* | **Tork Ladani, Safoura**: *La philosophie des sciences humaines: la relation réciproque entre la philosophie et la littérature* | **Heesen, Remco**: *Why journal editors play favorites* | **Casini, Lorenzo**: *How theoretical explorations explain. A Bayesian account (joint work with Radin Dardashti)* | **Fletcher, Samuel**: *On the alleged incommensurability of Newtonian and relativistic mass* |
| 18h00 – 19h30 | **Soler, Léna**: *Le problème de la contingence / inévitabilité des accomplissements scientifiques : la demande de l'inévitabiliste au contingentiste d'exhiber une alternative scientifique « réelle »* | **Séance plénière** **Plenary session** | | | |

| 30 JUIN / JUNE 30 | Amphipôle **348** (auditoire/auditory) | Amphipôle **315** | Amphipôle **315.1** | Amphipôle **319** | Amphipôle **321** |
|---|---|---|---|---|---|
| 09h00 – 10h30 | **Butterfield, Jeremy**: *Scientific realism and primordial cosmology: joint work with Feraz Azhar* | **Séance plénière** **Plenary session** | | | |
| *Pause café / Coffee break* | | | | | |
| 11h00 – 11h40 | Symposium: | **Le Bihan, Baptiste**: *Classer le monde empirique sans classeurs: comment concilier réalité de la classification et inexistence des sortes naturelles?* | **Ruphy, Stéphanie**: *Pluralist challenges to a science-based metaphysics* | **Ferrando, Tiziano**: *Towards a development of the metaphysics of primitive stuff* | **Romano, Davide**: *Why Bohmian non-locality is not a problem for us (classical objects)* |
| 11h40 – 12h20 | **Racovski, Thibault, Chavalarias, David, Huneman, Philippe, Bittencourt, Wellington & Fisler, Marie**: | **Tonnerre, Youna**: *Peut-on parler d'incommensurabilité structurale?* | **Felline, Laura**: *Mechanisms meet structural explanation* | **Kao, Molly**: *Einstein, Millikan and quantum theory: the evidential import of the photoelectric effect* | **Hubert, Mario**: *A primitive ontology without properties* |
| 12h20 – 13h00 | *Approches phylogénétiques et quantitatives des concepts scientifiques* | **Herzog, Michael & Doerig, Adrien**: *Why our best theories of perception lead to anti-reductionism* | **Kostic, Daniel**: *Mathematical features and ontic commitments in topological explanation* | **Fahrbach, Ludwig**: *We think, they thought* | **Oldofredi, Andrea**: *Particles creation and annihilation: a Bohmian approach* |
| *Pause midi / Lunch break* | | | | | |
| 14h30 – 15h20 | SPS **Assemblée Générale & *Prix Jeunes Chercheurs*** | | | | |
| 15h30 – 16h10 | GWP SPS Symposium: | **Dieli, Anna Maria**: *Individus darwiniens et sélection multi-niveau: enquête sur l'individualité de la cellule cancéreuse* | **Malecka, Magdalena**: *Scientific imperialism: an attempt at a definition* | **Pellet, François**: *Biological reality in theoretical science: the tower of Hanoi case* | **Wüthrich, Christian**: *The atemporal emergence of temporality* |
| 16h10 – 16h50 | **Reydon, Thomas, Love, Alan & Kaiser, Marie I.**: | **Lefèvre, Victor**: *L'organicisme en écologie: pseudo-science ou progrès scientifique?* | **Thoron, Sylvie**: *Is economics becoming a science of morality?* | **McFarland, Andrew**: *Causal powers and isomeric chemical kinds* | **Accorinti, Hernan & Martínez González, Juan Camilo**: *Theories and models: an approach from quantum chemistry* |
| 16h50 – 17h30 | *Towards a practice-oriented metaphysics of science* | **Lequin, Mathilde**: *Y a-t-il eu une "révolution paléobiologique" en paléoanthropologie?* | **Rivelli, Luca**: Antimodularity: *computational complexity may hinder scientific explanation* | **Held, Carsten**: *Antecedent strengthening and ceteris paribus laws* | **Scholl, Raphael**: *Stability without stasis: ambition and modesty of realism about true causes* |
| *Pause café / Coffee break* | | | | | |
| 18h00 – 19h30 | **Hooker, Clifford**: *Re-modelling scientific change: complex systems frames innovative problem solving* | **Séance plénière** **Plenary session** | | | |

| I JUILLET / JULY I | Amphipôle **348** (*auditoire/auditory*) | Amphipôle **315** | Amphipôle **315.1** | Amphipôle **319** | Amphipôle **321** |
|---|---|---|---|---|---|
| 09h00 – 10h30 | **Neander, Karen**: *The theory-theory of concepts* | **Séance plénière** **Plenary session** | | | |
| *Pause café / Coffee break* | | | | | |
| 11h00 – 11h40 | <u>Symposium</u>: **Merlin, Francesca, Pontarotti, Gaëlle, Weitzman, Jonathan & Rial-Sebbag, Emmanuelle**: *From genetics to epigenetics: what has changed?* | **Lopez, Olga**: *Michel Serres: l'histoire des sciences, un modèle de communication* | **Clavien, Christine**: *An evolutionary and mechanistic explanation of moral intuitions* | **Fumagalli, Roberto**: *Who is afraid of scientific imperialism?* | **Doboszewski, Juliusz**: *Determinism, epistemic holes and truncated spacetimes* |
| 11h40 – 12h20 | | **Giovannetti, Gabriel**: *Les principes de coordination: tentative d'élaboration d'un empirisme historique.* | **Lipko, Paula & Córdoba, Mariana**: *The patterns of life, and a new response to the species problem* | **Ivanova, Milena**: *Poincaré on the role of beauty in science* | **Baas, Augustin**: *The hypothetical nature of quantum randomness* |
| 12h20 – 13h00 | | **Ruyant, Quentin**: *L'empirisme modal* | **Hladky, Michal** *Functions in biology and in technology: in defence of a unified account* | **Bschir, Karim**: *Does epistemic pluralism foster scientific progress?* | **Farr, Matt**: *Causation and time reversal* |
| *Pause midi / Lunch break* | | | | | |
| 14h30 – 15h10 | <u>Symposium</u>: **Moya Diez, Ivan, Bertoldi, Nicola & Vagelli, Matteo**: *Technique, styles et évolution dans les études d'épistémologie historique des sciences de la vie* | **Barton, Adrien**: *Une ontologie dispositionnelle du changement* | **Ferreira Ruiz, María José**: *A property-cluster kind approach to life* | **Lemoine, Philippe**: *Scientific realism, approximate truth and the argument from underdetermination* | **Ardourel, Vincent**: *Théories finitistes des transitions de phase, émergence et idéalisations infinies* |
| 15h10 – 15h50 | | **Walter, Christian**: *La résistance au changement dans la modélisation mathématique en finance: l'exemple de la représentation brownienne de 1950 à 2000* | **Mossio, Matteo**: *Understanding biological stability: an organicist perspective* | **Cordoba, Mariana & Accorinti, Hérnan & Lopez, Cristian**: *How to deal with truth in pluralism within philosophy of sciences: boundaries and scopes* | **Stamenkovic, Philippe**: *De l' « utilité négative » de la philosophie d'Ernst Cassirer: application à l'argument EPR* |
| 15h50 – 16h30 | | **Israel-Jost, Vincent & Jebeile, Julie**: *Traitement des données et simulation numérique: quelle différence?* | **Wilks, Anna Frammartino**: *How function changed science* | **Dardashti, Radin**: *The epistemology of no-go theorems* | **Grégis, Fabien**: *La notion de précision expérimentale dans les ajustements des constantes de la physique* |
| *Pause café / Coffee break* | | | | | |
| 17h00 – 18h30 | **Norton, John**: *How Einstein did not discover* | **Séance plénière** **Plenary session** | | | |

**"SOCIAL DINNER"**
**19h30 / 20h**

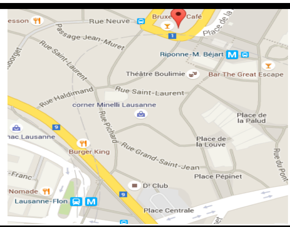
***Le Vaudois***

http://www.levaudois.ch/

Place de la Riponne, 1

*Comment y aller :*

Marcher 10 minutes depuis la station métro ***Flon*** (direction nord/montant) …
<u>**ou**</u> prendre l'autre métro (métro M2, direction *Croisettes*) depuis ***Flon*** jusqu'à la station ***Riponne***.




*How to get there:*

10 minutes walk from metro station ***Flon*** (north/uphill) …
<u>**or**</u> take the other metro (metro M2, direction *Croisettes*) from ***Flon*** to station ***Riponne***.

***Inscription nécessaire*** :

*Participer au « Social Dinner » demande l'inscription jusqu'au 20 Juin !*

***Registration required***:

*Joining the "Social Dinner" requires a registration by June 20 !*

(vous pouvez également utiliser l'**index** à la fin / you may also use the **index** at the end)

**Mercredi 29 juin / June 29 Wednesday**


| | |
|---|---|
| **matinée / morning** | |
| Salle / Room 348 (auditoire/auditory) | **p. 15** |
| Salle / Room 315 | **p. 18** |
| Salle / Room 315.1 | **p. 23** |
| Salle / Room 319 | **p. 27** |
| Salle / Room 321 | **p. 31** |
| **après-midi / afternoon** | |
| Salle / Room 348 (auditoire/auditory) | **p. 35** |
| Salle / Room 315 | **p. 42** |
| Salle / Room 315.1 | **p. 48** |
| Salle / Room 319 | **p. 53** |
| Salle / Room 321 | **p. 59** |


**Jeudi 30 juin / June 30 Thursday**


| | |
|---|---|
| **matinée / morning** | |
| Salle / Room 348 (auditoire/auditory) | **p. 63** |
| Salle / Room 315 | **p. 66** |
| Salle / Room 315.1 | **p. 70** |
| Salle / Room 319 | **p. 73** |
| Salle / Room 321 | **p. 76** |
| **après-midi / afternoon** | |
| Salle / Room 348 (auditoire/auditory) | **p. 80** |
| Salle / Room 315 | **p. 84** |
| Salle / Room 315.1 | **p. 88** |
| Salle / Room 319 | **p. 92** |
| Salle / Room 321 | **p. 95** |


**Vendredi 01 juillet / July 01 Friday**


| | |
|---|---|
| **matinée / morning** | |
| Salle / Room 348 (auditoire/auditory) | **p. 99** |
| Salle / Room 315 | **p. 102** |
| Salle / Room 315.1 | **p. 106** |
| Salle / Room 319 | **p. 110** |
| Salle / Room 321 | **p. 114** |
| **après-midi / afternoon** | |
| Salle / Room 348 (auditoire/auditory) | **p. 119** |
| Salle / Room 315 | **p. 123** |
| Salle / Room 315.1 | **p. 126** |
| Salle / Room 319 | **p. 130** |
| Salle / Room 321 | **p. 133** |


(vous pouvez également utiliser l'**index** à la fin / you may also use the **index** at the end)

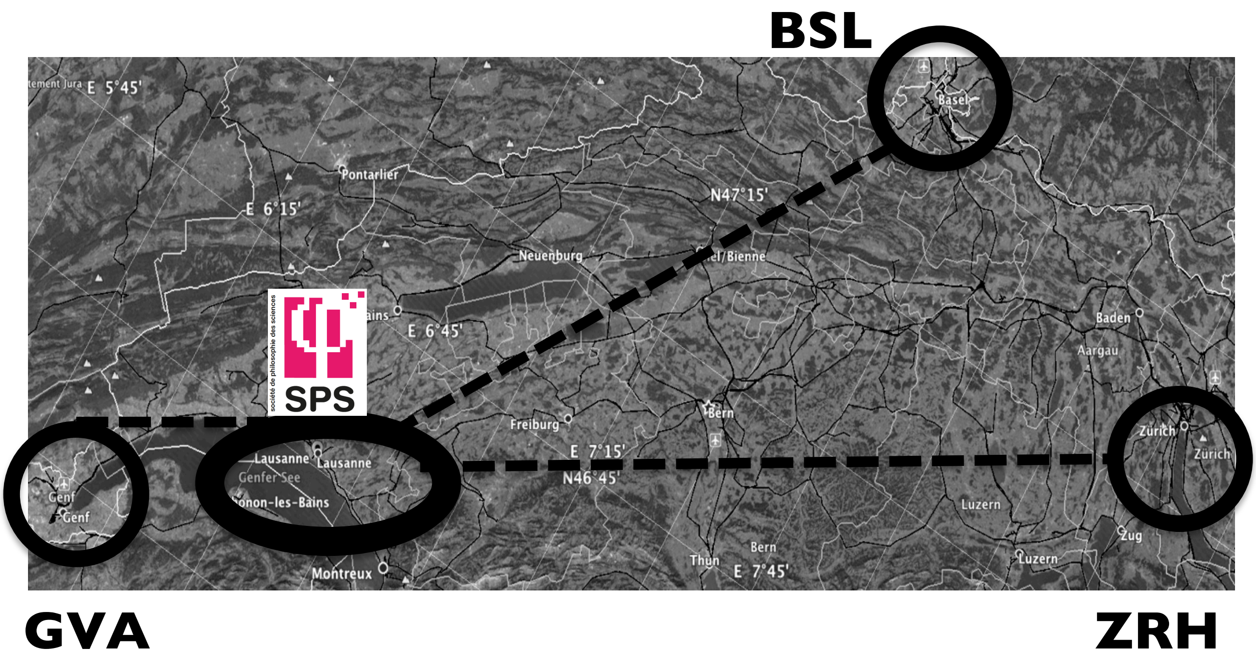
## Arriver à Lausanne / Arriving in Lausanne

### Arriver à Lausanne :

Par train depuis **Genève** aéroport : 2-3 trains par heure, environ 45min de trajet.
Par train depuis **Bâle** aéroport : 2-3 trains par heure, environ 2h30 de trajet.
Par train depuis **Zurich** aéroport : 2-3 trains par heure, environ 2h30 de trajet.

Important : il faut acheter le billet du train avant monter dans le train ; les guichets et les automats se trouvent toujours à l'entrée de la gare; les billets peuvent aussi être achétés en ligne.

Site web pour les horaires et billets : http://www.cff.ch/home.html



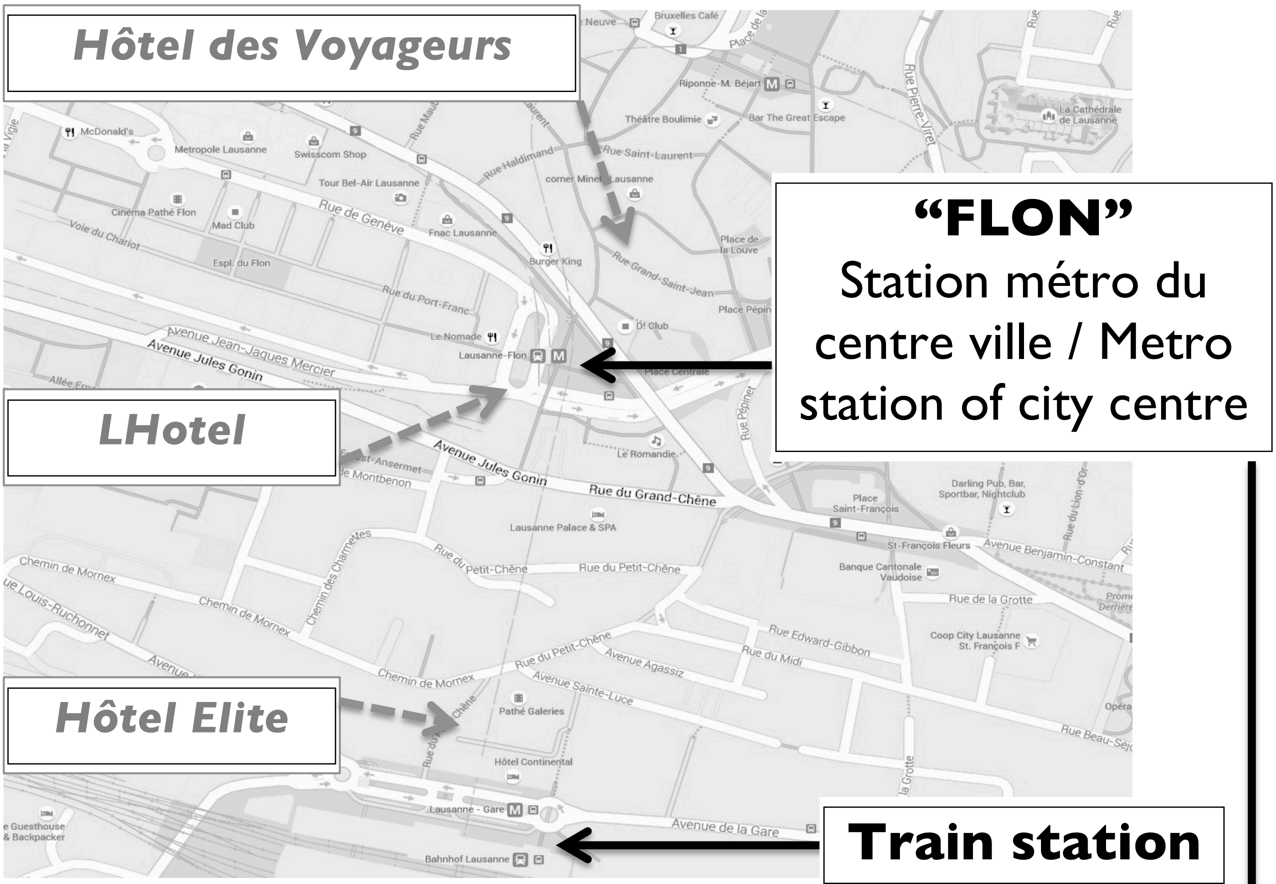

### Arriving at Lausanne:

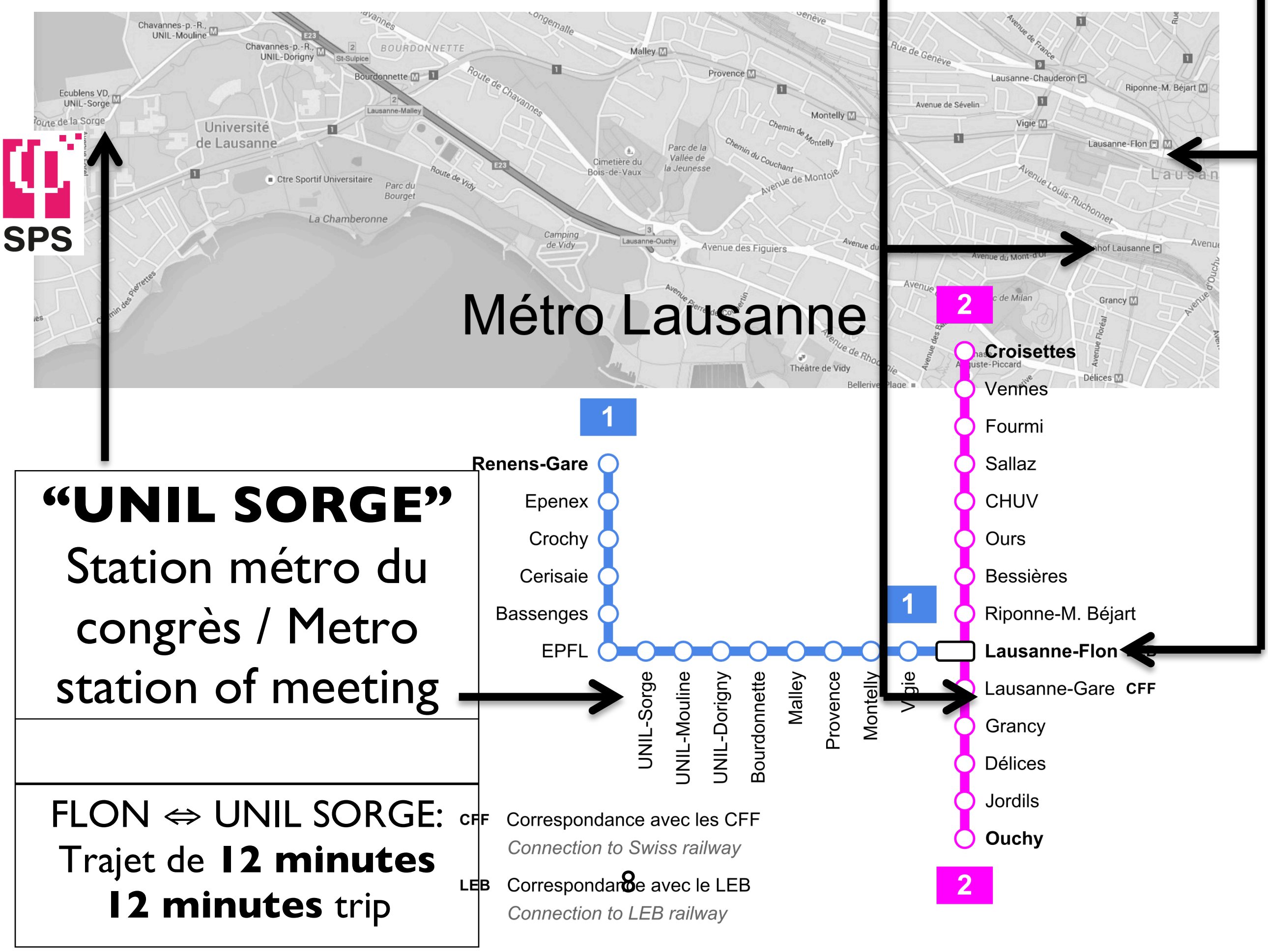
By train from **Geneva** airport: 2-3 trains per hour, trip about 45min.
By train from **Basel** airport: 2-3 trains per hour, trip about 2h30.
By train from **Zurich** airport: 2-3 trains per hour, trip about 2h30.

Important: you need to buy your ticket before entering the train; you find counters and selling machines at the entry of the train station; tickets can also be purchased online.

Website for schedule and tickets: http://www.sbb.ch/en/home.html

## Lausanne & arriver au congrès / Lausanne & arriving at the meeting



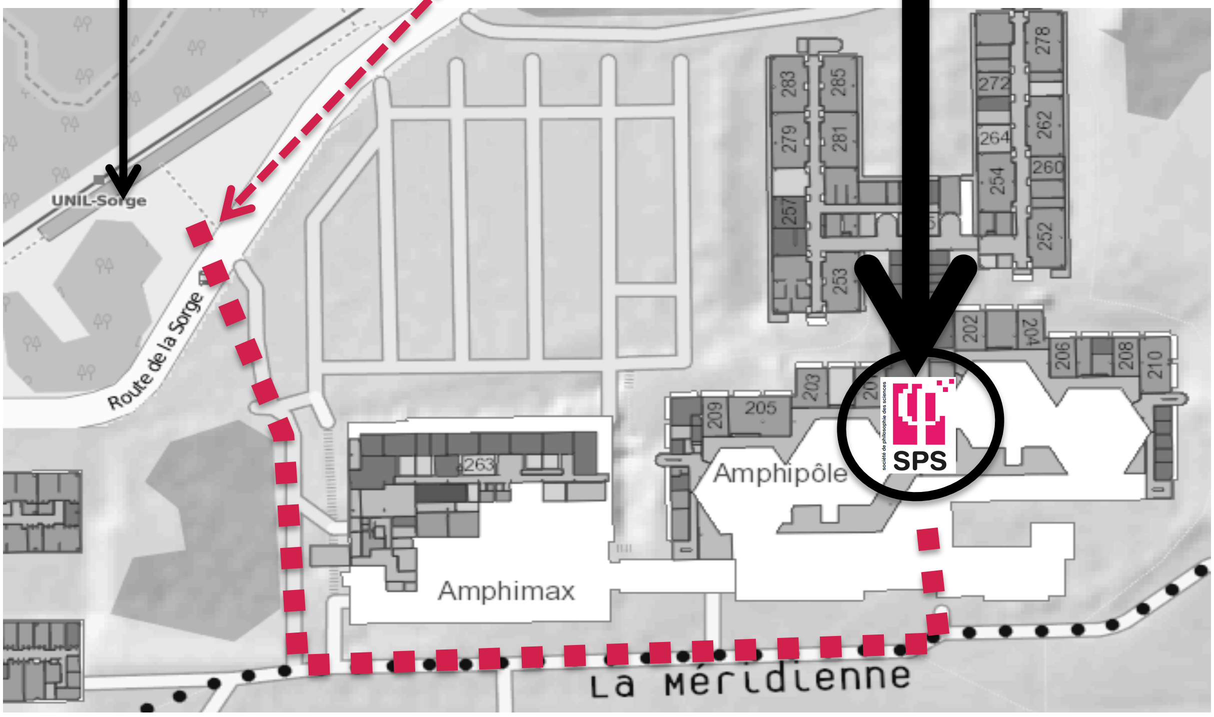
## Plan du congrès / Map of the meeting

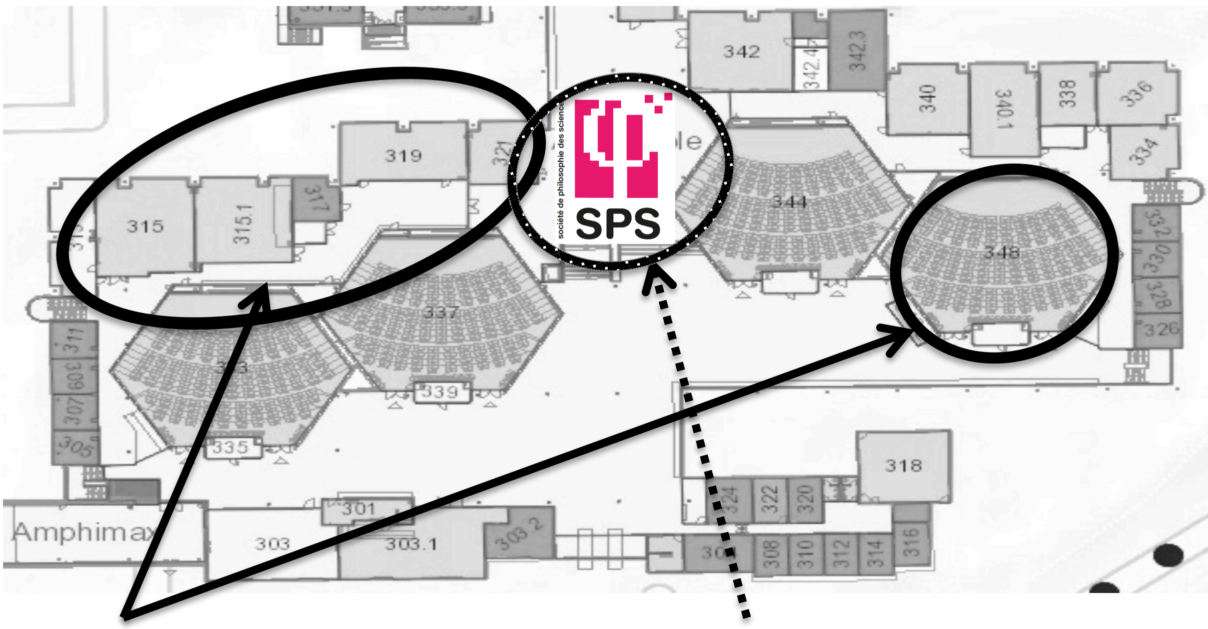
**"UNIL SORGE"**
Station métro du congrès / Metro station of meeting

**3 min**

**Congrès SPS / SPS Meeting**

Bâtiment **Amphipôle / Amphipôle** building




**Salles / Rooms**

**Accueil / Reception desk + Pause café / Coffee breaks**

# Informations curciales / Crucial information

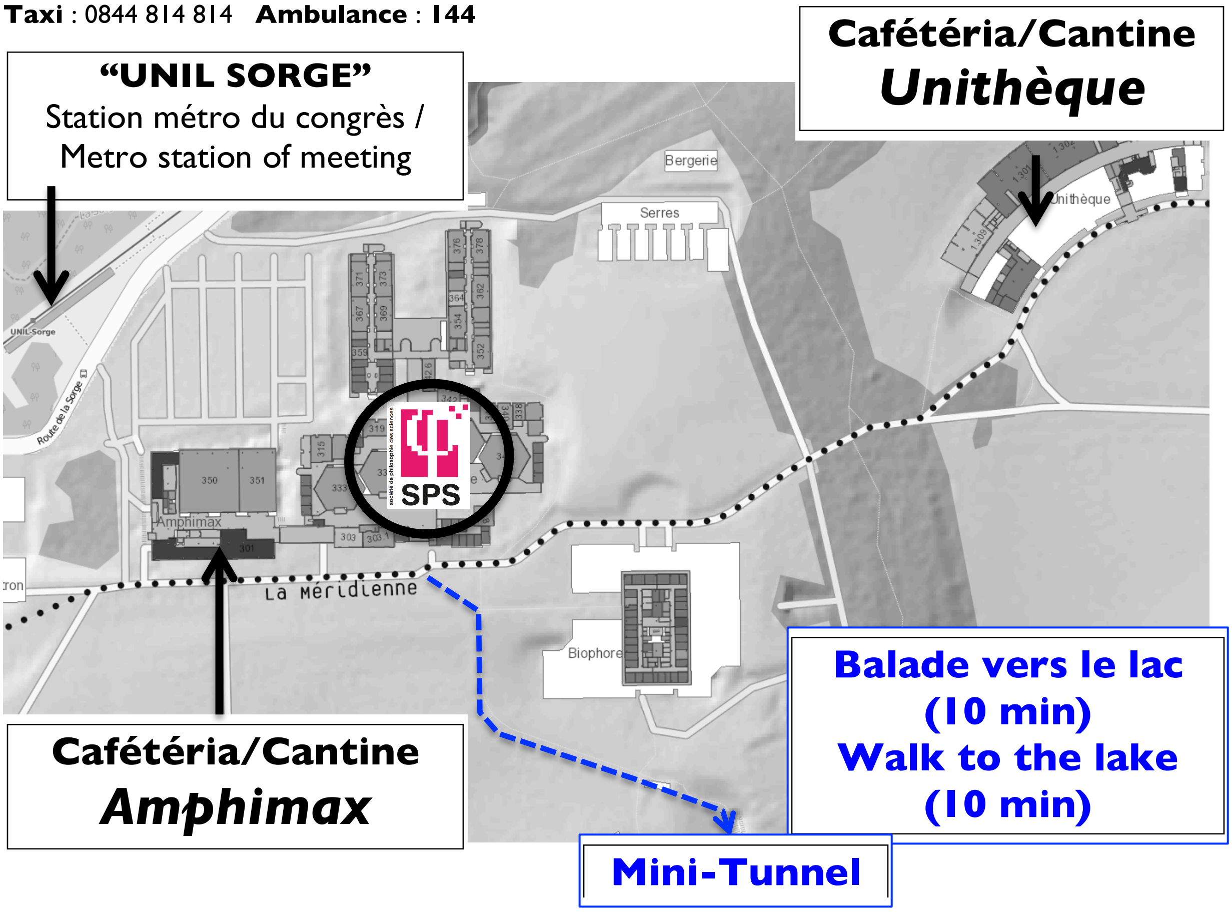
Les **pauses café** se trouvent à côté de l'accueil.

**Vos publications/livres** apportés peuvent être exposés à côté de l'acceuil, fermées à clé pendant la nuit.

**Internet / WIFI** : connectez-vous soit via **eduroam** (si vous avez un accès par votre université) soit via le réseau « **guest-unil** » avec le mot de passe : **SPS62016** (Il y a également plusieurs ordinateurs en libre accès dans le bâtiment).

Le **repas de midi** n'est pas organisé. Il y a une cafétéria/cantine dans les bâtiments **Amphimax** et **Unithèque** (voir sur la carte). Le prix de repas est toujours environ CHF 11 ; à **payer cash** en CHF ou EUR. *Les salles de conférences sont fermées à clé lors des pauses midi et vous pouvez donc y laisser vos affaires ; n'hésitez pas à faire une balade vers le lac (10-12min à pied ; voir la carte).*

Sauf pendant la pause de midi il y a toujours quelqu'un à l'accueil entre 8h30 et 17h, mais au cas où :
**Taxi** : 0844 814 814 **Ambulance** : **144**




The **coffee breaks** take place next to the reception desk.

**Your publications/books** can be put on display next to the reception desk, looked during the night.

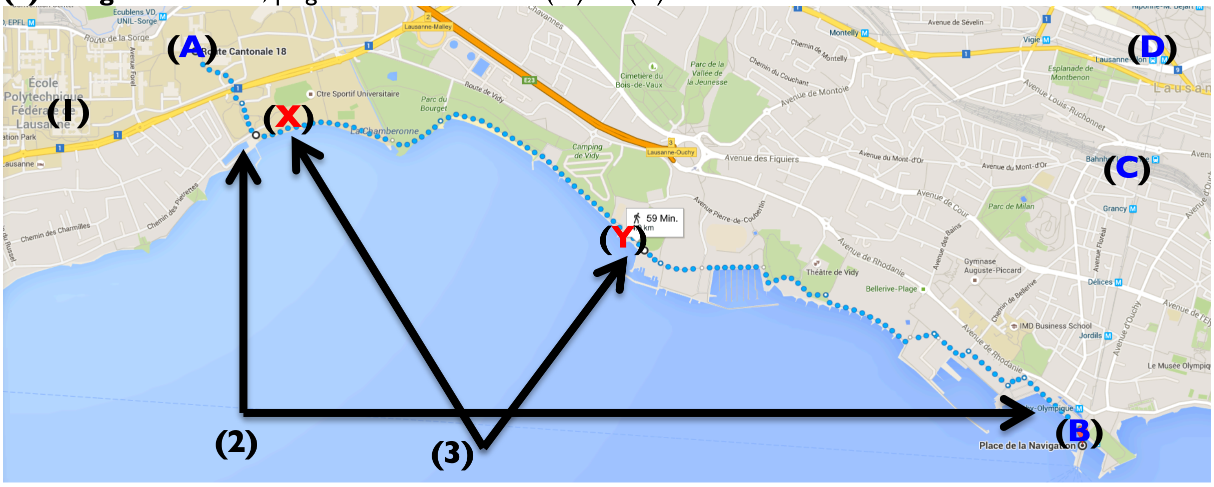
**Internet / WIFI**: connect yourself either by **eduroam** (if you have an access by your university) of by the network named "**guest-unil**" with the password: **SPS62016** (There also are some freely accessible computers in the building).

**Lunch** is not organized. There are cafetarais in the building **Amphimax** and **Unithèque** (see at the map). The price always is around CHF 11; you can only pay **cash** in CHF or EUR. *The conference rooms are locked during the lunch break so that you can leave your belonging there; do not hesitate to take a walk to the lake (10-12 min one-way walk; see map).*

Except during the lunch breaks, there always is someone from 8h30 to 17h at the reception desk, but just in case: **Taxi** : 0844 814 814 **Emergency**: **144**

# « Visiter » Lausanne / “Visit” Lausanne (*Suggestions*)

**(1)** ***Architecture***: ***Rolex Learning Center*** balade de 10 min depuis le congrès (**A**)
**(2)** ***Rentrer au bord du lac***: voir ligne pointillée qui commence au congrès (**A**), au bord du lac pour environ 1h jusqu'au port « **Ouchy** » (**B**) où on peut prendre le métro qui monte jusqu'à la **gare** (**C**) ou le **Flon** (**D**).
**(3)** **Nager** dans le lac, plages notamment entre (**X**) ou (**Y**).




**(1)** ***Architecture***: ***Rolex Learning Center*** 10 min walk from meeting (**A**)
**(2)** ***Walking back at the lake***: voir dotted line starting at the meeting (**A**), continue at the border of the lake for roughly 1h until the harbor « **Ouchy** » (**B**) where you may take the metro going uphil until the train station **gare** (**C**) or **Flon** (**D**).
**(3)** **Swimming** in the lake, beaches notably in between (**X**) ou (**Y**).

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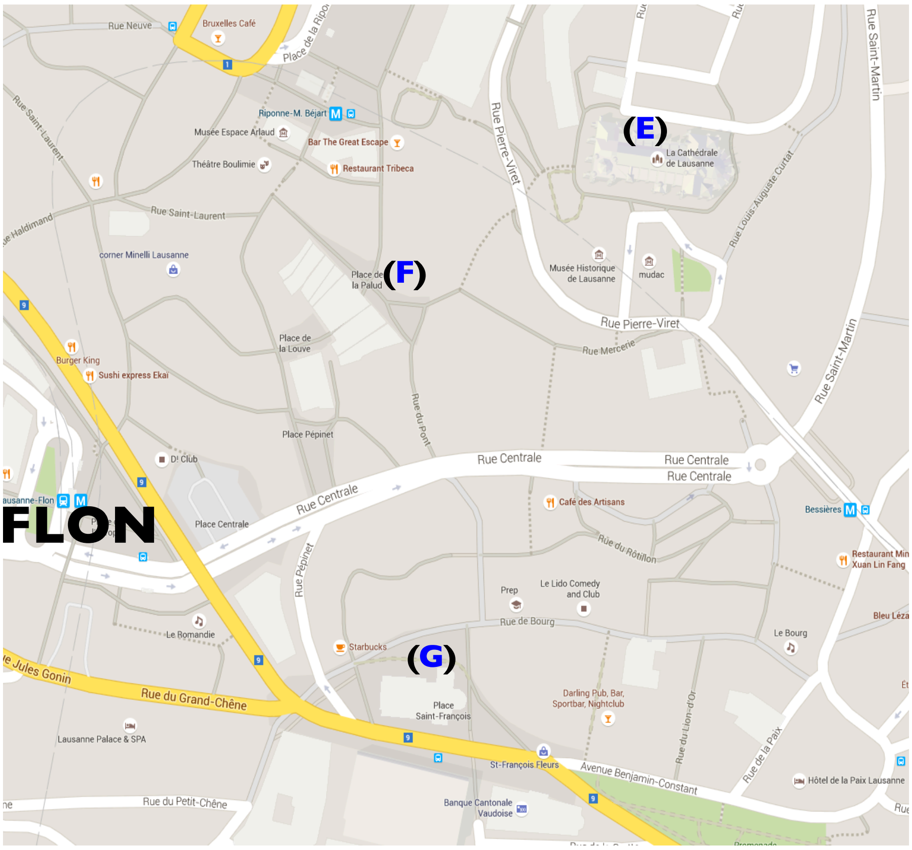
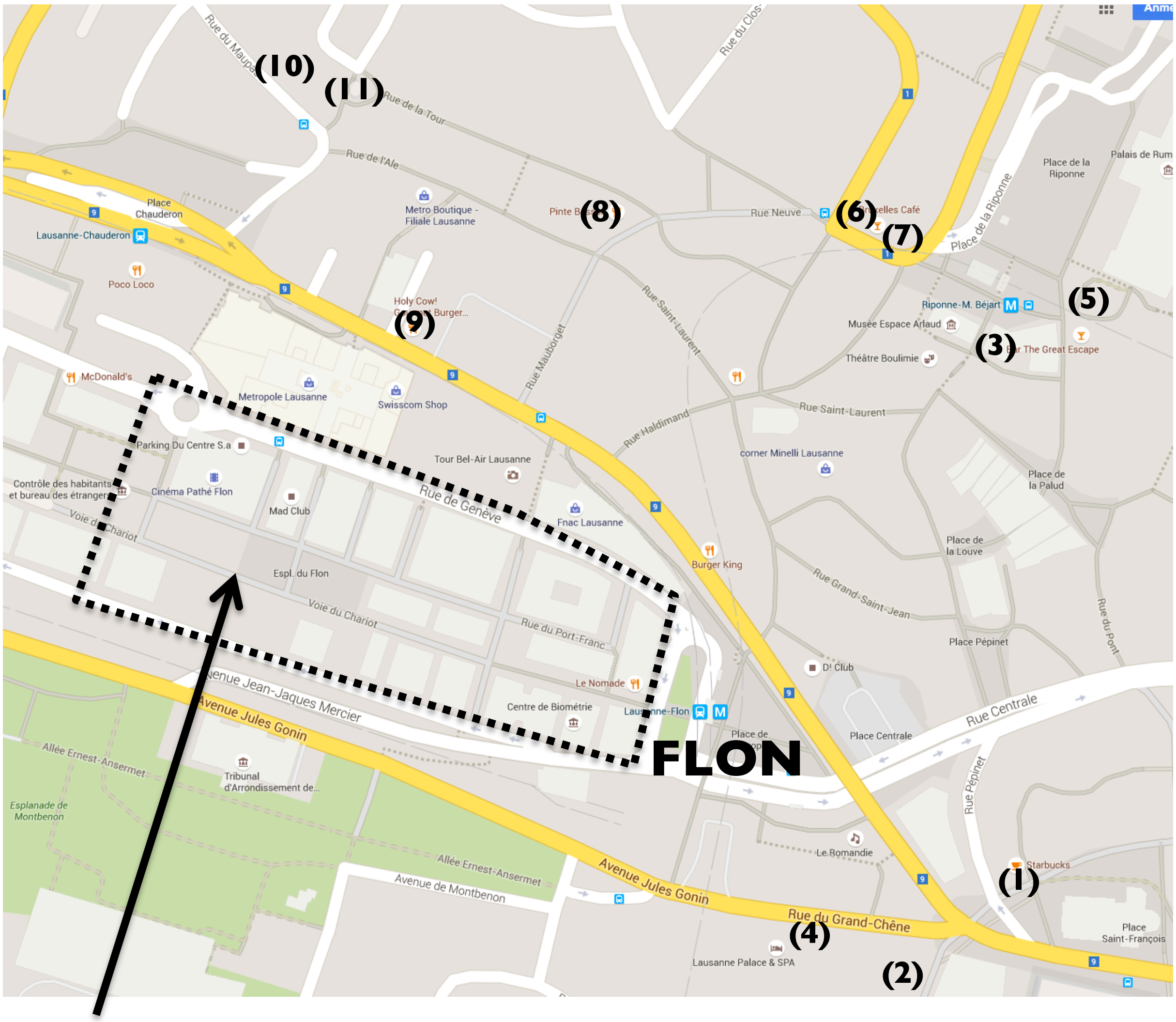
**(4)** Se **balader** dans le joli **centre ville** et dans la **veille ville** de Lausanne, notamment autour de la **cathédrale** (**E**), autour de la ***Place de la Palud*** (**F**) et ***Place Saint-Francois* avec la *Rue de Bourg*** (**G**) ou descendre par métro jusqu'au port (**B**) (voir sur la carte en haut).

**(4)** **Walking** in the lovely **city centre** and in the **old town** of Lausanne, notably around the **cathedral** (**E**), around ***Place de la Palud*** (**F**) and ***Place Saint-Francois* with the street *Rue de Bourg*** (**G**) – or take the metro to the harbor (**B**) (see on the upper map).



## **Restaurants & Drinks** (*Suggestions*)

**(1)** Restaurant, style: "Brasserie&Pinte traditionnel": **Café Romand** (Place Saint-François 2)
Tel: 021 312 63 75 http://www.cafe-romand.ch/
**(2)** Restaurant, style: "Bavarian food & beer": **La Bavaria** (Rue du Petit-Chêne 10)
Tel: 021 323 39 13 http://labavaria.ch/
**(3)** Restaurant, style: "elegant but relaxed": **Tribeca** (Place de la Riponne 4)
Tel: 021 311 11 33 http://www.tribeca-lausanne.ch/
**(4)** Restaurant, style: "Brasserie/brewery chic": **Brasserie du Grand Chêne** (Rue du Grand-Chêne 7)
Tel: 021 331 32 24 http://www.lausanne-palace.com/uk/index.php#la-brasserie-du-grand-chene.php
**(5)** Bar, style: "*English Pub Beer&Burger&Soccer&Terrasse*": **The Great Escape** (Rue de la Madeleine 18)
Tel: 021 312 31 94 http://www.the-great.ch/home.php
**(6)** Bar/Restaurant, style: "*brasserie belge / Belgian brewery*": **Bruxelles Café** (Place de la Riponne 1)
Tel: 021 311 33 01 http://www.brasserie-lausanne.ch/fr
**(7)** Bar/Restaurant, style: "*traditionnel & local*": **Le Vaudois** (Place de la Riponne 1)
Tel: 021 331 22 22 http://www.levaudois.ch/ (***Restaurant for "Social Dinner" 1 Juillet / July 1***)
**(8)** Bar/Restaurent, style: "vieux / old": **Pinte Besson** (Rue de l'Ale 4)
Tel: 021 312 59 69 http://www.pinte-besson.com/en/
**(9)** Burger: **Holy Cow** (Rue des Terreaux 10) http://www.holycow.ch/
**(10)** Bar & cinema, style: "*alternative*": **Zinema** (Rue du Maupas 4) http://www.zinema.ch/
**(11)** Bar, style: "*small & alternative*" **Hydromel** (Rue de la Tour 41) with club "*underground / electro*" (~ 23h – 5h) next to it: **La Ruche** http://www.la-ruche.ch/




**Quartier Flon**, style "*very modern* …" (several bars, cafés, restaurants, cinema, Mad club, galeries …)

# **Restaurants & Drinks** (*Suggestions*)

Déjà sur la page précédénte / Already on the previous page:

**(1)** Restaurant, style: "Brasserie&Pinte traditionnel": **Café Romand** (Place Saint-François 2)
Tel: 021 312 63 75 http://www.cafe-romand.ch/

**(2)** Restaurant, style: "Bavarian food & beer": **La Bavaria** (Rue du Petit-Chêne 10)
Tel: 021 323 39 13 http://labavaria.ch/

**(3)** Restaurant, style: "elegant but relaxed": **Tribeca** (Place de la Riponne 4)
Tel: 021 311 11 33 http://www.tribeca-lausanne.ch/

**(4)** Restaurent, style: "Brasserie/brewery chic": **Brasserie du Grand Chêne** (Rue du Grand-Chêne 7)
Tel: 021 331 32 24 http://www.lausanne-palace.com/uk/index.php#la-brasserie-du-grand-chene.php

**(5)** Bar, style: "*English Pub Beer&Burger&Soccer&Terrasse*": **The Great Escape** (Rue de la Madeleine 18)
Tel: 021 312 31 94 http://www.the-great.ch/home.php

**(6)** Bar/Restaurant, style: "*brasserie belge / Belgian brewery*": **Bruxelles Café** (Place de la Riponne 1)
Tel: 021 311 33 01 http://www.brasserie-lausanne.ch/fr

**(7)** Bar/Restaurant, style: "*traditionnel & local*": **Le Vaudois** (Place de la Riponne 1)
Tel: 021 331 22 22 http://www.levaudois.ch/ (***Restaurant for "Social Dinner" 1 Juillet / July 1***)

**(8)** Restaurant, style: "chic": **La Suite** (Rue du Petit-Chêne 28)
Tel: 021 320 60 30 http://www.lasuite.ch/

**(9)** Pizzeria, style: "très sympa": **Chez Mario** (Rue de Bourg 28)
Tel: 021 323 74 04 http://www.chezmario.ch/

**(10)** Restaurant & Bar, style: "lovely hipster": **Café des Artisans** (Rue Centrale 16)
Tel: 021 311 06 00

**(11)** Restaurant & Bar, style: "Fondu & local": **L'éveché** (Rue Louis-Curtat 4)
Tel: 021 323 93 23 http://www.leveche.ch/public/index.php

**(12)** Bar-Terrasse-Mini-Resto, style: "extremely cool place": **Le Bourg Plage** (située sous l'arche du Pont Bessières / Under the bridge "Pont Bessière") Tel: 021 311 67 53 http://www.le-bourg.ch/bourg-plage/

**(13)** Bar/Restaurant, style: "hipster & excellent tapas": **Café Saint Pierre** (Place Benjamin-Constant 1)
Tel: 021 323 36 36 http://www.cafesaintpierre.ch/ (*bars/restaurants en face / just opposite to*)

**(14)** Bar, style: "eSport": **qwertz** (Rue de la Grotte 3) qwertz.gg

**(15)** Bar, style: "apéro & afterdinner": **Bar Tabac** (Rue Beau-Séjour 7) Tel: 021 312 33 16 http://bartabac.ch

**(16)** Bar/Restaurant, style: "apéro sympa": **P'tit Central** (Rue Central 9) Tel: 021 312 80 75

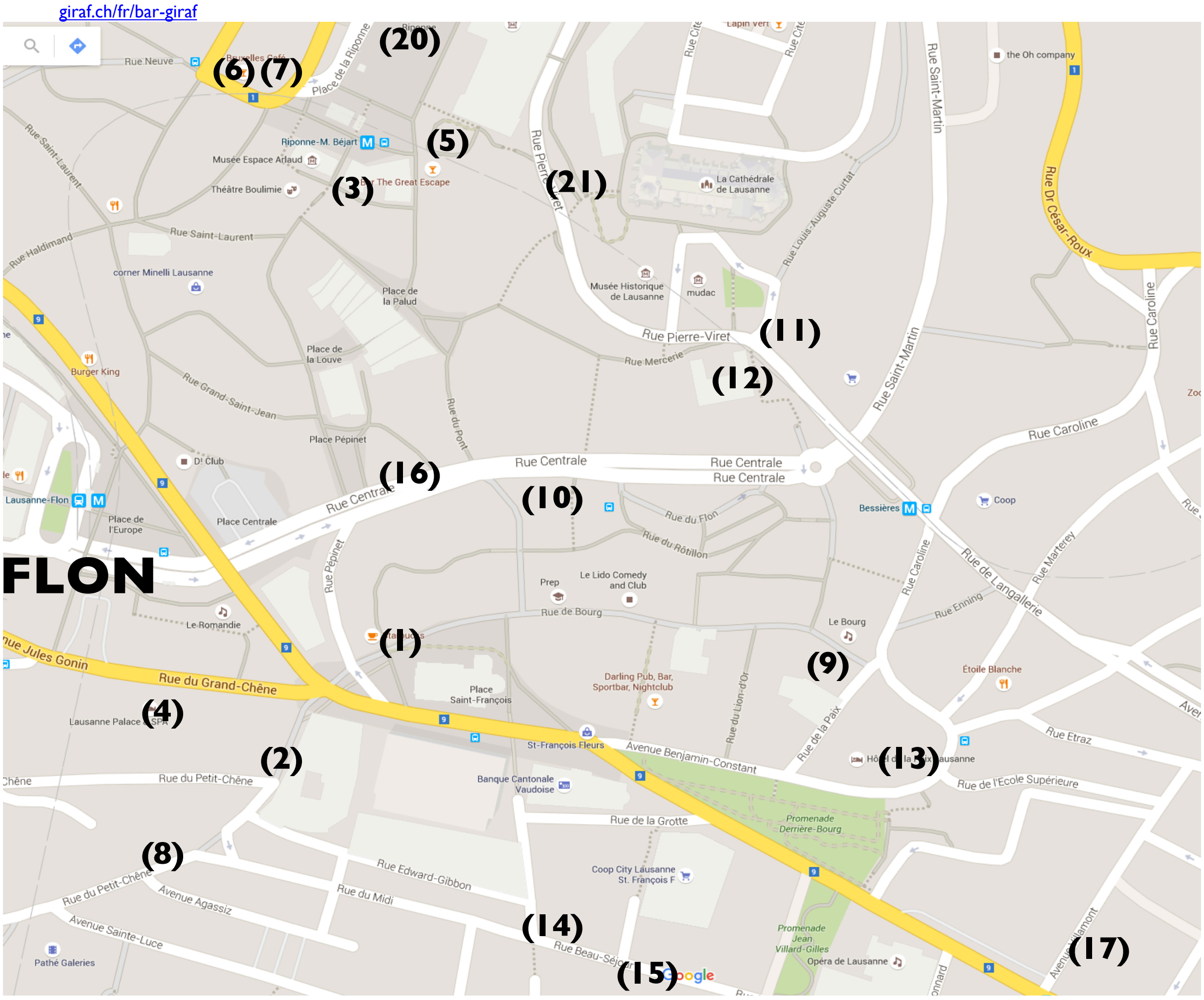
**(17)** Bar, style: "alternative & hidden treseaure": **La cléf** (Avenue Villamont 17) Tel: 021 312 31 28

**(18)** Bar/Restaurant, style: "hipster near the train station": **Café de Grancy** (Av. du Rond-Point 1) Tel: 021 616 86 66 http://www.cafedegrancy.ch

**(19)** Bar/Restaurant, style: "hipster near the train station, with a Balkan/Mediterranean touch": **Café du Simplon** (Rue du Simplon 17) Tel: 021 616 31 04 http://www.cafedusimplon.ch

**(20)** Bar, style: "outside" **La Grenette** (Place de la Riponne) http://lagrenettedelausanne.ch

**(21)** Bar, stlye: "eighties near the cathedral": **Bar Giraf** (Rue Pierre-Viret 6) Tel: 021 323 52 90 http://www.restaurant-vieux-lausanne-bar-giraf.ch/fr/bar-giraf




⇓ **(18 + 19)**

## « Social Dinner » le vendredi / "Social Dinner" on Friday

**Inscription jusqu'au 20 juin !!! / Registration by June 20 !!!**

***Le Vaudois*** (Place de la Riponne, 1) Tel 021 331 22 22 http://www.levaudois.ch/

***Comment y aller*** : Marcher 10 minutes depuis la station métro ***Flon*** (direction nord/montant) … **ou** prendre l'autre métro (métro M2, direction *Croisettes*) depuis ***Flon*** jusqu'à la station ***Riponne***.
***How to get there***: 10 minutes walk from metro station ***Flon*** (north/uphill) … **or** take the other metro (metro M2, direction *Croisettes*) from ***Flon*** to station ***Riponne***.


**Menu**

Tomate et mozzarella à l'huile d'olive et basilic (Tomato & mozzarella at olive oil & basil)

Bœuf braisé au vin rouge et pâtes au beurre (Braised beef with pasta)
*ou / or*
Ravioli farcis aux épinards et ricotta à la sauce tomate maison (Ravioli at spinach, ricotta cheese and tomato sauce)

Salade de fruits (Fruit salad)

Boissons inclus (included drinks) :
- Minérale gazeuse et non gazeuse (1 litre pour 2 personnes)
- Vin rouge ou blanc (3dl par personne)
- Café ou thé

**Merci de payer toutes les boissons supplémentaires !!!**
**Thanks for paying all further drinks !!!**

**Mercredi matinée 29 juin / Wednesday morning June 29**
Salle / Room 348 (auditoire/auditory)

Séance plénière / Plenary session:

*La double visée des chercheurs et le changement dans les sciences*
*(The scientists' dual target and scientific change)*

**Andler, Daniel**, daniel.andler@gmail.com, Université Paris-Sorbonne & Ecole normale supérieure

*Résumé*

Par-delà d'importantes différences, les philosophes des sciences qui s'intéressent (encore) aux caractéristiques générales de la science la conçoivent comme une relation entre deux entités, la Nature et le Scientifique ou la Communauté scientifique, la seconde menant sur la première une enquête empirique, et formant ainsi une représentation qu'elle s'efforce sans cesse d'améliorer. À cette conception « monoscopique » je propose de substituer une conception « biscopique », selon laquelle le Scientifique, ou la Communauté scientifique, mènent une double enquête, l'une portant sur la Nature, l'autre sur le corpus scientifique, à savoir la totalité des traces publiques laissées par les scientifiques. Ce modèle ne se réduit pas, comme on pourrait le penser, à une re-description du premier, et il est mieux à même de rendre compte des formes les plus fréquentes de changement dans les sciences. Les recherches sur la mémoire serviront à illustrer et à défendre le modèle.

*Abstract*

Beyond significant differences, philosophers of science who are (still) interested in general features of science all tend to conceive of it as a relation between two entities, Nature and the Scientist or the Scientific Community: the latter conducts an empirical inquiry on the former, and thus constructs a representation which it constantly attempts to improve. I suggest we reject this "monoscopic" view in favor of a "biscopic" model, in which the Scientist, or the Scientific Community, conducts two inquiries, one bearing on Nature, the other on the scientific corpus, viz. the totality of public traces left by scientists. This second model is no mere re-description of the first, as one might be tempted to think, and it provides a better account of the most frequent forms of scientific change. Memory research will be used to illustrate and defend it.

Symposium:

*Evolution de la vision sur la diversité du vivant*

**Bary, Sophie**, sophie.bary@gmail.com, Muséum National d'Histoire Naturelle
**Barberousse, Anouk**, Anouk.Barberousse@paris-sorbonne.fr, Université Paris 4
**Faugère, Elsa**, elsa.faugere@avignon.inra.fr, INRA, UR 767

Découvrir et décrire la diversité du vivant est une pratique scientifique ancienne qui date de l'Antiquité grecque et qui sera au cœur de l'histoire naturelle des XVIIIème et XIXème siècles. . Aujourd'hui, l'étude scientifique de la biodiversité implique une grande diversité de disciplines qui posent une large diversité de questions: inventaires, délimitation d'écosystèmes, étude des relations au sein des communautés, étude des transferts de matière et d'énergie, étude des dynamiques populationnelles, étude des spéciations, modélisation spatiale. L'étude scientifique de la biodiversité est traversée par un changement majeur en raison de la prise de conscience de l'importance de la biodiversité pour le futur de la planète. Cependant, ce changement est difficile à décrire et à analyser en raison de la multiplicité des éléments pertinents, qui concernent aussi bien l'entreprise scientifique que le contexte économique, social et politique de l'étude de la biodiversité. Le but de ce symposium est de proposer des pistes de réflexion pour mieux comprendre une évolution scientifique de grande importance sociétale, économique et politique. Nous insisterons en particulier sur les moteurs du changement, aussi bien internes qu'externes, sur les tiraillements disciplinaires impliqués, et sur la difficulté qu'il y a à construire des outils méthodologiques adéquats pour analyser ce changement en cours.

L'un des aspects les plus visibles du changement en cours dans l'étude de la biodiversité est l'utilisation du mot lui-même. Introduit en 1986, il a immédiatement été adopté aussi bien à l'intérieur de la communauté scientifique qu'à l'extérieur. Le fait que l'avenir de la biodiversité soit devenu un sujet de préoccupation majeure pour des institutions internationales a contribué à faire de l'étude de la biodiversité un enjeu scientifique majeur. Cependant, l'étude de la biodiversité ne fait pas l'objet d'une pratique scientifique unifiée, puisqu'elle n'est pas prise en charge par une seule discipline et qu'elle correspond donc à différentes représentations scientifiques.

Le contraste entre la connaissance effective de la biodiversité, qui est faible, et l'ampleur de l'enjeu économique, social et politique qu'elle représente, a provoqué des tensions au sein de la communauté des scientifiques. En effet, même si tous les scientifiques estiment pertinent la connaissance de la diversité des taxons, le temps long de leur description apparaît incompatible avec le temps court des actions de conservation. L'engouement qu'a suscité, à raison, l'identification moléculaire des taxons (Barcoding), peut être perçu aussi comme révélateur de ces tensions. Plus qu'un outil complémentaire de l'approche morphologique, sa rapidité a favorisé des propositions d'identification "tout génétique". Afin d'appréhender l'évolution de l'étude scientifique de la biodiversité, nous insisterons sur la mise en place du projet Barcode Of Life et sur ses réquisits épistémologiques.

Nous traiterons aussi de la perception de la crise de la biodiversité par les communautés scientifiques. Enfin, nous chercherons à caractériser la nature des changements associés à cette dynamique : progrès techniques, découvertes, incitation sociétale et économique.

Nous tenterons d'interroger si, au travers de la dynamique de cette vision scientifique et sociétale de la diversité du vivant, des éléments internes à la communauté scientifique peuvent être mis en évidence.

*Qu'est-ce que le projet de Barcoding of Life change à la représentation de la biodiversité ?* (**Anouk Barberrousse**)

Le projet de Barcoding of Life (BoL, Hebert et al., 2003) a pour but de permettre l'association rapide d'un spécimen à un nom d'espèce à partir du séquençage in situ d'un de ses gènes. Il ne s'agit encore que d'un projet, puisque le moyen de séquencer rapidement et sur place le gène qui a été choisi pour mener à bien la tâche d'identification n'existe pas encore ; cependant, la base de données constitutive de ce projet s'est développée rapidement et autorise un premier travail réflexif sur ses effets (http://www.barcodinglife.com/). Ainsi, on peut se demander si le développement du BoL a déjà transformé en profondeur les pratiques de la taxinomie et l'image de la biodiversité que les taxinomistes contribuent à former.

Le but de cet exposé est de décrire, à travers l'étude de la construction des bases de données du BoL, la transformation en cours des rapports entre données et connaissances dans le domaine de la taxinomie. Comme les promoteurs du BoL y insistent, sa mise en oeuvre marque l'entrée de l'étude de la biodiversité dans l'ère des big data. Mais d'autres biologistes soupçonnent cet engouement pour la modernité technologique d'avoir pour effet de détériorer la valeur épistémologique de la connaissance taxinomique qui en découle. Ainsi, selon le slogan d'Ebach et Holdrege (2005), « le Barcoding produit de l'information, pas de la connaissance ». Si c'est le cas, le développement du BoL comme norme d'établissement de connaissances taxinomiques constituera une perte par rapport au développement du travail taxinomique traditionnel. Je chercherai à montrer comment les taxinomistes peuvent s'appuyer sur le passé de leur discipline, et sur leur confrontation ancienne avec de grands ensembles de données, pour garantir que le lien entre données ou information et connaissances ne change pas de nature avec l'utilisation du BoL.

Cette enquête s'appuiera sur une analyse de la production de connaissances taxinomiques dans le cadre traditionnel et dans le cadre du BoL. J'analyserai la structure complexe du passage entre données et établissement ou révision d'une hypothèses taxinomique dans les deux cas. Je montrerai que, si la nature des données change d'un cadre à l'autre, en revanche, le lien entre données et hypothèses taxinomiques n'est pas différent. La principale transformation qui apparaît avec le BoL est que les biologistes sans formation taxinomique peuvent s'emparer plus facilement de ce domaine, puisque la constitution de connaissances biologiques à partir de données génétiques est maintenant une pratique courante dans l'ensemble de la biologie. Cependant, passer par-dessus l'expertise taxinomique n'est pas sans conséquences sur la qualité des connaissances produites. Je défendrai ainsi la thèse selon laquelle le BoL peut avoir deux effets contraires : accélérer le développement de la connaissance taxinomique d'une part, mais également en dégrader la qualité si le lien entre données et hypothèses taxinomiques n'est pas suffisamment contrôlé. Ainsi, deux évolutions différentes peuvent se dessiner pour la connaissance future de la biodiversité, l'une qui s'appuie sur la continuité avec les pratiques taxinomiques traditionnelles, et l'autre, en rupture avec ses pratiques, qui fragilise le lien entre données et connaissances.

*Comment de grandes expéditions naturalistes renouvellent la vision que les scientifiques et les profanes ont de la biodiversité* (**Elsa Faugère**)

Dans le cadre de ce symposium qui s'intéresse à l'évolution de la vision sur la diversité du vivant, je propose une communication centrée sur l'une des plus anciennes disciplines scientifiques en charge de l'étude de cette diversité : la taxonomie. Cette vieille discipline de l'histoire naturelle connaît, depuis une vingtaine d'années des changements majeurs, tant dans ses pratiques et techniques proprement dites que dans ses modalités d'accès aux terrains de collecte. Depuis le milieu des années 1980, la biodiversité est devenue un enjeu sociétal, économique et politique central, notamment dans les rapports entre pays du nord et pays du sud, détenteurs de la plus grande richesse biologique de la planète. Considérée par certains comme l'or vert des pays du sud, nationalisée par la Convention sur la Diversité Biologique de 1992 qui, de patrimoine commun de l'humanité l'a transformée en propriété des Etats souverains, et soumise, selon les biologistes, à une 6ème crise d'extinction, l'accès à la biodiversité se négocie aujourd'hui parfois âprement.

Dans cet exposé, je m'intéresserai à des biologistes français qui ont inventé un nouveau type de grandes expéditions naturalistes dont l'objectif est d'inventorier la biodiversité des pays du sud avant qu'elle ne disparaisse définitivement de la surface de la Terre. Je montrerai comment ces expéditions s'inscrivent dans une ancienne tradition d'expéditions naturalistes et d'exploration de la diversité du vivant tout en renouvelant le genre par l'ouverture que les organisateurs font vis-à-vis d'une pluralité d'acteurs sociaux : mécènes, journalistes, documentaristes, photographes, ethnologues, sociologues, etc.

En parvenant à embarquer des dizaines de participants, professionnels et amateurs, à chaque expédition, en multipliant les missions et zones de collecte dans les pays du sud, en médiatisant leurs découvertes auprès de différents publics, du nord et du sud, les organisateurs de ces grandes expéditions naturalistes participent de manière très active au renouvellement de la perception que les scientifiques mais aussi les profanes ont de la biodiversité.

*Exemple de l'exploration de la faune des profondeurs des océans* (**Sophie Bary**)

Je propose de caractériser les grandes étapes de la vision portée par les scientifiques et par la société sur la diversité de la faune des profondeurs des océans. La connaissance de la biodiversité des profondeurs de la mer est un enjeu scientifique (aujourd'hui encore nous avons exploré moins de 5% des océans) et économique (pour ses potentialités minières et halieutiques). La connaissance de cette faune est récente, elle commence au 19e siècle. Cette biodiversité des profondeurs sera analysée par des communautés scientifiques différentes ce qui fera émergée des représentations différentes pour des raisons techniques et disciplinaires.

Mon analyse s'appuie sur un corpus de documents associé à un programme d'exploration naturaliste du muséum de Paris, Tropical Deep-Sea Benthos, qui a lieu depuis 1976 et qui s'intéresse à la faune des profondeurs depuis 1976. Cette analyse me permet de rendre compte d'une évolution de cette représentation scientifique et de la mettre en perspective par rapport aux autres éléments de connaissance sur ce milieu. Je tenterai donc de caractériser ces différences de représentation et d'interroger ce qui vient d'une évolution interne aux questionnements d'investigation scientifique (e.g. les outils, les méthodes, la composition disciplinaire de la communauté scientifique) et ce qui vient davantage d'une évolution liée au regard sociétal (e.g. sur valorisation d'un écosystème par rapport à un autre pour ses potentiels miniers).

Comme souligné précédemment, l'exploration de la faune des profondeurs est relativement récente. L'hypothèse dominante à ses débuts est qu'il n'y a pas d'êtres vivants au-delà de 600 mètres de profondeur, c'est-à-dire qu'il y a une limite d'existence à la vie liée à l'absence de lumière. Les développements techniques du 20e siècle, comme les submersibles et les robots-sous-marins, ont contribué à révéler une diversité visible du vivant mais on ne comprend toujours pas comment la vie peut émerger d'un milieu si hostile (forte pression, absence de lumière, température faible). Il faudra attendre, dans les années 80, la découverte des sources hydrothermales pour révéler que les fonds marins sont dynamiques, que la faune peut y être abondante, et qu'elle a sa propre production primaire. Les conditions de vie de ces sources sont extrêmes (températures très élevées, présence de métaux lourds) et seront autant de défis techniques pour prélever la faune qui y vit. Ce milieu apparaitra vite prometteur pour son environnement riche en sulfures polymétalliques. A partir de cette découverte, il y aura une communauté qui se focalisera sur l'analyse de la faune autour de ces sources, différente de la communauté naturaliste qui explore la faune autre des profondeurs. Les enjeux de l'exploitation de ces dernières vont avoir une incidence sur la dynamique et les lieux à explorer. Ainsi, des représentations différentes émergeront du milieu des profondeurs des océans.

*La transformation de la notion de subalternation dans la théorie musicale de Marin Mersenne*

**Basilico, Brenda**, brendavbasilico@gmail.com, Université de Lille 3

La question sur la distinction entre différents domaines de connaissances et sur les relations que ceux-ci entretiennent configure l'emploi de la notion de subalternation. Il s'agit de la traduction latine alterum sub altero de l'expression thatéron upo thatéron employée par Aristote dans les Analytiques seconds afin de déterminer la place, le rôle et la méthode des sciences de l'optique et de la musique à l'égard de la géométrie et de l'arithmétique. Le sens qu'Aristote attribue à cette formule constitue la pierre de touche de la discussion autour des schémas de classification disciplinaire et du statut des sciences mathématiques au XVIème siècle qui se prolonge également au XVIIème siècle, ce qui est à l'origine de plusieurs études historiographiques qui, en général, ont une tendance à souligner les continuités entre l'épistémologie scientifique dominante dans le contexte renaissant - d'influence aristotélicienne - et l'émergence d'une nouvelle méthode au XVIIème siècle. La complexité de la notion de subalternation – ainsi que celle inhérente à la catégorie d'aristotélisme - invite à une réflexion sur la façon dont cette conception s'insère et opère, au début du XVIIème siècle dans le plan théorique, en tant que sujet de discussion philosophique d'ordre épistémologique, dans le domaine pratique, dans la résolution des problèmes d'ordre technique et dans les plans politique, institutionnel et religieux dans la mesure où l'organisation de l'enseignement répond inévitablement aux besoins pratiques et techniques et à la nécessité de conserver les dogmes de l'Eglise, surtout dans un contexte de troubles religieux.

Nous proposons ici une étude sur les mutations opérées dans la conception de subalternation de la musique aux mathématiques dans le contexte de l'œuvre philosophique et scientifique du Père Minime Marin Mersenne. Nous croyons que sa recherche musicale conduit à un véritable questionnement du paradigme d'épistéme dominant au début du XVIIème siècle qui soumet la science de la musique aux principes de l'arithmétique ; questionnement qui, nous insistons, n'est pas étranger au contexte politique, religieux et institutionnel mentionné ci-dessus. En effet, nous nous concentrerons, premièrement, sur le rôle des mathématiques subalternes et subalternantes dans la stratégie apologétique du Père Minime contre les ennemis de la religion. Dans ce cadre, Mersenne soutient que la connaissance des vérités mathématiques nécessite, premièrement, une procédure syllogistique qui soit capable de suivre l'ordre des idées éternelles à travers la lumière de la raison, qui fait abstraction de la matière - étant ainsi à l'abri des arguments sceptiques à propos de la connaissance sensible. Mais elle nécessite, deuxièmement - et de manière plus manifeste dans le cas des mathématiques subalternes -, une conformité avec les objets extérieurs . Nous considérons particulièrement important d'observer ici la façon dont les sciences subalternes - et surtout la musique - puissent s'ériger sur des fondements certains dans la mesure où elles fournissent une connaissance sur la réalité physique en empruntant les principes des mathématiques subalternantes ou pures. La musique revête une importance essentielle dans son dédoublement en science et art, en théorie et pratique : d'une part, elle est fondée sur des vérités mathématiques, connues par Dieu et archétypes de la Création ; d'autre part, elle capable de produire des effets sur le tempérament humain et avoir des conséquences sur l'ordre socio politique et religieux. La conception d'une harmonie universelle et ses fondements thélogico-métaphysiques ouvrent la voie d'une recherche musicale capable d'atteindre la certitude et la vérité prétendant aussi d'établir des critères objectifs pour juger la beauté musicale.

Or si les besoins de son apologétique conduisent à mettre l'accent sur les fondements mathématiques de la musique et de l'esthétique musicale, à partir des années 1630, le rapprochement de la musique à la physique devient problématique mais, en même temps, inéluctable. Mersenne se demande si la musique est, en fait, non seulement subalterne à l'arithmétique mais aussi à la géométrie, tout en soulignant la valeur scientifique de la recherche de Johannes Kepler et ses implications cosmologiques au moment de rejeter les élucubrations mystiques et inexactes de Robert Fludd . Mais il se demande également si elle ne partagerait pas cette relation de subalternation avec la physique car son objet matériel est le son, défini par Mersenne comme des « battements de l'air perçus par l'oreille ». En effet, ses recherches à propos de la nature et des propriétés du son se déplacent vers le terrain de la physique, où les principes arithmétiques s'avèrent parfois incapables d'assurer la certitude des démonstrations. Pour montrer ce changement de perspective, nous nous attarderons sur les différentes solutions envisagées par Mersenne aux difficultés techniques touchant l'accord et le tempérament des instruments. Mersenne considère la grande utilité d'adopter un tempérament égal, lequel n'est pas susceptible d'être justifié mathématiquement - étant fondé sur une radix surda et irrationnelle -, mais uniquement et exclusivement sur le phénomène du battement résultant des relations acoustiques qui ne répondent pas à l'exactitude des rapports numériques dont les différences seraient imperceptibles à l'oreille humaine. Les fondements acoustiques de la pratique du tempérament égal mettent en relief les problématiques qui entraine la continuité de la conception de la musique comme subalterne à l'arithmétique. Par conséquent, les mathématiques ne constituent pas la pierre de touche pour régler avec certitude la question du tempérament car la musique est capable de rendre audibles les nombres sourds. La question finale est de savoir la façon dont Mersenne arrive à concilier ce changement de perspective avec les principes théologiques et métaphysiques de son épistémologie et avec le souci apologétique qui est présent dans l'ensemble de son œuvre.

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*Ch-ch-changes: diachronic identity and the immune system as a sensor of change*

*(A tribute to David Bowie)*

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Because the living world is in constant change, a major challenge for biology is to determine how diachronic identity is maintained in living beings, that is, what makes a living being the same through time despite continuous change. Two important questions are at stake: first, what counts as a biological individual (the problem of biological individuality), and second how to follow this changing biological individual through time (the problem of biological identity). Many philosophers of biology have addressed this issue (Hull, 1978; Sober, 2000; Okasha, 2006; Godfrey-Smith, 2009), but almost all of them have done so from the point of view of evolutionary biology. In this talk, I suggest that another biological field, namely immunology, can shed light on the problems of biological individuality and identity, and that the articulation of an immunological approach with approaches based on other biological domains could contribute decisively to the resolution of these problems.

More precisely, I will defend two theses:

1) A scientific thesis, according to which immune systems are, across species, systems that sense the rate of change in the organism. Indeed, recent data suggest that the immune system, far from responding to nonself (Burnet, 1969), or even danger (Matzinger, 2002), detects in fact significant modifications in the host, regardless of their origin. For example, the immune system generally eliminates malignant tumors, despite their endogenous origin; conversely, genetically foreign entities that interact with the immune system in small quantities and in a progressive manner will not trigger an effector immune response. I will show here that, from guard mechanisms in plant's immunity to cases of barrier disruption detection in animals, increasing evidence confirms that the immune system senses changes, through different pathways.

Moreover, several well-characterized examples show that, at least in some cases, the immune system detects specifically the rate of change in a stimulus (its derivative through time). If the discontinuity theory of immunity (Pradeu et al., 2013) is correct, then many immune cells, in particular natural killer cells, macrophages, and T and B lymphocytes (Grossman and Paul, 2015), are activated according to the rate at which their molecular ligands change. Interestingly, many cases of biological systems that detect the rate of change in a stimulus have been described in nature, in particular in the neurosciences and in molecular biology (Casci, 2010). Perhaps, by examining these examples, important insights could be gained about how immune systems are activated in different contexts.

2) A philosophical thesis, according to which the immune system, as a change detection system, plays a crucial role in the delineation of the organism's boundaries and the maintaining of its identity through time. Contrary to what has been suggested by previous frameworks, in particular the self-nonself framework, the immune system can tolerate very important quantities of foreign antigens, which explain, in particular, how the billions of bacteria that live within an organism are not immunologically rejected. More generally, the immune system constantly adapts to a variable environment and contributes to the construction of the ever-changing organism. Overall, then, the immune system can tolerate change, and yet, if the view presented here is correct, it has evolved to detect and eliminate elements that change rapidly, accepting only progressive modifications. I will explain this immunological contribution to the issues of biological individuality and identity, and show how it can be articulated with lessons taken from other biological domains, in particular evolution.

One of the main topics of David Bowie's (1947-2016) music and public life is perpetual metamorphosis. Similarly, as emphasized in particular by Hull (1978), some organisms can undergo metamorphosis or other massive changes while maintaining their individuality. I will conclude by showing that these situations constitute a potential challenge for the claim made here that the immune system constantly monitors sudden modifications, and I will try to respond to this challenge.


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*Ignace Meyerson et la psychologie historique ou la psychologie comme zone de sables mouvants*


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Ignace Meyerson (1888-1983) est un médecin psychologue et philosophe français d'origine polonaise. Neveu de l'épistémologue Emile Meyerson, il le rejoint à Paris à l'âge de 18 ans, en 1906. A Paris, Ignace Meyerson est vite familiarisé avec le cercle de son oncle, cercle qui compte des scientifiques aussi divers que renommés[1] et contexte dans lequel se dessinera sa pensée. Figure méconnue, il est périodiquement oublié puis à nouveau redécouvert. C'est en grande partie à Françoise Parot, historienne de la psychologie, que l'on doit le fait qu'il puisse être connu aujourd'hui du « grand public », puisqu'elle avait fait publier un cours que Meyerson avait donné à l'Ecole Pratique des Hautes Etudes (EPHE) sous le titre *Existe-t-il une nature humaine ?* (Synthélabo, 2000), livre pour lequel elle avait par ailleurs écrit une longue et belle introduction. Françoise Parot a également animé un colloque à l'occasion de l'ouverture des archives Meyerson, dont elle s'est également occupée. On peut retrouver les actes de ce colloque, réunis par Parot, dans le livre *Pour une psychologie historique* (PUF, 1996).

D'autres auteurs se sont intéressés à la figure de Meyerson depuis - des historiens comme Ricardo di Donato, des philosophes comme Frédéric Fruteau de Laclos, Paul Mengal, Jerome Bruner, ou des psychologues comme Brassac ou Pizarroso. Si Meyerson est aujourd'hui méconnu, il a toutefois joué un rôle important dans la psychologie française de l'entre-deux guerres, quand il œuvrait avec Mauss, Durkheim, l'historien Seignobos et le philosophe et psychologue Charles Blondel à établir une science de l'homme et de la société qui étudie l'homme en ce qu'il a de spécifiquement humain et qui ne le découpe pas en rondelles disciplinaires – étudier donc « l'homme total », selon l'expression de Mauss, dans sa réalité concrète. Dans ce sens, en tant que secrétaire du *Journal de psychologie normale et pathologique* et de la Société de Psychologie, Meyerson a fait de ces institutions des lieux de l'interdisciplinarité, voire de la transdisciplinarité. En même temps, Meyerson occupe dès 1923 le poste d'assistant d'Henri Delacroix à la Chaire de Psychologie Générale de la Sorbonne pour la direction de laquelle il postulera plus tard, et il est aussi directeur adjoint du laboratoire de psychophysiologie d'Henri Piéron à l'Institut de Psychologie - récemment créé pour autonomiser la psychologie de la philosophie dont elle dépendait jusqu'alors.

En 1940, Meyerson est révoqué car il est juif. Il part alors pour Toulouse, où il enseigne la psychologie clandestinement et où il entre aussi dans la Résistance, avec l'historien Jean-Pierre Vernant. Sa position concernant la possibilité de la psychologie comme science s'esquisse dès les années 20 à Paris, mais prend sa forme définitive dans la thèse qu'il rédige en 1947 dans le but d'obtenir la Chaire de Psychologie Générale de la Sorbonne. Cependant, cette chaire sera finalement attribuée à Daniel Lagache et Meyerson ne reviendra à Paris que trois ans plus tard, avec l'aide de ses amis historiens Lucien Febvre et Fernand Braudel et de l'helléniste Gernet (di Donato, 1995), quand Febvre et Braudel vont lui proposer un poste au sein de la VIème section de l'EPHE, qu'ils viennent de fonder en 1946 et que Febvre présidait. Mais, une fois revenu à Paris après la guerre,

[1] Entre autres, des scientifiques comme Pierre Curie, des médecins comme Louis Lapicque, des historiens comme Charles Seignobos et des philosophes comme André Lalande, Léon Brunschvicg ou Henri Delacroix (Pizarroso, 2013).

Meyerson est marginalisé par Piéron et par son successeur, Paul Fraisse, qui contribue à l'éclatement de la discipline en créant des diplômes pour des sous-disciplines de la psychologie (Parot, 2000a).

Les auteurs qui s'intéressent à la figure de Meyerson présentent sa thèse sous différents angles, allant d'une perspective critique (Laclos, 2007a ; 2007b) à une perspective de renouvellement de la psychologie (Laclos, 2007a ; Pizarroso, 2008); je vais pour ma part me concentrer sur la place qu'occupe le changement dans la psychologie historique, pour être en accord avec le thème de ce congrès.

Dans sa thèse, appelée *Les fonctions psychologiques et les œuvres* et publiée sous forme de livre aux éditions Vrin en 1948, Meyerson met les bases d'une psychologie historique, objective et comparative : historique, car elle étudie les changements des fonctions psychologiques au cours de l'histoire ; objective, car elle part d'une matière précise, que l'on peut observer et analyser et qui est constituée par les arts, les sciences, les mythes, les religions – ce que Meyerson appelle *les œuvres*, et comparative, car elle tente de discerner des niveaux, et tout spécialement le niveau de base de l'Homme, ce qui le distingue des animaux supérieurs. En ce sens, Meyerson avait travaillé pendant plusieurs années sur l'utilisation de l'instrument chez le singe avec Paul Guillaume, dans le but de dégager de l'étude expérimentale les niveaux d'intelligence que l'homme a su dépasser.

En s'appuyant sur une définition de l'homme qu'il hérite des philosophes comme Cassirer et Dilthey et sensible également à la philosophie de Wilhelm von Humboldt, Meyerson a pour ambition de rendre compte dans la psychologie historique de ce qui est spécifiquement humain – l'esprit. Mais si l'esprit est pour Meyerson ce qui constitue le « spécifiquement humain » il n'en est pas moins difficile à spécifier et l'auteur pose dès l'introduction de sa thèse la question qui l'animera, à savoir quels sont les aspects fonctionnels permanents de l'esprit, qu'est-ce qu'on peut considérer comme son équipement primaire, qu'est-ce qui change. Comme pour Cassirer, l'homme se définit pour Meyerson comme un animal symbolique, un animal « bavard » - comme il l'affirme dans une lettre de 1948 à son ami psychologue suédois David Katz - qui ne vit pas dans l'immédiat, mais toujours dans une relation médiate avec le monde : l'homme est pour Meyerson « le seul animal qui ne soit pas en prise directe avec le réel » (1995 [1948], p. 119) et qui « n'agit que par le moyen d'intermédiaires, de médiateurs, instruments matériels ou instruments mentaux » (*loc. cit.*).

Mais ces médiateurs ne sont pas « fixes ». Selon Ignace Meyerson, la fonction principale de l'esprit est l'objectivation, la capacité de l'homme de mettre en forme le monde qu'il habite, le fait de vivre dans une relation avec le monde médiatisée par les symboles. L'esprit ne cesse de prendre forme, de s'objectiver, de se réaliser dans les œuvres, il n'existe même que parce qu'il s'objective. Mais si les œuvres apparaissent comme des signes qui marquent des opérations de l'esprit, elles sont à considérer également comme des signifiants, car elles participent à l'élaboration de cette pensée même en l'orientant « Et toute œuvre créée devient instrument à son tour » (*Op. cit.,* p. 107).

Pour Meyerson, l'objectivation se définit tout d'abord par l'intentionnalité, le fait que notre pensée est toujours pensée de quelque chose, « une *direction*[2] vers autre chose que le pur état mental ». Si la pensée est intentionnelle, affirme Meyerson, « ce n'est pas de ses propres opérations qu'elle est consciente d'abord, mais de ses produits » (*Op. cit.*, p. 31). L'objectivation se définit alors également comme « une tendance qu'a la pensée à extérioriser ses créations, ou plus exactement à les considérer comme des réalités extérieures ; et dans le cas où cette projection est le plus poussée, affirme Meyerson, l'objet acquiert une véritable indépendance ; on peut le décrire, on peut apprendre indéfiniment de lui » (*loc. cit.*).

Pour Meyerson, « l'esprit projette au-dehors, traite comme des concrets, comme des objets, ses propres productions » (cité par di Donato, 1995, p. 244). Du fait que nos états s'objectivent nous avons tendance à les hypostasier, alors qu'ils deviennent à leur tour des instruments, des instruments qui sont de plus modelables et repensables à partir de l'expérience. Ce qui nous fait penser que l'esprit a une forme immuable, précise et achevée est selon Meyerson la fixité des œuvres, le fait qu'elles représentent ce qui est clair.

Esprit et objet se trouvent pour Meyerson dans une relation d'interdépendance, une relation qui ne laisse pas de place pour la fixité de l'esprit - ni du monde, puisqu'ils se modèlent réciproquement sans cesse. À ce titre, la psychologie historique de Meyerson sera qualifiée d'interactionniste par le philosophe Paul Mengal (1996), car l'œuvre n'est pas seulement construction, elle est aussi interaction : l'homme est à son tour transformé par le résultat de son action sur son milieu, physique comme social.

La conception que Meyerson a de l'esprit est donc marquée par l'idée de changement : « L'histoire nous apprend que l'organisation sociale, les institutions, les faits religieux, les techniques et les sciences, les arts n'ont cessé de changer au cours des siècles. De là un paradoxe, poursuit Meyerson, ou même une contradiction : comment un esprit, toujours le même, a-t-il pu créer des œuvres différentes ? La réponse la plus plausible et la plus sensée qu'on puisse faire : c'est que *l'esprit n'a pas toujours été le même*[3] » (2000 [1975-1976], p. 304). Pour

[2] Souligné par l'auteur.
[3] Souligné par l'auteur.

comprendre l'esprit, il faut selon Meyerson retracer son histoire dans une démarche transdisciplinaire. Les fonctions psychologiques doivent être inférées à partir de l'étude historique et comparative des œuvres, mais rien ne peut nous garantir la validité de ces inférences.

En abordant une conception de l'esprit nouvelle, Meyerson cherche à voir si cela ne change pas l'esprit lui-même : « Nous essayons de ne pas maintenir nos vieilles coutumes [nos pratiques discursives sur les fonctions psychologiques, par exemple], afin de voir si on pourrait ne pas maintenir le monde » (cité par di Donato, 1995, p 228). En renonçant à l'idée d'un esprit stable et immuable et en soutenant le caractère continuellement changeant et discontinu des fonctions psychologiques, et plus largement de l'esprit dans l'histoire, Meyerson fait du psychologique un domaine dans lequel il est difficile de trouver un ancrage, une zone de sables mouvants (Pizarroso, 2008).

Toute théorie psychologique, ou même toute philosophie, devient ainsi relative à une époque, à une culture, et elle n'est donc pas un témoignage sur un univers absolu et abstrait. C'est donc d'une conception historiciste du monde qu'il s'agit ici, qui renvoie à l'idée des conceptions du monde (Weltanschauungen) relatives à un certain esprit du temps. Ainsi, puisque son objet d'étude ne cesse de changer au fil du temps, la psychologie n'est pas une science définitive, mais toujours modelée par la pratique de la vie.

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**Mercredi matinée 29 juin / Wednesday June morning 29**
Salle / Room 315.1

*Will the economic crisis produce a paradigm shift in economics?*

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I wish to suggest that with the growing acceptance, over the last two centuries, of social and political liberalism, economic theory has tried to accommodate itself to that position but, in so doing, has led us to a view that is at odds with what has been happening in many other disciplines. I blame, for this, the relentless insistence on the idea, that if individuals are left, insofar as possible, to their own devices, the economy will self organise into a state that has satisfactory welfare properties. This, I claim is backed neither by empirical evidence nor by theory. It has become an assumption.

At the same time economic theorists were convinced that they were moving in the direction of becoming a science with the same characteristics and standing, as the established sciences such as physics. Direct claims to this were made by Walras and many of his followers, and even Schumpeter the inventor of the idea of "creative destruction" vaunted the merits of equilibrium theory as moving economics forward on the road to a science. Yet the twin facets of the developments of economics, the belief in the self stabilising properties of the economic system, and the efforts to build a scientific model of that system as one in which the actions of self interested individuals would lead to a desirable "equilibrium", were to turn out to be in direct conflict.

I will explain the evolution in economic theory of the idea of the "invisible hand". Then I will explain the failure of modern axiomatic economic theory to show the existence of any such self equilibrating tendency. Without any results to show how an economy will move from a disequilibrium position to an equilibrium, we have concentrated on the properties of equilibria. This is because of the insistence of macroeconomists that explanations of aggregate economic phenomena must be based on "sound micro-foundations". But what theorists have shown is that within that standard framework it is impossible to obtain any results on stability, and as a result equilibrium analysis, as it stands, is no more than an interesting intellectual exercise. Yet, the famous Fundamental Theorems of Welfare Economics which show desirable properties of equilibria, do not have anything to say about how such equilibria are attained. I will also evoke, in this regard, the misrepresentations that are made of these theoretical results and how they have been used to suggest incorrectly, that we have shown the existence of some self organizing property. In standard economic models crises are said to be generated by exogenous shocks and do not come from within the system.

The current crisis has produced a number of calls, particularly from economic policy makers, for a fundamental change in economic theory. I will argue that we should analyse the economy as a complex adaptive system in which it is the interaction between individuals who have simple behavioural rules and who are purposeful but not fully rational in the economic sense that is the key to understanding how the economy evolves. Changing our two hundred year old paradigm to thinking of the economy as a complex adaptive system allows us to consider economies out of equilibrium and the fact that theywill not necessarily converge to an equilibrium with the desirable properties specified in the Fundamental Welfare Theorems. Indeed, they may self organise into states which are far from optimal. Such systems with their feedbacks are unpredictable and policy measures can generate unexpected consequences. However, they do allow us to understand the emergence of crises without recourse to rare and unexplained "exogenous shocks". Accepting this may lead to more realistic and more modest economic theory. Yet there is far from a consensus among economists, despite the current crisis, that there is a need for any such "paradigm shift".

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*On the confirmational relevance of neuroeconomics*

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About ten years ago did emerge the field of neuroeconomics (see Glimcher & Fehr, 2014). Neuroeconomics is commonly viewed as a study of decision making which combines the concepts, methods and tools from three main disciplines: neuroscience, psychology and economics. Since its very beginning, neuroeconomics has given rise to intense methodological discussions involving economists (e.g., Gul & Pesendorfer 2005/2008, Harrison 2008, Bernheim 2009), neuroeconomists (e.g., Camerer, 2007, 2008, 2013; Krabjich & Dean, 2015) and also philosophers of science and/or economics (e.g., Hausman 2008, Economics and Philosophy 2008, Journal of Economic Methodology 2010, Clarke 2014). Several aspects of neuroeconomics have been tackled in these discussions. The main issue at stake is to determine the potential and relevance of neuroscience for economics. 'Relevance" can mean several things. One may wonder whether neuroscience can help to predict economic phenomena (predictive relevance), to measure economics' constructs, to explain economic phenomena (explanatory relevance) or to confirm and disconfirm economic hypothesis and models (confirmational relevance).

This paper deals specifically with the issue of confirmational relevance. The denial of confirmational relevance is one of the major claims put forward by those who are skeptical about the potential of neuroeconomics for economics (the 'neuro-skeptics', for short). By contrast, 'neuro-enthousiasts' argue repeatedly that neuroeconomics is confirmationally relevant for economics. The issue is critical for the whole debate about the relevance and potential of neuroeconomics for economics: if one accepts the confirmational relevance of neuroeconomics, it becomes hard to deny that it could help to improve economics "in its own terms". In this paper, my goal is to determine under which conditions (if at all) evidence coming from the neuroscientific study of decision making can help to assess empirically choice models and assumptions that one finds in economics.

In the first, preliminary, part of the paper, I will introduce to the choice models on which economics relies. I will stress the extant disagreements on their interpretations, and explain how works the dominant methodology whose evidential base is purely behavioral. In the second part, I shall reconstruct as a simple argument the neuro-skeptical challenge to confirmational relevance. It will be shown that the challenge relies on two key premises: one general premise on the nature of confirmational relationships, and a specific one on the features of conventional choice models. From the discussions of these two premises, I will conclude that confirmation relevance faces a true difficulty, namely that choice models severely under-constraint data at the neural level. In the third section, however, I will argue that, in principle, two kinds of confirmational relations could emerge. The first one is "bottom-up" (from the neural level to choice models) and is mediated by neuro-oriented models. The second one is "top-down" (from choice models to the neural level) and relies crucially on auxiliary hypothesis which are necessary to bridge the gap between them. If time permits, in a last section, I will draw a comparison between the issue at stake and analog debates that take place in cognitive psychology about the confirmational relevance of neuroscience (see for example, Hanson & Bunzl, 2010; special issue of Perspectives on Psychological Science, 2013).


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*The logic of scientific discovery in macroeconomics*


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There have been a number of findings in macroeconomics that appear to be worthy of the title of scientific discovery: the Keynesian discovery of the tendency of competitive market economies to cause unemployment; the neoclassical discovery that applications of policy rules cannot cause rational expectations of prices to differ from actual prices; the neo-Keynesian discovery that barriers to price adjustments can cause monetary changes to have real effects etc. What is the logic that these discoveries follow? Theorists find it convenient to describe this logic in Lakatosian terms: Blaug (1976) presents classical and Keynesian economics as scientific research programs (SRPs) and the latter as theoretically and empirically progressive in comparison with the former; Maddock (1984) and Weintraub (1988) characterize neoclassical economics as a theoretically and empirically progressive SRP; and it's only a small step to think of neo-Keynesian economics as an SRP that is theoretically and empirically progressive in comparison with neo-classical economics. But there is a snag in describing the logic of macroeconomic discovery in Lakatosian terms. Empirical progress presupposes empirical corroboration, and it is not so clear whether empirical corroboration is forthcoming in macroeconomics.

Hoover (2001: 1) is right when saying that the ultimate justification of macroeconomics is to provide knowledge on which to base policy. But knowledge on which to base macroeconomic policy requires that there be relations of direct type-level causation between aggregate quantities, and that there be evidence in support of such relations. And the problem is that the evidence that can be provided in support of such relations is too inconclusive in principle to turn causal belief into knowledge. It is too inconclusive in principle because it derives from conditions (I1) – (I4) of Woodward's (2003: 98) definition of the term 'intervention variable', and because in macroeconomics, the probability of the conjunction of these conditions is necessarily smaller than 0.5.

The probability of the conjunction of these conditions is not necessarily smaller than 0.5 in disciplines like physics, the medical sciences or even microeconomics. In macroeconomics, however, it is necessarily smaller than 0.5 because of a pair of conditions that is peculiar to macroeconomics, that is always present in macroeconomics, and that renders the satisfaction of the conjunction of conditions (I1) – (I4) unlikely. The first of these conditions is that the subjects of investigation (a plurality of economic systems monitored during a particular time interval or a plurality of time intervals during which one particular system is monitored) are too diverse and small in number for applications of randomization techniques to lead to equal distributions of nuisance variables. The second condition relates to the presence of variables that potentially type-level cause X or Y and cannot be controlled because they are determined by the decisions of individual firms or agents, given the situations in which they are placed.

If the ultimate justification of macroeconomics is to provide knowledge on which to base policy, if knowledge on which to base policy requires that there be relations of direct type-level causation between aggregate quantities, and if the evidence that can be provided in support of these relations is too inconclusive in principle, then the logic of macroeconomic discovery has to be described in Kuhnian, not Lakatosian terms. The Kuhnian logic is not as different from the Lakatosian one as is sometimes suggested: just as an SRP has a hard core, a protective belt and a progressive and degenerating phase, a Kuhnian paradigm can fall into and emerge from crisis and is characterized by (theoretical or mathematical) principles that are held on to for long, while the empirical statements and auxiliary assumptions at its periphery are more easily given up in the process of normal science activity. It is likewise mistaken to think that the difference between SRPs and paradigms relates to the time span during which an SRP or paradigm comes into being. The decisive difference between SRPs and paradigms is that in the case of the former, scientific discovery is driven by theoretical and empirical progress, while in the case of the latter it is driven only by theoretical progress and theoretical progress by ideology.

The Kuhnian logic of macroeconomic discovery is a descriptive account of the historical development of the macroeconomic discipline and not a normative methodology. One may accordingly wonder whether it is normatively desirable. The fact that macroeconomists adjust or modify the theoretical foundations of the prevalent paradigm only in response to anomalies, and that they often fail to predict real-economy crises or to propose adequate means of mitigating them suggests that the answer is negative: failure to predict real-economy crises or to propose adequate means of mitigating them comes with the high social cost of output declines, high inflation or failed policies. It is of course questionable whether the prediction of real-economy crises is within human power, or whether the occasional occurrence of a severe crisis isn't something that we simply have to put up with. But the paper assumes that macroeconomists can improve their ability to predict real-economy crises and to propose adequate means of mitigating them. And it argues that they can improve that ability by adopting a different methodology: Popper's situational analysis.

Popper's situational analysis has been dismissed as confused (Latsis 1983) or irrelevant to contemporary concerns (Marchionni 2009). And it seems that such dismissals are not entirely unjustified. The paper, however, attempts to provide a consistent interpretation of situational analysis that gives rise to a methodology that might pass for a viable alternative to the Kuhnian logic. It will argue that situational analysis passes for a viable alternative to the Kuhnian logic if a good situational analyst is understood as a researcher who (a) realizes that ideologies might lead to preferences of (Keynesian, neo-classical, neo-Keynesian) models that inadequately capture the situation at hand (and is therefore able to roll back the influence of ideologies as much as possible); (b) can develop tentative models for the situation at hand (and is therefore familiar with adequate models for as many social situations as possible – a familiarity that is acquired through the study of causes that like financial market imperfections, can be ignored in many situations but have been relevant in earlier situations and might become relevant again); and (c) accepts the rationality principle only conditionally (and is therefore protected against anomalies in the shape of patterns of irrational behavior).

The presentation of the paper will first give an outline of the paper as a whole and then concentrate on its central claim, i.e. the claim that in macroeconomics, evidence that can be provided in support of relations of direct type-level causation is in principle too inconclusive to turn causal belief into knowledge. It will, more specifically, analyze three accounts that can be defended as adequate accounts of macroeconomic causality: Hoover's and Woodward's interventionist accounts and the potential outcomes approach that Angrist and Kuersteiner (2011) have introduced into macroeconomics more recently. And it will criticize the inference methods that come along with these accounts: natural experiments in the case of Woodward, and the empirical procedures that Hoover (2001: chs. 8-10) and Angrist and Kuersteiner (2011) propose to test for claims of direct type-level causation in macroeconomics.

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**Mercredi matinée 29 juin / Wednesday morning June 29**
Salle / Room 319

*Change in the decision sciences*

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Science may change by generalizing a model of a phenomenon. A model may adopt idealizations to control for some factors that affect the phenomenon it treats. Relaxing an idealization generalizes the model for cases without the idealization's control of factors affecting the targeted phenomenon.

The decision sciences advance both descriptive and normative accounts of choice. A model in normative decision theory typically adopts idealizations about agents and their decision problems and then formulates standards of rationality for the agents' choices. A common model, using several idealizations, claims that a rational choice maximizes utility.

A preliminary version of the common model assumes that an agent is cognitively ideal, fully informed, and faces a decision problem with a finite, Archimedean set of options to which the agent can assign a precise utility that evaluates an option comprehensively according to the agent's evaluation of the option's world, that is, the possible world that would be realized if the option were realized, a world the model assumes exists. In this preliminary model, an agent's maximizing utility amounts to the agent's maximizing informed utility, that is, adopting an option whose world, as represented by a proposition that for every feature of a world that the agent cares about specifies whether the world has that feature, receives a utility at least as great as the utility of any other option's world.

Normative decision theory advances by relaxing the preliminary model's idealization that the agent is fully informed, while retaining the other idealizations. Instead of assuming that the agent is certain of the world that would be realized if an option were realized, a more general model assumes only that the agent assigns a probability to each world that might be the option's world. Then an agent's maximizing utility is equivalent to the agent's adopting an option such that the expected utility of the option's world is at least as great as the expected utility of any other option's world. That is, the probability-weighted average of the utilities of the possible worlds that might be the option's world is at least as great as the analogous probability-weighted average for any other option. Relaxing the idealization of complete information generalizes the preliminary model so that it accommodates cases in which an agent's information is incomplete as well as cases in which it is complete.

Normative decision theory's standard model supposes that an agent is cognitive ideal so that the agent may effortlessly evaluate each option's world to assign it a utility. Relaxing the idealization that an agent is cognitively ideal is a way to advance. An improved model extends treatment of rational choice to agents who can effortlessly assign an option a utility only if the option's evaluation processes a number of considerations smaller than some limit. In the generalized model, a rational agent, for efficiency, evaluates an option by evaluating, not the option's world, but the part of an option's world that distinguishes it from the worlds of other options. For example, the agent may put aside events in the option's world that occur prior to the agent's current decision problem. These events occur in every option's world and do not distinguish the options' worlds. An efficient, general evaluation of options reviews only events that distinguish the options' worlds.

A model that accommodates limits on effortless processing of considerations to evaluate options needs, first, a way of characterizing the considerations that distinguish options' worlds, second, a type of utility that evaluates just the distinguishing features of an option's world, and, third, a justification of an evaluation of options that treats just the distinguishing features of options' worlds.

The events that distinguish an option's world are those that occur if the option were realized but do not occur given every option. According to a familiar usage of the term "consequences," these events are the option's consequences. An option's consequences in this sense exclude past events and also future events that no option can influence. An option's consequences stand in a type of causal relation to the option, but are not the same as the option's effects, because, for instance, an option is a consequence of itself but is not an effect of itself.

An option's comprehensive utility, a widely used type of utility, evaluates an option by evaluating the option's world. The scope of its evaluation is comprehensive. Its being comprehensive ensures its not omitting any features of an option's world that is relevant to the option's evaluation in a decision problem. An alternative type of utility evaluates an option by evaluating just the distinctive features of the option's world, that is, the option's consequences. I call the type of utility with this narrow evaluative scope "causal utility." In cases of incomplete information, an option's causal utility equals the option's expected causal utility.

According to causal decision theory, which I assume, an option's comprehensive utility is a probability-weighted average of the utilities of the worlds that might be the option's world if the option were realized. For the sake of efficiency, causal decision theory may replace an option's comprehensive utility with the option's causal utility. A justification of the replacement shows that the ranking of options according to their causal utilities is the same as their ranking according to their comprehensive utilities. The demonstration establishes, in the case of complete information, that the comprehensive utility of an option is a sum of the option's causal utility and a certain type of utility of the events in the option's world that are not its consequences. Then it establishes that the second factor is the same for all options, so that an option's rank according to the first factor, that is, its causal utility, is the same as its rank according to the sum of utilities, that is, its comprehensive utility.

Normative decision theory's method of generalizing its models by relaxing their idealizations illustrates a prevalent type of change in science. Initial idealizations simplify a model's treatment of a phenomenon. Succeeding models generalize by relaxing idealizations.

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*Change in science: the case of the development of the atomic theory from Democritus to Dalton*

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In this communication, we present a historical overview of the development of atomic theories, from Democritus to Dalton. Two categories have been established to regroup the conceptions that followed one another: the initial atomism and the empirical atomism. In each of these two categories, several conceptions were developed. Such a review allows the confrontation of the students' conceptions with the atomic aspect of matter and those conceptions developed during history in order to give rise to conceptual conflicts.

Science is the result of a long process of development of which the historical stages are fairly well known. This cannot be said of the process at the starting points of these stages which are indeed subject to multiple interpretations by philosophers and historians of science. While being based on Thomas Kuhn's distinction between normal science and revolutionary science (Kuhn, 1972), today, the image of science is defined in a different way and the interpretation of the scientific work is more nuanced. In the first case, scientific progress is described like a continuous and homogeneous evolution. The work of the "normal" scientist subscribes to "a paradigm that he has especially contributed to specify, to extend and to strengthen. It is not the factor of any scientific revolution. It slips toward the anonymity of acquired truths, and of unshakable certainties in its domain and at his level." (Merleau-Ponty, 1979). In the second case, this evolution advances by sudden sweeps, progress requiring then a radical paradigmatic change, summoning up deep conceptual adjustments: "The true scientific revolutions are much more than new discoveries; they alter the basic concepts of science." (Segre, 1980)

When an epistemological rupture appears, a radically different model of reality calls for "bothersome concepts" and "troubling" ways of thinking. It is an event which the established science endeavors "to fight", generally with ad hoc means of little scientific value. The model that follows the rupture has only in common with the one (or those) that precedes (e) its (their) attempt to describe the same reality: "It is necessary to note immediately that the "atoms" of the ancients are certainly not the same as those of today [...]. The Greek atomists believed that all matter was made of "atoms" and probably thought that all the varied forms of matter could be explained by different configurations (and movements?) of "atoms." What we believe nowadays resembles vaguely to these ideas, but there is certainly an enormous difference between our quantitative theories and the nebulous speculations of the ancient Greeks." (Wichmann, 1971)

This rupture takes place at the level of global science as those of its divisions. It is at the start a very particular problem that will provoke creative action that finally blooms into a new common conceptual frame.

In other words, the rupture takes place at the level of the concepts, principles and methods, when new starting points are discovered and that ancient ones are questioned. In general, these are very particular phenomena, "details", which cannot be integrated or explained in the consensual frame of reference that takes place further ahead and provoke this general upheaval. It is only later that a creative series can take place and continues based on this new approach, but, it can never bring anything that is essentially new. It is in this way that the rupture seems to us to be the main reason for the recorded progress. We are going to develop this thesis calling on the epistemological rupture concept as it has been introduced by the philosopher Gaston Bachelard.

Three parts constitute the body of this communication. They correspond to the various stages of the atomic doctrines developed before the advent of contemporary atomic theory but not necessarily corresponding to homogeneous chronological periods. The scientific discoveries intersect and overlap one another, the birth of a theory not necessarily giving way to the "death" of another. Nevertheless, one can define the stages that brought on models whose assimilation requires deep conceptual changes (Feynman, 1979; Leprince-Ringuet, 1973).

It seems therefore more suitable, for the clarity of our account and for the purpose of our reasoning, to classify the atomic doctrines following their common epistemology, disregarding otherwise the strict chronology of the events. Thus, we identify the doctrines as follows: the initial atomism that is to say that it is in the beginning and then, the empirical atomism. Each of these stages is described under its philosophical, historical and epistemological aspects while taking care to highlight the ruptures that are created in the thought and the methods.

*Conclusion*

This study made it possible to bring out the epistemological ruptures that marked the development of the initial atomism and the empirical atomism. It attempted to show that the atomic models from Democritus to Dalton are in logical discontinuity since the modes of construction of the knowledge, imagined by these thinkers, are not always founded on the same epistemological premises. In addition, this study of different models will allow in a teaching and learning context to identify in the discourse of teachers and students the notion of epistemological rupture in the development of initial atomism and empirical atomism. The notion of epistemological rupture is not part of the conceptual repertory of the majority of teachers of sciences in secondary schools and teachers of physics and chemistry in Quebec colleges. Indeed, a study achieved with the cooperation of these teachers demonstrated the absence of a representation of discontinuity in theories developed during this part of history regarding the particle aspect of matter (Métioui & Trudel, 2012) and the atomic theory (Métioui & Trudel, 2015). This also goes for pre-service elementary and secondary teachers according to whom the scientific theories of today are basically an improvement of the old theories (Métioui & Trudel, 2013; Trudel & Métioui, 2011). This knowledge will also bring out the social dimension of science since science is not solely anchored in a logical-mathematical setting, as it is thought by most teachers in training (Métioui & Trudel, 2013). Finally, it will widen what one could call the scientific culture of teachers and students which is necessary to understand the process by which science goes through in its development.


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*Change in science and changing science*


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There are at least two ways in which one can understand the general idea of "change in science". First, one can construe it as change occurring in the world – e.g. changing things through time – as it is studied by science. Or one can consider it as science itself -its theories, concepts or laws- changing through history. Though both these understandings are not necessarily related to one another, and though there are also usually tied to different kinds of rational inquiry -scientific and philosophical, respectively-, one can wonder whether some "change in science" in the first sense can come to entail a "change in science" in the second sense. To put it differently, one can raise the question whether there would exist some worldly changes that have such a special nature that they inexorably lead to -or impose- a change in the way we do science about them.

In this talk, I will make a case for a positive answer to this question, that is, I will show that there are indeed worldly changes that impose, in order to be properly understood, a changing science. To this aim, I will structure my talk in the following way:

- *First*, I will claim that the technical concept of "emergence" can be appealed to as a theoretical tool to provide a general cartography of the ways in which worldly things can be said to change. More particularly, I will lay down the structure of such a cartography using three dimensions that are usually used to distinguish between different varieties of emergence, namely the epistemological/ontological, synchronic/diachronic and weak/strong distinctions. Because these distinctions cut across each other, they can help compartmentalize the conceptual landscape of emergence -and hence (worldly) change- in eight different (but not necessarily exclusive) regions.

- *Second*, I will argue that the notion of emergence associated with one of these regions is appropriate to support the claim that some worldly changes do impose a changing science. In particular, I will argue that a (currently largely under-explored) form of diachronic, weakly ontological version of emergence can do the job. I will precisely characterize such a form of emergence -following Paul Humphreys' recent proposal (Humphreys, unpublished)- and emphasize its merits as a way of capturing the idea of (worldly) change in an interesting -i.e. nor too liberal nor too radical- way. Furthermore, I will stress the way in which diachronic, weakly ontological (worldly) change does induce strong epistemic effects on the very science one uses to study such (worldly) change.

- *Finally*, I will argue that there actually exist physical systems that experience change in the relevant, diachronic and weakly ontological sense, to the effect that they lead to the advent of a changing science. In support of this contention, I will appeal to some recent works of the Nobel prize winner physicist Robert Laughlin (1999, 2000a, 2000b, 2005), who has relentlessly claimed that the physical changes at play in phenomena like the fractional quantum Hall effect should urge scientists to adopt a new kind of physics

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*Real patterns without underlying stuff*

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A central issue in the ontological debate on quantum mechanics is the question whether the high-dimensional space on which the quantum mechanical wave function is defined (the so-called configuration space) represents the fundamental space in which physical reality unfolds. Whoever wants to give an affirmative answer to that question faces the challenge of recovering our experience of a three-dimensional world from a high-dimensional underlying ontology. In my talk, I will discuss (and sketch a solution to) two problems that affect the two most popular attempts to do this (proposed by David Wallace and David Albert, respectively; see, e.g., Wallace 2012, chapter 2 and Albert 2015, chapter 6).

Despite considerable differences in focus and context, Wallace's and Albert's proposals share the basic idea that the three-dimensional structures with which we are familiar are to be viewed as certain dynamical patterns in the wave function of the universe. A first problem with this view, as pointed out by Alyssa Ney (2015, p. 3118), is that the notion of a pattern seems to presuppose the existence of some underlying stuff, something of which there is a pattern. Indeed, Wallace borrows the relevant concept of a pattern from Daniel Dennett (1991), and all the examples Wallace and Dennett use to illustrate this idea deal with patterns as spatiotemporal arrangements of some kind of matter. But what is required if one starts with nothing but the wave function is a story of how we get matter (situated in ordinary space and time) in the first place, so its existence must not be presupposed in the concept of a pattern.

In response, I first remark that there is no conceptual obstacle to applying the notion of a pattern beyond the realm of spatiotemporally located entities. For example, we readily understand what it means for there to be a pattern in a sequence of natural numbers. What is needed, then, in order to overcome Ney's criticism, is a sufficiently precise way to explicate this extended application of the pattern concept beyond the spatiotemporal realm. I will attempt to demonstrate that the technically precise notion of a pattern developed by James Ladyman and Don Ross (2007, chapter 4) is fit for that task.

In his discussion of Albert's proposal, Peter Lewis (2013, p. 116) emphasizes a second problem for the appeal to patterns in the wave function as an explanation of three-dimensionality. He complains that the patterns on which Albert relies for identifying three-dimensional structures appear only under one arbitrary choice of coordinates, and such patterns are generally regarded as artefacts of that choice rather than facts about the world. This would render the pattern story rather unattractive, at least to those who want to maintain realism about ordinary physical objects.

Like the first problem, this second one can also be traced back to Dennett's (1991) seminal paper, which, despite its title ("Real patterns") has often been interpreted as advocating an instrumentalist (rather than realist) stance towards the patterns. Against this, Ladyman and Ross (2007, chapter 4) argue for a realistic reading of Dennett, and I will apply this realism in order to counter Lewis's critique. By means of a simple mathematical example, I will illustrate how the coordinate-dependence to which Lewis alerts us is compatible with realism. The point is that whether or not a pattern appears to us may depend on the choice of coordinates, but whether or not it is there does not.

The upshot of this discussion is that the alleged obstacles to identifying the objects of our experience with patterns in the wave function can be overcome.

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*Scientific realism, theory change and primitive ontology*

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In this paper I connect the recent debate on the philosophy of quantum mechanics concerning the nature of the wave-function to the historical debate in the philosophy of science regarding scientific realism. Being realist about quantum mechanics is particularly challenging when focusing on the wave-function (see [Albert Ney 2013]). According to the wave-function ontology (WFO) approach, the wave-function is a concrete physical entity1. In contrast, according to the primitive ontology (PO) approach it is not. In this paper, I argue that the PO approach can naturally be interpreted as an instance of the so-called 'explanationism' realism [Kitcher 1993], [Psillos 1999] . If so, the PO approach, unlike the WFO approach, does not fall prey of the pessimistic meta-induction argument (PMI) against realism.

*1-The Pessimistic Meta Induction and Restricted Realism*

Scientific realism is, roughly put, the view that scientific theories give us a (nearly) truthful description of the world. The main argument for scientific realism, the no-miracle argument, takes the empirically success of a theory to be an indication of its truth. In contrast, the PMI argument claims that our current theories are more likely to be false, given that many past theories were successful but turned out to be false. One way to respond to the PMI challenge is to restrict realism, and argue that in theory change some of the components of the old theory get carried over to the new theory. If one can show that the retained components are directly responsible for the empirical success of the theory, then the PMI is blocked. There are various ways to restrict or limit realism, one example of which is 'explanationism' realism. Psillos and Kitcher distinguish between 'presuppositional' and 'working posits' of a theory. Psillos' idea is to focus on the mechanism of empirical success of past theories in order to show that the entities that brought about such success, the working posits, were preserved in theory change, and these are the ones a realist is justified to believe in. The other theoretical constituents are 'idle' components, which make no difference to the theory's success and thus the realist has no need to commit to.

*2-Primitive Ontology, the Classical-to-Quantum Transition and Metaphysical Neutrality*

Scientific realism has often been defended discussing theories other than quantum mechanics. For instance, Psillos and Kitcher discuss the caloric theory and theories of ether, and argue that their success did not depend on the notions caloric and ether (i.e. they are presuppositional posits), and that the entities responsible for the theories' success got carried over to the new theories. Here, I wish to show how the PO theories provide examples of quantum theories with the same (or suitably similar) 'working posits' as classical mechanics. That is, the PO carries over through theory change and it is the sole responsible for theory success. Because of this, assuming that this kind of strategy is successful in defending scientific realism, the PMI argument is blocked: the realist is justified in believing that the PO is real because it is preserved in theory change and does all the work to explain empirical success of theories.

In quantum theories understood within the PO framework, matter is represented by entities in three-dimensional space (or four-dimensional space-time), which are the PO of the theory [Allori 2013]. An example of PO is particles, as in Bohmian mechanics, but one could also have a continuous three-dimensional matter field localized where the macroscopic objects are, like in GRWm. Or one could have discrete spatio-temporal events, like in GRWf. In contrast, the wave-function does not represent matter, but is necessary to implement the law of temporal evolution of the PO. Since the PO of classical mechanics is particles, it is (suitably) preserved in moving to quantum theories: during the classical-to-quantum transition there is a change in the laws of matter, while matter is (fundamentally) always (appropriately) described by 'stuff' in three-dimensional space (or four-dimensional space-time). In addition, it has been argued that the PO alone accounts for the empirical success of both classical and quantum theories, given that the (macroscopic) experimental results are always a function of the (microscopic) PO and never of the wave-function [Allori Goldstein Tumilka Zanghi 2014].

All primitive ontologists (or supporters of suitably related views) maintain that one should be realist about the PO, but they have different ideas about the wave-function, which has been considered, among other things, a law-like object, a disposition, a property, or a new kind of entity. Be that as it may, one can be 'metaphysically neutral' with respect to the wave function: one does not need to postulate the existence of the wave-function in order to account for the success of the theory. In other words, while the PO is a working posit, the wave-function is a presuppositional posit of quantum theories.

*3-A New Argument for the PO Approach*

To summarize, I have shown how the PO approach may naturally be seen as an instance of 'explanationism' realism in which one restricts realism to the PO: since the PO is carried over in theory change, and it is the sole responsible for the theory empirical success, then one is justified in believing it exists. Therefore, the PO approach provides a straightforward strategy to block the PMI in the quantum framework. This provides to the PO approach an important advantage over the alternative WFO approach, according to which the wave-function is a concrete physical field and should be regarded as representative of matter. The wave-function is an object that lives in the highly dimensional configuration space, and as such is a very different entity from classical particles. In this way, there is no continuity of working posits between classical and quantum mechanics, and the strategy to avoid the PMI argument suggested above within the PO framework is not available in this context.

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## *OSR: emergence or micro-physicalism?*

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As Steven French puts it, Ontic Structural Realism (OSR) is motivated by "two sets of problems that 'standard' realism is seen to face. The first has to do with apparent ontological shifts associated with theory change that can be observed throughout the history of science. The second is associated with the implications – again ontological – of modern physics" (French, 2010:90).

We will focus on the second set of problems. Our main goal is to evaluate the Ontic Structural Realism's resources to deal with the contemporary debate about the ontological and epistemological theories of emergence and reductionism. If OSR intends to rehabilitate a realist stance in the contemporary philosophy of science, being faithful to modern physics and its ontological implications, then OSR must be able to address that contemporary debate.

Besides, by exploring this issue we are deepening the analysis of the very ontology of the physical reality, proposed by OSR: structures, relations, relata, as well as their distinct modes of composition. How do relations compose structures, or how are structures formed out from relations and their relata? Supervenience, as it is well known by now, merely states an asymmetric relation of dependence and/or co-variation between a supervenience basis and a supervenient phenomenon, leaving unanswered the reason why such a relation holds.

Hence, what are the qualitative and/or nomological relations between the different levels of composition and structuration of reality, including the macro and micro-levels within Physics itself? Is OSR just another contemporary form of microphysicalism? And if so, does OSR envisage it in an eliminativist or in an epiphenomenalist way?

A part/whole micro-physicalist kind of reduction – the kind of reduction that directly opposes emergence – states that any higher-level systemic property must be explained in terms of (i) the intrinsic properties of the systems' parts taken as isolated systems, (ii) the laws that obtain over those properties, (iii) and some laws of composition that establish how those properties have to be combined in order to account for the properties of the system as a whole (Hüttemann, 2004: 35).

As one of us already defended (Santos, 2015), a naturalistic account of emergence can only defy this model of micro-reductionism if one assumes the possibility of a qualitative change of the system's components in virtue of their own interactions.

But which structure's components could undergo such a qualitative change? Within the ontological framework advocated by the 'thin-objects' version model of a moderate OSR, objects and relations are aids to be only conceptually distinct, since relations are just modes of objects – ways in which objects exist. So there are fundamental objects, "but at least some central ways in which the fundamental physical objects exist are relations so that these objects do not have any existence – and in particular not any identity – independently of the structure they are part of" (Esfeld and Lam, 2010).

Adopting such a view, one can try to argue that a given structure can instantiate a new type of property that is not manifested at the level of the structure's components, that is, the relations and their relata. Then, one can move a step forward, and try to explain the emergence a structure's new property and its micro-irreducibility, by virtue of a qualitative change at the level of those 'thin-objects', as relata of the relations that compose the structure.

Nevertheless, it seems there is one main objection to such an attempt. If there is a fundamental physical level of mereologically simple elements, those elements could never be subject of qualitative changes.

At this point we are addressing another main tenet of OSR. But does OSR really need to be committed to fundamentalism? Moreover, is fundamentalism coherent with the above quoted OSR's initial motivation? From the recent concerns with fundamentalism raised notably by Schaffer (2003), Markosian (2005) and McKenzie (2014), we will try to argue that both from mereological and supervenience relations, OSR faces severe difficulties to sustain the fundamentalist thesis. Indeed, we will argue, non-fundamentalism is not only compatible with the nowadays forms of OSR, but is also coherent with OSR's initial motivations.

We will conclude that if no ontological status is ascribed to objects as relata of relations, and fundamentalism is not challenged, is it clear that emergence has no place in the OSR's view of the world. Still, these are two open questions to OSR.

So, how must OSR address the existence of the different levels of composition and structuration of reality, including the ones within the realm of Physics itself? What kind of theory of explanation and/or reduction can or must OSR endorse? Are the laws of macrophysics just epiphenomenal outcomes of the fundamental laws and properties of the fundamental or more basic microstructures of the quantum realm? Or can there emerge new types of properties and laws, without putting at risk the unity of the world, and the unity of this new scientific structural realism?

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*Les fonctions en chimie*

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Le terme de « fonction » possède une variété de sens spécifiques aux différents domaines scientifiques dans lesquels il est employé. Dans les sciences fondamentales, on peut distinguer trois emplois, en mathématiques, en biologie, et en chimie. Si le concept de fonction mathématique est peu controversé, la fonction biologique fait en revanche l'objet de débats depuis quarante ans, entre approche systémique (Cummins, 1975) et approche étiologique (Wright, 1973 ; Neander, 1991). Les usages mathématique et biologique du terme sont communément considérés comme hétérogènes.

La chimie possède utilise aussi le terme « fonction »; cet usage ne semble pas avoir été jamais en philosophie des sciences. Au sens moderne, une fonction chimique est définie par un « groupe fonctionnel », c'est-à-dire un ensemble défini et structuré d'atomes qui sont le lieu de réactions spécifiques. Malgré son utilisation omniprésente en synthèse chimique, la fonction chimique n'est quasiment jamais définie en elle-même dans les ouvrages d'enseignement. Le concept de fonction semble pourtant porter à débat : des divergences de définition apparaissent à la fois dans son utilisation à travers l'histoire de la chimie et dans son interprétation actuelle.

Afin de mieux comprendre les enjeux du concept de fonction en chimie, nous rappelons d'abord les conditions dans lesquelles la notion de « fonction chimique » est apparue et a été traitée dans la chimie du 19e siècle (notamment dans le contexte de la chimie organique de Berthelot). Puis nous dressons un tableau de l'utilisation du concept aujourd'hui, en nous appuyant sur des ouvrages de référence et sur une enquête réalisée auprès de chimistes. Cette enquête historique nous permet de caractériser plusieurs conceptions de la fonction en chimie, engageant des attitudes différentes quant à la définition de ca concept, l'ontologie qu'elle mobilise, et son utilité. Cette enquête révèle une bipolarisation des conceptions relatives aux fonctions chimiques à plusieurs niveaux de réflexion. Tout d'abord, la définition même de la fonction chimique peut se référer soit à la description d'une structure moléculaire, soit à la réactivité de la molécule considérée. Ces approches, structurelle et dispositionnelle, se répondent en second lieu pour affiner et rendre plus exacte la classification moléculaire

Enfin, deux visions s'opposent quant statut épistémique des fonctions : certains chimistes considèrent la fonction comme une réalité naturelle qui émerge d'une organisation anatomique de la molécule, d'autres utilisent le concept comme un pur outil didactique qui facilite et l'enseignement et la recherche. Nous concluons notre enquête en confrontant le concept de fonction chimique avec les définitions philosophiques des fonctions qui ont été discutées en philosophie des sciences au cours des quatre dernières décennies (depuis les contributions fondatrices de Wright et de Cummins), dans un contexte scientifique ne prenant en compte que la biologie et, dans une certaine mesure, la psychologie. Le concept chimique de fonction ne peut assurément pas entrer dans le cadre des théories étiologiques ou évolutionnistes des fonctions. Nous examinons dans quelle mesure l'approche systémique, qui conçoit les fonctions en termes de "rôle causal" dans un système englobant peuvent accueillir la notion chimique de fonction. Nous en tirons argument pour relativiser les théories étiologiques-évolutionnistes de la fonction, et soutenir qu'elles ne proposent pas tant une définition du concept de fonction qu'une élucidation des raisons pour lesquelles le raisonnement fonctionnel est si important dans les sciences de la vie, et de la forme particulière qu'il y prend.

*Le bricolage évolutif : un modèle pertinent des mutations dans les sciences*

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Le généticien français François Jacob proposait d'expliquer l'évolution des espèces par le jeu des réaménagements moléculaires survenant dans la structure du noyau cellulaire des organismes. La diversité biologique résulte, selon lui, des combinaisons que la nature y réalise, au gré des opportunités, entre des pièces puisées dans un fonds biochimique stable, constitué il y a des millions d'années, à l'aube de la vie sur terre. Ainsi l'évolution n'est pas provoquée par l'apparition de nouveaux matériaux biochimiques, mais par le réinvestissement de pièces anciennes, récupérées et réassociées dans des combinaisons originales.. Ce" bricolage évolutif" montre comment, dans l'évolution, la nature produit le neuf à partir de l'ancien, par une méthode qui, si elle ne lui dénie pas toute initiative créatrice, la place néanmoins dans une situation où elle ne crée rien ex nihilo et ne suit pas un projet prédéfini.

Ce modèle génétique paraît adapté à l'interprétation des révolutions scientifiques, notamment celles provoquées, non par l'émergence de théories nouvelles, mais par la résurgence d'hypothèses connues jadis, puis abandonnées faute de preuves suffisantes. Or tel est le cas des principaux changements qu'ont connus les sciences physiques et biologiques. L'héliocentrisme, promu par Copernic, et l'évolution, ressuscitée par le darwinisme, offrent une illustration de la force révolutionnaire de la résurgence des théories désuètes.

Le retour des théories est toutefois un processus complexe, qui n'est jamais un cycle purement itératif. Il repose sur des mutations conceptuelles majeures nourries aux acquis intermédiaires de la science. Là encore, les modèles biologiques nous parlent: une théorie vaincue et abandonnée ne revient jamais à son état d'innocence initiale. Elle resurgit toujours réinvestie dans de nouveaux atours. Ainsi, la structure .de l'évolution biologique et celle des idées scientifiques montrent la force évolutive des processus mutationnels.

L'histoire des sciences est en effet un processus mutationnel, dans lequel les théories sont constamment interrogées et déstabilisées pour être finalement destituées et remplacées par d'autres tout aussi fragiles et provisoires. L'explication "émergentiste", qui paraît la plus largement partagée par les histories des sciences, souligne que les révolutions sont des moments où apparaissent des théories nouvelles, plus performantes que les précédentes. Elle explique ainsi les progrès scientifiques par le renouvellement du répertoire théorique des disciplines.

A vrai dire, il conviendrait de nuancer la portée du modèle "émergentiste", et de dénoncer comme l'un des plus grands mythes structurants de l'épistémologie historique, l'idée que le neuf recouvre toujours le nouveau. Le progrès est-il toujours porté par une succession de théories inédites, et les théories déchues se trouvent-elles toujours irrémédiablement condamnées? D'illustres historiens comme Comte, Bachelard, Koyré ou Kuhn paraissent décrire les progrès scientifiques comme une succession irréversible de théories de mieux en mieux fondées, et tournée vers un avenir qui, au fur et à mesure qu'il se réalise, relègue définitivement son passé. Or la double hypothèse selon laquelle, d'une part, les progrès scientifiques sont portés par l'émergence de théories inédites et, d'autre part, les processus historiques entraînent la disqualification irréversible des théories déchues, semble radicalement contrariée par les épisodes majeurs de l'histoire des science: les plus grands bouleversements ont été provoqués dans les sciences par la résurgence d'idées jadis vaincues et abandonnées. La révolution héliocentrique et la révolution évolutionniste sont en effet de lointaines réplications d'intuitions pythagoriciennes et épicuriennes.

Il apparaît que la science progresse en recourant parfois à son passé, en réhabilitant ce qu'elle a d'abord rejeté. Elle ne construit pas son avenir sur les ruines du passé, mais trouve plutôt bien souvent dans le fonds hétéroclite de sa mémoire, constitué de vieilles intuitions métaphysiques, des croyances magico-religieuses, de théories scientifiques surannées,, la matière de sa propre reconstruction.

Les théories résurgentes sont des théories revisitées. La science contemporaine ne les conserve qu'en adaptant leurs concepts fondamentaux à ses réalités évolutives.

En réalité, le recyclage des concepts démodés est un phénomène qui n'est pas toujours lié à des épisodes de récurrence théorique. Les théories modernes s'enrichissent constamment de concepts anciens, dont elles corrigent les premières représentations intuitives. On peut dire que les corrections apportées sont parfois si profondes que, sous le même nom, on ne désigne plus le même concept, la même idée scientifique. Mais en gardant les noms anciens d'atome, d'éther, de substance, de planète, d'héliocentrisme, etc., dont le sens actuel est si éloigné de leurs premières représentations, n'a-t-on pas voulu reconnaître l'ascendance des intuitions préscientifiques? Au-delà du nom, il y a immanquablement l'héritage, le lien de filiation, la marque d'une paternité... Les concepts "mutants", qui traversent les épisodes fragmentés de la science, confirment bien l'idée que la science, comme la nature évolutive, fait du neuf par le recyclage de l'ancien. Il y a en effet une similitude frappante entre le bricolage moléculaire effectué par la nature au cours de l'évolution et la réutilisation par la science de pièces conceptuelles  démodées  pour constituer de nouvelles théories.

Nonobstant les limites que comporte toute analogie entre les activités conscientes et rationnelles de l'homme et les processus erratiques et aveugles de la nature, on découvre néanmoins dans la structure des changements en science, la présence des indicateurs majeurs d'un processus mutationnel, tel que l'a décrit François Jacob.

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*L'addiction est-elle une maladie ? Un débat théorique et ses enjeux pratiques.*

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Il y a une controverse autour de la question de l'addiction : est elle une maladie ou une mauvaise habitude ? De coupable, transgressant des normes morales, l'addict devient, dès le 18ème siècle (Rush,1785) un malade soumis à des besoins irrépressibles, le craving, et vivant sous la menace du sevrage (Jellinek,1960). Craving et sevrage étant la conséquence des modifications du cerveau par certaines substances dont la liste s'allonge avec le développement de la pharmacologie.

Le modèle maladie implique 1.des objets aux propriétés pharmacologiques définies 2. la prépondérance du craving et du sevrage, consécutifs aux effets neurobiologiques des objets précédents 3. la chronicité et l'irréversibilité (Leshner,2009).

Or, les points 2 et 3 ont été remis en cause dès la fin du 20ème siècle (Szasz,1974; Peele,1985; Fingarette,1988). La prise en charge de patients présentant les traits caractéristiques de l'addiction mais sans prise de substance (jeux, sport, travail…) est venu, encore, renforcer les doutes sur la pertinence du modèle poussant à l'élargissement de la catégorie à des objets quelconques et débouchant sur une définition relationnelle du phénomène et une multiplication des modèles (West & Brown,2013).

Par ailleurs, le récent DSM V remodèle le phénomène et les critères diagnostics. Il revendique une vision dimensionnelle de l'addiction, avec une facilitation du diagnostic à l'entrée par rapport aux manuels précédents. Sevrage et craving sont mis au même niveau que les troubles existentiels, introduits dès le DSM III-R (et dont l'importance n'a fait que croître dans les manuels), renforçant le point de vue d'une addiction conçue comme conduite, ou expérience. Le nombre de critères réunis par le patient permet de quantifier la sévérité de l'addiction qui peut évoluer, des degrés de « rémission » sont définis qui mettent fin à la chronicité obligatoire. Tous les critères sont sur un pied d'égalité, aucun n'est requis, seul le nombre compte ; l'addiction semble devenir un Spectrum disorder. Enfin  cette conception intègre une addiction comportementale, le jeu pathologique.

Nous montrerons que ce double mouvement, dirigé vers la reconnaissance d'une addiction sans drogue et vers la gradation spectrale n'a pas apaisé la querelle entre les tenants du modèle maladie et ses opposants car elle ne repose pas sur le caractère pathologique de l'addiction, elle est étiologique et à visée pratique.

Contrairement aux apparences, l'opposition ne porte pas sur le point de savoir si l'addiction est une maladie. L'addiction, est bien reconnue comme un trouble au sens de Wakefield, reposant sur une dysfonction préjudiciable (Wakefield, 1980). Il y a dysfonctionnement du système qui gère les comportements orientés vers des buts, mais pour les partisans des deux options c'est le niveau pertinent de description qui diffère ;  le du substrat neurobiologique pour le modèle maladie (maladie du cerveau) alors que ses opposants s'intéressent à des niveaux autres, cognitifs, comportementaux… (maladie de l'addict). Il y a bien préjudice, il n'est pas uniquement dû au contexte normatif comme certains « anti » semblent le penser, même s'il s'inscrit dans un environnement qui a son importance.

Le point d'achoppement porte in fine sur le modèle explicatif et ses conséquences (prise en charge, évolution…). L'article Addiction is a brain disease and it matters de Leshner dans Science en 2009, véritable manifeste en réponse aux thèses « anti », ne porte que sur les addictions pharmacologiques et les formes extrêmes, chroniques, incurables, à partir desquelles les explications neurobiologiques ont été tirées, explications neurobiologiques qui définissent réciproquement l'addiction. Les "anti" réfutent cette restriction et acceptent les addictions « légères », transitoires, aux substances et les addictions comportementales pour lesquels les modèles biologiques manquent.

De plus le DSM V se présente comme un outil neutre sur le plan étiologique alors qu'il ne l'est pas. Il se limite lui aussi essentiellement aux addictions pharmacologiques et pose donc un a priori étiologique qui fige la controverse.

La dispute découle donc, d'abord, de la difficulté de proposer des modèles étiologiques en psychiatrie (Schaffner,2008) et c'est l'insuffisance du modèle étiologique, réductionniste, proposé qui pose problème, imposant un tableau restreint aux cas extrêmes du phénomène, de plus en plus en contradiction avec l'épidémiologie et les pratiques de prise en charge (Peele,1998).

Pour finir, les données actuelles nous semblent aller dans le sens d'une seule addiction, avec ou sans substance. En plus de l'action de l'extérieur, des substances pharmacologiques sur le substrat biologique, une action interne au substrat biologique lui même, liée à sa fonction (évaluation des actions, valeur donnée à l'expérience vécue) est nécessaire ; des événements dont on doit rendre compte du point de vue expérientiel-motivationnel sont des facteurs causaux. Or, si cela permet d'envisager un modèle explicatif multi-niveaux de l'addiction, intégrant point de vue biologique et existentiel, cela éloigne de la perspective d'une réduction stricte, l'explication est multi niveau parce qu'on n'a su à ce jour au moins réduire les dimensions ajoutées. Or, l'autre nœud de la controverse est pratique. L'usage des stratégies de décomposition et de localisation dans la recherche d'une explication du phénomène se heurte aux difficultés inhérentes à l'exercice (Bechtel & Richardson,1993). Le modèle actuel est incomplet, mais familier, l'ouverture qui s'annonce implique de nouveaux outils et pratiques et le risque d'une dissolution de l'addiction qui lui enlèverait limites et spécificité. Les « anti » critiquent le modèle actuel parce que pour eux il ne permet pas de prendre en charge la grande majorité des patients car il repose sur une vision réductionniste étriquée qui ne rendant pas compte de la variabilité des situations, implique des solutions inadaptées (Peele,1998;Valleur &Matysiak,2002;Heymann,2009), et c'est pour cette même raison que les tenants du modèle maladie résistent au changement ; la perspective réductionniste leur donne des explications garantes de l'efficacité de la prise en charge et la complexification pourrait les leur enlever (Leshner,2009).

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*Explorer les limites de la phénoménologie de la médecine*

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La phénoménologie de la médecine est une sous-discipline populaire de la philosophie de la médecine. L'enjeu de la phénoménologie de la médecine est à la fois descriptif et prescriptif : il s'agit pour ses auteurs (Toombs 1987, 1988, 2001 ; Carel 2008, 2011, 2012 ; Svenaeus 2000, 2009) d'une part d'utiliser les ressources conceptuelles de la phénoménologie pour décrire la pratique médicale, et d'autre part de proposer, grâce à ce point de vue phénoménologique, une amélioration de la médecine. Ces approches phénoménologiques se développent au sein d'un mouvement général dans la philosophie de la médecine, qui vise à humaniser la médecine, c'est-à-dire selon leur terme, à dépasser ce qu'elles considèrent comme la crise actuelle de la médecine (Marcum 2008). Selon ces auteurs, le modèle de la médecine occidentale, qu'ils appellent biomédecine ou « médecine scientifique », ne permet pas de prendre en compte le patient dans son individualité et sa subjectivité, déshumanisant ainsi – selon leur terme – la médecine. Par subjectivité, ils entendent l'ensemble des aspects psychologiques, émotionnels et sociaux, c'est-à-dire selon leur vocabulaire, les aspects proprement humains. La médecine actuelle – la biomédecine – présupposerait une thèse réductionniste (désignée par ces auteurs sous le terme imprécis de « naturalisme ») selon laquelle seule l'aspect physique et biologique compte, réduisant ainsi le patient à son corps physique et objectif. Face à cela, la phénoménologie permettrait au contraire de remettre au cœur des préoccupations la subjectivité des patients, notamment selon ces auteurs, grâce au primat donné par la phénoménologie à l'expérience subjective. Havi Carel écrit par exemple que "(…) la phénoménologie – la description de l'expérience vécue – est l'approche la plus utile pour améliorer et prolonger l'approche naturaliste de la maladie (…) L'importance donnée par la phénoménologie à l'expérience du patient et à l'environnement profondément humain de la vie quotidienne, présente une nouvelle approche de la maladie." (Carel 2008 : 10).

La phénoménologie a ainsi deux rôles : d'une part prêter ses concepts à la description de la pratique médicale (ci-dessus par exemple, « l'expérience vécue »), et d'autre part prescrire une nouvelle pratique médicale fondée sur l'apport de la phénoménologie. En effet, selon ces auteurs, les concepts de la phénoménologie permettraient une meilleure description et compréhension de l'expérience de la maladie. Par exemple S.K. Toombs reprend à son compte le concept husserlien de réduction phénoménologique (parfois aussi appelé réduction ou épochè éidétique), qu'elle définit selon les termes suivants : « En exécutant la réduction phénoménologique, l'individu rend explicite l'activité de l'expérience elle-même. Il ne s'occupe plus de l'objet-en-tant-que-tel mais de l'objet-en-tant-qu'il-est-perçu […] l'approche éidétique offre au médecin un moyen d'examiner l'expérience de la maladie et de dépasser le paradigme scientifique traditionnel de la maladie»

(Toombs 197 : 221)

L'approche phénoménologique permettrait au médecin de comprendre l'expérience de son patient – autrement décrite par ces approches comme permettant le développement d'une relation d'empathie – et ainsi d'améliorer la prise en charge des « problèmes existentiels » de celui-ci (Toombs 1987 : 235).

Malgré le consensus apparent autour de ces approches, j'entends ici remettre en question cet usage actuel de la phénoménologie dans la philosophie de la médecine. Comme l'a récemment montré Jonathan Sholl (2015), l'un des premiers problèmes de ces approches se situe au niveau de la critique proposée de la position adverse – ce qu'elles appellent le naturalisme. La définition qui en est donnée reste en effet largement imprécise : elle omet la distinction philosophique usuelle entre naturalisme métaphysique et naturalisme méthodologique, rendant ainsi la cible de ces approches ambiguë (le sujet est-il l'existence de la maladie ou la compréhension de celle-ci ?). En plus d'être imprécis, le naturalisme tel que le comprenne ces auteurs ne se ramène qu'à des thèses réductionnistes nullement défendues ailleurs dans la philosophie de la médecine. Notamment, même Christopher Boorse accepte un naturalisme modéré où se joignent concept pratique et concept théorique de la maladie (Boorse 1975, Giroux 2010). Par ailleurs je montrerai qu'il est réducteur de présupposer que le naturalisme ne peut par définition prendre en compte l'expérience subjective de la maladie – c'est-à-dire les aspects psychologiques, sociaux et émotionnels, selon le vocabulaire propre de ces auteurs. L'objet de ma première partie sera donc de montrer en quoi la critique du naturalisme menée par la phénoménologie de la médecine repose sur la construction d'un épouvantail.

Le second problème de ces approches n'est pas moins important et se caractérise par le manque de rigueur dans l'utilisation de la phénoménologie et de ses concepts : ce sera l'objet de ma deuxième partie. Il s'agira d'abord de montrer que la phénoménologie présentée dans ces approches est souvent « allégée » ou « faible » (selon les adjectifs proposés par T. Gergel 2012) car elle identifie et cantonne la phénoménologie à l'étude de l'expérience subjective (au sens des aspects psychologiques et sociaux). Or cette étude n'est ni une spécificité de la phénoménologie ni son objet principal. Ainsi c'est bien la psychologie qui étudie l'expérience subjective et les états psychologiques, tandis que la phénoménologie rejette depuis Husserl tout psychologisme (nous le verrons, elle ne s'intéresse pas à des états psychologiques mais à des structures de l'expérience). D'autre part, plusieurs concepts phénoménologiques utilisés dans la littérature font l'objet de contresens : c'est notamment le cas pour le concept d'épochè ou de réduction phénoménologique. Comme je le rappellerai, celui-ci appartient à un projet purement philosophique, ce qui le rend difficilement utilisable pour améliorer la pratique de la médecine. Enfin, certains problèmes philosophiques importants ont été occultés au profit d'un usage faussement simple de certains concepts, notamment celui d'empathie. Ainsi Carel et Toombs semblent penser qu'il suffit d'invoquer l'idée d'empathie pour décrire la compréhension du patient par son médecin grâce à la phénoménologie. Pourtant le concept d'empathie dans la phénoménologie est à l'inverse l'expression d'un problème fondamental ignoré par celles-ci : comment appréhender l'autre dans son altérité et sa subjectivé propre ? Si comme le disent Toombs et Carel, la « réalité de la maladie » se trouve dans l'expérience subjective du patient, comment un médecin peut-il la comprendre ? L'empathie est-elle possible ? La phénoménologie, au lieu de servir à prescrire une nouvelle pratique médicale, apparait à la place plus à même de traiter de questions philosophiques fondamentales.

Pour résumer, il s'agira d'étudier dans une première partie la critique du naturalisme menée par la phénoménologie de la médecine, puis dans une seconde, d'étudier la fidélité de ces approches à la phénoménologie et son projet.

Séance plénière / Plenary session:

*Le problème de la contingence / inévitabilité des accomplissements scientifiques :*
*la demande de l'inévitabiliste au contingentiste d'exhiber une alternative scientifique « réelle »*

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*Résumé*

Le problème général dont relèvera la réflexion est celui de la contingence / inévitabilité des accomplissements scientifiques, en particulier de ce qui compte comme *connaissance* scientifique. En première approche, ce problème pointe vers des questions du type : y a-t-il quelque chose d'inévitable dans notre science et dans toute entreprise scientifique digne de ce nom, et si oui quoi, et en quel sens de « inévitable » ? Ou bien certains accomplissements de notre science, en particulier ce que nous tenons pour nos connaissances les plus solidement établies, auraient-ils pu être autres, significativement différents des nôtres, voire inconciliables avec les nôtres ? Une entreprise humaine relevant de la science en un sens suffisamment proche du nôtre, performante dans le même sens et au même degré que notre science, aurait-elle pu légitimement valider des résultats incompatibles avec ce qui a pour nous valeur de résultats scientifiques robustes, de telle sorte que nos résultats devraient être reconnus contingents ?

Ian Hacking peut être désigné comme celui qui, au tournant des années 2000, a le premier encouragé les analystes des sciences à considérer ce problème comme un chapitre autonome de la philosophie des sciences, au même titre que, par exemple, le problème beaucoup mieux identifié et beaucoup plus discuté du réalisme scientifique – le deuxième problème étant lié au premier mais ne s'y réduisant pas. Hacking a élaboré une première conceptualisation du problème de la contingence / inévitabilité, et a introduit le lexique du « contingentisme » et de l'« inévitabilisme » pour repérer les deux pôles antagonistes mis en jeu.

Le but de l'intervention sera de reconstruire et d'évaluer un certain nombre d'arguments paradigmatiques constitutifs du débat entre « contingentistes » et « inévitabilistes ». Le point de départ, et l'épicentre de la discussion, sera une réaction typique de l'inévitabiliste aux affirmations contingentistes. Cette réaction, Ian Hacking l'a saisie de manière imagée et percutante au moyen de l'expression anglaise « put up or shut up ! » (ci-après abrégée comme « PUSU »). En substance, l'inévitabiliste signifie par là au contingentiste que s'en tenir à invoquer, à titre de sciences alternatives incompatibles avec notre science, de simples *possibilités* abstraites, « contrefactuelles » ou « purement logiques », est totalement insuffisant, complètement gratuit, et pour tout dire « nul et non avenu » du point de vue de l'argumentation philosophique. Simultanément, l'inévitabiliste désigne au contingentisme la tâche qu'il devrait réussir à accomplir : le contingentiste devrait exhiber une alternative scientifique *réelle*, actualisée et crédible aux yeux des spécialistes, et non pas se contenter de faire appel à des candidates imaginaires, virtuelles, forgées pour les besoins de la cause par le philosophe, et qu'aucun praticien des sciences n'est prêt à prendre au sérieux. L'inévitabiliste conçoit cette demande adressée au contingentiste comme une légitime exigence d'étayage empirique : comme une épreuve empirique que le contingentiste devrait passer avec succès pour que sa position puisse commencer à être considérée et discutée. L'inévitabiliste a tendance à considérer sa propre position comme la position qu'il est raisonnable d'adopter « par défaut ». La charge de la preuve lui apparait être dans le camp contingentiste, et il érige la demande PUSU en condition préliminaire du débat : tant que le contingentiste n'aura pas réussi à relever le défi PUSU, il se sent facilement dégagé de tout devoir épistémique envers le contingentisme, c'est-à-dire autorisé à ignorer celui-ci et ses prétendus arguments. Sur fond de tels présupposés, l'incapacité des contingentistes à répondre de manière satisfaisante à l'injonction PUSU est communément perçue comme un argument fort contre le contingentisme, peut-être même comme le principal argument dont dispose l'inévitabiliste pour réduire le contingentiste au silence et pour être dispensé jusqu'à nouvel ordre d'avoir à s'en préoccuper.

L'essentiel du propos sera consacré à présenter, analyser et discuter cette demande adressée par l'inévitabiliste au contingentiste, en vue, in fine, d'évaluer la force de l'« argument PUSU ». La présentation prendra la forme d'un dialogue entre deux représentants prototypiques, d'une part de l'inévitabilisme, et d'autre part du contingentisme. Le dialogue portera sur deux tentatives contingentistes pour exhiber une science alternative « réelle », l'une inspirée des écrits de Andrew Pickering sur la physique des particules, l'autre des travaux de James Cushing ayant trait à la mécanique quantique. L'analyse de ce dialogue dégagera des formes générales paradigmatiques d'arguments et de contre-arguments mobilisés dans le problème de la contingence / inévitabilité des accomplissements scientifiques, et identifiera un certain nombre de difficultés constitutives de ce problème qui, dans les échanges entre contingentistes et inévitabilistes, se transforment fréquemment en points de divergences irréductibles entre les deux camps.

Je montrerai que le prétendu « argument PUSU » n'a pas le pouvoir que les inévitabilistes et le « sens commun » sont portés à lui accorder, et soutiendrai que les contingentistes en particulier, mais plus largement la philosophie des sciences en général, ont tout à gagner à reconnaitre que la demande PUSU n'est à l'examen *pas* une épreuve empirique recevable, et une fois cela reconnu, à refuser de faire droit à cette demande. Si l'examen des tentatives contingentistes pour répondre à la demande, ainsi que des réactions inévitabilistes que suscitent ces tentatives, est hautement instructif, en fin de parcours, cet examen met en évidence qu'il est vain pour le contingentiste de s'acharner à poursuivre dans cette voie. En effet, la demande PUSU telle que l'inévitabiliste la conçoit et la précise au fil de l'échange ne *peut pas* être satisfaite. Les sens dans lesquels elle ne peut être satisfaite seront précisés, en rapport avec l'analyse des raisons de cet état de choses. La plus fondamentale de ces raisons réside dans le régime moniste qui caractérise notre science. On peut soutenir que ce régime n'a lui-même rien d'inévitable, et qu'un régime plus pluraliste nous conduirait à voir sous un autre jour l'ensemble du problème de la contingence / inévitabilité en général, et la question de la demande PUSU en particulier. Mais tant que prévaut le monisme, il faut refuser à la demande PUSU le statut que lui assigne l'inévitabiliste, à savoir celui d'une épreuve empirique significative que le contingentisme se devrait de passer avec succès pour que l'inévitabilisme soit détrôné de son statut de position « par défaut » et que ses sympathisants aient alors à s'engager dans le débat. Une perspective philosophique soucieuse d'interroger ce qui va de soi plutôt que de l'assumer de manière non critique ne devrait ni traiter l'inévitabilisme comme la position par défaut, ni présupposer que la charge de la preuve est dans le camp contingentiste, mais bien plutôt s'employer à scruter quels arguments l'« instinct inévitabiliste » est susceptible de mobiliser en sa faveur, et confronter ceux-ci aux arguments contingentistes sans éliminer d'emblée par principe ceux qui impliquent des contrefactuelles.

*Choix rationnel : définir et estimer son "intérêt"*

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Dans la plupart des problèmes épineux de théorie des jeux et de décisions rationnelles, se pose une question du type "qu'ai-je intérêt à faire?". Mais que signifie "avoir intérêt... à choisir l'action a plutôt que l'action b"? Voilà une question dont la réponse ne fait toujours pas consensus au sein de la communauté plurielle des chercheurs sur le sujet (théoriciens des jeux, philosophes, mathématiciens, logiciens...). Le paradoxe de St-Petersburg ou encore le dilemme du prisonnier montrent que la question de "l'intérêt" est bien centrale. Les raisonnements comme: "l'espérance de gain est infinie, donc, quelque soit le montant demandé pour faire une partie, j'ai intérêt à jouer" ou encore "si l'autre me dénonce, j'ai intérêt à le dénoncer et si l'autre ne me dénonce pas, j'ai intérêt à le dénoncer, donc, quelque soit le choix de l'autre, j'ai intérêt à le dénoncer" sont bien connus mais leurs "rationalités" (ou, tout du moins, leurs pertinences) ne font pas l'unanimité.

Le concept d'intérêt est en réalité flou, pluriel. De quel "intérêt" parle-t-on et combien vaut-il (à combien doit-on l'estimer)? Voilà deux questions qu'il convient de résoudre au moment de prendre une décision - de faire un choix rationnel. Pour le paradoxe de St-Petersburg (avec des gains d'un, deux, quatre, huit euros...), si l'on définit l'intérêt à jouer par l'espérance mathématique (généralisée), alors celui-ci est infini. Cela signifie que pour jouer, nous devrions rationnellement être d'accord pour payer n'importe quelle somme : 100 euros, 10000 euros... Peu importe que personne ne soit en réalité disposé à payer de telles sommes pour jouer, là n'est pas la question. L'important est que l'intérêt soit défini et quantifié. Libre alors, à chacun, d'accepter cette définition, sa pertinence et ses conséquences. Si par exemple, on définit l'intérêt à jouer lorsque la médiane des gains est strictement supérieure à la médiane des pertes, alors l'intérêt à jouer sera bien plus faible (ici, moins d'un euro). Cet intérêt sera néanmoins défini et quantifié, que le joueur s'y reconnaisse ou pas.

Dans cette communication, nous revenons sur deux autres paradoxes ouverts.

Les deux portefeuilles (wallet paradox) : le professeur Smith déjeune avec deux étudiants en math.

- Professeur Smith: Laissez-moi vous montrer un nouveau jeu. Posez vos portefeuilles sur la table. Nous allons compter quelle somme a chacun. Celui qui a la plus petite somme gagne tout l'argent de l'autre portefeuille.

- Joe: Hum... Si j'ai plus que Jill, elle gagnera juste ce que j'ai. Mais si elle a plus que moi, je gagnerai plus que ce que j'ai. Aussi, je gagnerai plus que ce que je peux perdre. Le jeu est en ma faveur.

- Jill: Si j'ai plus que Joe, il gagnera juste ce que j'ai. Mais s'il a plus que moi, je gagnerai plus que ce que j'ai. Le jeu est en ma faveur.

Qu'en pensez-vous?

Les deux enveloppes (two-envelope paradox) : vous êtes face à deux enveloppes non-distinguables qui contiennent de l'argent. L'une contient le double de l'autre. Vous choisissez une enveloppe, vous l'ouvrez et trouvez 20 euros. Vous avez la possibilité de partir avec cette enveloppe là ou d'échanger avec l'autre enveloppe.

Vous pensez qu'il y a autant de chances qu'il y ait 10 euros ou 40 euros dans l'autre enveloppe. Comme vous avez plus à gagner qu'à perdre en changeant, vous avez donc intérêt à changer. Pourtant, si vous aviez choisi l'autre enveloppe au départ, partant de la somme contenue dans cette enveloppe, vous auriez conclu à l'intérêt de changer pour prendre finalement l'autre (donc celle en main actuellement).

Alors que devez-vous penser, que devez-vous faire?

Nous défendons l'idée que pour les deux portefeuilles, aucun intérêt n'est quantifiable et que l'indifférence à jouer s'impose. Cependant, dans une variante (où le professeur Smith regarde en secret le contenu des deux portefeuilles, observe que l'un a 40 euros et l'autre 50 et annonce aux deux étudiants que la différence entre les deux sommes est de 10 euros), l'intérêt basé sur l'espérance de gain devient quantifiable (i.e. épistémiquement estimable) pour chaque étudiant et la décision de jouer devient rationnelle. Il en est de même pour les deux enveloppes, dès que l'enveloppe en main est ouverte et le montant connu du joueur, l'intérêt basé sur l'espérance de gain devient quantifiable et décider de changer d'enveloppe devient rationnel.

Plus généralement, le concept sous-jacent est celui de la préférence stricte. Il semble, in fine, que l'action a soit strictement préférée à l'action b si et seulement si il existe une somme S, quantifiable et connue du joueur (à l'instant de sa prise de décision), telle que: Eu(b)+u(S)<Eu(a), où u(.) est une utilité strictement croissante non bornée et Eu(.), l'utilité espérée. Le joueur rationnel doit être alors disposé à payer toute somme strictement inférieure à S pour réaliser l'action a plutôt que l'action b. Pour les deux portefeuilles (dans sa version originale), il est rationnel de croire que l'on a plus à gagner qu'à perdre. Mais l'intérêt à jouer n'est pas quantifiable (i.e. on est incapable de proposer une somme rationalisable à payer pour jouer). Cet intérêt non-quantifiable n'est alors pas suffisant pour rompre l'indifférence à jouer. Ce principe de l'intérêt non-quantifiable s'applique également pour les deux enveloppes, avant l'ouverture de l'enveloppe en main. En revanche, une fois l'enveloppe en main ouverte, on est en mesure de quantifier la somme rationnelle maximale à payer pour changer d'enveloppe. Aussi, l'intérêt est estimable et l'on est en mesure de décider selon cet intérêt. Il ne suffit pas de savoir (ou croire) que l'on a plus à gagner qu'à perdre pour avoir intérêt à faire une action plutôt qu'une autre, il faut être en mesure de quantifier cet intérêt.

Ce qui est particulièrement troublant dans ces deux paradoxes, c'est que la décision semble indépendante de l'information donnée. Dans la variante des deux portefeuilles, la connaissance de la différence des sommes d'argent en jeu est suffisante pour rationaliser la décision de jouer. Dans les deux enveloppes, c'est la connaissance du montant en main qui joue ce même rôle pour changer d'enveloppe. Attention, cette indépendance est uniquement due à l'hypothèse d'une utilité linéaire (par exemple, la fonction identité qui conduit à quantifier son intérêt via l'espérance mathématique de gain). En effet, pour une fonction d'utilité non linéaire, la décision dépendra clairement de la valeur de l'intérêt estimé ainsi que des gains et pertes possibles. Au moment effectif du choix entre deux actions a et b, la quantification de l'intérêt u(S), de Eu(a) et Eu(b), est suffisante pour rationaliser son choix.

Ne serait-il pas nécessaire et suffisant pour qu'un choix soit rationnel que le concept d'intérêt soit défini et quantifié factuellement ?

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*La relativité individuelle de la santé et de la maladie : Canguilhem et la médecine personnalisée*

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Les progrès de la médecine moléculaire, particulièrement dans le cadre du développement des thérapies dites ciblées en cancérologie, conduiraient à une définition plus précise des maladies et à envisager une modification de la nosologie en direction d'une redéfinition moléculaire de certaines maladies. Ceci pourrait conduire à la constitution de classes de maladie de plus en plus rares, à des strates de plus en plus restreintes. Certains vont jusqu'à défendre le « principe de la maladie unique » : maladie et malade ne feraient qu'un. Par ailleurs, dans une approche qui se veut dynamique et plus holiste, la médecine systémique ou médecine des 4P (Préventive, Prédictive, Participative et Personnalisée) propose une personnalisation qui repose sur l'observation de la transition entre santé et maladie pour chaque individu à partir du relevé suivi et répété de nombreuses caractéristiques multidimensionnelles (du génome en passant par l'exposome, le clinicome et des données concernant la qualité de vie).

Ces formes de personnalisation font directement écho aux thèses fortes de Georges Canguilhem pour lequel il n'y a pas de définition objective ou absolue du normal mais seulement une normativité biologique universelle qui, pour chaque individualité organique, prend une forme strictement individuelle. Toutefois, cela le conduisait à affirmer alors l'impossibilité d'une science de la pathologie. Or les approches issues de la médecine personnalisée prétendent à une scientificité de cette individualisation. L'objectif de cet article est d'examiner les formes d'individualisation de la santé et de la maladie qu'induisent les différentes orientations prises (médecine stratifiée et médecine des 4P principalement) par la médecine dite « personnalisée » et de confronter ces conceptions à celle défendue par Canguilhem.

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La question de la liberté de la recherche scientifique s'impose avec force à nos démocraties contemporaines. Les problématiques qu'elle soulève, foisonnantes et multiformes, irriguent et enrichissent le vaste champ d'investigation que constitue l'étude des liens 'science-société'. Par exemple, le versant éthique de la question est abondamment discuté, notamment dans le cadre juridique. Le problème est alors de penser l'articulation d'un droit à la recherche avec le corpus des libertés fondamentales organisant nos sociétés.

Un autre angle d'attaque s'intéresse aux décisions de politique scientifique. Quel degré d'autonomie offrir à la sphère scientifique dans la détermination des grandes orientations de la recherche ? Le problème se pose en particulier si l'on veut chercher à défendre une forme de démocratisation des choix en la matière (Kitcher, 2010). La crainte souvent invoquée est alors la dissolution de l'idéal de science pure et désintéressée dans des exigences pratiques reflétant les besoins immédiats de la société. La science, pour être féconde tant au niveau épistémique que pratique, doit se tenir à l'écart des pressions exercées par le monde socio-économique qui l'environne. A l'appui de cette thèse, on retrouve chez de nombreux auteurs de diverses époques un même type d'argument, que l'on peut nommer argument d'imprévisibilité (Polanyi, 1951 ; Braben, 2008). Sa formulation repose sur une idée simple: on ne peut pas prédire les résultats d'une recherche en cours. Une fois cela admis, toute tentative de programmation, de finalisation, s'écroule d'elle-même sous la force de ce paradoxe indépassable: comment prévoir l'imprévisible ? Orienter la recherche, c'est donc se priver de directions d'investigation qui auraient pu se révéler fructueuses. Une formulation plus technique de l'argument souligne le fait que les grandes découvertes et inventions sont sérendipiennes : elles surgissent là où on ne les attend pas. Dans ce cadre, les solutions à un problème pratique se trouvent certainement dans une recherche qui n'est pas jugée, a priori, utile. Derrière son apparente simplicité, la mobilisation de cet argument pour défendre la liberté de recherche dissimule des hypothèses d'arrière plan rarement explicitées. Notamment, il suppose qu'une science autonome dessine une espace privilégié pour réaliser des découvertes sérendipiennes. De fait, il semble évident que la possibilité de changer librement les directions de l'enquête est ici un facteur clé. La défense de la liberté de recherche fondée sur la notion de sérendipité met l'accent sur cette liberté institutionnelle d'ouvrir de nouvelles voies de recherche. Cependant, en amont, se situe un aspect rarement abordé du problème: comment sont générées les observations inattendues, "sérendipiennes", à même de diversifier les directions prises par l'investigation scientifique ? La question des conditions facilitant la genèse du neuf, du surprenant, est cependant cruciale si l'on veut étudier de manière rigoureuse l'argument d'imprévisibilité, notamment dans sa version centrée sur la sérendipité. La problématique autour de laquelle s'articule cette contribution est donc la suivante: une science finalisée par des exigences pratiques est-elle moins encline à générer des observations inattendues, novatrices, qu'une science librement fondée sur la pure curiosité ?

La première étape de notre analyse se penchera sur l'interprétation souvent caricaturale de la notion de finalisation. L'idée même de contrôle externe par une exigence d'utilité est souvent assimilée à une tentative de réduction de la science à un travail d'ingénierie, consistant à appliquer à la résolution de problèmes pratiques un réservoir de connaissances. En tant que vision alternative de ce que peut être une recherche finalisée par des objectifs pratiques, nous pensons que la notion de science inspirée par l'usage est un outil intéressant (Stokes, 1997). Cette dernière peut être pensée non pas comme programmant des objectifs pratiques, mais comme inscrivant des objets, des phénomènes particuliers dans le champs de la science. Outre le cas archétypal des travaux de Pasteur sur la vaccination, l'étude des effets de certaines molécules sur la croissance tumorale (chimiothérapie) est un bon exemple. Notre problématique revient donc à s'interroger sur la genèse de l'inattendu dans le cadre des science pures et inspirées par l'usage.

Nous nous proposons alors de tracer quelques pistes possibles d'investigation autour de cette problématique nouvellement formulée. L'analyse menée par Carrier (2004), cherchant à présenter certaines caractéristiques méthodologiques de la science orientée par l'application nous servira de fil directeur. Les notions de modèles locaux (local models), d'expérimentation 'en conditions réelles' (real-world experiments) et de relations causales contextualisées (contextualized causal relationships) seront discutées dans le cadre de la notion de sérendipité. Nous les illustrerons à l'aide d'exemples pris dans le champs de la biomédecine, et de la cancérologie en particulier. Nous verrons alors que la science inspirée par l'usage peut être considérée comme un espace privilégié pour le genèse d'observations sérendipiennes. A l'inverse, nous soulignerons l'idée selon laquelle la science 'fondamentale' contemporaine, caractérisée notamment par l'émergence du style de laboratoire (Hacking, 1992), tend à centrer son analyse sur des phénomènes sélectionnés, stabilisés, maîtrisés. Le caractère réglé des expériences qui sont menées dans ce cadre tendent à générer des «résultats plus souvent attendus que surprenants» (Hacking, 1992, p.37).

Cette contribution a donc pour objectif de discuter l'argument d'imprévisibilité en se centrant sur la question de la genèse de la nouveauté. Si une certaine souplesse est nécessaire pour permettre la réorientation d'une recherche, le principe de libre enquête, porté par la seule curiosité des scientifiques, n'optimise pas nécessairement la diversité épistémique pourtant intuitivement associée à la science autonome. Les objets, les phénomènes introduits dans le cadre d'une science dite inspirée par l'usage (donc potentiellement imposés de manière externe) peuvent être une source importante de découvertes sérendipiennes.

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*La philosophie des sciences humaines : La relation réciproque entre la philosophie et la littérature dans l'œuvre du grand philosophe persan Shahâb-od-Din Sohrawardi*

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*Introduction*

La philosophie des sciences humaines permet de découvrir la vérité humaine. Les sciences humaines ont toujours cherché à présenter des méthodes efficaces pour connaître mieux l'homme et ses capacités, tout en présentant les divers aspects de sa vie. Une des branches la plus importante des sciences humaines, c'est la littérature – et cette contribution vis à montrer son rôle pour la philosophie des sciences humaines. Quelle est la philosophie de la littérature? Et comment peut-elle aider à découvrir la vérité humaine?

La notion de philosophie, ainsi que le nombre d'aspects philosophiques contenus dans la littérature a varié d'une période à l'autre. Bien des écrivains et des poètes ont essayé d'exprimer leurs idées philosophiques dans leurs ouvrages. Réciproquement, les genres littéraires sont utilisés par les philosophes pour présenter leur philosophie.

Dans cet article, nous analyserons un cas particulier pour montrer la relation réciproque entre la philosophie et la littérature: Shahâb-od-Din Sohrawardi, grand philosophe iranien du XII$^{e}$ siècle. Nous pourrons voir comment ce philosophe de l'école d'Ishrâq a pu faire connaître sa théosophie aux lecteurs par ses récits allégoriques. Notre méthode pour cette étude sera une méthode référentielle et analytique. Ainsi, grâce aux ouvrages écrits sur les ouvrages philosophiques de Sohrawardi, nous analyserons comment ce grand philosophe s'est servi du récit allégorique pour exprimer sa théosophie ou comment il a pu réaliser l'un des objectifs de la littérature dans ses récits philosophiques.

*1. La relation réciproque entre la philosophie et la littérature*

La littérature, «c'est l'ensemble des ouvrages où les meilleures pensées sont exprimées dans les meilleures formes.» (Zarrinkoub, 1369: 6). Dans cette définition, Zarrinkoup a considéré à la fois le «contenu» et la «forme» des ouvrages littéraires. (Akbari Biraq et Asadiyân, 1390: 4). D'après une autre définition, tirée du *Dictionnaire de Robert*, la littérature, ce sont «les œuvres écrites, dans la mesure où elles portent la marque de préoccupations esthétiques.» (Rey, 1988: 744). Ce terme nous rappelle les noms de Racine, Voltaire, Hâfez, Sa'di et leurs ouvrages. En fait, la littérature comprend toutes les œuvres dans lesquelles les problèmes sociaux et toutes les idées d'une période historique sont peints d'une façon esthétique et grâce aux figures du style. Alors, dans les ouvrages littéraires, la philosophie et les pensées de l'écrivain s'expriment sous une belle forme. Ainsi, parfois, le but de la littérature, c'est l'expression des pensées philosophiques de l'auteur. D'où, la relation entre la littérature et la philosophie.

La philosophie est «une science qui répond aux questions fondamentales de l'homme et une recherche pour trouver la vérité.» (Akbari Biraq et Asadiyân, 1390: 4). Ce terme nous rappelle les noms de Socrate et Aristote ainsi que leurs ouvrages. Selon la définition de Robert, la philosophie, c'est «1- l'ensemble des questions que l'être humain peut se poser sur lui-même et examen des réponses qu'il peut y apporter; vision systématique et générale du monde […] 2- systèmes d'idées qui cherchent à établir les fondements d'une science.» (Rey, 1988: 941). Dans le second cas, nous pouvons citer la philosophie de la littérature dont il est question dans cet article.

La philosophie de la littérature étudie les thèmes et les procédés de la littérature et les concepts philosophiques se trouvent aussi parmi les thèmes littéraires. D'ailleurs, parfois, les philosophes ont recours à la littérature et aux genres littéraires pour exprimer leurs pensées philosophiques.

Ainsi, il y a toujours eu une relation réciproque entre la littérature et la philosophie. Pourtant, Platon les séparait. Selon lui, «le monde est divisé en deux parties: le monde sensible et le monde raisonnable. La littérature appartient au premier et la philosophie, au second.» (Aflâtoun, 1374, Livres II et III). Dans le livre X de *La République*, il oppose les poètes aux philosophes. Mais, il a recours aux formes littéraires pour exprimer ses plus importantes idées philosophiques dans cet ouvrage. En fait, tous les dialogues de cet ouvrage sont écrits dans la forme littéraire de «la dialectique». (Akbari Biraq et Asadiyân, 1390: 9) Cet ouvrage révèle ainsi la relation étroite entre la philosophie et la littérature.

Une des raisons de cette relation étroite entre ces deux sciences, c'est l'usage du langage métaphorique par tous les deux. En fait, «la philosophie est une forme de la littérature et le langage métaphorique lie l'une à l'autre. Car, tous les deux sont construits d'une même matière: «les mots»» (*Ibid*.: 12).

La métaphore est une figure du style littéraire qui consiste «à désigner un objet ou une idée par un terme convenant à un autre objet ou une autre idée en raison d'une ressemblance perçue par l'esprit.» (Eterstein, 1998: 274). En fait, «la métaphore recourt à l'imagination, c'est-à-dire, à la capacité de saisir des analogies.» (*Ibid*.: 275). Elle a pour effet de substituer une connaissance symbolique à la connaissance rationnelle du monde (*Ibid*.) et elle permet «de mieux faire comprendre une réalité abstraite en l'assimilant à une réalité concrète.» (*Ibid*.). C'est ainsi qu'elle est utilisée par le philosophe pour concrétiser ses idées abstraites.

Selon Daryouch Achouri, Nietzche est le premier qui ait connu l'aspect métaphorique de la langue et qui ait souligné l'importance du style et sa relation structurelle avec le sens: «Il a montré dans quelle mesure, la métaphore, qui semble appartenir au langage poétique, joue un grand rôle dans le langage apparemment solide de la philosophie, qui veut être le pur sens. Comprenant cette vérité, il a pu effacer la frontière apparemment impassable entre le langage philosophique et le langage poétique et dans son ouvrage intitulé *«Ainsi dit Zoroastre»*, il a exprimé ses idées et ses pensées philosophiques par recours à un langage poétique.» (Achouri, 1388; cité dans Akbari Biraq et Asadiyân, 1390: 9-10).

En effet, d'une part, les philosophes utilisent des images, des métaphores, des symboles et même des genres littéraires pour exprimer leur philosophie et d'autre part, dans tout ouvrage littéraire, on trouve une pensée, donc, une philosophie. (*Ibid*.: 13). Réciproquement, la littérature devient ainsi l'outil de la philosophie et la philosophie, l'outil de la littérature.

De nombreux ouvrages littéraires contiennent des notions philosophiques. Du côté de la littérature russe, nous pouvons citer les romans de Dostoïevski et Tolstoï. «Dans la littérature persane, c'est Hâfez qui traite des sujets philosophiques à travers son *Divân*» (Khorramchahi, 1372, V.1: 30). En fait, dans leurs poèmes, les poètes classiques persans traitaient fréquemment des thèmes philosophiques et théologiques comme le déterminisme et l'autorité, le destin etc. (Yasrébi, 1370).

De cette manière, la littérature devient un outil pour le philosophe afin qu'il puisse transmettre ses concepts philosophiques au lecteur. En France, Sartre, dans *La Nausée*, et Camus, dans la plupart de ses pièces de théâtre, éliminent les frontières de la philosophie et de la littérature. (Ahmadi, 1383: 532). En fait, la littérature est considérée en tant que l'outil du philosophe pour exprimer ses concepts philosophiques. Ainsi, le philosophe utilise le langage littéraire et les figures du style pour exprimer ses idées philosophiques dans une forme littéraire. Mais, la beauté de la prose d'un philosophe n'ajoute rien aux valeurs philosophiques de ses idées. En fait, les philosophes utilisent les diverses figures du style pour exprimer leur philosophie, mais l'usage de ces figures du style ne modifie pas le contenu de leur pensée. Mais, dans la littérature, la forme influe sur le contenu. (Akbari Biraq et Asadiyân, 1390: 12-13).

Donc, «la littérature est l'outil de la philosophie» veut dire que le philosophe utilise le langage et les formes littéraires pour exprimer sa philosophie et «la philosophie est l'outil de la littérature» veut dire que les pensées et les concepts philosophiques peuvent être traités en tant que des sujets d'un ouvrage littéraire. En fait, l'un des objectifs de la littérature, c'est l'expression des concepts concernant l'existence humaine qui sont traités dans la philosophie. Même, parfois les auteurs utilisent les termes de la philosophie dans leurs ouvrages littéraires. Mais, il faut faire attention que la manière de l'interprétation d'un écrivain d'un sujet philosophique est tout à fait différente de la manière d'interprétation d'un philosophe du même sujet et même, nous pouvons dire que le langage littéraire d'un écrivain influence sa philosophie et que la littérature, c'est une sorte de la connaissance de l'existence qui a son propre discours.

Cette relation entre la littérature et la philosophie est aussi observée chez les philosophes islamiques. Ainsi, Sohrawardi (1155-1191), le grand philosophe islamique, qui fait l'objet de cet article, a-t-il utilisé l'allégorie pour exprimer sa théosophie d'*Ishrâq* dans *L'Archange empourpré*. Dans la partie suivante, nous allons voir comment il a pu transférer sa théosophie aux lecteurs à travers ce récit allégorique.

### *2. La philosophie et la littérature: Sohrawardi et sa théosophie d'Ishrâq*

Sohrawardi, surnommé le «Sheikh de l'Ishrâq», né en 1155 à Sohraward en Iran et mort en 1191 à Alep en Syrie, est le fondateur de la théosophie d'Ishrâq. Son ouvrage principal s'intitule *Hikmat-al-Ishrâq* (*La théosophie orientale*).

«Le mot *Ishrâq* signifie illuminer. Dans le langage théosophique, *Ishrâq* signifie «dévoilement intuitif» ou l'«illumination de l'âme par les intelligences». Dans la théosophie de l'Ishrâq, cette «illumination» et ce «dévoilement intuitif» des vérités du monde ne sont possible que dans le cadre d'une purification intérieure constante de son âme.» (Hedjazi, 2010) Cette théosophie est basée sur les concepts de la raison, de la lumière et de l'intuition.

D'après la définition du *Dictionnaire de Robert*, l'intuition est une «forme de connaissance immédiate qui ne recourt pas au raisonnement.» (Rey, 1988: 696). Dans la théosophie de l'Ishrâq, l'intuition joue un rôle très important. Pourtant, cette théosophie est basée à la fois sur la compréhension intuitive et l'argumentation logique de la vérité du monde. L'intuition joue également un rôle important dans la littérature. Elle y est opposée à la raison. Comme celle-ci, elle est l'un des modes d'accès au réel que propose la littérature et nous savons que les poètes accèdent aux vérités du monde grâce à l'intuition.

Sohrawardi a écrit beaucoup d'ouvrages dans lesquels il pose sa théosophie d'une façon allégorique. Parmi ses ouvrages persans, nous pouvons citer *L'Archange empourpré*. Il y a utilisé l'allégorie pour exprimer sa théosophie. Dans une partie de cet ouvrage, il utilise les récits du *Livre des Rois* de Ferdowsi pour concrétiser sa théosophie. Il y relate le récit de Zâl, le père de Rostam, héros épique iranien, qui était laissé dans la montagne symbolique de Qâf. Là, il a écrit: «Qâf est le nom d'une montagne au sommet de laquelle Simorq, le symbole de la lumière divine, réside. Zâl, qui fut né avec des cheveux blancs, et, qui symbolise la raison et la pureté, fut laissé au pied de cette montagne … » (Chafi'ï, 1394). Ainsi, il a pu exprimer sa théosophie grâce aux symboles qui se trouvent dans la littérature persane et réaliser l'un des objectifs de la philosophie de la littérature qui est l'expression des sujets philosophiques à l'aide des symboles, des images et des métaphores.

*Conclusion :* Dans cet article, nous avons vu qu'il y a une relation réciproque entre la philosophie et la littérature. La philosophie fournit les thèmes et les sujets des ouvrages littéraires et la littérature fournit les images et les formes des ouvrages philosophiques. Ainsi, les auteurs et les poètes traitent des sujets philosophiques dans leurs ouvrages et les philosophes utilisent les formes littéraires pour exprimer leur philosophie. Dans ce sens, nous avons constaté que le grand philosophe islamique, Sohrawardi, a utilisé l'allégorie pour présenter sa théosophie de *l'Ishrâq* dans *L'Archange empourpré*.

La relation entre la philosophie et la littérature peut être considérée comme une des branches principale de la philosophie de la littérature. La philosophie de la littérature est une science très compliquée. Car, la littérature est un domaine compliqué et beaucoup de concepts concernant l'homme y sont traités. De sorte qu'entre les différentes sciences comme la philosophie, la sociologie, la politique etc. et la littérature s'établit une sorte d'interférence. C'est pourquoi la philosophie de la littérature doit examiner, outre les sujets littéraires comme l'imagination, les figures du style, la narration, la description et les thèmes littéraires, la relation qui existe entre la littérature et les autres sciences tout en déterminant les limites de cette relation. Bien sûr, les critiques littéraires le font depuis des années grâce à leurs théories littéraires. Pourtant, la création de la philosophie de la littérature semble très nécessaire. Car, la philosophie de la littérature peut influencer la création littéraire et fournir une méthode qui soit utilisable à la fois pour la critique des ouvrages littéraires et pour l'écriture de ces ouvrages.


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*Human minds don't extend*

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Contra Clark and Chalmers (1998), I will argue that human minds don't extend. My view leaves open the metaphysical possibility of extended human minds. It also leaves open whether non-human minds may extend. And it also leaves open whether systems partly constituted by human minds may possess non-human minds of their own. That human minds don't extend is an empirical claim. I support it by undermining the justification Clark and Chalmers (1998) give for the opposite claim that human minds do extend. I will only discuss one of their strategies for arguing human minds extend, which concerns dispositional beliefs (such as believing that Paris is the capital of France even when you are not consciously entertaining that thought).

In support of extended beliefs, Clark and Chalmers (1998, p.12) propose a thought experiment. This involves a patient suffering from Alzheimer's disease, Otto, who always carries with him a notebook, and consults it when needed. The notebook helps him remember what healthy people remember effortlessly, e.g., what MoMA's address is. Clark and Chalmers then propose a Parity Principle, according to which: if an external memory-like resource (like Otto's notebook) guides reasoning or behavior in roughly the same ways as a biologically internal resource does, the former should be considered just as much (or as little) a part of the mind as the latter. For the purpose of argument, I will endorse both Otto's description, and the Parity Principle.

To see the Parity Principle at work, suppose the following. Otto uses information stored in his notebook in ways conducive to behavior or reasoning. Suppose these ways are broadly similar to the ways Inga, a tourist with unimpaired memory, uses information stored in her biomemory. Both Otto and Inga wish to get to MoMA. Otto consults his notebook for directions, Inga uses her biomemory. Information stored in Inga's biomemory partly constitutes Inga's belief that MoMA is on 52nd St.. So, by the Parity Principle, information stored in Otto's notebook partly constitutes Otto's belief that MoMA is on 52nd St. If something outside Otto's organismic boundaries (a notebook) were necessary to subserve his belief that MoMA is on 52nd St., then Otto's mind would extend.

My reply to this line of thought is that, for the Parity Principle to apply, information stored in Otto's notebook must be used in sufficiently similar ways to information stored in Inga's biomemory - in guiding reasoning ('Where is MoMa?') and action ('Go to MoMA'). I will argue the similarities are insufficient, because there are relevant differences. I first survey two accounts of these by Weiskopf (2008) and Rupert (2004), and then propose my own.

Information stored in Inga's memory is more integrated that that in Otto's notebook (Weiskopf 2008). It can be updated faster, and more coherently. Whereas Otto would need to rewrite entries in his notebook, and to then rewrite all related entries. However, we have no reason to think that highly integrated information always serves speed and accuracy in information-processing. On the contrary, Clark's (2008) notion of soft assembly codes for the possibility that on-the-fly, unstable, goal-driven local computations may, at least sometimes, aid retrieval of long-term beliefs with increased accuracy and speed.

Weiskopf is right to focus on informational differences underwriting Inga's and Otto's beliefs. Otherwise one would beg the question against partisans of the Parity Principle. However, I advocate zooming in not on fact-driven belief-update, but on the role concepts play in beliefs. I follow the basic thought that beliefs are made of concepts, indexed to the time and place of their deployment. (For example, believing that cheeseburgers contain beef uses at least these concepts: of cheeseburger, beef, containing, almost anytime, and almost everywhere.) If there is a relevant difference in the way Inga uses her concepts and Otto uses his, then the Parity Principle fails to apply.

The way information is stored on paper in Otto's case, and in the brain in Inga's case, is clearly different. Rupert (2004) reviews a number of psychological effects evinced in experimental research on human subjects, which Otto would not exemplify, precisely because his relevant memory is external to his body, and stored in his notebook. However, Wheeler (2010) is right to point out that if some experimental subjects failed to exhibit such psychological effects (what was learned last would not be easiest to remember, for example), we would not count such subjects as lacking minds, or lacking beliefs. Rather, we would revisit our previous (tenuous) generalizations that all human (bio)memory is subject to such effects.

Clark (2010) dramatizes Wheeler's point by asking us to imagine an entire species which updated beliefs differently (contra Weiskopf) or which did not exhibit recency or negative transfer effects (contra Rupert).

Clark then persuasively suggests that we would still wish to say the species in question has beliefs. Yet Clark overstates his case. If we found this different species, we would say they have beliefs, but not that they have human beliefs. The existence of exceptions making us reconsider generalizations of memory effects (Wheeler) is entirely compatible with denying we got the cognitive psychology of human memory wrong. This leads to making a difference between whether minds extend, and whether human minds extend. Clark's rejoinder fails to show that human minds extend.

To say Otto plus his notebook don't make a human, hence the ensemble's beliefs are not human, would beg the question against Clark. Rather, the point is that human beliefs are made possible by the way humans memorize concepts. Although all his facts are in the notebook, Otto's concepts are in his head. Otherwise, Otto would be unable to read his notebook to begin with. Inga's memory of facts is, however, where her memory of concepts is too – in her head. This is the relevant difference between Otto and Inga.

Otto's beliefs are where his concepts are, just like Inga's, so Otto's mind does not extend. You might protest: Otto wouldn't believe MoMA is on 52nd St. unless his notebook said so; that factual information partly constitutes Otto's belief. To reply: this confuses two debates. On one debate, minds do or don't extend. On another debate, one does, or does not, interpret external signs by pairing them up with internal representations which denote the same things: the word "table" is paired with a person's concept of table. I am concerned with the first debate. The objection entices to take a stand on the second debate. But that is a red herring. Clark (2008) may be right that reading is not matching words with concepts, even if he is wrong about extended minds, Otto's concepts do not extend into the words written in Otto's notebook, even if they couldn't exist without them. The two debates are independent of one another. Information in Otto's notebook is a useful instrument by which to enrich or renew his conceptual repertoire. But the instrument is not part of the end product, Otto's dispositional beliefs don't extend, and neither does his mind.

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## *What a brain state is not*

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Are mental states brain states? Questions like this are a defining feature of the landscape of contemporary philosophy of mind. The range of possible answers to this question—identity theory, functionalism, eliminativism, reductionism—is well-rehearsed. Determining which answer is the right one requires that we accurately characterize both the relata—mental states and brain states—and the relationship that holds between them. To date much of the focus has been on characterizing mental states and the relationship they bear to brain states. Comparatively little attention has been focused on the concept of a brain state. And yet nearly every approach to the question of whether mental states are brain states depends on several key presuppositions about the brain: that there are brain states; that brain states are the sorts of things that the natural sciences discover, classify, and invoke in explanations; and that the philosopher's methods and the scientist's methods of individuating, classifying, and grouping brain states are roughly similar.

Here, I argue that we should be skeptical about the philosopher's concept of a brain state, because the concept depends on a series of intertwined conceptual confusions about biology that must be disentangled if we are to understand and evaluate the claim that mental states are brain states. Brain states, if there are such things, are states of biological creatures. Biological creatures are products of evolution. If philosophers are to sensibly and responsibly appeal to brain states, they ought to be responsive to the relevant empirical methods for individuating and classifying biological entities and to the philosophical problems raised by those endeavors. Here, I argue that they are not. Philosophers of mind operate on an implicit understanding of biology that manages to overly reify brain states as natural, biological kinds. So long as philosophers of mind continue to invoke the idea of a brain state without any further analysis of what a brain state is and what role it plays in the biological sciences, we are unlikely to make any significant progress on the project of determining whether mental states are brain states.

I begin by considering what philosophers of mind have said to date about the nature of brain states. The original identity theorists—Smart, Place, and Feigl—rarely used the term at all, preferring language that focuses on processes rather than states (Feigl 1958; Place 1956; Smart 1959). This is noteworthy for a few reasons. First, they do not claim that mental states are identical to brain states; they rarely discuss "brain states" at all. Second, their focus on processes rather than states seems to better reflect how neuroscientists think about the brain, in terms of temporally extended and dynamic mechanisms and processes rather than full descriptions of static time-slices of brains. Third, they understood that these terms were filler terms that would eventually be filled in by a more mature neuroscience. And yet that is not what has happened: phrases like "brain state" and "c-fiber firing" have assumed a central role in discussion of the metaphysics of mind.

Six decades on, we are now in a position to ask whether "brain state" is a concept that plays some explanatory role in a more mature neuroscience. And yet as Bechtel and Mundale (1999) have argued, brain states appear to be a philosopher's fiction, playing no real explanatory role in the science. If "brain state" means what philosophers have typically taken it to mean, then the identity theory is all but guaranteed to turn out to be false, but not for any interesting reasons: science will never show that mental states are identical to brain states for the simple reason that neuroscientists do not investigate brain states. But this is not so much a defect with identity theory as it is a problem with the dialectic that has emerged surrounding it. I argue that a focus on processes and mechanisms, rather than states, is bound to be more fruitful.

Having shifted our focus from states to mechanisms, I ask how we might determine whether mental states are identical to brain mechanisms or processes. We might begin, as some have, by asking whether a given mental function is realized by more than one neurobiological mechanism. Whether a mental function is singly or multiply realized requires determining whether individual mechanisms—across individuals and species—are sufficiently similar or distinct, so as to warrant lumping or splitting them into the same type or distinct types.

The problem with this approach is that the metaphysician's and the biologist's understanding of what it would mean for two mechanisms to be the same diverge rather sharply. In the absence of information about how the mechanism evolved to do what it does, the metaphysician will proceed by looking for any structural or functional difference between mechanisms. The metaphysician will then rarely find reasons to lump two mechanisms together into the same type, because the biological world is rife with variation. Some argue that this biodiversity is evidence for massive multiple realization and thus a major obstacle for the identity theory (Aizawa and Gillett 2009). But I think a different conclusion can and should be drawn if we attend more carefully to how biologists think about similarity and difference.

The biologist, in constrast, will determine whether two mechanisms are of the same type in part by thinking about how the mechanism evolved to do what it does. One way that biologists individuate kinds is by way of homology: structures are grouped together as members of a type if they are similar because they evolved from a common ancestor. This method of classification will yield members of a kind that might look drastically different superficially (e.g., metacarpals in humans and horses) but are nonetheless the same in the sense that matters to biologists. Homology is a rich source of biological similarity, and yet this is a method of classification that has been almost entirely overlooked by philosophers of mind.

Philosophers would do well to adopt homology thinking about neural processes. And yet they have avoided it almost entirely. This oversight is puzzling: philosophers of mind endeavor to answer important questions about brains. Brains are products of evolution, and so one would think that evolutionary biology would have a central place, along with neuroscience, in theorizing about the metaphysics of mind. And yet philosophy of mind has proceeded almost entirely in ignorance of evolutionary biology. If philosophers of mind are to sensibly and responsibly assess the similarity and difference of neural processes, they ought to be responsive to the relevant empirical methods for individuating and classifying biological entities.

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*Decisions and choices in computational linguistics systems: a social epistemology of automated codification*

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With the exponential development of individual and infrastructural computer use, many aspects of human sociability and behaviour have been captured through data. Most of web data are said to be of textual nature, hence an increased popularity and dynamicity of text analysis tools and methods. According to François Rastier (1996), modern linguistics has two issues, one rooting in grammar and logic, the other one in hermeneutics. Although they have given birth to several computational application such as in quantitative analysis of textual data and computational linguistics, none one these schools of thought have set the design of these techniques as a purpose for themselves. Just as phonetics was reborn amongst engineers and computer scientists as speech analytics (Grossetti & Boë 2008), linguistics is reinvented as natural language processing. NLP is a domain of research without exact boundaries which occupies nevertheless a growing part of the so-called data sciences.

Through the example of textual data analysis, I would like to make a contribution in explaining what the data sciences are from the perspective of philosophy of science and what kind of knowledge they might help us formulate, assuming that they produce results of actual epistemic value. This contribution relies on both an epistemological analysis of data science techniques as described in scientific reviews and conference, and a field work amongst a software editor (where I currently work at) specialised in natural language processing. This investigation questions the status of informatics as both a science and a technology. The role of computation in social sciences and humanities (and in our case, in linguistics) can be compared to the role of mathematics in founding modern Galilean sciences: in a scientific context, statistics as an instrument is the material and practical condition of possibility of any knowledge. It is a necessary but not sufficient criterion for scientificity; it must take place in a theoretical framework which provides scientific concepts and interpretation solutions.

However, just as mathematics and statistics, data sciences have their own norms of design and development which may or may not contradict scientific ones. Moreover, data sciences are, after all, called "sciences" and might also refer themselves to scientific norms. Just as Bachimont (1996) examined the epistemological status of artificial intelligence and its tension between being a cognitive science and a computational technology, I would like to show how data sciences present the same tension, notably by following their own agenda while being regained by industrial and scholar institutions. More precisely, their agenda is in some case the agenda of software and online companies. In those cases, knowledge production appears to be a byproduct of decision-oriented pragmatic considerations. However, data science techniques do produce results of epistemic value, although the interpretation of those results is not normalised.

Through the example of textual data processing at Proxem, the software editor where I work at, I show how the best philosophical conception to describe the epistemic regime of the data sciences could be an anarchist 'anything goes' à la Feyerabend, or a Peircean pragmatism where knowledge is defined by a capacity to decide and act. The linguistic resources (ontologies, thesauri, taxonomies…) used by Proxem for its computation rely on borrowings from several techniques, methods, approaches and conceptions of language; the software that is actually produced is the result of various epistemic cultures including those of computer scientists, computational linguists and social scientists; the interpretation and validation of the results is based on a social epistemology where the analyst, the customer and the engineers take some part.

Besides, while some engineers are familiar with theoretical and computational linguistics, what they do as a company is very different from a scholar linguist's work; as a consequence, the knowledge they produce cannot be considered of linguistic nature. However, they build on linguistic objects to deduct sociological-like assertions about customer behaviours and expectations, in what appears to be a shift in meaning and interpretation.

This example of industrial natural language processing illustrates the different epistemic regimes of the data sciences, which I tried to disentangle. It reminds us that knowledge does not come as a raw product of computation and data analysis, but as a result of a conceptual framework of hypotheses and significations which might be largely inspired by common sense or a pragmatic call to corporate decision-taking, but remains a necessary condition for sense-making from data.

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*When journal editors play favorites*

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Journal editors occupy an important position in the scientific landscape. By making the final decision on which papers get published in their journal and which papers do not, they have a significant influence on what work is given attention and what work is ignored in their field (Crane1967).

In this paper I investigate the following question: should the editor be informed about the identity of the author when she is deciding whether to publish a particular paper? Under a single- or double-blind reviewing procedure, the editor has access to information about the author, whereas under a triple-blind reviewing procedure she does not. So in other words the question is: should journals practice triple-blind reviewing?

Two kinds of arguments have been given in favor of triple-blind reviewing. One focuses on the treatment of the author by the editor. On this kind of argument, revealing identity information to the editor will lead the editor to (partially) base her judgment on irrelevant information (such as the gender of the author, or whether or not the editor is friends with the author). This harms the author, and is thus bad.

The second kind of argument focuses on the effect on the journal and its readers. Again, the idea is that the editor will base she judgment on identity information if given the chance to do so. But now the further claim is that as a result the journal will accept worse papers. After all, if a decision to accept or reject a paper is influenced by the editor's biases, this suggests that a departure has been made from a putative "objectively correct" decision. This harms the readers of the journal, and is thus bad.

Here I provide a philosophical discussion of the reviewing procedure to assess these arguments. I distinguish between two different ways the editor's judgment may be affected if the author's identity is revealed to her. First, the editor may treat authors she knows differently from authors she does not know. Second, the editor may treat authors differently based on their membership of some group (e.g., gender bias). My discussion focuses on the following three claims.

My first claim is that the first kind of differential treatment the editor may display (based on whether she knows a particular author) actually benefits rather than harms the readers of the journal. This benefit is the result of a reduction in editorial uncertainty about the quality of submitted papers when she knows their authors. I construct a model to show in a formally precise way how such a benefit might arise---surprisingly, no assumption that the scientists the editor knows are somehow "better scientists" is required---and I cite empirical evidence that such a benefit indeed does arise. However, this benefit only applies in certain fields. I argue that in other fields (in particular, mathematics and the humanities) no significant reduction of uncertainty---and hence no benefit to the readers---occurs.

My second claim is that either kind of differential treatment the editor may display (based on whether she knows authors or based on bias against certain groups) harms authors. I argue that any instance of such differential treatment constitutes an epistemic injustice in the sense of Fricker (2007) against the disadvantaged author. If the editor is to be (epistemically) just, she should prevent such differential treatment, which can be done through triple-blind reviewing. So I endorse an argument of the first of the two kinds I identified above: triple-blind reviewing is preferable because not doing so harms authors.

My third claim is that whether differential treatment also harms the journal and its readers depends on a number of factors. Differential treatment by the editor based on whether she knows a particular author may benefit readers, whereas differential treatment based on bias against certain groups may harm them. Whether there is an overall benefit or harm depends on the strength of the editor's bias, the relative sizes of the different groups, and other factors, as I illustrate using the model. As a result I do not in general endorse the second kind of argument, that triple-blind reviewing is preferable because readers of the journal are harmed otherwise. However, I do endorse this argument for fields like mathematics and the humanities, where I claim that the benefits of differential treatment (based on uncertainty reduction) do not apply.

Note that, in considering the ethical and epistemic effects of triple-blind reviewing, a distinction is made between the effects on the author and the effects on the readers of the journal. This reflects a growing understanding that in order to study the social epistemology of science, what is good for an individual inquirer must be distinguished from what is good for the wider scientific community (Kitcher 1993, Strevens 2003, Mayo-Wilson et al. 2011).

Zollman (2009) has studied the effects of different editorial policies on the number of papers published and the selection criteria for publication, but he does not focus specifically on the editor's decisions and the uncertainty she faces. Economists have studied models in which editor decisions play an important role (Ellison 2002, Faria 2005, Besancenot et al. 2012), but they have not distinguished between papers written by scientists the editor knows and papers by scientists unknown to her, and neither have they been concerned with biases the editor may be subject to. And some other economists have done empirical work investigating the differences between papers with and without an author-editor connection (Laband and Piette 1994, Medoff 2003, Smith and Dombrowski 1998), but they do not provide a model that can explain these differences. This paper thus fills a gap in the literature.


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*Change and tradition. For a new interpretation of Francesco Redi's experiments on the generation of insects (1668).*

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The Italian naturalist Francesco Redi (1626-1697) is known as the first scientist to have experimentally disproved the thousand-year belief in spontaneous generation. Born in Arezzo (Tuscany), Redi was educated at the University of Pisa, where he received his degree in Philosophy and Medicine. In the 1650's he became the court physician to the Grand Duke of Tuscany Ferdinando II de' Medici. Redi will then spend all his life at the Medici court, where he held a prominent role not only as physician, but also as poet and lexicographer, becoming the head of the illustrious Accademia della Crusca devoted to the study and the preservation of the purest Italian language. He also attended the Accademia del Cimento ('the Academy of the Experiment'), the scientific academy founded by the Grand Duke and his brother Leopoldo, who were animated by the desire to make Galileo's legacy revive and both had a genuine, 'undeniable passion for science' (Feingold 2009: 231).

Redi is currently considered as a radical empiricist who abruptly interrupted the Aristotelian tradition and dramatically widened the horizons of modern Western culture (Bernardi 1996: 59). In particular, Redi is known for 'the experiment of the sealed jars', with which he observed that pieces of meat put in airtight jars bred no maggots, while only the meat left in open jars bred maggots. His conclusion was that 'putrefied matter [...] has no other office than that of serving as a place, or suitable nest, where animals deposit their eggs' and, more generally, that the generation of living beings always originates from the seed of their own species (Redi 1909: 27).Clearly, such a statement is correct; however, I will challenge the truthfulness of the accounts Redi gives in the '*Esperimenti intorno alla generazione degl'insetti*' ('Experiments on the generation of insects'), published in Florence in 1668.

Redi opens his treatise with an important plea for experimental method, stressing the prominence of ocular evidence against theorical speculations. For instance, he writes: 'I have always taken the greatest care to convince myself of facts with my own eyes by means of accurate and continued experiments before submitting them to my mind as a matter of reflection' (Redi 1909: 20). Nevertheless, an accurate comparison with seventeenth-century sources shows how, actually, Redi's treatise has to be read in strict correlation with the contemporary controversies on spontaneous generation. In fact, despite presenting himself as a pure empiricist, at the beginning of his treatise, Redi undertakes a sort of survey about the main theories on spontaneous generation, although in a rather implicit manner. Indeed, what is noteworthy is the very allusive nature of Redi's survey. First, he hints quite generically at 'ancient and modern schools', who held that the earth itself has the power to generate animals and plants without seed (Redi 1909: 23). Likely, Redi is referring here to the neo-Platonic theory of the *anima terrae* (earth-soul) dating back to the Renaissance philosopher Marsilio Ficino (1433-1499) (cfr. Hirai 2011).

Then, Redi hints at those who think that spontaneous generation is caused 'by the generative principle residing in the sensitive and vegetative soul, of which particles remain alive in the dead bodies of animals and plants *as sealed in a jar*' ('come in un vaso'). Now, this passage clearly echoes the theories of Fortunio Liceti (1577-1657), the author of the successful treatise '*De spontaneo viventium ortu'* (Vicenza, 1618). Liceti thought that living beings could only arise from matter that had been somehow alive, that is, corpses, rotten fruits and plants, food waste and excrements. For him, animated particles remained in organic matter in a quiescent state, as sealed in a jar ('ut in vase'), but could be brought to new life thanks to heat.

Finally, Redi hints at 'another class of wise persons who hold it to be true that generation proceeds from certain minute agglomerations of atoms, which contain the seed of all things. These persons say further that the seed was created by God at the beginning of the world and scattered in all directions' (Redi 1909: 24). Despite Redi's allusive tone, it seems to me evident that he is referring to the theories on generation of the French philosopher Pierre Gassendi (1592-1655). If we compare the original passage of Redi's treatise with Gassendi's writings on spontaneous generation, we can see that indeed the passage in Italian is nothing but the translation of Gassendi's thought, according to which Nature is permeated with invisible primordial seeds able to arrange and animate atoms. Strange enough, Redi ascribes a similar view also to William Harvey (1572-1657), claiming that for the English scientist too spontaneous generation is due to mysterious invisible seeds floating in the air.

What I will suggest is that only assuming that Redi's target were the above mentioned theories, the considerable inconsistencies we read in his accounts will find their explanation. I am referring, for instance, to the passage where Redi states that, in order to avoid the contact with flies, he 'even had pieces of meat put under ground, but though remaining buried for weeks, they never bred worms'. But it seems evident to me that although the meat was not exposed to the contact with flies, it would had certainly lured all the worms, larvae, insects and fungi that proliferate under ground. I believe that actually Redi, reporting this experiment, aimed at contrasting the Neo-platonic doctrine of the *anima terrae*, according to which, as we have seen, the earth has the power to generate life.

A polemical intent seems to have inspired even the experiments of the sealed jars. In this case too, Redi's accounts seem hardly plausible. For instance, as Bruno Basile has already stressed, the jars used by Redi were not sterile (Basile 1987: 151), nor was the meat sealed inside. Further, it is worth remembering that, mostly, the meat used for the experiment was taken from untamed animals (such as snakes, eels and freshwater fish). Redi says nothing about their caught and their preservation. Then, can we possibly be certain that no fly had come in contact with the corpses before they were sealed in the jars? Can we possibly be certain that the animals were free from parasites? In another passage, the Italian naturalist claims that excrements and fruits sealed in a container never generate worms. But it has to be noticed that at Redi's time no animal and no plant could have been treated with paracitides. In my opinion, such accounts were in reality intended to demonstrate that decaying organic matter never breeds insects in order to antagonise Liceti's theories.

Redi's also tells us something highly surprising about the eggs laid by the flies in the containers left accessible to them. In particular, he writes to have enclosed some fish in a box full of holes and that after four hours he had observed (with the unaided eye!) many tiny eggs adhering in bunches to the joints and around all the holes in the interior of the box (Redi 1909: 31). Surely, this is rather unusual, since flies normally inoculate their eggs (or larvae) into rotting matter and that is even the main explanation - I daresay - for the belief in spontaneous generation, since maggots seem literally to burst out from rotten meat.

However, according to Redi, fly eggs are perfectly perceptible and even tangible: 'I crushed them [the fly eggs] between my nails and the cracked shell emitted a kind of whitish liquid', he writes. Again, what I believe is that Redi is not giving us an objective scientific account, but, quite the opposite, he is here attempting to contrast the most advanced theories on generation of his time, namely Gassendi and Harvey's ones.

In fact, the experiment of the sealed jars could prove that inside rotten corpses there are no such vital principles able to bring matter again to life (Liceti's theory) and that the appearance of the maggots on the meat is due to an external cause. However, how could Redi prove that maggots actually originate from fly eggs and not from invisible seeds floating in the air? Hence, for Redi it was crucial to show to his readers that maggots do hatch from fly eggs. That's why he reports to have seen and touched real eggs, complete with shell and albumen.

Moreover, Redi explicitly tells us that he repeated the experiment in order to reply to the objection that he had used sealed jars into which the air could not penetrate or circulate (Redi 1909: 36). This time he put the meat in a large vase covered with a fine gauze, which allowed the air to enter. But in this case too he writes something rather implausible, namely that the flies, unable to reach the meat, laid their larvae on the gauze or even dropped them in the air.

The last aspect I wish to enlighten about Redi's refutation of the belief in spontaneous generation is its resemblance with the theories of a rather neglected naturalist, the German theologician Wolfgang Franzius (or Frantzius, Franz, 1564-1628). As Redi will do later, in his '*Animalium historia sacra*' (Wittenberg, 1612) Franzius had in fact claimed that insects cannot originate from filth or from rotting meat, but only from eggs laid by individuals of the same species (Franzius 1653: 545-547).

Redi knew Franzius' work and, definitely, the only difference between the two is that the Italian (supposedly) proved his theory by the means of ocular evidence, while Franzius exclusively relied on metaphysical explanations, namely that God would not allow anomalies in natural generation (cfr. Roggen 2007: 135-136). But, as we have seen, Redi's accounts display several unclear points and, finally, the experiments he performed seem to have more a persuasive, rhetorical nature than a probative one - a very interesting parallel could be drawn with Harvey's argumentation in '*On the motion of the heart and blood*' (see Cattani 2011).

Concluding, I don't believe, as Paula Findlen does, that with his entertaining and witty treatise Redi attempted 'to bring the new experimental culture to the attention of his patrons' (Findlen 1993: 40). Quite the opposite, what I believe is that Redi artfully fashioned himself as a scientist of Galilean stamp in order to appeal the taste of his patrons. Then, we must acknowledge Redi's rhetorical ability, but also underline how historiography seems prone to glorify any anti-traditional attitude - in particular, for the early modern period, any anti-Aristotelian attitude - and exalt what appears to be the revolutionary discovery of a solitary genius.

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*How thought experiments cause change in science*

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In this article we will examine two ways in which scientific thought experiments (hereafter TEs) can cause change in science. A historical and contextual analysis of Galileo Pisa tower and Maxwell's demon shows that TEs can play an important role in theoretical change; either causing it directly by refuting an existing theory, or suggesting new theoretical avenues by exploring a theory. We conclude with some morals concerning most epistemic accounts of scientific TEs.

First, TEs can bring about or cause a scientific change by refuting an existing theory; Galileo Pisa tower is a very interesting case and is treated as such in the current literature. Nevertheless, as Palmieri (2005) claims, we are usually presented with a "cartoon reconstruction devoid of any historical meaning." In fact, this TE is misrepresented; many, following Brown (1986), analyze it as confirming Galileo's law of free fall, conferring it a function of theory choice, reminiscent to the one supposedly played by crucial experiments. A historical analysis shows that Galileo's TE had only a refutational function, i.e., it was purported to refute Aristotle's theory, without defending or proposing another theory, at least not on it's own. To see this we have to look at how it was used by Galileo:

Galileo first introduced this TE as early as 1590 in De Motu (probably influenced by a similar TE by Jean-Baptiste Benedetti 1553). Four decades later in his Two new sciences (1638, and few years earlier in Postils to Rocco), he reused it for the same refutational purpose. In both occurrences, Galileo's principle aim was to show that absolute weight couldn't cause a difference in the speed of free fall, for mobiles of same material, thus refuting Aristotle's theory. The difference between these two occurrences is what followed from this refutation:

In 1590 and with an Archimedean analogy, Galileo argued in favor of a "restricted" theory of free fall; the mobile's specific weight is treated as the cause of a divergence in speed of fall. That is, the young Galileo was arguing for an early theory of free fall that establishes a proportionality between speed of fall and the mobile's specific weight (to be exact, minus that of the medium), even in void: Mobiles of different material fall at different speeds, while mobiles of the same material fall with identical speeds, regardless of their absolute weight.

While in the 1630's, with a complex argumentative strategy that starts with, but is not limited to the TE, Galileo argued in favor of a general theory of free fall: Specific weight is no longer treated as causing a difference in speed of fall, and thus in void all mobiles fall at the same speed.

This shows that Galileo's TE did not, on it's own, result in theory choice proper, but only in refutation. Nevertheless its refutational function did cause a theoretical change; it showed that Aristotle's theory could not describe free falling mobiles. In addition, this TE had also an indirect exploratory function; Galileo, in both occurrences, was in fact looking for the causes of the divergence of speed of fall of any two free falling mobiles, in plenum and in void. The TE denies that absolute weight can cause a divergence in speed of fall, but remains silent on the effects of other causal factors. Factors then explored in each argumentative strategy.

Second, scientific TEs can bring about or cause a theoretical change by exploring a new theoretical avenue. We saw the exploratory function that followed Galileo Pisa tower TE in both occurrences. Here we will examine a TE where theory exploration is its main function, while also refuting a theoretical interpretation:

Maxwell's demon is a canonical example of theory exploration. When Maxwell conceived his TE (1867), the classical phenomenological second law of thermodynamics was formulated without reference to the microscopic constitution of gases. In fact it was originally formulated in the context of the caloric theory of heat, which defines heat as a moving substance. With the development of the kinetic theory, heat became defined in terms of molecular motion. It was thought by Clausius, and early Boltzmann, that the second law could be grounded on a dynamical method that traced each molecule in its course. Maxwell thought differently, and used his demon to limit the scope of the second law of thermodynamics to aggregates of molecules; to the macroscopic domain. In other words, Maxwell used his demon to propose a "statistical", as opposed to a dynamical interpretation of the second law. He did not however give us any quantitative formulation of this law or an explicit formulation of its concepts.

The TE reestablished external coherence between a statistical interpretation of thermodynamics and kinetic theory. Coherence lost with a dynamical interpretation of the second law. It enables, and in fact enabled, a further reflection on statistical mechanics and its foundations. Finally, the vague nature of the demon in the TE opened new avenues of exploration of physically possible demons and explanations of their failure. Looking at the current scientific and philosophical literature on Maxwell's demon, one directly notices the ongoing exploratory function of this TE, in its different versions, a century and a half later.

In analyzing both cases, we will underline the role of the imagined "particulars" in the scenario of TEs: Galileo, in choosing two mobiles of the same material, was justified to neglect the effect of air resistance without assuming the existence of a void. Maxwell in imagining an under-specified sorting mechanism, later labeled demon, was justified to neglect the physical possibility, or near realizability of such a device.

This analysis of the functions of TEs and the role of the particulars in TEs allows us to draw some morals relative to the epistemic accounts of TEs: Many are too restrictive by demanding a nomologically possible scenario for all successful TEs (Kuhn, Sorensen, Laymon, Nersessian, Buzzoni and Gendler). Others, like Brown's, attributes an unjustifiably strong epistemic function to some TEs. Finally, Norton's argument account with the elimination thesis, misrepresent, or at least lessens, the role of some specifically chosen particulars in the scenarios of TEs.

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*Real, thought and numerical experiments: the experimental triangle*

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How experimental is a thought experiment? This is a much discussed question in the literature on thought experimentation, which has received answers that range from the denial of the experimental character of thought experiments (e.g., Humphreys 1993; Norton 2004), to claims that thought experiments are a kind of experimentation (e.g., Sorensen 1992; Buzzoni 2004). The question about the experimental side of thought experimentation clearly brings with it the issue about the relationship between thought experiments and experiments concretely conducted, for instance in laboratories. Let us call the latter "real experiments".

Although getting clear about the relationship between thought and real experiments seems to be an important issue in the debate, in my opinion a deep analysis is precisely what is missing. Very often the experimental side of thought experiments has not been evaluated per se: thought experiments have been judged from the standards of real experiments, rather than on the basis of a broad definition of experiment that can include both types of experimentation.

The analysis of the experimental side of thought experiments seems to be influenced by a widespread bias about the intrinsic epistemological superiority of real experiments. By following this bias we risk focusing on the features proper to "true" experiments (i.e., real experiments) that thought experiments lack. For instance, a typical plea for real experimentation would stress that, insofar as thought experimentation does not directly examine nature, it is less reliable and lacks justificatory power. The upshot of this line of reasoning is that thought experiments should be employed only when real experiments are not available, otherwise they are useless. A terminological attitude that can fall prey to such a bias is to consider thought experiments as imaginary experiments: just as imaginary friends are not genuine friends, thought experiments would not be genuine experiments, but mere imaginary visualizations of experiments.

It is interesting to notice that if we replace the expression "thought experiment" with "numerical experiment" in the above, we can retrieve an analogous set of considerations which actually feature in the literature on computer simulations. This is just one among several parallelisms that can be drawn between thought experiments and numerical experiments. For instance, some (e.g., Gilbert & Troitzsch 1999; Beisbart & Norton 2012) argue that real experiments and numerical experiments could not possibly differ from each other more; while others (e.g., Dowling 1999; Barberousse et al. 2009) regard numerical experiments as a genuine experimental practice. Moreover, it has been pointed out that the analysis of the relationship between numerical experiments and real experiments is often influenced by the widespread bias about the intrinsic epistemological superiority of real experiments (see Winsberg 2009 and Parker 2009).

Although some authors have commented in passing on the parallelism between thought experimentation and numerical experimentation (e.g., Sorensen 1992; Nersessian 1993; Stöltzner 2003; Buzzoni 2004; Cooper 2005) and others have suggested that numerical experiments can be seen as a type of thought experiments (e.g., Di Paolo et al 2000; Swan 2000) and will even replace the latter (Chandrasekharan et al. 2013), the "trading zone" between thought experiments and numerical experiments has been sparsely considered (e.g., Staüdner 1998; Velasco 2002; Lenhard 2011 and El Skaf & Imbert 2013). Most works have primarily focused their attention on either thought or numerical experiments and on the links with real experiments. However, a triangular comparison is bound to be mutually illuminating.

The aim of this paper is to complete the triangulation and deepen our understanding of the relationships between real experiments, thought experiments and numerical experiments from an unbiased perspective. I will show that the characteristics shared by the three scientific tools suffice to consider them all as "experiments", modulo certain distinctive, distinguishing features. It will emerge that thought experiments and numerical experiments are much closer to each other compared to real experiments. Nevertheless, the upshot will be that all three are fundamental to scientific research, contra the provocative view that computer modelling will replace thought experimentation.

The completion of the triangulation will come in three steps. The first section sets the stage. I shall claim that a deep analysis of the relationships between these three scientific tools has been compromised by a bias for the epistemological superiority of real experiments. My proposal is that real experiments, thought experiments and numerical experiments should be put on the same level in order to allow an unbiased triangular comparison. The latter will fruitfully help to identify similarities and differences among the three tools, which will be the topic of the second and the third sections respectively.

The second section focuses on five basic experimental features: (1) "method of variation"; (2) epistemological holism; (3) refinement of theoretical positions; (4) reproducibility and (5) fallibility. Appealing to the literatures and comparing different examples, I shall dwell on each feature and show that they are common to real experiments, thought experiments and numerical experiments.

The third section draws on the most obvious difference between the three practices, namely the nature of the laboratory in which they take place. It is important not to fall in the aforementioned bias, though: we must resist the temptation of thinking that thought experiments and numerical experiments are employed only because real experiments cannot be made, and to consider this lack of implementation as a fault. Matter really matters: we should not underestimate the hardware component of both thought experiments (i.e., the mind) and numerical experiments (i.e., the computer). Here is why I disagree with the accounts that consider thought experiments and numerical experiments in mere abstract terms. Thought experiments and numerical experiments are tools of scientific enquiry with an undeniable empirical flavour.

*How theoretical explorations explain. A Bayesian account (joint work with Radin Dardashti)*

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It is sometimes argued that theoretical explorations (sensitivity analysis, robustness analysis, etc.) are key to how so-called 'minimal' models explain. This seems puzzling because they provide no novel empirical evidence but merely study the models' behaviour. Here, we provide an account of how theoretical explorations explain, which we illustrate with reference to two minimal models of asset pricing in finance. The first half of the argument shows that the way theoretical explorations explain in such models is not adequately rationalized by Batterman (2001)'s renormalization account of explanation, which Batterman and Rice (2014) claim to be applicable to all minimal models. The second half argues that theoretical explorations explain in virtue of the analogical character of the models. The explanation is rationalized in a Bayesian framework.

*Quantum Humeanism*

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For a long time, it was assumed that quantum entanglement refutes Humeanism, in particular David Lewis's thesis of Humean supervenience (e.g. Lewis 1986, introduction), because the entangled wave functions of quantum systems are not compatible with an ontology that admits only local matters of particular fact (e.g. Maudlin 2007, ch. 2). In recent years, however, it has become clear that this judgement is unfounded. The door through which Humeanism can enter quantum physics without having to change the ontology of there being only local matters of particular fact in ordinary space-time are quantum theories that base themselves on what is known as a primitive ontology of an always definite, spatial configuration of matter in order to solve the famous quantum measurement problem. The aim of this contribution is to show how these quantum theories are hospitable to Humeanism and to base two general claims on this case study, namely (a) that all what is needed for an ontology of physics are spatial or spatio-temporal relations between primitive objects and (b) that, contrary to a widespread belief, the objects of fundamental physics remain constant through theory change, but the dynamical structures that capture the evolution of these objects change.

The argument for endorsing the primitive ontology approach to quantum physics is independent of Humeanism. The argument is the solution to the measurement problem that this approach offers in terms of there always being a definite spatial configuration of matter (no superpositions), with the wave function and its entanglement concerning the evolution of that configuration; a law that links the wave function with that evolution is therefore required in addition to the law describing the evolution of the wave function in configuration space (such as the Schrödinger equation) (see notably Maudlin 2015). The most developed primitive ontology approach is Bohmian mechanics, which advocates an ontology of point particles that always move on definite trajectories, with these trajectories being fixed by the universal wave function through what is known as the guiding equation (see Dürr et al. 2013). As Dürr et al. (2013, ch. 12) argue, the universal wave function then has the status of being nomological instead of being a physical entity on a par with the particles. Its being nomological opens the door for applying the major philosophical approaches on laws to the wave function, whereby the argument for Humeanism about laws has to be independent of quantum physics.

In particular, a Bohmian Humeanism has been set out recently (see Miller 2014, Esfeld 2014, Callender 2015 and Bhogal and Perry 2015). In brief, given the trajectories that the particles take, the regularities that the particle trajectories throughout the whole history of the universe exhibit make it that a wave function variable figures in the dynamical laws capturing these trajectories, such that a certain wave function applies to the particle configuration at any time. On quantum (Bohmian) Humeanism, thus, there are no relations of entanglement in nature over and above the distance relations among the point particles. In other words, there is no quantum state in nature over and above the particle positions and their change. The way in which the particles move is such that it manifests certain stable correlations so that, if we set out to represent their motion in a manner that is both simple and informative, we have to write down an entangled quantum state and a law in which an entangled wave function figures.

Nonetheless, Bohmian quantum Humeanism requires amending Lewis's thesis of Humean supervenience. The Bohmian particles are characterized only by their position in space – that is, by the spatial relations among them, which make up their configuration. There is no place for natural intrinsic properties instantiated at points of space-time in Bohmian mechanics, since even the classical parameters of mass and charge are situated on the level of the wave function instead of belonging to the particles taken individually (see Brown et al. 1996). But this is good news for Humeanism, since if there are no natural, categorical intrinsic properties, the objections from quidditism and humility no longer apply. The supervenience basis does not become arbitrary by cutting off natural, intrinsic properties: spatial relations enjoy a privileged status as forming the supervenience basis, since they unify the world. Furthermore, they can individuate the fundamental physical objects (i.e. the point particles), provided that their configuration is not entirely symmetrical. As Hall (2009, § 5.2) has shown, the same treatment – i.e. nomological status instead of intrinsic properties – can be applied to mass and charge in classical mechanics in order to remove the objections from quidditism and humility. We thus obtain the following view: the objects remain the same from classical to quantum physics – namely point particles characterized by their spatial arrangement –, whereas our hypotheses about what the laws are and which dynamical parameters figure in them change from classical physics (force laws) to quantum physics (law with entangled wave function).


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*For an approximate continuity of structure between Newtonian and Bohmian mechanics*


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Structural Realism endorses a notion of continuity of structure across different theories that needs clarification (Worrall 1989). In light of this, my contribution aims to provide an example of such continuity. More specifically, my talk concerns Newtonian and Bohmian Mechanics and will raise the question of whether it is possible to see a continuity of structure between the two theories in case we endorse Ontic Structural Realism.

I will open my talk with a brief summary of the position of Structural Realism (SR) and Ontic Structural Realism (OSR). Structural Realism claims that there is continuity between shifts of different Scientific Theories and that this continuity is of structure and not of content (Worrall 1989). Ontic Structural Realism endorses the statement that all that really exists is structure (Ladyman 1998). For this reason, in a scientific theory what is ontologically significant is its structure alone. In this view, the ontological status of objects is not necessarily completely denied, but re-conceptualized in structural terms (French and Ladyman 2003). What instead is denied is a metaphysics according to which the object is defined as a substratum with intrinsic properties. According to an ontic structuralist reconceptualization, then, objects are defined through the relations they entertain: objects do not enter into some relations in virtue of the properties they have; on the contrary, they seem to have certain ontological features because of the relations they enter into (Cei and French 2015). These relations are spelled out by the laws of the theory through mathematical equations. Laws indeed, are not grounded in intrinsic and dispositional properties of the objects, on the contrary, they are the features of the fundamental structure of the theory. The objects seem to manifest some properties only because of the law-like structure they are in. Because of this account of law given by OSR, we can say that two theories present a continuity of structure when the laws remain invariant. However, this is hardly ever the case. Often, on the contrary, the equations of one theory reappear as limiting cases of another one, which means that the latter tends to the former as some quantity tends to some limit. I claim that these cases present an approximate continuity of structure.

In the second part of my talk, I will present the case of the shift between Newtonian and Bohmian Mechanics. Bohmian Mechanics seems to be the most suitable quantum theory that can account for a continuity from Newtonian Mechanics to the quantum domain. This is because both theories share a primitive ontology that consists of particles in motions. However, if we re-conceptualize the ontology in terms of structure, and structure alone, in order to detect a continuity between the two theories we need a continuity of structure. Hence, the question would be whether or not the two theories rely on the same (kind of) structure and whether it is possible to recover a continuity between the two structures, in case they differ. Both theories present a non-local dynamics according to which each particle trajectory depends on all the other particles of the system. Moreover, in both theories this dependence is simultaneous. Hence, it seems that both ontologies present the same kind of underlying structure, which relates all the particles (Esfeld et al. forthcoming). However, it is clear that the structures underlying the two theories cannot be the same, since the laws of the two theories are different. Most importantly, I will show that while in Newtonian Mechanics the structure that relates all the particles of the system is actually reducible to many one-to-one relations, in Bohmian Mechanics the structure is not separable (Hubert forthcoming). This is due to the peculiar character of entanglement given by the wave-function. Hence, while for the Newtonian case the dynamical structure underlying the theory is simply non-local, in the Bohmian case it is holistic, since in the latter the whole is prior to the parts. (Here I disagree with the claim endorsed in Esfeld et al. forthcoming, according to which both Newtonian and Bohmian Mechanics present dynamical holism.) Concerning the concept of particles, in an ontic structural realist perspective, particles 'break down into the web of relations that are held together in themselves' through the laws of the theory (Cei and French 2015, p.36). This means that the shift from a non-local dynamics to a holistic one implies a shift of the conception of particles as well. While in Newtonian Mechanics it would still be tempting to endorse a Moderate Ontic Structural Realism (Esfeld and Lam 2008), according to which we retain the notion of particles as individuals, in Bohmian mechanics it is ontologically proper to talk only about all the particles taken as a whole, hence about the particle configuration. The question now is whether it is still possible to recover a continuity of structure between the two theories even though they do not share the same (kind of) structure. I will claim that, if we endorse ontic structural realism, the answer is completely up to the physicists who work in the classical limit of Bohmian mechanics. In case it is possible to regard the Newtonian laws as limiting cases of the Bohmian system when some quantity goes to zero (for example the quantum potential or the Planck constant, as suggested in the literature (Allori et al. 2002, Holland 1993), then we can still talk of a continuity. However, I would define this continuity as approximate, since the two theories do not share the same dynamical structure.

In my conclusion, I will reiterate the claim that Newtonian and Bohmian Mechanics do not share the same kind of dynamical structure, mainly because one is non-local and the other is holistic. However, if Physics can explain the conditions under which a Bohmian system becomes classical, and so how the Bohmian laws become Newtonian, then it is true that there is an approximate continuity of structure between the two theories. This case provides a clear example of what Structural Realism means by continuity of structure across different theories.


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*On the alleged incommensurability of Newtonian and relativistic mass*


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One of the enduring debates about scientific change concerns the extent to which there is conceptual continuity across successive theories. The same term as used in different theories often on its face appears to have ultimately different extensions. Despite some ostensive overlap, the traditional story goes, they are embedded in a different network of terms that, holistically, grants it a different meaning. There has also been a more recent resurgence of debate regarding limiting-type relationships between theories, especially in physics, and whether these count as reductive relationships. This debate has concerned to what extent one theory can be the limit of another, and whether, if it is, this explains the limit theory. Although these two debates are not always explicitly connected, one of my goals is to show how a particular sort of positive solution to the reduction question can also contribute to understanding the extent of conceptual continuity and discontinuity between theories related by a limit. In particular, I apply some relatively new (to the philosophical literature) topological tools for understanding the limiting relationship between Newtonian and relativistic kinematics to what is perhaps the most well-known alleged example of conceptual incommensurability, that between the Newtonian and relativistic concepts of mass. My main contention is that the mass concept in the two theories of kinematics is essentially the same.

Famously, of course, both Kuhn and Feyerabend provided historical evidence that, in the mathematical framework used to formulate Newtonian and relativistic kinematics at the latter's inception in 1905, these concepts were not the same. I do not intend here to dispute their historical claims. Rather, my contention is based on a reconstruction of both theories in light of the best mathematical frameworks for describing them now, that of four-dimensional differential (affine) geometry. Thus, I do not intend to dispute here how historical actors involved in the construction, elaboration, and propagation of relativity theory. Instead, I wish to show that however the situation appeared to these actors, there is a way of describing and understanding these theories and their relationship that makes completely transparent the commonality of their concepts of mass.

One of the interesting conclusions to draw from this is that the usual understanding of incommensurability is likely too tied to the contingent and accidental features of the particular language in which a theory may be described—that is, it is too tied to the syntactic conception of theories that dominated philosophy of science in the 1960s. While there continues to be debate about the merits of the semantic view of theories, the syntactic view's successor, almost all seem to be in agreement that capturing the structure of a theory involves in large part aspects that are invariant (or at least appropriately covariant) across choice of language. Taking this into account shows that the essential differences are not so invariant. This moral is important for the reduction literature, too, for one potential objection to the claim that Newtonian kinematics is the reductive limit of relativistic kinematics is that the incommensurability of their mass concepts prevents the limit from being reductive, i.e., explanatory. Thus showing the commonality of the mass concepts is also important for understanding the explanatory relationship between the theories.

The technical portion of my argument proceeds in three phases. The first involves formulating both Newtonian and (special) relativistic kinematics in the framework of four-dimensional differential (affine) geometry, with the worldlines of particles as certain (timelike) piecewise smooth one-dimensional submanifolds. In both kinematical theories, mass is a non-negative parameter that, when associated with a worldline, specifies the degree to which the worldline departs from being a geodesics—following locally straight ("unforced") motion. The mass parameter then in both theories enters into the expression of the particle's four-momentum as a kind of normalization constant. I point out that there is a degree of convention not normally recognized in how it so enters, but that the choice of convention is essentially irrelevant when considering the details of simple particle collisions. The completion of this formulation reveals that mass plays the same functional roles in both kinematical theories; the only substantive difference lies in different spacetime structures that determine spatial distances and temporal lengths.

These different structures are nonetheless related, and in the second technical phase, I show how the Newtonian structure arises at the limit of the relativistic structure. This limit is constructed mathematically, by considering sequences of relativistic spacetimes (with various particles and observers within) that converge to Newtonian spacetimes, the sense of convergence being given by an appropriately chosen topology on the joint class of spacetimes. Because the Newtonian and relativistic spacetimes have a common conceptual interpretation, as revealed in the first phase, the topology can be easily interpreted as encoding similarity of empirical predictions. Thus a convergent sequence of relativistic spacetimes does not indicate a sequence in which the speed of light grows without bound, but rather one in which the measurements of the fixed observers can be better and better approximated by those of a certain hypothetical idealized Newtonian observer.

The third phase responds to a natural objection to the above account, namely that it has not explained the significant difference of Einstein's mass-energy relation, E=mc2. Here I build on previous work by Rindler, Lange, and Flores, as well as on the conventional elements mentioned above, to explain the significance of the most famous equation not asserting the identity of mass and energy, but either as defining energy or stating an energy content associated with mass. The analysis of classical "fission" experiments can then be made where change in mass is interpreted only as a change in effective mass, a conceptual move also available in the Newtonian framework. Lastly, I gesture towards how this analysis extends to the Newtonian and general relativistic theories of gravitation, the former in its Newton-Cartan form, where the presence of the same sort of mass can be understood as having the same sort of influence on spacetime geometry.

**Jeudi matinée 30 juin / Thursday morning June 30**
Salle / Room 348 (auditoire/auditory)

Séance plénière / Plenary session:

*Scientific realism and primordial cosmology: joint work with Feraz Azhar*

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*Abstract*

We discuss scientific realism from the perspective of modern cosmology, especially primordial cosmology: i.e. the cosmological investigation of the very early universe.

We first state our allegiance to scientific realism, and review some of what is now known about the early universe: meaning, roughly, from a thousandth of a second after the Big Bang onwards(!).

Then we take up two issues about primordial cosmology, i.e. the very early universe, where 'very early' means, roughly, much earlier (logarithmically) than one second after the Big Bang: say, less that $10^{-11}$ seconds. Both issues illustrate that familiar philosophical threat to scientific realism, the under-determination of theory by data---on a cosmic scale.

The first issue concerns the difficulty of observationally probing the very early universe: more specifically, the putative inflationary epoch. The second issue concerns difficulties about confirming a cosmological theory that postulates a multiverse, i.e. a set of domains (universes) each of whose inhabitants (if any) cannot directly observe, or otherwise causally interact with, other domains.

Symposium:

*Approches phylogénétiques et quantitatives des concepts scientifiques*

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*Présentation générale*

L'exportation de théories et de méthodes issues de la biologie évolutive vers les sciences humaines et sociales a une histoire riche et complexe. Parmi les tentatives récentes on notera: (au niveau des processus) la mémétique, de la théorie dawkinsienne de l'évolution des réplicateurs génétiques au domaine de la culture ; ou (au niveau des patterns) l'acclimatation de méthodes phylogénétiques, initialement utilisés pour établir des relations de parenté entre espèces biologiques, à l'évolution des langues (p.ex. Gray et Jordan, 2000). De manière plus spécifique, l'histoire des sciences a reçu un éclairage nouveau par un tel échange de méthodes, grâce à la vision sélectionniste du changement scientifique développée par David Hull, qui recourt à la sélection naturelle pour expliquer l'évolution des sciences via notamment les processus de compétition entre théories.

Ces méthodes - l'approche sélectionniste aussi bien que l'approche phylogénétique - importent une dimension quantitative dans l'étude de leur objet. Or un tel élément apparaît aujourd'hui remarquablement pertinent pour l'étude de la science et de son évolution, du fait du caractère de plus en plus monumental de la production scientifique et des archives qui la recueillent. La numérisation systématique des publications et le développement d'outils informatiques d'analyse textuelle rendent en effet possible de multiples approches quantitatives en histoire et philosophie des sciences.

Ce symposium propose de donner un aperçu des possibilités et des limites des approches phylogénétiques et quantitatives pour l'histoire des concepts et théories scientifiques, mais aussi pour la reformulation de problèmes philosophiques classiques tels que le changement de théorie ou de paradigme et ses justifications, la forme du progrès scientifique, ou encore la délimitation de frontières et de recoupements entre théories rivales, en sciences naturelles ou sociales. Les exposés couvriront à la fois des questions de méthode, et des applications concrètes à des domaines précis.

*Analyser l'évolution des sciences avec la reconstruction de phylomemies, quels enjeux pour la philosophie des sciences ?* (**David Chavalarias**)

La science est l'une des productions culturelles les mieux structurées. C'est aussi l'un des premiers domaines d'activité à avoir vu sa production presque intégralement numérisée. Pour cette raison, elle constitue un objet d'étude privilégié pour l'analyse et la compréhension des dynamiques culturelles à partir de l'analyse des traces numériques (motifs d'innovations, dynamiques collectives, morphogenèse des réseaux socio-sémantiques, processus de différentiation, etc.).

Avec la convergence des méthodes de fouille de données textuelles, de l'essor du calcul haute performance et de l'analyse des réseaux dynamiques complexes, se sont développées des approches quantitatives des sciences qui s'intéressent à l'évolution des champs scientifiques et des concepts qu'ils mobilisent. Nous présenterons l'une de ces approches, la reconstruction de la phylomemie des sciences, qui permet de décrire et de visualiser la morphogenèse des champs scientifiques de manière multi- échelle et de suivre les contextes d'usages des termes scientifiques. L'histoire des sciences traditionnelle est principalement celle des ``Grands Hommes'', la reconstruction des phylomemies permet de décrire une évolution en intégrant la multitude des contributions de la communauté scientifique.

Alors que de nombreux philosophes ont théorisé le développement des connaissances scientifiques, nous poserons la question de savoir si ces approches quantitatives sont des objets appropriés pour corroborer ou réfuter les théories en philosophie des sciences.

*Analyse phylomemétique de la recherche sur la nouveauté évolutionnaire entre 1965 et 2015* (**Thibault Racovski** & **Philippe Huneman**)

Au cours des trois dernières décennies se sont multipliées les tentatives de remise en cause ou de modifications du paradigme dominant en biologie de l'évolution établi entre les années 1930 et 1950 et généralement appelé Synthèse Moderne ; la forme la plus récente de cette contestation étant l'appel à la création et l'esquisse d'une Synthèse dite Etendue (Laland et al., 2015; Pigliucci, Müller (éds.) 2010). Ce débat théorique se projette également sur le plan historiographique, notamment à travers l'idée que l'histoire récente de la biologie a été essentiellement écrite du point de vue des vainqueurs, c'est-à-dire du point de vue de la Synthèse Moderne (Amundson, 2005). Le développement de cette contestation coïncide avec l'émergence puis l'institutionnalisation de l'Evo-dévo, un ensemble de recherches multidisciplinaires unifiées par l'idée d'intégrer l'étude du développement biologique à celle de l'évolution, contrastant avec la séparation relative de ces deux domaines instaurée par la Synthèse Moderne.

Le problème de l'apparition des nouveautés ou innovations (novelty, innovation, en anglais) en biologie de l'évolution – soit des traits tels que les ailes des oiseaux ou les pétales des angiospermes, soit des traits qui ne sont pas simplement quantitativement distincts de leurs prédécesseurs - tient une place centrale dans l'argumentation des défenseurs d'une Synthèse Etendue et plus généralement dans le domaine de l'Evo-dévo. Deux assertions sont récurrentes dans cette littérature : l'idée que le paradigme dominant en biologie de l'évolution aurait négligé le problème de la nouveauté ; l'idée que ce paradigme, efficace pour comprendre l'évolution des adaptations, ne possède pas les ressources théoriques nécessaires pour expliquer la nouveauté alors que les méthodes et concepts récents issus de l'Evo-dévo le permettent (Müller et Newman, 2003; Raff, 2000; Wagner, Chiu, Laubichler, 2000).

Pour évaluer ces assertions dont la virulence doit beaucoup au contexte d'affrontement théorique, une histoire détaillée de la recherche sur le problème de la nouveauté est nécessaire. Nous appliquons ici la méthode quantitative de reconstruction de phylomémies (Chavalarias et Cointet, 2013) à la recherche sur la nouveauté de ces 50 dernières années. Cette analyse quasi exhaustive du corpus scientifique permettra de corroborer des hypothèses formulées par de précédents travaux historiques non quantitatifs (p.ex. Love, 2003, 2005) ou par nous-mêmes, telles que : l'hypothèse d'une négligence du problème de la nouveauté avant les années 1980 ; l'hypothèse d'un glissement du concept d' « innovation clé » pris dans le cadre de l'étude des patterns macroévolutifs vers le concept de nouveauté lié à l'étude des mécanismes du développement et de leur évolution ; enfin l'hypothèse d'une domination des études sur les animaux (et surtout sur les vertébrés) par rapport aux études sur les plantes, champignons et microbes. Il s'agira plus généralement de produire une cartographie de l'évolution des concepts liés à l'étude de la nouveauté.

*Une approche par réseaux sémantiques des conceptions internalistes et externalistes en Evo-dévo, vers une Synthèse Evolutionnaire Etendue* (**Wellington Bittencourt**)

Cette contribution vise à élaborer une cartographie du champ conceptuel de l'Evo-dévo (biologie de l'évolution et du développement) à partir de l'analyse de publications avec la méthode des réseaux sémantiques. Ces réseaux sont utilisés comme outils computationnels d'analyse afin de produire une vision détaillée du cadre conceptuel développé dans les livres étudiés.

A travers cette cartographie conceptuelle de l'Evo-dévo nous visons deux objectifs : 1) Intégrer d'une part l'approche externaliste et populationnelle développée par les architectes de la Synthèse Moderne et d'autre part les concepts émergents de l'Evo-dévo relevant d'une approche internaliste et structurelle. 2) Proposer, à partir de notre cadre analytique de la restructuration théorique en cours en biologie de l'évolution, une contribution à l'entreprise collective de création d'une nouvelle Synthèse Etendue.

Le but final de notre travail théorique est d'établir des liens entre l'étude des facteurs structurels déterminant le développement de la forme des organismes et l'étude des facteurs fonctionnels déterminants leur adaptation, ceci afin de contribuer à une compréhension unifiée du rôle du développement dans l'évolution des être vivants.

*L'arbre classificatoire, un outil transdisciplinaire au service de l'analyse conceptuelle* (**Marie Fisler**)

L'arbre classificatoire est une méthode issue de la biologie. Elle y est utilisée pour classer les espèces en fonction ce qu'elles partagent d'original. Pris dans un cadre évolutionniste, ces points communs sont la trace de leurs liens de parenté. Cependant, l'arbre classificatoire n'a pas besoin d'un présupposé généalogique pour être utilisé. Réalisé suivant le principe de parcimonie, il rationalise les partages. Il permet de voir, parmi un ensemble d'auteurs ou d'idées, qui partage quoi avec qui. Ces regroupements permettent de déterminer des ensembles, des « écoles » de pensée parmi ces auteurs, des ensembles d'idées.

En philosophie politique, on distingue souvent des groupes de penseurs : gauche, droite, centre, monarchistes, démocrates, républicains, etc. Cependant, ces groupes ne sont pas rigoureusement formalisés : l'on ne sait pas précisément sur quels ensembles de points communs des idées sont regroupées, ni si les groupes qu'elles forment peuvent s'inclure les uns dans les autres : la pensée citoyenne est-elle une pensée républicaine ? Qu'est-ce qu'une pensée républicaine ? Quels critères permettent-ils de répondre ?

Dans le passé, en Histoire Naturelle, il en était de même : on distinguait d'une part ce qui est vivant, de l'autre ce qui ne l'est pas. Parmi les êtres vivants, certains étaient capables de se mouvoir. Ils ont été appelés « animaux ». D'autres ne le pouvaient pas : ils ont été nommés « végétaux ». Et l'arrangement de certaines espèces était problématique : où classer par exemple les coraux, les éponges ou les unicellulaires ? Des points de contact entre certaines catégories ont alors parfois été créés : les zoophytes, par exemple, espèce mi-animales, mi-végétales.

La méthode cladistique permet ainsi, en créant des ensembles formalisés rigoureux, de savoir précisément qui partage quoi d'original avec qui. Certains groupes, préalablement reconnus par les spécialistes du domaine d'étude, sont reconnus sur l'arbre des idées ainsi réalisé. Mais d'autres de ces groupes peuvent ne pas être retrouvés, et disparaître. D'autres enfin peuvent apparaître, qui n'avaient encore jamais été reconnus en histoire des idées. L'arbre classificatoire et la méthode permettant son élaboration sont ainsi des modes d'étude novateurs des pensées politiques. Mais cette méthode permet également, a posteriori, d'examiner les circulations et échanges d'idées entre les auteurs. Il permet ainsi de reconstruire l'histoire, les enjeux et les déplacements d'un concept.

La communication proposée présentera dans un premier temps cette méthode : comment se lit, comment s'élabore, comment se construit un arbre suivant la méthode de parcimonie. Dans un second temps, deux études illustreront le propos. La première consiste en une classification des pensées politiques en langue française, entre 1793 et 1871. La seconde consiste en une catégorisation des correspondants de Pierre Bayle.

*Classer le monde empirique sans classeurs :*
*Comment concilier réalité de la classification et inexistence des sortes naturelles ?*

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*Introduction*

L'une des fonctions des sciences empiriques est d'offrir une classification des entités qui constituent ou habitent le monde empirique. La biologie est, à cet égard, paradigmatique. Au travers de la classification linnéenne ou de la classification phylogénétique, elle exhibe une pluralité de taxons, des sortes naturelles (natural kinds) hiérarchisées (les espèces, les ordres, les règnes, etc.). La fonction de classification associée aux sortes naturelles fonde d'autres fonctions, la plus importante étant la fonction d'inférence inductive1. L'appartenance d'un individu à une sorte naturelle permet de faire des prédictions sur le comportement de l'individu en question.

La réalité de ces sortes naturelles fait débat, certains philosophes défendant que les classifications et les « classeurs » (i.e. les sortes naturelles) sont des constructions à partir de conventions linguistiques et scientifiques, aboutissant à la position conventionnaliste à propos de la classification et des sortes naturelles. Comme l'écrivent de façon imagée Bird et Tobin (2015, section 1.1.2) : « les classifications des botanistes se suivent pas plus les articulations de la nature que ne le font les classifications des cuisiniers». J'accepterai la thèse selon laquelle les sortes naturelles n'existent pas en la motivant brièvement (section 1.). Je défendrai ensuite la thèse selon laquelle ce conventionnalisme à propos des sortes naturelles est compatible avec un réalisme de la classification (section 2.). Je conclurai en montrant comment cette approche permet d'aboutir à une interprétation élégante du pluralisme taxinomique (section 3.).

*1. Motiver le conventionnalisme des sortes*

Une première raison de trouver le conventionnalisme attrayant, assez commune, est le rejet des essences et, en particulier, des propriétés essentielles. Cette position que l'on retrouve sous différentes formes (par exemple, le conventionnalisme modal en philosophie du langage et des modalités (Sidelle 1989), ou l'anti-essentialisme en philosophie du langage et en philosophie de la biologie (Mayr (1970), Ereshefsky (2010)) implique de rejeter ensuite les sortes naturelles pour la raison suivante : les sortes naturelles dont individuées par l'existence de propriétés essentielles à ces sortes, des propriétés qui sont spécifiques à cette sorte. S'il n'existe pas de propriétés essentielles dans le monde empirique, alors il s'ensuit qu'il n'existe pas de sortes naturelles.

Deuxièmement, une thèse relativement populaire en philosophie de la biologie est le pluralisme taxinomique (Dupré 1993, Ereshefsky 2000, Kitcher 1984, Mishler et Brandon 1987). Selon cette thèse, il existe une pluralité de classifications également pertinentes ou correctes du monde empirique. Le pluralisme taxinomique amène à reconnaître l'égale pertinence ou exactitude de classifications en compétition : on peut penser ici en particulier à la classification linnéenne et à la classification phylogénétique. Or, s'il existe une pluralité de sortes naturelles menant à des classifications incompatibles, il semble que ces dernières soient de nature conventionnelle et qu'elles ne recoupent pas les articulations objectives du royaume empirique.

*2. Evacuer les sortes naturelles*

Le conventionnalisme de la classification implique qu'il n'existe aucune structure objective qui ne puisse être dévoilée par l'enquête empirique (anti-réalisme scientifique). Cette implication me paraît intenable : il est important de développer une conception des classifications qui soit compatible avec l'existence de classifications objectives (mind-independent) de la réalité. Le conventionnalisme des sortes est ainsi acceptable dès lors qu'il n'implique pas un conventionnalisme de la classification. Tout le problème consiste alors à récupérer les fonctions de classification et de généralisation inductive traditionnellement attribuée aux sortes naturelles, en proposant un nouveau dispositif théorique qui permette d'expliquer le fonctionnement des inférences scientifiques en l'absence de toute référence à des sortes naturelles.

Ce problème peut être formulé de la manière suivante : de par son appartenance à une sorte naturelle S, un individu x possède nécessairement un ensemble de propriétés P, puisque ces dernières sont celles de tout membre de S. La connaissance de l'appartenance d'un individu x à S permet donc d'inférer que, nécessairement, x possède l'ensemble des propriétés P, et ainsi de faire des prédictions sur le comportement futur (ou passé) de x. D'un point de vue métaphysique, la fonction inférentielle est donc localisée dans la combinaison de l'instantiation des propriétés P par S, et de l'appartenance de x à S. Si l'on élimine S, conformément à ce schéma, on perd ipso facto sa fonction inférentielle – ce qui est pour le moins fâcheux.

Ma suggestion consiste à fonder la fonction inférentielle dans l'existence de relations directes entre les propriétés P, sans la médiation de S. Si l'individu x possède une propriété P1 membre de l'ensemble P alors, nécessairement, l'individu x possède les autres propriétés Pn de l'ensemble P. La connexion nécessaire entre P1 et les autres Pn, ici, ne découle pas de l'appartenance commune de P1 et des autres Pn à une sorte naturelle S. La connexion nécessaire est le résultat d'une relation de dépendance ontologique connectant directement les propriétés en question. Cette nouvelle description ontologique permet d'évacuer l'existence des sortes naturelles et des propriétés modales. Certes, il existe toujours de la modalité dans cette ontologie, puisque les relations de dépendance ontologique sont des dispositifs modaux, mais ces dispositifs sont plus faibles, évitant de postuler des essences, des propriétés modales et des sortes naturelles. Il existe des propriétés, et certaines de ces propriétés sont connectées par une seule et unique relation de dépendance ontologique (causale ou nomique par exemple, suivant la théorie de la nécessitation naturelle adoptée). (Note : Il est possible de développer une explication équivalente de co-variations systématique en substituant à la relation de dépendance ontologique une relation de dépendance probabiliste primitive. Il faut donc introduire deux relations de dépendance distinctes entre les propriétés pour étendre l'explication aux phénomènes probabilistes et statistiques). A cet égard, ces relations permettent de réaliser une économie ontologique intéressante en évitant de postuler l'existence de créatures métaphysiques exotiques telles que les sortes naturelles.

*3. Une explication du pluralisme de la classification*

Si le débat sur les sortes naturelles se fait vif en biologie, notamment avec le conflit entre la classification linnéenne et la classification phylogénétique, la biologique n'est pas la seule discipline qui doive s'affronter au problème de la classification. Mauro Murzi (2007) et Stéphanie Ruphy (2013), par exemple, ont montré récemment que même au sein des sciences physiques, il existe des domaines clairement pluralistes. En effet, en astrophysique, les étoiles sont regroupées diversement au sein d'une pluralité de classifications suivant qu'on les classe en fonction de propriétés accessibles à partir du spectre visible (type spectral, classe de luminosité), ou en fonction de propriétés structurelles accessibles par l'analyse des signaux ultraviolets ou infrarouges (comme la température).

Le modèle ontologique que je propose permet d'adopter le pluralisme de la classification, en physique comme en biologie, sans pour autant verser dans la thèse selon laquelle le monde empirique n'admet pas de classifications objectives. Les catégories sortales scientifiques ne dénotent pas des sortes naturelles mais des associations de propriétés naturelles dont la co-variation systématique ou statistique s'explique par l'existence de relations de dépendance ontologique (causales ou nomiques) entre ces propriétés.

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## *Peut-on parler d'incommensurabilité structurale ?*

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Mots clés : incommensurabilité, réalisme structural, continuité, révolution scientifique, changement scientifique

En 1962, au sein de deux publications indépendantes, Thomas Kuhn et Paul Feyerabend suggèrent l'idée provocatrice selon laquelle les théories scientifiques qui se succèdent au cours de l'histoire sont « incommensurables ». Une telle affirmation leur vaudra de nombreuses critiques. On leur reproche notamment de promouvoir une vision irrationnelle du changement scientifique, faisant la part belle au relativisme; ce qui amènera Theocharis et Psimopoulos (1987) à les qualifier de « pires ennemis de la science ». Un demi-siècle plus tard, la thèse de l'incommensurabilité a donné lieu à une variété de discussions, conduites en des termes différents. C'est que les théories peuvent être incommensurables en plusieurs sens. Deux types d'incommensurabilité sont, en particulier, distingués au sein de la littérature contemporaine[1] : d'une part, l'incommensurabilité sémantique, due au changement de signification des termes théoriques et qui remet en cause la possibilité de comparer les théories au niveau de leur contenu; d'autre part, l'incommensurabilité méthodologique, due à l'absence de normes d'évaluation fixes et objectives, qui remet en cause la rationalité du choix entre théories scientifiques concurrentes. Cependant, quel que soit l'objet de la discussion, l'enjeu reste le même : (ré)établir une continuité entre les théories successives.

Si les discussions perdurent, un point, au moins, semble définitivement acquis : l'existence d'une continuité structurelle, ou mathématique, entre théories successives. Cette thèse a, notamment, été défendue par John Worrall, dans un article de 1989. Son idée est la suivante : si certains éléments d'une théorie sont abandonnés au cours du changement scientifique, la majeure partie du contenu mathématique est, quant à elle, conservée. On retrouve, en effet, des équations identiques d'une théorie à une autre et il est souvent possible de déduire du formalisme de nouvelles théories, le formalisme de théories plus anciennes, reproduisant les prédictions de ces théories dans les cas limites où certaines quantités peuvent être négligées[2]. De ce fait, Worrall défend que la structure logico-mathématique portée par les équations[3] se conserve au cours du changement scientifique ; les nouvelles théories incorporant la structure mathématique des théories qui les précèdent.

Le but de mon exposé est d'interroger l'existence de cette continuité structurelle. Je montrerai qu'en dépit d'arguments convaincants en sa faveur, elle peut être remise en cause. On ne peut donc l'accepter sans une étude plus précise et détaillée des relations logico-mathématiques qu'entretiennent les théories successives, ni sans une analyse précise de ce que l'on entend par « structure mathématique ».

Je commencerai par souligner le fait que cette continuité de structure possède de nombreux arguments forts en sa faveur. D'une part, elle permet d'expliquer le succès prédictif des théories, par-delà les changements et les révolutions scientifiques[4]. D'autre part, elle semble être la plus à même de rendre compte de la pratique scientifique contemporaine[5].

Je défendrai, néanmoins, dans une seconde partie, qu'aussi bien étayée qu'elle puisse être, cette continuité de structure peut être rejetée en faveur d'une incommensurabilité, que je qualifierai de « structurale ». Je mettrai, ainsi, en évidence l'existence possible d'une discontinuité au niveau même des structures formelles des théories. Deux arguments en particulier seront dégagés. Je montrerai, dans un premier temps, que l'idée selon laquelle une théorie remplacée constitue un cas limite de la théorie qui la remplace présente un tableau simplifié et trompeur des relations entre théories[6]. Dans un second temps, je rejetterai la possibilité d'une distinction nette entre structure formelle ou mathématique d'une théorie d'un côté, et contenu ou interprétation théorique de l'autre[7].

[1] Voir, par exemple, Sankey, H. et Hoyningen-Huene, P. (2001), ou encore Soler L. (2004).

[2] Pour exemple, on peut citer la théorie de la Gravitation Universelle de Newton, qui est généralement vue comme un cas limite de la théorie de la Relativité Générale d'Einstein, et qui permet de reproduire les prédictions de cette dernière lorsque les phénomènes étudiés ne font pas intervenir des vitesses proches de celle de la lumière.

[3] La structure mathématique exprimée par les équations de la théorie quand les termes de ces équations ne sont pas interprétés.

[4] On retrouve ici la version structurale de l'argument du miracle présentée par Worrall (1989).

[5] On constate aujourd'hui en astrophysique et en cosmologie, qu'une part importante de la recherche, ainsi que des nouvelles connaissances, qui devraient en principe être exclusivement produites à partir de la théorie « einsteinienne » de la gravitation, le sont pourtant, en même temps, en partant d'un cadre « newtonien ». C'est précisément le cas chaque fois que les phénomènes étudiés ne font pas intervenir des vitesses proches de celle de la lumière, rendant superflue l'utilisation des équations de la Relativité Générale. L'expérience GRANIT (Transitions GRAvitationnelles Induites du Neutron) - tel que je le montrerai - constitue un cas particulièrement significatif de cet entrelacement des paradigmes au niveau théorique, étayant l'idée d'une continuité structurelle entre les théories.

[6] Je m'appuierai, ici, sur l'analyse menée par Joshua Rosaler (2013) dans sa thèse de doctorat portant sur la relation entre théories en physique et notamment sur la notion de réduction inter-théorique.

[7] Je m'appuierai ici, bien qu'à des fins contraires, sur la critique opérée par Psillos de la distinction entre la nature d'une entité, ou d'un processus, et sa structure (1999).

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*Why our best theories of perception lead to anti-reductionism*

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Basic percepts and observation sentences, such as "the voltmeter is at 7A", provide the ground truth for realistic theories. Reduction is the second backbone of these theories linking, for example, neuroscience to physics.

First, we will show, by mathematical proof, that reduction is impossible if ontology is complex. We will provide a toy example which illustrates this point: a hypothetical animal has a sensor, which reacts to red and green light only. When red light is presented, the animal deterministically lifts the right back limb. For green light, the left one. Inputs and outputs are causally linked by a "brain" with just a few (binary) neurons. Even though inputs, outputs, and brain activity are fully available for millions of observations of a "scientist", it is impossible to decode the output from the brain activity. Hence, neither sensation nor motor actions can be reduced to the underlying neural activity- even though input and output are perfectly correlated.

Next, we outline the challenges any perceptual system needs to meet. For example, the light (luminance), which arrives at a photo-receptor of the retina, is a combination of the light shining on an object (illuminance) and the material properties of the object (reflectance). For a given luminance value, there are infinitely many illuminance-reflectance pairs, giving rise to this luminance value (an ill-posed problem). Hence, perception cannot be based on the raw input values. Second, we show how perceptual systems can solve such ill-posed problems. One conclusion of this analysis is that perception is inherently subjective, i.e., the metric of the perceptual system is not isomorphic to the metric of the physical space. We will argue that perception has evolved subjective metrics to exactly cope with the abundant complexity of the physical, non-reducible external world.

In conclusion, we propose that reduction is neither possible nor desirable.

**Jeudi matinée 30 juin / Thursday morning June 30**
Salle / Room 315.1

*Pluralist challenges to a science-based metaphysics*

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A widespread motivation for a science-based metaphysics is the idea that since metaphysics aims at getting objective truths and since science is precisely in the business of providing objective knowledge about the world, metaphysics should be very close to science, in one way or another. But is science really in the business of providing the kind of objective knowledge that metaphysicians value and aim for, that is, knowledge about 'the ultimate structure of reality' or about 'how the world really is'?

A naturalized approach (dear to proponents of a science-based metaphysics) to this question recommends looking at the actual state of science, and a commonly acknowledged feature of this state today is its disunity. Indeed, while the philosophy of science has for a significant part of its professionalized existence waved the (motley) banner of the unity of science, few would deny today that the philosophical tide has clearly turned in favour of the plurality of science.

My aim in this talk is to investigate which parts of the multifaceted project of a science-based metaphysics should be revised or even dropped in light of scientific pluralism. I will investigate in particular what is left of ontological objectivity in a pluralist, model-based view of science, when scientific knowledge is taken as inherently perspectival (e.g. Giere 2006) and when science can only provide us with a collection of idealized ontologies (e.g. Teller 2004). I will suggest at the end that a valuable aim of a science-based metaphysics is not so much to get at 'objective truths about the world' (a lost cause given the perspectival nature of scientific knowledge) than to grasp, in a Neo-Kantian (or Friedmanian) vein, the structures and external constraints of our modes of production of scientific knowledge and objectivity.

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*Mechanisms meet structural explanation*

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One of the major strengths of the New Mechanistic philosophy (Glennan 1996; Machamer et al. 2000) is the virtually ubiquitous application of mechanistic reasoning in scientific practice, which also makes mechanistic explanation (ME) plausibly the most successful account of scientific explanation currently available. However, there are some domains where mechanistic reasoning, and ME with it, drastically loses its predominance in the scientific enterprise. This is often the case in the philosophy of fundamental physics. On the other hand, those phenomena which are not explained through a ME are not necessarily unexplainable brute facts of nature: mechanically brute phenomena might be nonetheless explained by science. One partial aim of this paper is to give flesh to this claim by showing that some mechanically brute phenomena in the domain of fundamental physics are structurally explained.

Since its origin in Hughes' work (1989b, p. 175), a central role in the definition of structural explanation (SE) was played by the claim that SE is independent of any assumption about what types of entities and what types of processes lie within the theory's domain. On the one hand, this feature received an essentially anti-metaphysical connotation; on the other, it was also used as the key to understand the non-causal character of SE. Some central features of SE have therefore been shaped on the elaboration of such a characterization. The second partial aim of this paper is to reconsider Hughes' original characterization of the non-causal character of SE as a contrast with a merely metaphysical kind of causal explanation.

The paper's structure is as follows. In Sect.1, I introduce the issue of the limited applicability of ME in fundamental physical theories. Probably the mechanistic philosopher who has devoted the most attention to such problem is Glennan (1996, 2000, 2010, and more recently Kuhlmann and Glennan 2014), so his framing of the issue deserves a special attention. Independent of more general considerations, in Sect. 2 I provide two illustrating examples of phenomena that are typically seen as successfully explained by our best fundamental physical theories and yet, I will argue, are mechanically brute. More specifically, such phenomena are structurally explained. In Sect. 3, I elaborate on the contraposition between SE and ME and investigate the consequences of such contraposition for SE. It could be argued that the two accounts of explanation refer to antithetical general views of scientific explanation and cannot therefore be both true. To counter such a conclusion, in Sect. 4 I underline the common features of SE and ME and show that they might be assimilated by the same theory of explanation. More exactly, ME and SE can be both assimilated by an epistemic, model-based account of explanation that work by providing information about patterns of counterfactual dependences.

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*Mathematical features and ontic commitments in topological explanation*

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Many philosophers consider topological explanations to be mathematical (Huneman 2010, 2015; Jones 2014; Batterman and Rice 2014). In a very obvious sense they are mathematical, i.e. their explanans doesn't cite any causes, but rather cites some mathematical properties. However, there are philosophers who argue that topological explanations are not truly mathematical because the actual causal details of systems that are represented in topological models determine the topology (Bechtel and Levy 2013; Craver 2016). I call this objection the Ontic Commitment Thesis (OCT). Based on the OCT they claim that if topological explanations are explanatory they have to be ontic, but if they are ontic they are not sui generis mathematical explanation, but merely a variety of mechanistic ones.

I argue that topological explanations are in fact mathematical and that they are non-causal, i.e. they are not ontic and because of that they are distinct from other explanatory strategies such as mechanisms.

*What are topological explanations and what makes them distinctly mathematical?*

Explanations based upon topological models are mathematical in a very specific sense. In mathematical explanations the explanandum is mathematically derived from the model or a set of equations, but not from the actual causal interactions, as is the case in the ontic account of mechanistic explanation (Salmon 1984; Craver 2013), and to that effect the explanans of topological explanation is always a description of some mathematical property or sets of properties (Huneman 2010, 2015; Lange 2013, 2014; Pincock 2014). Famous examples of mathematical explanations are the Loddtka-Voltera equations or the cicada life cycle explanation.

Topological explanations are distinctly mathematical in exactly this sense. To understand better this claim, I will now give an example of topological explanation.

Topological explanations are derived from network or graph models. Such models are network or graph idealizations of real systems, which normally have two elements: vertices or nodes and edges or links/connections. A graph is a set of vertices connected through edges (Newman 2010). Vertices in different fields of investigation may represent different things. For example, in neuroscience vertices might represent neurons or brain regions and the edges, the synapses or functional connections between the regions.

The very first and influential topological model is the Watts and Strogatz (1998) small-world network model.

This model was built in such a way that starting from a ring lattice it has n vertices and k edges. The structural properties of such a graph are quantified by using its characteristic path length, which measures a typical separation between two vertices in the graph, i.e. how many edges have to be traversed in order to reach a vertex n2 from the vertex n1, which is expressed as L(p) and the clustering coefficient C(p) of a network or a graph, which measures the cliquishness of a typical neighbourhood of nodes, and to that effect it is a local property.

The small-world networks are then characterized by low L(p) values, which is due to a few long-range edges. Such 'short-paths' connect nodes that would otherwise be much farther apart and in effect shortening the path lengths between the whole neighbourhoods, and, neighbourhoods of neighbourhoods.

These features of network topology then allow us to understand the system dynamics as a function of its structure. For example, infectious disease will spread more rapidly though a population which instantiates a small-world topology, precisely because of these specific topological properties, i.e. the L(p) will be low and the C(p) will be high. This pattern of explanation will work for many types of real systems with small-world topology and for variety of different explananda, e.g. it will explain computational or metabolic economy, synchronicity, stability, robustness, resilience and many others.

But one might object that the actual causal interactions in a system determine its topology, and that to that effect it is possible to construct several different networks with exactly the same topological structure using different choices for edges. Suppose we get a small-world structure using relationships of familial relatedness, or predation relations, or the citation relations. Each one of these graphs have the same topological structure, say they are small-world topologies. Even though they have the same topology one could argue that topology isn't enough for explanation, we need to know what gives rise to this specific topology (and that varies across systems) in order to be able to use topology as an explanans (Craver 2016). This is the Ontic Commitment Thesis (OCT).

But this would be a misunderstanding, because the topological explanation is always given in terms of topological features, not in terms of how the system arrived at having them. Choosing what the nodes and edges will represent in a network is postulated when constructing a network, and is in a way rather arbitrary, i.e. many different networks can be constructed from the same system, for example the brain can be considered as a network of neurons as well as a network of brain regions (depending on the scale). Or depending on methodology for obtaining data, nodes in a network may represent surface sensors (in the EEG cap), or for fMRI data nodes can represent single voxels or aggregates of voxels that in effect represent anatomical or functional areas in the brain (Achard et al. 2006; Power et al. 2011). But regardless of the target system, the explanation is always given in terms of topological properties and not in terms of the decision what the network represents because such decision is a part of the explanandum.

This sort of an objection seems to be falsely presenting the exact relation between explanans and explanandum in topological explanation. Let's remind ourselves that "…the explanation includes an explanans, and some relationship or situation between the explanans and explanandum. The explanation explains by situating the explanandum to the explanans via that relationship." (Andersen 2015).

The OCT instead, shifts the focus to different explananda, because the questions why system has some specific topological properties and why given those topological properties it behaves in certain ways are separate issues. Thus the topological explanation don't require the OCT to be fully explanatory, and because of that it should be considered distinct from mechanisms.

**Jeudi matinée 30 juin / Thursday morning June 30**
Salle / Room 319

*Towards a development of the metaphysics of primitive stuff*

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In recent years the debate about scientific realism has been put forward by research on the ontological implications of quantum mechanics. My main focus will be the metaphysical framework of Moderate Ontic Structural Realism (Esfeld and Lam 2008) and Primitive Stuff Ontology (Esfeld et al. 2013, 2014). This research approach has many points in common with other proposals, on the one side Radical Ontic Structural Realism (French 2014), on the other Primitive Ontology (Dürr, Goldstein and Zanghì 2013, Allori 2013).

ROSR is a philosophical position motivated by science (especially quantum mechanics). Its promoters embrace a naturalized metaphysics, inspired and guided by scientific investigation, which establishes a limit between what could and could not be said about nature. Using French's terms (French 2014), one should adopt a "Viking approach" to metaphysics: get to the armchair philosopher's village, take what is needed and leave. The core claim of ROSR is that all there is in the world are structures (physical laws, mathematical structures and symmetries), with no need of objects to instantiate them. The PO was introduced (DGZ 2013) as a general philosophical framework initially for Bohmian Mechanics – a physical theory that proposes a clear solution to the problems of Quantum Mechanics – and then developed into a more general approach to other interpretations of quantum mechanics and to scientific theories in general (Allori 2013). Seminal work on the subject has been done by Bell (Bell 1975) with his theory of local beables, though in both cases the metaphysical aspects are not spelled out in a rigorous philosophical manner.

PSO+MOSR stand somewhere in between, analysing the more subtle philosophical consequences of the PO approach as the absence of intrinsic properties at the fundamental level, investigating the ontological status of spacetime, and clarifying the possible meaning of the wavefunction as a nomological entity. Against ROSR, they argue that the abandonment of objects as relata is not necessitated by physics, and highlight the overlooked difference between spatial and dynamical structure for the role they play in ontology and explanation.

My aim in this talk is to propose an elaboration and clarification of the metaphysical framework of PSO+MOSR in the light of recent developments in analytical metaphysics, using a more diplomatic approach than a Viking's while keeping an eye on the science.

The guiding ideas are the following:

- Parsimonious ontology: there is only one kind of substance, a network of spatial relations between matter points (Esfeld et al. 2014, Esfeld and Lam 2008). The relations and relata are interdependent, and there is no priority of one over the other.
- Fundamentality: chains of asymmetric ontological dependence must terminate (Shaffer 2010).
- Grounding: ontological dependence is spelled out in terms of metaphysical grounding.
- Relationalism about space and time: the Leibniz way, space as the order of coexistence and time as the order of succession of configurations of what coexists.
- Humeanism about laws: physical laws as the most simple and informative descriptions of a dynamical system.
- Truthmaker Principle: for every proposition *p* there exist some *x* such that *x* makes *p* true.

First, I will specify the difference between relata/relations of the fundamental network and relata/relations as they are usually conceived in the metaphysical debates, especially those concerning identity conditions and priority of relata over relations. Once the ground is settled, I will define in more details the ideas just presented and clarify the links between them. Then I will proceed by putting them together and try to tie them into a coherent metaphysical picture, and show the advantages of such a theory over ROSR and PO. I will consider the equations of a physical theory as truthbearers (something inherently representational in character). The thesis that I will try to develop is that every truthbearer is made true by a dynamical pattern, and that every dynamical pattern must have a proper part, which is metaphysically grounded in the fundamental network; this proper part is the primitive ontology, while the other part is the dynamical structure that captures the change in the fundamental network. Articulating this thesis with the language of analytical metaphysics, I intend to show how the different parts fit together nicely and agree with the physics.

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*Einstein, Millikan and quantum theory: the evidential import of the photoelectric effect*

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Most people now take for granted the importance of Einstein's explanation of the photoelectric effect for the development of quantum theory. However, when Einstein first put forth the hypothesis of light quanta in 1905, the scientific community's reaction was not enthusiastic. One of the factors contributing to this was likely the lack of precise experimental data available for the phenomena Einstein was addressing. When Millikan performed his experiments on the photoelectric effect almost a decade later, the impact was significant: the results were in striking agreement with Einstein's predictions, and scientists began to take the light quanta hypothesis much more seriously. Indeed, Einstein was awarded the Nobel Prize in 1921 not for his work on relativity, but for his account of the photoelectric effect. A philosophical analysis of Millikan's work can thus provide insight into how experiments can guide developing theories. In this paper, I argue that Millikan's work did not necessarily support the existence of photons, but nevertheless contributed significantly to the development of a quantum theory. This case study helps us better understand how a newly developing theory can be constrained and shaped by experiments during a transitional period.

In 1905, Einstein put forth the hypothesis that light is composed of discrete quanta of energy. While the primary motivation for this postulate arose in analogy with the kinetic theory of gases, he also considered how this hypothesis might account for phenomena such as the photoelectric effect, where `cathode rays', or streams of electrons, are emitted from a metal surface exposed to incident light. From the hypothesis, Einstein was able to derive an equation that entailed a relationship between V, the potential difference required to stop electrons from escaping the surface, and v, the frequency of the incident light. Specifically, he hypothesized that the V-v relationship would be linear and would have slope h/e, where h is Planck's constant, and e is the charge of an electron. At the time, the experiments on the photoelectric effect were only precise enough for Einstein to conclude that his prediction was satisfied to within an order of magnitude.

Millikan's 1916 determination of the value of h required several steps, one of which provided the verification of the V-v relationship. This work is now retrospectively taken to be good evidence for the photon theory of light, as evidenced by reports of the experiment in textbooks, and in statements by Millikan himself in retrospective accounts. Yet, at the time, Millikan was one of many to disagree with Einstein about what the underlying theory explaining the photoelectric effect might be. In 1916, Millikan was ready to accept Einstein's predicted linear relationship, yet still rejected the original explanation for that equation, namely, the hypothesis of light quanta. In fact, he explicitly discussed an alternative to the light quanta hypothesis that would explain why Einstein's equation was physically instantiated.

I argue that this is because the experimental results do not differentiate between alternative explanations of why the linear relationship holds. If we were to consider a general hypothesis that posited the existence of light quanta, we would still have to make reference to a more specific posit such as Einstein's equation in order to derive the linear relationship being tested. It would also be plausible to derive the linear relationship with an alternative account of the physical underpinnings of this behaviour. However, any such account would require the inclusion of the specific equation given by Einstein. Thus, the experimental results do not differentiate between the light quanta hypothesis and any other account of the mechanisms governing the behaviour of energy, but does support Einstein's equation.

It is also significant that the primary purpose of Millikan's paper was to use experiments on photoelectric phenomena to determine the value of h as accurately and precisely as possible. Millikan characterizes the inquiry as one that would allow him to ``assert whether or not Planck's h actually appeared in photoelectric phenomena as it has been usually assumed for ten years to do'' (1916, p. 360). He discusses the work of other scientists in terms of their determinations of h. Thus, we can infer that in Millikan's view, what scientists could really take away from Einstein's 1905 paper was the applicability of the parameter h to a new domain. This fact has been recognised by several historians of science, but it is significant for philosophers as well. I argue that Millikan's goal here can be understood as an attempt to provide a unification of photoelectric phenomena and blackbody radiation phenomena, by using the experimental results to provide information about the theoretical parameter h that is crucial in both domains.

It is thus misleading to claim that at the time Millikan's experiment was conducted, the hypothesis of light quanta was clearly vindicated, either historically or epistemically, even if we think it provides such evidence now. However, this work did support the importance of incorporating h into the description of the behaviour of energy in this domain, as well as constraining its numerical value. I conclude that Millikan's experiments contributed to the development of quantum theory by requiring specific quantum elements to be present in any future theory, but not by directly supporting the hypothesis of light quanta.

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## *We think, they thought*

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Scientific realism, the thesis that current successful theories are probably approximately true, is threatened by the pessimistic meta-induction (PMI), according to which realism is undermined by the history of science, which is full of theories that were once empirically successful but later refuted. Many realists have responded to the PMI by arguing that present theories are better than past theories, a fact which blocks the PMI. This response comes in different varieties: current theories enjoy more success than past theories, they result from better methods than past theories (Devitt 2011, Roush 2010), they are more unifying than past theories (Doppelt 2007), and so on.

Against this response the anti-realist can offer the following counterargument: people in the past could have reasoned in exactly the same way as the realist does today: “Our theories are more successful (result from better methods, are more unified, …) than past theories, and this difference blocks the PMI”; but this reasoning would have proven wrong by the theory refutations that subsequently ensued, and hence we should conclude that the above reasoning of realists today in response to the PMI also fails (Wray 2013). I aim to analyze this counterargument and show its limitations.

I start by noting that the imagined reasoning of people in the past is not exactly the same as the reasoning of realists today; rather there is a difference, namely a difference in success (I focus on the version referring to success.). Hence, the reasoning of people of the past is only analogous to the reasoning of the realists today; it proceeds upon lower levels of success. This means that the counter-argument of the anti-realist has to be supplemented by an inductive step from past to present, from the premise that the reasoning of people in the past failed to the conclusion that the reasoning of current realists fails. Furthermore, the force of this inductive step derives entirely from the subsequent theory refutations, because they are responsible for the failure of the reasoning in the past. It follows that the counterargument of the anti-realist boils down to a version of the PMI, namely an extrapolation of theory failure along degrees of success from past to present levels of success, or so I argue in my pa-per. In other words, the counterargument, which is third-level in the sense that it refers to pieces of reasoning that refer to theories that refer to the world, cannot have more force than this version of the PMI, which is second-level in the sense that it refers to theories that refer to the world.

Interpreted in this way, as an extrapolation of theory failure along degrees of success, the counter-argument of the anti-realist is easier to assess than in its original form. To assess it we have to deter-mine the pattern of change and stability among the respective best theories in the history of science up to the present, where this pattern should not be gauged by time, but by degrees of success (or improvement of methods, or degrees of unification). Having determined the pattern, we can form a judgment about what to project into the future: theory change, theory stability or neither. Once we have formed a judgment in this way, the counterargument of the anti-realist no longer has any force. When it is presented in its original form, “past people could have reasoned in the same way, but look what subsequently happened”, we can reply that the entire force of the counterargument originates from “what subsequently happened” and we already looked at what subsequently happened when we looked at the whole pattern of stability and change in the history of science and formed a judgment about the future development of science.

Finally, to reach such a judgment it is plainly relevant how long – in terms of increase in success, not in terms of time – current theories have been stable in the past. I close by offering some relevant observations suggesting we should project theory stability into the future.

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*Why Bohmian non-locality is not a problem for us (classical objects)*

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Bohmian mechanics is a realistic interpretation of quantum mechanics, that is, it supplies the standard mathematical framework with a clear ontology. Within Bohmian mechanics we can tell a physical story about the actual behavior of the quantum systems, while within the standard framework we can just calculate the predictions for the measurement outcomes. A Bohmian system is described by a configuration of particles and a wave function. As time evolves, the particles follow continuous trajectories in physical space, whose motion is guided by the wave function (evolving itself according to the Schrödinger equation).

The basic ontology of Bohmian mechanics is, thus, rather simple: particles that follow continuous trajectories in space through time. This seems to be ideal if we want to recover the classical world of everyday experience, whose basic ontology also reduces to particles that follow trajectories in space. However, Bohmian trajectories have a striking and novel feature respect to the classical ones: they are highly non-local, i.e., given a non-factorized N-particle system, the velocity of one particle depends from the positions of all the other particles in the configuration. This fact permits to account (together with the quantum equilibrium hypothesis) for the predictions of quantum mechanics, but brings with it an image of the world that is quite different from the classical one, where different systems seem to behave independently each other. It has been argued, indeed, that this novel feature of Bohmian mechanics cannot be compatible with the ordinary experience of the physical world, and that the non-local Bohmian trajectories have to be rejected if we want to maintain a realistic interpretation of nature.

I want to show, on the contrary, that non only Bohmian non-locality is necessary (according to Bell's theorem, every quantum theory that want to match with the empirical predictions has to be non-local), but that it is a crucial ingredient in order to explain the transition from a holistic dynamics (quantum world) to a local dynamics (classical world).

The scheme is the following one: in a realistic situation, a Bohmian system interacts with an external environment, that is, an external particle (e.g., an air molecule, a photon, a cosmic ray, …). This leads to entanglement between the system and the external particle, which in turn creates a superposition of spatially separated channels in the total wave function (system + environment). However, the system particle (X) and the external particle (Y) will enter just one of these channels. If the different channels have disjoint supports in configuration space (that is realistically achieved through a gradual process after many scattering interactions), then the dynamics of X (Y) will be guided just by the corresponding branch of the wave function in which the particles has entered before (say, ?_s(?_E)). Under these conditions, we can call ?_s the effective wave function of the system and ?_E the effective wave function of the external particle. So, the wave function of the system and that one of the environment have been "effectively factorized": the dynamics of X is only guided by ?_s and the dynamics of Y only by ?_E. It is worth noting that this is not a real factorization: the total wave function is still represented by a superposition of different states, but all these branches (the ones different from the effective wave functions of the two systems) do not affect anymore the dynamics of the particles X and Y. We, thus, have entered a local dynamical regime, since the trajectory of X does not depend from any external particle or any external branch of the wave function (and the same for Y).

The emergence of the effective wave functions for Bohmian subsystems is, thus, the turning point for the quantum to classical transition, and the Bohmian non-locality (mathematically expressed in the entangled state) is exactly what we need in order to accomplish that. The idea to recover an effective factorization through the interaction with environment dates back to Bohm & Hiley (1987), and it is now investigated in decoherence theory. Finally, we will seek to clarify the relationship between Bohmian mechanics and decoherence, finding out how the latter can be of a great help for the former in the quantum to classical connection.

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John S. Bell (2004) disagreed that quantum mechanics is an operational theory. Quantum mechanics cannot be solely about measurements and observations because this attitude leads to the measurement problem. Therefore he postulated certain objects which are supposed to exist independently of any observations. He calls these objects beables. And among those beables there is a distinct class: the local beables. The local beables are defined on bounded regions of space-time, and they directly generate all measurement results. Once you have them you have transformed quantum mechanics to a GRW collapse theory or to Bohmian mechanics.

Later, Dürr, Goldstein, and Zanghi (2013) formed the notion of a primitive ontology. The primitive ontology almost coincides with Bell's local beables; it adds the requirement that the objects are primitive. And Dürr, Goldstein, and Zanghi not only emphasized that quantum mechanics must postulate a primitive ontology, but they require that every physical theory from quantum field theory to quantum gravity has to do so, too.

I intend to update the notion of a primitive ontology to a primitive stuff ontology. The problem I see with a primitive ontology is its silence about properties. It's not clear whether the elements of a primitive ontology need to have properties in the first place. And if they happen to have properties the meaning or status of properties is not spelled out. Consequently, I would like to separate the objects from properties right from the outset: The primitive stuff ontology consists of propertyless primitive objects in three-dimensional space.

The most familiar primitive stuff are particles moving in Euclidean space. The particles are intrinsically identical and can only be distinguished by their location. Therefore, every per- mutation is physically and metaphysically indistinguishable. And this must be represented by the mathematical representation as well. A configuration of particles has to be written as a set {Q1, . . . , Qn} since the standard representation as an n-tuple (Q1, . . . , Qn) distinguishes the order of the particles.

In essence, physics is about the motion of primitive stuff, while one task of metaphysics is to argue whether properties exist and if they exist to explain what they are. Another task of metaphysics is to work out the fundamental ontology of the world, which is defined to consist of all primitive entities that are the building blocks the entire ontology.

The measurement problem is not solved by properties alone but by the postulated objects. Nevertheless, properties may explain why the objects behave as they do. I argue that physics only support dynamic-nomological properties; categorical properties are not part of physics. In the current philosophical literature, there are three big theories discussed which differ in the status of laws of nature and properties: Humeanism, primitivism about laws, and dispositionalism.

All those theories have in common that they postulate primitive stuff; the role of properties is different. Humeanism introduces categorical properties. Dispositionalism introduces dispositional properties. A primitivist about laws can dispense with properties because the laws are the dynamical efficacious entities. Ned Hall (2009) proposed a Neo-Humean version of Lewis's Humeanism by abandoning categorical properties. On the fundamental level only primitive stuff plus spatiotemporal relations remain.

In sum, the notion of a primitive stuff ontology helps us in forming the following taxonomy:

- Lewis's Humeanism: fundamental ontology = primitive stuff + local qualities + spatiotemporal relations.
- Neo-Humeanism: fundamental ontology = primitive stuff + spatiotemporal relations.
- Primitivism about laws: fundamental ontology = primitive stuff + laws of nature + space-time.
- Dispositionalism: fundamental ontology = primitive stuff + intrinsic properties + space-time.

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*Particles creation and annihilation: a Bohmian approach*

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Physics has always been concerned with questions regarding what are the ultimate constituents of matter and how they behave and interact. The Standard Model (SM) of particle physics is an answer to these questions, and nowadays is the most successful physical theory at our disposal. This model explains the fundamental structure of matter in terms of elementary fermions interacting through bosonic fields and comprehends three of the four fundamental forces in Nature: the electromagnetic, the weak and the strong interactions; only gravitational effects are not taken into account. Furthermore, its predictions have been corroborated with an extreme degree of accuracy, and recently remarkable experimental evidence for the existence of the last ingredient of the SM, the Higgs boson, have been obtained.

The SM is a Quantum Field Theory (QFT), in the sense that QFT is the mathematical framework in which SM is written. Thus, it provides an ontology in terms of fields, and it is by construction a unification of the axioms of quantum mechanics and special relativity. (It is worth noting that in experimental situations, in order to produce new particles from collisions, energies are needed to be at least as great as the rest masses of the produced particles, thus relativistic requirements must be necessarily taken into account. Moreover, SM predicts the existence of antiparticles which come from the negative solutions of the Dirac equation as consequence of the relativistic relation E=±?(p^2 c^2+m^2 c^4 ) present in it. These are only two of the several reasons according to which it is not possible to dismiss relativity in QFT.)

Despite of these significant triumphs, this theory inherits several conceptual problems that plague the standard interpretation of Quantum Mechanics (QM), such as the measurement problem or the role of the operators and of the observers/measurements. Thus, unfortunately mathematically ill-defined notions appear even in the fundamental structure of the SM.

However, among the foundations of QM and QFT, there exist models with a clear ontology, e.g. Bohmian Mechanics (BM) or the class of spontaneous collapse theories (GRWm, GRWf, rGWRf), in which such notions do not find any room within the derivations admissible from their axiomatic apparatus. The primary aim of this talk is to present the common structure of these theories, underlying the crucial role that a sharp ontology plays in order to obtain successful explanations of physical phenomena.

In second place, two models of Bohmian QFT will be presented as serious alternatives to the standard formulation of QFT in order to recover the physical content of the SM. Though standard QFT is generally defined as the combination of the axioms of quantum mechanics and Special Relativity (SR), there exists a class of non-relativistic models which are generalizations of Bohmian Mechanics to the phenomena of particles creation and annihilation reproducing the statistics of QFT experiments. In this talk, I will presents two models which share a particle ontology, being insensible to the conclusions of several no-go theorems which exclude the possibility of a proper particle theory in the context of QFT. (they involve specific relativistic constraints which are violated in BM) These are the Dirac sea approach and the Bell-type QFT. The former postulates an ontology of a finite and fixed number of fermions, which are defined as structureless particles with a specified position at every time. Within this model particles are never created or destroyed. The dynamics is completely deterministic and comprehends the usual Schrödinger equation for the motion of the wave function and a guiding equation describing particles' trajectories. Here bosonic degrees of freedom are not part of the fundamental ontology. The latter provides an ontology made of fermions and bosons both considered as elementary particles (with positions always defined). The dynamics for the configuration of particles is stochastic: here trajectories can begin and end, and random jump processes from a given configuration to another are inserted within a Bohmian-like guiding equation. These jumps are related to creation and annihilation of particles. Though this model does not provide a deterministic law of motion, it reproduces the statistics of the standard model considering equivariant Markov processes by construction.

Even though these models are not relativistic, they gain Lorentz invariant predictions, so that they are experimentally indistinguishable with respect to a genuinely relativistic theory.

These models show that it is mathematically possible to postulate a particle ontology even in QM and QFT, providing an image of the world approximately similar to that of classical physics. These results are achieved specifying a primitive ontology (determination of the fundamental entities the theory is about) and a set of dynamical variables which constraints the motion of the primitive variables. This strategy follows the methodology introduced in the mid-Seventies by the physicist John S. Bell. In several papers he explained how to construct a rigorous physical theory from a sharp metaphysics. This methodology divides the mathematical structure of a given theory in two parts: structures with a direct physical meaning and dynamical structures. The former ones are the formal counterparts of real physical objects postulated as primitive concepts according to a specific theory. Since they are always localized in space and time they are called local beables (for instance, in BM the local beables are particles' positions). These primitive variables cannot be defined in terms of other more basic notions and the explanation of every physical phenomenon is based on them. The dynamical structures are used to implement equations of motion for the former ones: they tell how these move in space and time via the specification of parameters such as mass, charge, energy, wave functions, etc. Considered together these two structures define the "architecture" of a physical theory.

In conclusion, the substantial aim of the talk is to underline the how a clear metaphysics at the fundamental level of construction of physical theories could be extremely useful in order to avoid the severe conceptual problems that plague the standard version of QM and SM (or more generally QFT) and to achieve rigorous physical theories. The Bohmian QFT models here considered are interesting examples of how this goal could be obtained.

Symposium:

*Towards a practice-oriented metaphysics of science*

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*Introduction*

Practice-oriented metaphysics of science is the project of developing metaphysical claims about what the world is like by examining how scientists study the world and what kind of knowledge they gain. Practice-oriented metaphysics of science deviates from traditional scientific metaphysics (e.g., Ladyman & Ross 2007) in two respects: It abandons the reductionistic assumption that physics is the only science that has a say about metaphysical issues and it is based on the assumption that philosophers should pay attention not only to the products of science, such as scientific theories, but to the whole variety of elements that constitute scientific practice (Waters 2014, Chang 2011).

While metaphysics of science is a burgeoning research field (e.g., Ladyman & Ross 2007; Maudlin 2007; Bird 2007; Ross, Ladyman & Kincaid 2013), practice-oriented metaphysics of science is still at its dawn. The aim of this symposium is to chart the territory of possible challenges and the prospects of developing this field. The key question of the symposium is the following: What should a practice-oriented metaphysics of science look like? This overarching question branches into a number of more concrete questions:

*Methods*: What does it mean to do metaphysics of science in a practice-oriented way? Which principles guide the selection of empirical information from and about scientific practice that are relevant to a particular metaphysical question? What are the strategies for coping with possible inconsistencies among different practices? Does metaphysics of scientific practice leave room for, or even require, assumptions derived from other sources, such as a priori or everyday intuitions?

*Products*: What characteristics do the results of practice-oriented metaphysics exhibit? Are they necessarily pluralistic? If so, does this force us to limit our metaphysics to the claim that the world is fundamentally disorganized? Or is it possible to account for the variety of scientific practice while retaining some of the monistic, parsimonious aspirations of traditional metaphysics? Is it necessary for metaphysical claims to remain local, or is it possible to integrate the results of different metaphysical projects into a universal metaphysics of scientific practice?

*Views of Metaphysics*: What view of metaphysics is involved in the idea of practice-oriented metaphysics of science? Can practice-oriented metaphysics of science count as "genuine" metaphysics at all? After all, in the tradition of Aristotelian metaphysics (which is currently gaining popularity again; e.g., Tahko 2012) the aim of metaphysics is to make claims about the ultimate nature of reality, which seems quite difficult when the focus is on localized practices with the inevitable perspectivalist or pragmatist slant that scientific practice seems to involve.

The aim of this symposium is to address some of these metaphilosophical questions in relation to concrete cases from a number of different areas in the philosophy of the life sciences. The three talks will focus on the notions of homology, individuality and genomic parts. By examining methods, products and views, our symposium will deliver a new image of how scientific practice is currently re-shaping well-entrenched metaphysical notions in the life sciences.

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*I. When are a priori assumptions warranted in the metaphysics of scientific practice?* (**Thomas Reydon**)

Philosophical work on kinds and classification, in particular in practice-oriented philosophy of science, has recently witnessed a turn toward the epistemology of kinds and classifications and away from metaphysical issues. However, abandoning the search for an account of the metaphysics of kinds and classifications would be too quick, as such an account is a crucial element of the explanation why some kinds and classifications are used in the sciences with more success than others, and why some ways of grouping things turn out not to be useful at all. After all, barring cases of epistemic luck the reason for the epistemic and practical success of kinds and classifications must be that they adequately connect to some or other feature(s) of the world.

While an account of kinds and classifications thus needs to encompass metaphysical elements, it is not clear whether practice-oriented philosophy of science is at all able to provide such elements. In this respect, practice-oriented philosophers of science face two problems. First, metaphysics cannot be read off from either epistemology or practice: simply examining scientific kinds and classifications and the ways in which investigators in the various areas of science employ them will not reveal their metaphysical underpinnings. Second, once the metaphysics is elucidated for individual cases, these different metaphysical pictures need to be unified into an overarching account of kinds and classifications – an issue which some authors hold to be insurmountable (cf., Dupré, 1993).

Thus it seems that at least some a priori considerations should be allowed to enter into the picture as guidelines for the metaphysical analyses of individual cases. But as a priori metaphysics is suspect from naturalistic and practice-oriented viewpoints, the challenge for a practice-oriented metaphysics of kinds and classification is to bring a priori considerations into play without removing the account unacceptably far removed from actual scientific practice. In this talk I addresses this challenge and question when a metaphysical account of a particular part of science would count as being unacceptably far removed from the actual practice it is applied to. My answer will hinge on the extent to which metaphysical claims can accommodate a variety of philosophical interpretations/reconstructions of a particular area of practice. The idea is that the less a metaphysical claim fits a wide variety of interpretations or reconstructions of a particular area of practice the more it should be considered unacceptably removed from that area of practice. I will use a case study on the classification of organismal morphological and genetic traits on the basis of homology to show how in this case the concept of sameness of kind can be interpreted in different ways and how, accordingly, a priori metaphysical assumptions about the relation of a classification or kind to the world can be ranked. The overarching aim is to explore what a thoroughly naturalistic and practice-oriented metaphysics of scientific kinds and classifications could look like.

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*II. From Individuation in Biology to Biological Individuality* (**Alan Love**)

Scientific metaphysics is based on the idea that metaphysics – the study of what the world is ultimately like – should be informed by the remarkable success of science. This success is often interpreted in terms of theoretical claims and explanatory models in the sciences. Many argue that the continuous rejection of these claims and models through history undermines the assumption that the sciences can provide a reliable basis for drawing metaphysical conclusions. Although scientific metaphysicians have responded to this skepticism in powerful ways (e.g. French 2014), a different method is to locate the success of sciences in their practices, such as genetic manipulation. Many practices survive the rejection of particular theoretical claims and explanatory models. The metaphysical implications of stable forms of successful practice are unclear because they involve local, partial theories of complex phenomena that do not yield integrated, comprehensive outlooks across different levels of organization. This clashes with the expectations of some scientific metaphysicians: "The raison d'être of a useful metaphysics is to show how the separately developed and justified pieces of science (at a given time) can be fitted together to compose a unified world-view" (Ladyman, et al. 2007).

One place where a practice-oriented metaphysics of science can probe the implications of stable forms of practice is individuality and relationships between parts and wholes. Philosophical analyses of individuals in biology have focused on theories of individuality that either account for what a biological individual is or provide different dimensions of biological individuality (Godfrey-Smith 2013; Clarke 2013). The primary considerations in these discussions derive from evolutionary theory, understood as a fundamental framework for all of biology, where the capacity of an object to undergo selection is paramount. How individuals are determined in practice—individuation—and what those practices look like in different investigative contexts have been largely neglected. To redress this neglect, I analyze individuation in the experimental practices of developmental biologists investigating the coordination of relative sizes between the whole organism and its constituent parts. My analysis demonstrates that different individuation practices arise from distinct investigative aims and lead to divergent conceptualizations of what qualifies as an individual (i.e., to different products). Different kinds of individuals are tracked successfully by different kinds of scientific practices, which do not depend on unified, theoretical answers to what counts as a biological individual (Kovaka 2015). This suggests that pluralism is one metaphysical implication of moving from individuation in practice to biological individuality – from biological practice to scientific metaphysics – and that expectations of unification are not naturalistically motivated.

*III. What Are the Parts of the Human Genome? – Metaphysics of Biological Practice and the Challenge of Pluralism* (**Marie I. Kaiser**)

Biological practice often confronts us with a plurality of sometimes conflicting approaches, explanatory strategies, models and theories, representational and classificatory schemes (Kellert, et al. 2006). This plurality poses a problem to the naturalistic metaphysician who draws on the explanatory and investigative practices of the biological sciences to develop metaphysical claims. If the aim of metaphysics is to make claims about the ultimate nature of reality then the practice-oriented metaphysician must find ways to cope with the plurality of biological practice and to avoid conflicting metaphysical claims.

This paper uses a case study in the metaphysics of biological practice to examine whether and how the challenge of pluralism can be meet. The metaphysical case study I am interested in is concerned with a special version of van Inwagen "special composition question" (1990, 21): Under which conditions is an object, such as a specific DNA sequence, a part of the human genome? In the case study, I approach this question by analyzing the practices of individuating genomic parts that characterize a recent branch of molecular biology, the ENCODE Project (ENCyclopedia Of DNA Elements; ENCODE 2012). For metaphysicians interested in biological part-whole relations, the ENCODE Project constitutes a very interesting and promising example because it is among the few cases in which biologists explicitly seek to identify part-whole relations. The aim of the case study is to provide a coherent account of the metaphysical assumptions that underlie ENCODE's practices of individuating genomic parts.

When developing this account, a metaphysician of biological practice encounters the problem that ENCODE's approach of picking out particular DNA sequences as parts because they display certain biochemical signatures is not the only approach of individuating genomic parts. In other fields, also genetic and evolutionary approaches are employed (Kellis, et al. 2014). I will argue that this plurality of approaches poses a dilemma for metaphysicians: either they stick to the goal of developing a single account of genomic parthood that applies universally and that carves the human genome at its joints, but then they have to ignore the existing plurality of individuation approaches, or they take biological practice seriously, but then they have to abandon the view that there exists a uniquely correct partitioning of the human genome into parts.


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Séance plénière / Plenary session:

*Re-modelling scientific change: complex systems frames innovative problem solving*

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*Abstract*

In the received philosophy of science we don't model scientific change, rather it is given to us as logical form. Logic is manifestly inadequate to this purpose. Whence any scientist would ask 'How else could I model scientific change more fruitfully?' This question is explored from a complex systems perspective and shown to present a compelling but distinctively difficult problem. More positively, a new proposed universal form for all innovative problem solving, from research to design, from crime detection to ethics, is presented and an initial assessment made of its potential for understanding scientific change.

*Individus darwiniens et sélection multi-niveau : enquête sur l'individualité de la cellule cancéreuse*

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Le cancer, également appelé tumeur maligne, est une maladie caractérisée par une prolifération aberrante des cellules et une capacité à envahir d'autres tissus. Dans le cancer, les cellules se développent hors de contrôle et deviennent envahissantes : il est donc généralement décrit comme une maladie de la cellule.

Mutation, compétition et sélection naturelle entre cellules sont les principales composantes du phénomène du cancer (Nowell 1976). Les mutations qui surviennent dans les cellules cancéreuses leur donnent un avantage sélectif sur les cellules normales : suite à une mutation, une cellule cancéreuse prolifère comme une lignée darwinienne. Les cellules cancéreuses peuvent en effet être décrites comme une population darwinienne soumise à la sélection naturelle. Dans cette description, les cellules cancéreuses satisfont les critères de l'évolution par sélection naturelle, c'est-à-dire, une variation héritable qui produit une différence de fitness : analyser le cancer comme un phénomène darwinien a généré des nouvelles perspectives sur l'étiologie de la maladie, sur sa pathogenèse ainsi que son traitement.

Dans cet exposé, je vais analyser l'utilité de la perspective darwinienne pour une définition de la cellule cancéreuse. Je voudrais montrer que, même si la description des cellules cancéreuses comme population darwinienne est théoriquement correcte, elle n'est pas utile pour comprendre pleinement leur identité. Je vais analyser Germain (2012) et Lean-Plutynski (2015) pour décrire la conception des cellules cancéreuses comme une population darwinienne ; ensuite, je vais essayer de montrer comment l'identité des cellules cancéreuses dépend de leur contexte. Le but est de faire un lien entre la sélection naturelle, le débat sur l'individualité, et l'identité des cellules cancéreuses comme un problème ontologique, épistémologique et thérapeutique.

Tout d'abord, je vais vérifier si les cellules cancéreuses satisfont les critères pour être des individus darwiniens (Germain 2012). En effet, en suivant l'analyse de Godfrey-Smith (2009), nous pourrions décrire la cellule cancéreuse comme ayant beaucoup de variations mais une très faible fidélité d'hérédité. En plus, on peut observer dans les tumeurs une forte coopération entre les cellules cancéreuses : cela signifie que les cellules cancéreuses ne sont pas vraiment égoïstes, mais qu'il existe un degré élevé de dépendance mutuelle entre elles. Enfin, la tumeur est caractérisée par une continuité du paysage de fitness (fitness landscape), ainsi que par une dépendance des différences reproductives des caractéristiques intrinsèques : la fitness des cellules cancéreuses individuelles dépend plutôt des caractéristiques intrinsèques que des signaux extérieurs. Grace à ce dernier caractère, les cellules cancéreuses sont "re-darwinisées" : leur fitness devient en partie autonome de celle de l'organisme.

Ensuite, je vais analyser la conception selon laquelle le cancer est un phénomène de sélection multi-niveau (Lean-Plutynski 2015). Dans cette perspective, le cancer est, en même temps, un objet et un sous-produit de la sélection. Dans la progression du cancer, ils sont en jeu différents niveaux de sélection : la sélection agissant à un niveau est en conflit avec la sélection à un autre niveau. Le cancer survient quand une population cellulaire est soumise à la sélection naturelle d'une manière qui détruit l'intégration au niveau de l'organisme. Cela signifie que la cellule devient la nouvelle unité de sélection, à la place de l'organisme : la fitness passe à niveau cellulaire (Lean-Plutynski 2015). En effet, les cellules cancéreuses qui sont les plus efficaces donnent origine à des métastases : dans ce dernier cas, la fitness est attribuée au groupe (toute la tumeur) dans son ensemble, ce qui permet de parler de sélection de group. Cela pourrait aussi expliquer, dans une certaine mesure, le phénomène des métastases : "Groups are more "fit" if and only if they propagate more groups." (Lean-Plutynski 2015).

Dans cet exposé je voudrais montrer que la définition de l'individualité évolutionnaire de la cellule cancéreuse est correcte mais insuffisante : par exemple, il est impossible de comprendre la spécificité d'une cellule cancéreuse à partir de ce point de vue. La cellule cancéreuse est une cellule normale qui commence à se différencier d'une manière aberrante : ainsi, elle donne naissance à des cellules hétérogènes, parfois d'un point de vue phénotypique et parfois d'un point de vue génotypique. En effet, les cellules tumorales peuvent montrer des profils phénotypiques différents : par exemple, la morphologie cellulaire, l'expression du gène, le métabolisme, la motilité, la prolifération et le potentiel métastatique. Ce phénomène est connu comme l' « hétérogénéité de la tumeur ». Cette hétérogénéité se produit à la fois entre tumeurs et au sein d'une même tumeur. Comprendre l'hétérogénéité tumorale peut aider à comprendre pourquoi l'identité d'une cellule dépend fortement de son contexte.

Par conséquence, je pense que pour expliquer qu'est-ce que c'est qu'une cellule cancéreuse, il faudrait chercher plutôt à définir son identité comme une identité relationnelle (Bertolaso 2013). La notion d'identité – du latin identitas – signifie la coïncidence d'une chose avec elle-même (Wiggins 2001). L'identité révèle ce qui permet la persistance d'une chose dans le temps et l'espace : c'est-à-dire, pourquoi une chose reste la même, tout en changeant au cours du temps et dans l'espace. Pour définir l'identité, les relations entre les parties sont cruciales : l'intégration organisationnelle entre les parties fait l'identité de l'ensemble. Il faudrait donc sortir d'un cadre où il y a des individus darwiniens à plusieurs niveaux pour définir un phénomène – le cancer – qui ne peut pas être compris sans faire référence à plusieurs niveaux en même temps.

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*L'organicisme en écologie : pseudo-science ou progrès scientifique ?*

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La science écologique est souvent présentée comme en continuité avec l'histoire naturelle. D'après (Stauffer 1960), le concept linnéen d'économie de la nature aurait préfiguré les théories écologiques du XXe siècle. (Cooper 2003) considère quant à lui que l'écologie des populations et des communautés est fille de la révolution darwinienne. Enfin, selon la plupart des historiens de l'écologie, se seraient greffés sur cette écologie aux sources darwino-linnéennes des apports de la cybernétique, de la systémique, et de la thermodynamique pour donner naissance à l'écologie des écosystèmes à partir des années 1930.

Cette historiographie classique néglige un épisode décisif de l'histoire des sciences, l'élaboration de la première théorie générale en écologie par Frederic Clements au début du XXe siècle (Clements 1904; Clements 1905; Clements 1916). Comme l'a montré (Eliot 2011), ce paradigme a été rétrospectivement taxé de pseudo-scientifique pour son usage d'une analogie entre organismes et unités écologiques. En accord avec Eliott, nous soutenons que le paradigme clementsien constitua au contraire un progrès scientifique. En rupture avec l'histoire naturelle, il fit passer la discipline de la seule description des communautés végétales à leur explication causale. Clements utilisa l'analogie avec les organismes tant pour soutenir que les unités écologiques sont des unités de sélection naturelle que des entités fonctionnellement intégrées. Cette seconde thèse est devenu un principe fondamental de l'écologie des écosystèmes. L'import par Clements du concept d'organisation biologique de la physiologie vers l'écologie initia ainsi un révolution scientifique au sens de (Kuhn 1962) en ce qu'il convainquît un nombre important d'écologues de l'époque de se consacrer à des problèmes nouveaux (succession écologique, individuation, stabilité, et normes de bon fonctionnement des écosystèmes). Le développement ultérieur de l'écologie des écosystèmes grâce aux outils de la cybernétique, de la systémique, et de la thermodynamique doit davantage s'interpréter comme un raffinement conceptuel au sein du paradigme initié par Clements plutôt qu'un rejet de celui-ci dans la mesure où l'écologie des écosystèmes conserve ces problèmes. Les théories de l'écologie des écosystèmes présuppose ainsi la pertinence de l'analogie entre organismes et écosystèmes. C'est pourquoi nous défendrons en conclusion que l'import d'outils théoriques depuis la physiologie peut toujours être source de progrès en écologie des écosystèmes. De manière plus générale, nous souhaitons montrer par cet exemple que les analogies en sciences, pour peu qu'elles soient prises au sérieux et non reléguées au rang de métaphores à but pédagogique, contribuent au progrès théorique et explicatif en fournissant des modèles d'intelligibilité de la nature.

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*Y a-t-il eu une « révolution paléobiologique » en paléoanthropologie?*

**Lequin, Mathilde**, mathildelequin@gmail.com, Université Toulouse Jean Jaurès, ERRAPHIS

Selon Sepkoski et Ruse (2009), la paléontologie a connu au cours du XXe siècle une « révolution paléobiologique » : longtemps cantonnée à la seule description de spécimens fossiles et rangée du côté de la géologie plutôt que de la biologie, la paléontologie a été redéfinie en tant que paléobiologie, à travers la synthèse néo-darwinienne, puis la méthode cladistique et la théorie des équilibres ponctués.

Cette « révolution paléobiologique » a-t-elle également eu lieu en paléoanthropologie? Cette discipline, qui étudie l'évolution humaine à partir de ses vestiges fossiles, s'est certes fait l'écho des changements majeurs qui ont scandé l'histoire de la paléontologie. Ainsi, l'esprit néo-darwinien du congrès « Origin and Evolution of Man » de 1950 marque la naissance d'une paléoanthropologie moderne, dans laquelle les concepts de sélection naturelle, d'adaptation et de variabilité des populations sont au premier plan. Depuis la fin des années 1970, la méthode de classification cladistique a été massivement mobilisée, jouant désormais un rôle structurel dans l'établissement de la phylogénie des homininés. Outre ce registre théorique, l'accroissement remarquable du registre fossile ainsi que le progrès des techniques d'analyse, permettant d'extraire davantage d'informations des spécimens, constituent également des vecteurs de changement dans cette discipline.

Au cours de cette présentation, je montrerai que la « révolution paléobiologique » reste pourtant inachevée en paléoanthropologie. L'augmentation spectaculaire du nombre de genres et d'espèces fossiles au cours des 20 dernières années, autant que la cyclicité des controverses sur l'origine et l'évolution des premiers homininés, semblent démentir l'intégration effective de la paléoanthropologie à la biologie de l'évolution actuelle. Comment expliquer cette résistance de la discipline à la perspective paléobiologique?

Je m'intéresserai d'abord aux difficultés épistémiques propres à la paléoanthropologie, en laissant de côté les contraintes générales inhérentes à la paléontologie. Trois exemples témoignent des limites que rencontre l'intégration de cette discipline à la biologie de l'évolution. 1) La théorie des équilibres ponctués, fondée sur l'étude de séries anatomiques de gastéropodes et d'arthropodes fossiles, est difficilement applicable à l'étude des homininés, pour lesquels on ne dispose pas de séries fossiles comparables. 2) La méthode cladistique n'a pas permis de faire progresser les débats relatifs à l'évolution posturale et locomotrice des homininés : les classifications actuelles ne prennent en compte que les caractères crâniens et dentaires, à l'exclusion des caractères postcrâniens, dont la signification phylogénétique est jugée peu claire. Pourtant, ces caractères postcrâniens jouent dans les faits un rôle déterminant dans l'assignation d'un spécimen à la lignée des homininés. 3) Les enseignements de la génétique sont limités, non pas seulement en raison des limites techniques actuelles (compromettant l'exploitation de l'ADN fossile au-delà de 200 000 ans), mais parce que la base génétique ne suffit pas à expliquer l'évolution du comportement postural et locomoteur.

Dans un deuxième temps, je mettrai en évidence les problèmes d'ordre épistémologique également impliqués dans l'inachèvement de la révolution paléobiologique en paléoanthropologie. Je montrerai que ces problèmes concernent principalement les modalités d'attribution de la signification fonctionnelle et phylogénétique des caractères morphologiques, mais aussi la distinction entre signal génétique et signal comportemental dans le processus d'interprétation des vestiges fossiles. J'illustrerai ces problèmes en analysant les lectures antagonistes développées par Tim White et Bernard Wood, qui reflètent deux écoles de pensée au sein de la communauté paléoanthropologique. Critiquant l'amplification artificielle du nombre de genres et d'espèces fossiles liée à un usage dogmatique de la méthode cladistique et de la théorie ponctuationniste, White (2009a) appelle à restaurer la révolution paléobiologique par un retour aux concepts fondamentaux du néo-darwinisme, tels que l'évolution phylétique et le primat du signal génétique (White, 2009b). Au contraire, selon Wood (2000, 2010), la réforme épistémologique de la discipline reste à entreprendre : la clarification du concept d'espèce (phénétique, phylogénétique ou monophylétique) et la prise en compte de possibles homoplasies au sein de la lignée des homininés apparaissent comme les principaux défis encore à relever.

A partir de ces deux lectures, je montrerai que la « révolution paléobiologique » associée au tournant néo-darwinien de 1950 est pour la paléoanthropologie actuelle une force d'inertie, alimentant un cadre interprétatif conservateur dans lequel les caractères considérés comme « humains » sont le plus souvent surdéterminés. Je remettrai également en question la pertinence de la notion de « révolution » comme catégorie permettant de penser le changement en paléoanthropologie. Ainsi, je démontrerai que le changement en paléoanthropologie ne peut être attendu ni de mises à jour théoriques, ni de futures découvertes fossiles, ni du progrès des techniques d'analyse : il exige de faire face à une série de problèmes conceptuels, liés pour les uns à des questionnements fondamentaux de la philosophie de la biologie, pour d'autres spécifiques à la paléoanthropologie (tels que le concept d' « humain » et les biais relatifs à l'anthropocentrisme et à l'anthropomorphisme). J'expliquerai d'autre part que le changement effectif en paléoanthropologie ne peut pas reposer uniquement sur l'effort d'intégration à la paléobiologie : il exige aussi la reconnaissance de la paléoanthropologie comme anthropologie.

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*Scientific imperialism: an attempt at a definition*

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The aim of the paper is to contribute to the debate on scientific imperialism in the philosophy of science (Clarke and Walsh (2009), Clarke and Walsh (2013), Kidd (2013) and Mäki (2013), inspired by the text of Dupré (1995)). The debate has revolved around the question of the permissibility of the application of scientific theories and methods outside the discipline in which they were initially introduced. Philosophers of science have attempted to clarify what it means for a theory or a discipline to be applied outside its own field or domain and whether such an application can be understood as imperialistic. Attempts to identify instances of scientific imperialism have sometimes been accompanied by formulating criteria for evaluation.

Dupré characterizes scientific imperialism as an application of a "successful scientific idea" "far beyond its original domain" (Dupré 2001, p. 74), so that this application cannot "provide much illumination". For Clarke &Walsh (2013) scientific imperialism is illegitimate occupation by one discipline of another discipline's territory. According to Mäki (2013), scientific imperialism is a phenomenon that may occur between two disciplines. Thus, for him, scientific imperialism is a "dynamic interdisciplinary relationship" (p. 325). Mäki distinguishes between certain types of imperialism: imperialism of scope, imperialism of style, and imperialism of standing.

Mäki argues that imperialistic advances should be constrained if they do not advance the pursuit of explanatory unification across disciplinary boundaries. Mäki's definition is an important advancement in the debate and I would like to build my own account upon it. However, I resign from conceiving of scientific imperialism in terms of relationships between disciplines and I argue that Mäki's imperialism of standing is crucial for identifying the instances of scientific trespassing that can be called imperialistic.

I argue that some novel application X of methods, theories, research programs becomes imperialistic when:

1) X is favoured (by members of the scientific community) at the expense of other methods, or theories, or research programs in terms of academic and non-academic prestige, power, resources and when
2) the attempt of justifying this favouring of X is made by claiming that X is…
 a) more 'progressive' than applications of other methods, or theories, or research programs (justified by the progress in science);
 b) more 'scientific' than applications of other methods, or theories, or research programs (justified by the progress of science);
3) and claim (2) is assumed to hold without providing argument for it.

I point out that scientific imperialism is an activity that is related both to a certain view on improvement and progress, as well as to a power to realize it. Otherwise, without pursuing this vision of improvement, scientific trespassing is only aggressive, or invasive – but not imperialistic. Without the power to actually have an effect, that is, to actually affect the standing between approaches, such an attempt is a mere scientific Don Quixoterie.

In the debate on the notion of scientific imperialism the link to the notion of scientific progress is often made. Dupré notices that imperialistic tendencies that manifest themselves in claims that a particular theory provides the key in the understanding of a given phenomenon, are often related to the attempts to provide explanatory unification that is presented as "unqualified scientific good". He criticizes such tendencies for introducing inappropriate methodology for studying a given phenomenon. Clarke and Walsh believe that there may be a progress in science, even though "there is no one definitive account of progress (…) What we decide to count as progress in science will be a matter of how we decide to weigh the various backward-looking factors that contribute to progress" (Clarke and Walsh 2013, p. 345). Mäki uses the notion of epistemic scientific progress, of advancement in knowledge, "including explanatory knowledge about the world: growth of explanatory unification" (Mäki 2013, p. 336) in order to assess scientific imperialism. He proposes four constraints on scientific imperialism (ontological, epistemological, axiological, institutional) that set conditions for acceptable scientific imperialism that contributes to scientific progress in the sense advocated by him. All of the preceding accounts are examples of how participants in the debate of scientific imperialism bring their convictions about scientific progress to the debate.

In my account the notion of scientific progress already forms part of any charge of scientific imperialism, as I argue that those novel applications of theories, methods, or research programs are imperialistic that are supported as being more progressive. Furthermore, my account shows that scientific imperialism always leads to epistemic loss, as “the expense at which one scientific approach is favoured over another” denotes an asymmetrical relation: being in this relation is beneficial for one entity because it is harmful/detrimental/disadvantageous for another entity. We always identify loss here (loss of opportunities, loss of funding, loss of societal relevance) faced by a certain theory, research program, approach. This loss has an epistemic dimension – it leads to forgoing a certain type of research and of knowledge.

Evaluation of scientific imperialism can involve two types of considerations. First, when analysing the expense at which one application of method, or theory, or research program is favoured over another, one necessarily has to adopt a standard of appropriate 'distribution' of standing. If someone opposes this standard, then this person can judge the novel application as unfair in the light of her view on the appropriate distribution of standing across scientific approaches in the organization of science. This standard could be defined in the light of someone’s views on the organization of science, as well as in the light of the view on the epistemic loss related to the institutional favouring of one approach. Second, one can analyse whether a novel application is illegitimate according to her views on scientific progress (progress of science, or progress in science). A critic can question the notion of scientific progress that an imperialist endorses (either by arguing to replace it by another notion, or by rejecting entirely the very notion of scientific progress).

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*Is economics becoming a science of morality?*

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One could be forgiven for thinking that mainstream economics had managed to evacuate morality from its analysis. The issues of social justice and distributive justice seem to have been confined to the field of social choice theory, a normative approach which, although of considerable interest in the academic world, had no influence on the rest of the profession and the recommendations that the latter addressed to policy makers. One might cite as a counter-example the influence of Amartya Sen in international bodies, but, we believe, this is the exception that proves the rule. Mainstream economics explained this sidelining of issues related to morality by invoking at least three justifications. First, because, taking as a canonical reference An Inquiry Into the Nature and Causes of the Wealth of Nations, it could argue as Adam Smith did, that the market does not need morality. Second, because economics seemed to have found in the Pareto optimality criterion the adequate tool to compare and judge social situations and thus to characterize itself as a science of public decision. Finally, because a technical justification was often used, the so called impossibility of interpersonal comparison of utilities. Thus, it would seem that an almost complete consensus has developed around the idea that economics could leave to one side any consideration of morality. Not that economics has ever been perfectly homogeneous and consensual, but controversies among economists have not been so much about morality as about market efficiency. Deaf to attacks from other social sciences, philosophy, political sciences and from the profane, while all of these discussed and debated the immorality of markets, economists have been wont to attribute such criticisms to a misunderstanding of the foundations of economic liberalism and a confusion between immorality on the one hand and irrationality and inefficiency on the other.

However, as we will argue here, this quasi consensus became increasingly fragile at the end of the XXth century. Research in experimental and behavioral economics has long shown the inability of the model of the economic agent to reproduce behavior observed in the laboratory and in economic reality. Furthermore, this literature has not been limited to providing evidence against the standard model of the economic agent, but has also generated new interpretations and models, which constitute the theory of social preferences. Fehr and Fischbacher (2004) define social preferences as “other-regarding preferences in the sense that individuals who exhibit them behave as if they value the payoff of relevant reference agents positively or negatively.” The most famous of these models has been proposed by Fehr and Schmidt (1999), the so called model of inequity aversion. The basic idea is simply to change the utility function by incorporating a component that represents the interest that the individual has in others’ payments. This component is simply the difference between the payment of the individual and that of the other or an average of those of others. The utility function is a weighted sum of the player’s material payment and this component. By doing so, without abandoning the dominant framework, the model casts doubt on the three justifications evoked above to eliminate morality from economics.

Can we say, nevertheless, that a positive theory of morality exists in economics? By a positive theory of morality we mean a theory that can explain how, what is referred to by the generic term "morality", emerges and shapes individuals’ behavior in society. The theory of social preferences has tried to expand the definition of the utility function in order to allow it to reproduce the pro-social behavior observed in experiments and which is now regarded as a stylized fact. But, the way it did it, with a minimum change in the basic model, poses certain problems of inconsistency. Furthermore, the models proposed, based on methodological individualism, try to explain how morality shapes agents' behavior without explaining how morality emerges. These models simply try to represent the idea that people value morality or, in cases that are best suited to this approach, tried to capture something in human nature, which would be an intrinsic characteristic of the individual, his essence, and which could generate pro-social and moral behavior. But morality is a social phenomenon. What would be a necessary change in economics in order to integrate a positive theory of morality?

For this it would be necessary that economics distances itself from methodological individualism. Indeed, our claim is that, the basic unit that has to be studied in order to understand morality, the indivisible unit, may not be the individual but the interaction between several individuals. If this is true, the research program of methodological individualism to explain moral behavior is as hopeless as trying to explain sexual reproduction by describing a unique representative human being. On the contrary, we think that it is worth seeking to explain an alternative approach which takes interactions seriously and which seeks for how morality emerges from the social environment. The ultimate objective of this approach would be to discover what we will call mechanisms, which could explain both how the interaction between agents generates morality and how this morality shapes interactions. However, can we conceive of these mechanisms? Is this approach a tractable alternative? In fact, we think that examples of lines of research, which, in our opinion, go in this direction, exist already. The first one is drawn from a historical literature, this is the Theory of Moral Sentiments by Adam Smith. The other line of research is drawn from a very recent domain of research, social neuroscience, which could have been inspired by Smith. We will discuss the possibility that economics will re-integrate the approach developed by its founder in the Theory of moral sentiments, now that it has been appropriated and re-interpreted by social neurosciences.

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*Antimodularity: computational complexity may hinder scientific explanation*

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I explore the pragmatic effects of computational constraints, specifically computational complexity, on certain common types of scientific explanation, mainly in the biological disciplines. The affected kinds of explanation are those which require a hierarchical modular description of the system under consideration: the excessive computational complexity of certain algorithmic tasks for modularity detection entails that even modern computational systems can fail to yield a valid modular description of the system under consideration, hindering its explanation. This effect of computational complexity on explanation is most likely to occur in sciences, such as systems biology, where the size of the dataset to be analyzed, combined with the computational cost of the algorithm for detection of hierarchical modularity in them could lead, in certain cases, to the impossibility of finding a valid coarse-grained, high-level, modular description of certain complex systems, hampering their functional or mechanistic explanation. I will argue that a generalized occurrence of such effects of computational complexity on explanations could in some way be viewed as the sign of a historical change toward an automated and less human-understandable science.

I first propose a general conception of modularity, which constitutes a necessary feature for a system to be explainable in certain ways. Modularity is a notion stemming from Herbert Simon's seminal works in the 60s on variable aggregation and near-decomposability (see Simon 1962), and consists in the possibility of decomposing a system (viewed as composed of discrete parts interrelated according to a specific chosen relation) into recognizable, sufficiently defined and persistent subsystems (the modules) each one composed of parts which are more strongly related to each other than to parts belonging to other modules. Hierarchical modularity is the possibility of describing a system as a full hierarchy of levels, each level composed of loosely interrelated modules, each module in turn possibly decomposable into sub-modules belonging to the lower level. Modularity is relative to the pragmatic choice of a relation between elementary parts of the system: varying the basic description of the system, its modular structure can vary.

Hierarchical and high-level modularity is needed not only for a posteriori explanation of a known phenomenon, but also during the phase of scientific discovery: specifically, as noted by James Woodward, during the search for casual relationships between parts of a mechanism either at low and and at a higher level.

Algorithmic detection of hierarchical modularity turns out to be plagued by computational intractability of the search for the best hierarchical description of a system (NP-completeness of optimal aggregation of variables, as per Winker 1992, NP-hardness of even approximate aggregation in general, as per Kreinovich and Shpak 2006, and NP-completeness of the optimization of modularity measure Q in community detection in networks, as per Brandes et al. 2008), and in any case by a high computational cost of the known approximate algorithms (see for instance Danon et al. 2005), circumstances which hinder its applicability on large enough systems. In these cases, for lack of a feasibly obtainable high-level modular description, the system can be only described in terms of its numerous elementary parts, possibly hampering the intelligibility of the obtained description.

To characterize the above situation, I propose the notion of "antimodularity", consisting in the unavailability of a hierarchical modular description of a system valid enough for explanatory purposes. This lack of a valid hierarchical multilevel modular description can be due either to the actual absence of modularity in the system's basic description, or to the impossibility to obtain a hierarchical modular description which fits the needs of the observer, an impossibility due to size of the system under assessment combined with computational cost of the algorithmic methods for modularity detection.

To show how antimodularity can affect scientific explanation, I assess its consequences on four models of scientific explanation: mechanistic, functional, deductive-nomologic, and topological explanation (the latter proposed by Philippe Huneman, as in Huneman 2010). Antimodularity, by impeding the obtainment of a full hierarchical description, negates the possibility of multi-level explanation, leaving us, in the worst case, with only a low-level description of the system in terms of its interrelated elementary parts, or, in better cases, with a higher-level but still fairly low-level modular description. Such a kind of description, in the case of large enough systems, can result unintelligible. As a consequence, antimodularity mostly damages mechanistic and functional explanations, which rely on multiple interrelated levels of description and require intelligibility (mechanistic explanation understood in an "epistemic" sense, as in Bechtel and Abrahamsen 2005 and Wright 2012, and functional explanation understood as in Cummins 1975). By entailing, in most interesting conditions, Mark Bedau's weak emergence (see Bedau 1997), antimodularity impedes deductive-nomological explanations, for lack of a law allowing for prediction of the system. Topological explanation, a form of non-causal, non-mechanistic explanation, is instead immune from the consequences of antimodularity: antimodularity, being itself a kind of topological property, should enable topological explanation.

I examine some cases in the scientific literature, in order to assess the likelihood of incurring in antimodularity in certain areas of scientific research, such as systems biology: antimodularity could occur in the explanation of large biological networks (e.g. genetic or metabolic ones) due to the excessive computational complexity (NP-completeness) of algorithms for optimizing community detection, and to the still very high computational cost of sufficiently accurate approximate algorithms for community and functional role detection in these networks.

More in general, a form of explanatory difficulty (which I would call "explanatory emergence") could derive from the computational complexity of tasks involved in finding structure in large scientific datasets. Where explanatory emergence occurs, the system is only describable at its lowest possible level, as a very large set of parts interrelated in intricate ways, and such descriptions could be so complex to possibly overcome human comprehension, ending up as unintelligible.

A general preliminary conclusion that can be drawn is this: the growing use of computational methods of analysis of big datasets in certain scientific disciplines could have already pushed, or can be on the verge of pushing certain disciplines towards a computationally-aided science characterized by the partial presence of automatically generated explanations, possibly severely lacking in human intelligibility where antimodularity occurs. These explanations could, in a self-sustaining fashion, progressively guide scientific research toward poorly human-understandable theories and goals. Such a trend would constitute, it seems, a major paradigm shift and a historical change in certain areas of science worth a serious preemptive philosophical investigation.


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*Biological reality in theoretical science: the Tower of Hanoi case*

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A recent trend in theoretical and engineering life sciences shows an increased interest in building more and more biologically realistic models. Nevertheless, this tendency is distinguished from a post-AI tradition which aims at building unrealistic models, and with another historical tradition which tries to provide general design principles.

My goal in this talk is to provide and discuss a framework for differentiating between these three trends. After introducing and illustrating them, I shall argue that they are, at first glance, best differentiated with respect to the way by which scientists justify one of the methods used in building models, namely the idealization method; the introduction of idealizations is justified by representational ideals specific to each of the three trends. Moreover, I will show how the Tower of Hanoi (henceforth ToH) case study in theoretical neuroscience questions this classification on basis of ideals that have been so far associated with each trend.

The talk is divided into two parts. The first part is devoted to provide a framework for distinguishing between what I discern to be three actual trends in theoretical biological science.

The first trend, labeled "biological strong realism", aims at building more and more biologically inspired models. An illustration of this trend is provided by the Human Brain Project whose goal is to simulate a whole individual brain on a supercomputer, and which complies with the following methodology: "the more details, the better".

I label the second trend "biological antirealism", for modelers try to build models that only map the correct input/output of the phenomenon of interest, regardless of the biological plausibility of the postulated internal structure of the model. An example of this trend is the Nernst equation (in its biological application), which aims at describing a neuron's resting potential and predicting a measured potential; this equation only captures an input/output profile of the phenomenon, for it states that different concentration gradients map onto different equilibrium potential values.

The third trend may be labeled "biological weak realism", because it aims at constructing models that are indeed similar to a biological phenomenon, but only include the causally relevant factors giving rise to it. For example, in synthetic biology, the Repressilator was constructed in such a way to include all and only the core components and interactions that produce the oscillatory behavior characteristic of gene-regulatory networks.

In the remaining of the first part, I shall argue that these three trends are best distinguished with respect to the way modelers justify their basic methodology used in building models; this methodology is usually called "idealization", that is, the technique of intentionally introducing false assumptions in a model to achieve certain goals.

On basis of our above example, it is clear that biological strong realists pragmatically justify their idealized models: if biological strong realism is distinguished by the practice of introducing simplifications such that to make the model tractable, but with the intention to remove any distortion, then its ideal is to build a precise, accurate or complete model of a phenomenon.

For biological antirealism, what resorts from the above illustration is that the rationale behind the idealization method is to construct a model with the ideal of sufficiency, simplicity or predictive power.

A synthetic model such as the Repressilator was able to show possible and optimal design principles of circadian clocks; the reasons that modelers give for introducing idealizations is their willingness to pursue ideals like explanatory power, universality or generality.

The second part of the talk is interested in questioning the intuitive framework presented in the first part for distinguishing between actual tendencies in theoretical biological science. I will show how the ToH task poses a serious difficulty on our above framework by arguing that this framework does not appropriately allocate the representational ideals specific to each trend.

One of the most recent computational models of the ToH task uses the sophisticated perceptual strategy algorithm, which has been proven to better reflect the human-like manner to solve the task. Implementing this algorithm on a computer requires the triangulation and compactness of as much data as possible provided by behavioral and fMRI studies. Modelers have assessed their model on basis that the idealizations were pragmatically useful for generating explanations appealing to the relevant mechanisms and for predicting fMRI data. From this example, one notices that, although this model seems to belong to the strong biological realistic trend, the ideals previously allocated to the two other trends are also present. Indeed, there are the representational ideals of simplicity, in so far as it is associated here with the ideal of compactness, predictive power, universality, in the sense that the model tries to triangulate or unify into a single computational model a wealth of data, and explanatory power.

From the biological antirealistic perspective, the ToH task is considered a "shortest path problem", which is a combinatorial optimization problem. Thus, a recurrent neural network has been constructed by using a Hopfield network on basis that it provides a complete characterization of the input/output and an explanation of the fact that the minimum number of moves to solve the task is 2n–1, where n stands for the number of disks. Again, we find here not only the ideals associated with biological antirealism, but also the ones associated with the two other trends: completeness and explanatory power with respect to the characterization of the ToH task as a shortest path problem.

GPS and Soar models were built to provide a canonical explanation (with only difference-making causes) of the problem solving task, of which the ToH task is considered a mere instantiation. Biological weak realists offer us a complete model of the problem solving task by including studies about the learning processes, and GPS model was deemed sufficient for executing the goal-recursion strategy. But, these latter two ideals have been said to be specific to the other trends.

To conclude, I shall propose a way out of the shortcomings highlighted in the second part. As a consequence of the considerations of the ToH case study, I will provide a more fine-grained categorization of the three trends by specifying the technique of idealization: while biological strong realism aims at an explanatory model where all data are judged relevant, biological antirealism and weak realism aim at explanatory models in the sense that they allow to save the phenomenon and include difference-making causes.

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*Causal powers and isomeric chemical kinds*

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Some philosophers have claimed that (natural) kinds can be construed as mereologically complex structural properties. This essay examines several strategies aimed at construing a certain class of natural kinds, namely isomeric chemical kinds, in accordance with this view. In particular, the essay examines views which posit structural proper parts in addition to microconstitutive parts to individuate isomeric chemical kinds. It then goes on to argue that the phenomenon of chirality in stereochemistry gives the proponent of kinds-as-complex-properties evidence for positing the existence of causal-cum-dispositional individuating proper parts, in addition to structural parts, for chemical enantiomeric kinds.

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*Antecedent strengthening and ceteris paribus laws*

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Antecedent-strengthening is a trivial theorem of classical logic but its equivalent in informal reasoning often fails. Intuitively, the effect is closely related to the problem of ceteris paribus laws and one recent explanation (Graham Priest, Introduction to Non-Classical Logic, 2008) explicitly makes reference to a ceteris paribus qualifier. On a second look, however, the relation seems to dissolve. As a matter of fact both problems are most plausibly explained by referring to features of the natural-language conditional distinguishing it from the material one – but to different features. The failure of antecedent strengthening is best explained by assuming that a naïve reasoner evaluates the conditional constituting the conclusion via the Ramsey test and makes a tacit assumption such that the net result is an inconsistent set of premises – from which set classical logic infers anything (and thus also the conclusion conditional's consequent), while the naïve reasoner infers nothing. On the other hand, the ceteris paribus qualifier is best explained by assuming that an indicative conditional in some (but by no means in all) cases can express that a sufficient condition obtains – which makes it possible for 'if A

and B, then B' to express an informative (possibly false) claim instead of a logical triviality. Thus, both explanations manifestly invoke different features of the indicative conditional and it becomes hard to see the connection that seemed so obvious in the beginning. The paper aims to find an answer to the question whether, and eventually how, the two problems and solutions are related, after all.

An example will illustrate both effects. Consider Rudy, a bird about whom you do not have any direct, i.e. perceptual information. You are told that Rudy is a raven. Since you believe that ravens are black, you infer that Rudy is black. Now, you are told that Rudy is an albino and retract your inference. In your first reasoning, you accepted the following conditional: 'If Rudy is a raven, then he is black', but, as evidenced by your retracting the original inference, from that you do not infer that 'if Rudy is a raven and is an albino, then he is black'. Thus, you do not accept an instance of an informal equivalent of antecedent-strengthening. Here is an explanation. You have the background belief that albino birds do not have the natural feather coloration of their species such that you would accept 'if Rudy is an albino, then he is not black'. On the other hand, you evaluate conditionals via the Ramsey test, i.e. by hypothetically adding the antecedent to your stock of beliefs and judging whether you should believe the consequent. By this procedure, you evaluate 'if Rudy is a raven and is an albino, then he is black', given 'if Rudy is an albino, then he is black'. Thus, you hypothetically add 'Rudy is a raven and is an albino' to your givens. Adding 'Rudy is an albino' triggers your background belief that 'if Rudy is an albino, then he is not black', such that all in all your givens consist of 'if Rudy is a raven, then he is black', 'if Rudy is an albino, he is not black', 'Rudy is a raven and is an albino', which is inconsistent (assuming that your intuitive conditionals validate modus ponens). In contrast with the classical logician, from an inconsistent belief set you do not infer anything, in particular not 'Rudy is black'. Thus, you take 'If Rudy is a raven and is an albino, then he is black' not to follow from 'if Rudy is a raven, then he is black'.

It is easy to see how the failure of antecedent-strengthening is related to ceteris paribus laws. There was a law 'ravens are black' involved in your reasoning. This law, obviously, was a ceteris paribus law. You believe that ravens are black but you do not take this to hold literally for all ravens (think of albino ravens), i.e. you believe that ceteris paribus, all ravens are black. Only if this qualifier is ignored, it follows from 'all ravens are black' and 'Rudy is a raven' that if Rudy is a raven and an albino, then he is black. So antecedent-strengthening fails iff the law backing up the premise conditional is not qualified by a ceteris paribus operator.

The ceteris paribus law you believe is a universally quantified conditional allowing exceptions. Certain properties of ravens ground exceptions, others don't. Rudy's being an albino or having his feather color changed artificially ground exceptions, but Rudy's having two wings does not. In the literature, it is generally acknowledged that the list of exceptions for a ceteris paribus law is potentially endless – at least for some important cases of such laws in the life and social sciences. Moreover, it is often assumed that the features on the list of exceptions allow of no common characterization – but this is due to the fact that giving the obvious characterization leads to a conclusion that is thought to be unacceptable. To wit, all of Rudy's properties founding exceptions are properties that do or can interfere with his being black. Thus, instead of instances 'if Rudy is a raven and is not an albino, then he is black', 'if Rudy is a raven and has not been painted yellow, then he is black', and so on, we have 'if Rudy is a raven and nothing prevents hat he is black, then he is black'. This explication of the ceteris paribus law is generally thought to reduce it to vacuity and hence is resisted in the literature. This thought rests on the plausible assumption that 'nothing prevents that Rudy is black' implies 'Rudy is black' but also on the less plausible assumption that 'if A and B, then B' has the same truth-conditions as 'if it is true that A and B, then it is true that B'. In the paper, I propose different truth-conditions; they will clarify the relation between the bare indicative conditional and the CP-qualified conditional that is a ceteris paribus law.

*The atemporal emergence of temporality*

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Cosmological models based on Einstein's general relativity (GR) generically contain an 'initial' singularity -- the big bang. Similarly, black hole models in GR are also singular. That these two important groups of relativistic models suffer from these mathematical pathologies, which are not considered to faithfully describe the corresponding physical situation, thus leads to the general conviction that GR must ultimately be replaced by a more fundamental theory of gravity capable of adequately describing the very early universe as well as black holes. As results from ordinary quantum mechanics have inspired a hope that a quantum theory of gravity may smooth out these singularities.

However, as physicists have proceeded in search of this elusive quantum theory of gravity, it has become increasingly clear that the ontology of such a fundamental theory will not contain anything resembling a spacetime. Space and time, it appears, are absent at the fundamental level and only 'emerge' as effective phenomena at a coarse-grained scale. That spacetime emerges from whatever non-spatiotemporal structure a quantum theory of gravity postulates is required to explain why GR has been as successful as it has been in describing gravity as a feature of spacetime. Any theory striving to supplant GR must explain this success, and must thus show how GR is an effective theory. In other words, it must establish the emergence of classical spacetime from the fundamental structure atemporally, i.e., without conceiving of this emergence as a dynamical process in time.

Since it is precisely the initial big bang singularity in the cosmological models that requires an adequate understanding captured in a quantum theory of gravity, there must also be a temporal emergence of classical spacetime: from what, if anything, did the very early universe come? In other words, there must be a transition -- a dynamical process -- from an earlier quantum state, which lacks any correspondence to a classical emergent state, to a later 'classical' cosmology well described by relativistic spacetime. Clearly, this temporal emergence is grounded in the atemporal emergence.

So why is there something rather than nothing? More precisely, how does the universe evolve from what is classically 'before time', according to the more fundamental quantum theory, into the current regime in which GR holds? Unsurprisingly, different approaches to quantum gravity offer different answers. The goal of this paper is to consider one such approach and its treatment of the very early universe, viz. loop quantum gravity and the cosmological models based on it -- so-called 'loop quantum cosmology' --, where we appear to find a vindication of our initial hope that an appropriate quantum theory will wash out the big bang singularity.

In order to glimpse at the yet unknown final version of loop quantum gravity, loop quantum cosmology freezes all but one degree of freedom, a 'cosmological' scale factor, which is interpreted as a cosmological time clocking the evolution of the universe. It is found that in isotropic and homogeneous models of loop quantum cosmology the classical singularity seems to disappear in two senses. First, unlike in the classical analogue, the curvature does not increase beyond any bound for arbitrarily small scale factors. Second, there exists a principled way of extending the models through the initial singularity into a mirror universe. Naively put, we could say that the dynamical evolution in these models leads to a contracting and subsequently expanding universe. However, the mirror universe behind the big bang affords at least two interpretations: either the tunneling from an earlier shrinking universe to our later expanding one; or as the twin birth of two expanding universes, connected by a shared initial quantum state, without, it seems, a semi-classical interpretation.

A number of subtleties must be navigated before we can meaningfully speak about what happened 'before the big bang'. First, the fact that the state vector becomes indeterminate for a vanishing scale factor suggests that the dynamical evolution around the classical big bang cannot be fully 'deterministic' despite its regularity. I argue that the failure of 'determinism' implies that the singularity is not full resolved, pace the claims made in the literature that it is. This 'quasi-determinism' can be characterized measure-theoretically as an 'initial' state determining almost all states. Second, the general problem of entering a sector of the Hilbert space whose states do not appear to have corresponding semi-classical states and thus seem to elude a classical interpretation means that there really is no physical time to be identified for states in this sector. Physical time emerges as an aspect of the relativistic spacetime that describes the late universe. As this physical time arguably grows in the direction 'away' from the natively quantum state also for the mirror universe, only the second interpretation survives scrutiny. We are thus left with a model of twin universes arising out of the same quantum soup, expanding as they mature.

The ulterior motive of this paper is to exemplify how a careful analysis of the relation between temporal and atemporal ways in which relativistic spacetimes can emerge from an underlying, non-spatiotemporal quantum structure leads to philosophically rich questions. This paper is part of a larger joint project with Nick Huggett addressing these different senses of emergence and their relation in different approaches to quantum gravity.

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*Theories and models: an approach from quantum chemistry*

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In the twentieth century, theoretical physics was implicitly adopted as the paradigm for the philosophy of science. This led to a theory-centered perspective, according to which scientific knowledge is primarily encoded in theories, whereas models only appear in specific applications. It is only in recent decades that the notion of model has gained interest in the philosophy of science. Disputing the leading role of theories, philosophers of science have recognized that models constitute a fundamental methodological resource of modern science. However, the relation between theories and 96i mis96s96 still a matter of debate. The present work tries to provide a fresh perspective to the debate by considering an example coming from quantum chemistry.

According to the traditional conceptions of scientific theories, theories apply to particular situations through the use of specific models. This supposes that models depend on theories because they are designed to make the application of the theory possible. Thus, models in science are merely mediators between theories and reality. From this viewpoint, a model is always a model of a certain theory because it is its "truth-maker". Any simplification or approximation introduced to build the model should derive from or be legitimated by the theory, since only in this case the supporting evidence can be transferred from the model to the theory.

This latter position was challenged by the so-called "toolbox" conception of scientific theories, which, from an instrumentalist stance, calls into question the dependence of models on a specific theory. The arena of the debate between the two views was mainly the model developed by Fritz and Heinz London in 1933 to explain the Meissner effect in superconductors. The richness of this case relies in the fact that the approximations introduced to account for the Meissner effect were not derived (nor could be derived) from the theory. Hence, the modeling process would be ruled not by the theory but by the phenomenon itself. Furthermore, the assumptions required for the construction of the model implied a sort of contradiction with the current theory : at odds with electromagnetism, a ferromagnetic material was considered as diamagnetic.

The London brothers' model became the paradigmatic example in the discussions between the supporters of the traditional view about models, such as Steven French, James Ladyman, Otavio Bueno and Newton da Costa (French y Ladyman 1997, 1998, 1999, da Costa y French 2000, 2003, French 1999, Bueno, French & Ladyman 2002), and the defenders of the instrumentalist conception, primarily represented by Nancy Cartwright, Mauricio Suárez and Towfic Shomar (Cartwright, Shomar & Suárez 1995, Suárez 1999, 2009, Suárez & Cartwright 2008). According to Cartwright and Suárez, the London brothers' model challenges the assumption that models depend on theories and, as a consequence, are their truth-makers. In turn, French and Ladyman (1997) try to minimize the consequences of the model by claiming that the independence of the model with respect to the theory is merely relative and historical.

The purpose of the present aim is to participate in the debate in two interrelated steps :

(i) First, we analyze the discussions around the London brothers' model from a critical viewpoint. Our aim is to show that the debate has reached a kind of dead end as the consequence of disagreements about the interpretation of the very notion of independence and its role in the constitution of scientific models.

(ii) Second, we intend to contribute to find a way out of the dead end by appealing to a new example, not yet sufficiently discussed in the current literature : the case of the molecular models used in quantum chemistry. Those models integrate two incompatible theoretical domains : quantum and classical. Quantum theory provides the Schrödinger equation to determine the energy levels of the molecule. The classical domain, through structural chemistry, establishes the geometry of the molecule, given by the fixed position of the nuclei in space. The "clamped-nucleus approximation", introduced by Born-Oppenheimer (1927), is incompatible with the Heisenberg uncertainty principle, which establishes the impossibility to assign simultaneously a definite position and a definite momentum to a quantum particle (see Hughes 1989).

The analysis of the molecular models of quantum chemistry supplies a new perspective to address the problem of the relation between theories and models. This perspective shows that the independence of models from theories cannot be considered, as the traditional view holds, as a merely relative and historical situation that will be overcome with further theoretical development. By contrast, the case of models in quantum chemistry reveals a conceptual independence that is constitutive of the modeling process. The existence of models that integrate incompatible theories constructively and in an empirically successful manner provides an argument to call into question the traditional view, according to which a model is always a model of a certain theory, on which it depends.

Leaving physics as the main arena of the debate and entering the less explored realm of chemistry, a new domain is opened to study the relation between theories and models. In fact, quantum chemistry manifests tolerance of theory incompatibility not only regarding the Born-Oppenheimer approximation, but also in the different models of molecule used in the practice of the discipline to describe molecular structure : Valence Bond and Molecular Orbital. Besides the fact that both adopt the clamped-nucleus approximation, the two models include different conceptual and qualitative assumptions about the very nature of molecules. These peculiarities of quantum chemistry lead us to wonder whether the theory-centered view of science is rooted in a kind of "imperialism" of physics that permeated the traditional philosophy of science. Perhaps turning our attention to other scientific disciplines, such as chemistry, allows us to approach to the scientific activity of modeling from a perspective completely different than that offered by theoretical physics.


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*Stability without stasis: ambition and modesty of realism about true causes*


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Realists about true causes (Novick, this session) argue that stable claims in the life sciences meet the *vera causa* ideal: it must be demonstrated (1) that a putative cause exists, (2) that it is competent to produce a particular type of effect, and (3) that is responsible for that effect in a particular instance. Life scientists themselves recognize claims that are established to this high standard as much more robust than those whose main virtue is their explanatory power.

In addition to identifying stable claims, *vera causa* realism also offers many resources for understanding instability and controversy. It gives an account of the revisions and expansion we should expect around established true causes. For instance, biochemists in the middle of the 20th century established quite firmly that DNA is a cause of hereditary traits in pneumococci. But this was accepted only after a long series of experiments and extended debate, after which most researchers came to agree that DNA could be extracted to a sufficient level of purity for its causal competence to be demonstrated, and that the experiments successfully excluded confounders. However, this hard-earned consensus about a true cause did not stifle the further growth of knowledge. To the contrary: it provided a framework for it.

First of all, there was the possibility of *alternative pathways* of heredity. Even though a heritable polysaccharide capsule had been transferred from one strain of pneumococci to another, this could have been merely a superficial trait. The question remained whether "deeper", species-specific characteristics had the same physical basis, or whether they were, perhaps, inherited via a more complex protein or nucleoprotein. In the *vera causa* framework, this is the question of responsibility: a cause of hereditary traits had been found, but how many instances of such traits were in fact attributable to that cause? Second, causes are generally not by themselves sufficient for bringing about an effect: for DNA to play its causal role, a number of *co-factors* are required that enable it to be duplicated and transcribed in a coordinated way. DNA does not do much without polymerases, helicases, topoisomerases, and a host of other proteins, all of which required extensive research and shaped our view of how DNA exerts its effects. Third, the *intermediate steps* linking DNA to phenotypic traits remained entirely open. It was far from clear that the lacunae would be filled in the way that they have been. It was initially speculated, for instance, that DNA itself possessed catalytic activity by which it directed cellular function and differentiation. There were also some mechanisms that explained away the experimental results. Some researchers suspected, for instance, that the explanation for the transformative effect of pneumococcal DNA was almost the reverse of what we now know to be the case: DNA was taken to enclose and insulate a protein that was the true carrier of hereditary information, so that our interventions (such as transferring or destroying DNA) only made it appear as if DNA produced hereditary effects.

In summary, realism about *verae causae* is far from a throwback to an overreaching, static view of scientific knowledge. Claims about true causes can turn out to rest on mistaken causal inferences (for instance, if confounding occurred), in which case revisions are indicated: scientists do not accept experimental results lightly. But even where the ideal is in fact met, the result is anything but stasis. Claims about true causes invite the search for additional ones: alternative pathways, co-factors and intermediate steps must be investigated. There is thus a rich dynamic of growth in our knowledge even while some claims that meet the *vera causa* ideal can be accepted as simply and stably true.

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**Vendredi matinée 01 juillet / Friday morning July 01**
Salle / Room 348 (auditoire/auditory)

Séance plénière / Plenary session:

*The theory-theory of concepts*

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*Abstract*

The theory-theory of concepts in developmental psychology tells us that the learning of certain concepts involves the development of mental theories, a process described as being much like that involved in theory change in science. Some philosophical objections to the theory-theory are easy to answer if we combine it with a dual-aspect semantics, in which the mental theory directly determines the intensional rather than the extensional content. The hard problem is providing the accompanying theory of reference. This paper discusses aspects of this problem.

Symposium:

*From genetics to epigenetics: what has changed?*

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*Introduction*

The blooming field of epigenetics is often presented as an innovative research domain bringing about a revolutionary change with respect to genetics. In this symposium, two philosophers, a biologist, and a lawyer will develop an interdisciplinary analysis of the relationship between these two disciplines. More precisely, each of them will compare genetics and epigenetics under some specific aspect (epistemological, philosophical, conceptual, and juridical) in order to identify the major modifications provoked by the rise of epigenetics as well as some features that have remained unchanged. Among the discussed topics will figure the kind of explanation and the methodological approach used to account for development in genetic and epigenetics terms (Merlin), the articulation of genetics and epigenetics with some key philosophical concepts such as individuality and personal identity (Pontarotti), the influence of technological advances on the conceptual framework of epigenetics (Weitzmann), and the need of different legal frameworks to deal with genetic and epigenetic information (Rial-Sebbag).

*Epigenetics and the explanation of development: the mirage of opening the developmental black box* (**Francesca Merlin**)

Epigenetics is a relatively new research domain having its origin, both as a word and as a field, in Waddington's work on development. By "epigenetics", he meant the study of the set of causal processes through which gene activity causes the phenotype to emerge (1942): his aim was to shed light into such a developmental black box. During the last twenty years, epigenetics has often been presented as a revolutionary field, showing that the relation between genotype and phenotype cannot be reduced to the information coded in the DNA sequence:

there is no genetic program for development, no predetermined epigenesis. Gene expression, cellular differentiation and, more generally, development tightly depend on a variety of chromatin biochemical marks that are dynamically involved in the construction of the phenotype. This represents a big challenge for the kind of explanation largely operating in classical and molecular genetics that provides a privileged causal role to the DNA sequence (genes) as the sole or main source of information. Despite that, we argue that recent research in epigenetics has not produced any apparent epistemological change with respect to genetics, at least for now, in the way it accounts for (the construction of) phenotypic traits. Development is absent and the recurring references to Waddington betray this author's motivation to bridge the gap between genetics and embryology. Indeed, the developmental black box containing the whole complex of causal processes connecting the genotype to the phenotype is still closed: for the moment, biologists look for correlations between differences in epigenetic marks and differences in phenotypic features. In this way, epigenetic research has made possible to add an epigenetic layer to traditional genetic explanations. However, we argue, the kind of explanation used today to account for the outcome of development has not changed: it reduces the complexity of the developmental process and its phenotypic result to the DNA sequence plus epigenetic marks (in particular, DNA methylation marks) as inputs to development. So, it is not surprising to see in the literature of the field several mentions of the "epigenetic program" driving, with the genetic one, gene expression and cellular differentiation. Having shown that, we shall be charitable because epigenetics is rather new as a research field. For instance, biologists do not yet know enough about the way epigenetic marks are propagated from cell to cell during development and transmitted from across generations. This is particularly true for histone marks. Thus, the way explanations of development are built in this field could (and should) be considered as a first step towards a more comprehensive and dynamical account of the construction of individual organisms through development.

*Epigenetics and personal identity: breaking free from biological reductionism?* (**Gaëlle Pontarotti**)

Studying molecular marks that control gene expression, the fast-growing field of molecular epigenetics could, according to some authors, shed new light on the question of personal identity. Indeed, specialists of behavioral epigenetics claim that epigenetic mechanisms could engrave the quality of social environment at the biological level (Champagne, 2010) and consequently mediate the effects of early-life experiences on adult – physiological and psychological – traits. Bolder papers published in large-audience magazines present epigenetics as a Promethean field of research that could deliver us from genetic determinism (Cloud, 2010), notably in rewriting the rules of disease, heredity and identity (Watters, 2006). In this view, context-sensitive epigenetic marks could participate in the construction of features related to personal identities.

In this presentation, I question the articulation of the concept of personal identity with molecular biology, in the context of genetics and of epigenetics. More precisely, I ask whether the field of epigenetics induces a change in the way we think about the links between biological constitution and personal features. Does the conceptual framework of epigenetics go beyond the both deterministic and reductionist approach that was notably entailed by molecular genetics and the metaphor of a highly constraining "genetic program"? In other words, does it nullify the thesis of a determination of personal features by biological constitutions? Does it further avoid the partial "molecularization" of personal identity?

Starting from an analysis of the concept of personal identity, I argue that whereas the dynamic and context-sensitive aspects of epigenetic processes seem to liberate us from genetic determinism, the introduction of epigenetic marks into the debate on personal identity reveals some confusion between distinct levels of analysis (molecular and psychological). I suggest that far from going beyond the partial "molecularization" of personal identity involved by molecular genetics, such confusion could grounds a new form of reductionism. In this context, I propose two alternative and non-reductionist ways to assess how epigenetics could shed light on the question of personal identity: the first consists in building an analogy between the dynamic construction of epigenetic profiles and personal identities; the second consists in analyzing the epigenetic mechanisms involved in brain processes responsible for the construction of a coherent image of the self.

*Advances in Epigenetics: the path from conceptual framework to molecular precision* (**Jonathan Weitzman**)

Definitions of the word "epigenetics" abound: they span over half a century of active research, from the landscape metaphor first portrayed in the black-and-white drawings of Conrad Waddington which define the genotype-phenotype interactions in developmental biology, to the colourful complexity of datasets emerging from the post-genomic boom in sequencing technologies. Indeed, epigenetics takes on different meanings in the different fields of biology. The debate about exactly what the word "epigenetics" means is likely to continue for several years, attesting to the interest that the field is attracting because of its conceptual framework and its continuous development of powerful technological tools. The debate becomes even more complex when the word is used beyond the biological community.

In general, epigenetics explores how genetically identical entities, whether cells or whole organisms, display different characteristics, and how these are inherited. The past century witnessed amazing advances in our understanding of genetics, but secrets remain hidden within the genome. Epigenetics research is now blossoming, offering a potential panacea for these post-genome blues. There is a trend to place Epigenetics in conflict with Genetics, as a new way to solve the unresolved genetic mysteries of the past century. Driven by the unfolding of molecular biology and recent technological progress, the term has evolved significantly and shifted from a conceptual framework to a mechanistic understanding. This shift was accompanied by much hype and raised hopes that epigenetics might hold both the key to deciphering the molecular underpinning of complex, non-Mendelian diseases and offer novel therapeutic approaches for a large panel of pathologies.

These advances have encouraged some to refer to the "Epigenetic Revolution", with dreams of a paradigm shift that will herald in a post-genomic and post-Central-Dogma era. This hype has led to much confusion and risks not living up the expectations, in the same way that genetics and genomics promised more than they could deliver. I will argue that the real change is the development of new technological approaches that allow us to ask old questions using new tools. The 'Nature vs Nurture' debate has been rephrased in the context of post-genomic understandings of the interaction between Genome and Environment. Now that the human genome sequence is defined, the tools of epigenetics are encouraging us to define more precisely what we mean by environment. We can now trace the impact of environmental factors on the genome with precise and molecular precision and monitor the dynamics of epigenetic marks over time. These mechanistic approaches will also allow us to determine to what extent epigenetic marks and epigenetic phenomena are reversible. The combined technological advances continue to shed light on the relationship between genotype and phenotype.

*Is epigenetic information a new legal entity?* (**Emmanuelle Rial-Sebbag**)

The generation of data in the field of epigenetics raised unexpected legal issues on how the law should cover them. As genetics and epigenetics are in close relationship, a "natural" position should have been to reason by analogy when talking about epigenetic information, in including it under the scope of the genetic information legal regime. The latter consists in framing genetic information apart from the other kinds of health information due to its potential discriminatory aspects and its interest when using it in a family context. Hence, as heritable and identifiable information, its protection is based on the need to protect individuals against potential misuse of their genetic information and to ensure the respect for confidentiality when communicating with third parties even with the family members. This position is adopted in the French regulatory framework but also at the European level such as the Oviedo Convention (article 11). Despite the adoption of these rules genetic information is not yet defined in law (except in the Data protection regulation but only from a technical perspective). On the contrary, US law has adopted a clear definition of it in the GINA act based on corroborative evidence. The definition of epigenetic information differs from the previous one as, even if a certain degree of heritability can be observed, this information can be reversible notably by acting on the environmental factor. The place and influence of the environment (defined at large) is crucial for the epigenetic field. Thus, the emergence of epigenetic information is questioning its legal categorization: does it fall under the same legal regime or do we need to create another category and another legal framework? Already discussed by Ruth Chadwick and al (Personalized medicine, 2013) epigenetic and genetic information are sharing an area in terms of legal regime but should be distinguished in terms of responsibility (responsibility of individuals for genetics / collective responsibility for epigenetics). Therefore, in the context of epigenetics, and unlike with genetics, the legal framework should move from the individuals to the environment. In other words, defining epigenetics information should emphasize on its environmental dimension (and the collective responsibility) as this dimension overturns the well-known legal model in the field of genetics. This is no longer the protection of the individual which is prominent in the epigenetic model, but the environmental dimension. However and even if some criteria can be described for epigenetic information (Personal information relating to health which is strongly influenced by the environment and with potential implications for future generations) we will discuss the opportunity to anticipate, from a legal perspective, on this new object and we will propose to focus on some improvement of the definition of genetic information as a primarily solution.

*Michel Serres: L'histoire des sciences, un modèle de communication*

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Quand on découvre pour la première fois le texte de Michel Serres Eléments d'Histoire des Sciences (1989) qu'il écrit aux côtés d'autres historiens bien connus, on doit admettre qu'il suit, dans cette œuvre, un parcours orthodoxe : il commence avec l'histoire des sciences de l'Antiquité et exalte en particulier Archimède, puis continue avec le Moyen Age pour arriver enfin à la modernité des sciences inaugurée par Galilée. Dans cette période moderne, on voit apparaître les grands personnages de la science : Linné et Darwin, Lavoisier et Pasteur, entre autres. Nous attirons ainsi l'attention sur ce texte, pour lequel Michel Serres joue le rôle de compilateur, pour mieux indiquer la profonde modification que va suivre sa démarche intellectuelle. Donc, si à partir de cette étude, il est possible de supposer ou d'imaginer les textes que continuerait à écrire logiquement M. Serres, on se trompe complètement, puisque au contraire, il aura une trajectoire intellectuelle complétement singulière qui lui permettra de proposer un nouveau regard sur l'histoire des sciences. De cette vision particulière, nous pouvons exalter trois aspects. En premier lieu, il faut indiquer que M. Serres renonce à faire une épique des Sciences en les isolant des autres savoirs et il tente par tous les moyens de lier, à différents moments, les sciences sociales et les sciences exactes. De cette manière, il retire de l'importance aux savoirs scientifiques qui, depuis la modernité, sont devenus le discours hégémonique. La perspective de M. Serres cherche à échapper aux externalistes et internalistes (termes de Georges Canguilhem), pour proposer une composition différente : les sciences comme des expressions de leur époque dont les problèmes sont aussi présents dans les autres savoirs non scientifiques. C'est par ce biais que M. Serres efface les murs entre les différents domaines du savoir, en démontrant que les séparations répondent parfois à des relations de pouvoir, plutôt que de savoir.

En deuxième lieu, M. Serres n'a pas l'angoisse habituelle qu'ont les historiens face à l'anachronisme. Alors ainsi, en surmontant ce démon et en négligeant toutes les critiques qui peuvent lui être dirigées, il invente une nouvelle méthode d'écriture dans l'histoire des sciences qui échappe au progrès des sciences et propose, en contrepartie, la rencontre des savoirs dans des moments historiques parfois très éloignés. C'est la raison pour laquelle il peut, dans un livre comme La naissance de la physique, nous montrer l'importance de l'atomisme ancien et d'Archimède pour le calcul différentiel et les modèles dissipatifs contemporains, ou bien pour la théorie des fluides et la théorie du chaos. De cette manière, M. Serres nous enseigne qu'il travaille à partir de problèmes et que, pour tenter de nous y faire réfléchir, il peut mettre en relation des théoriciens complétement distants dans le temps, mais qui ont pourtant des affinités, puisque leurs problèmes sont liés. De cette manière, on peut comprendre une certaine intemporalité des sciences parce qu'elles ne sont pas uniquement inscrites dans leur temps, mais aussi à des champs problématiques qui les dépassent et que seul le regard de l'historien peut retrouver.

Enfin, dans un troisième moment, nous voulons attirer l'attention sur la composition du temps propre à M. Serres et qui est lié aux aspects précédents. C'est ainsi qu'il part du présent pour aller vers le passé et non l'inverse. Cette méthode de travail, éloigne l'histoire des sciences des discours purement érudits, pour tenter de répondre aux questions que se pose sa société. De cette manière, on retrouve, chez M. Serres, l'actualité des problèmes des sciences contemporaines ou bien encore les transitions sociales du monde actuel qui vont lui servir pour interpeller différents moments historiques et ainsi nous indiquer que nos préoccupations ne sont pas nécessairement nouvelles. Il montre de même ainsi que, parfois, elles nous rendent contemporains des sujets de périodes historiques très éloignés. Sous cette perspective du temps, il peut aussi nous indiquer la profonde singularité du présent et nous démontrer le compromis que nous devons effectuer pour modifier nos cerveaux et nos pratiques par rapport à la matérialité de notre monde.

Ainsi, avec des exemples ponctuels, nous chercherons à montrer comment M. Serres inaugure une conception de l'histoire des sciences qui peut servir à plusieurs aspects : trouver les rapports entre les sciences exactes et les sciences sociales, reconnaître les rencontres entre les savoirs théoriques et les savoirs appliqués ou bien encore arriver à de nouvelles compositions entre le présent et le passé des sciences.

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Duhem, dans la conclusion de son ouvrage sur la théorie physique, affirme que « faire l'histoire d'un principe physique, c'est, en même temps, en faire l'analyse logique » (Duhem, Pierre, La théorie physique: son objet, sa structure, Vrin, 2007, p. 368). Il est important toutefois de constater que, selon le physicien, cette affirmation n'est pas valable en ce qui concerne les principes mathématiques, ceux de la géométrie par exemple, où « l'enseignement peut se donner de manière entièrement logique »(Ibid, p. 367). Plus qu'un lien entre analyse logique et analyse historique – qu'il serait nécessaire de caractériser plus en détail – la phrase de Duhem pose une différence fondamentale entre les principes de la physique et ceux des mathématiques pures.

Mais en quoi consiste exactement cette différence ? Elle réside dans le fait que « le seul moyen de relier les jugements formels de la théorie [physique] à la matière des faits que ces jugements doivent représenter [...] c'est de justifier chaque hypothèse essentielle par son histoire » (Ibid, p. 368). Dans cette affirmation transparaît une compréhension des théories physiques chère aux Empiristes Logiques, comme Schlick, Reichenbach, Carnap, ou encore Hempel, selon qui les théories physiques sont constituées par un ensemble de principes généraux qu'il faut ensuite relier au socle de l'expérience grâce à des « principes de liaison » (Reichenbach parle de « principes de coordination » ou de « définition coordinatives », et Hempel de « principes de liaison » (bridge principles), dans les Éléments d'épistémologie). Mais l'innovation de Duhem transparaît dans l'affirmation que les principes doivent leur justification à l'histoire ; c'est-à-dire ni à la logique, ni à l'expérimentation.

On pourrait se demander s'il y a là réellement un écart avec la conception empiriste logique, car en tout état de cause, pour les philosophes de ce courant, les « principes de liaison » ne sont pas autre chose que des conventions, dont la justification ne provient en effet ni de la logique, ni de l'expérimentation, mais de considérations de simplicité et de commodité, dont l'histoire peut nous donner la source et la pertinence en fonction du contexte. Cependant Duhem s'écarte bien de la doxa empiriste logique en ce qu'il considère l'histoire non pas seulement pertinente vis-à-vis du contexte de découverte (ce que les empiristes accorderaient volontiers) mais surtout vis-à-vis du contexte de justification (Duhem dit bien qu'il faut « justifier [...] par [l'] histoire »).

Cette distinction, opérée par Reichenbach (Voir Reichenbach, Hans, Experience and prediction: an analysis of the foundations and the structure of knowledge, Chicago, 1976), et en effet essentielle pour l'empirisme logique car elle permet d'écarter des processus de validation des connaissances les aspects historiques et sociaux, sans leur ôter leur pertinence pour la compréhension de la science en général. De ce point de vue, Duhem est en accord total avec l'empirisme logique à venir, puisqu'il se sert lui-même de la distinction pour le savoir mathématique : « L'histoire des Mathématiques est, assurément, l'objet d'une curiosité légitime ; mais elle n'est point essentielle à l'intelligence des Mathématiques ».

Mais pourquoi l'histoire serait-elle amenée à jouer un rôle fondateur pour la connaissance physique, et non pas pour la connaissance mathématique ? Et comment l'histoire peut-elle seulement justifier une hypothèse physique ?

Il nous semble que ces deux questions peuvent recevoir une réponse à travers l'étude du premier livre de Reichenbach, La théorie de la Relativité et la connaissance a priori (Reichenbach, Hans, The theory of relativity and a priori knowledge, Berkeley, Etats-Unis, 1965). Dans ce livre publié en 1920, Reichenbach se propose de montrer que la récente théorie de la Relativité Générale permet de réfuter une grande partie de la philosophie kantienne ; selon l'expression de Michael Friedman (Voir Friedman, Michael, Reconsidering logical positivism, Cambridge, Cambridge University Press, 1999), il y introduit une « relativisation » du concept d' a priori. Reichenbach explique en effet que a priori chez Kant possède deux significations : « nécessairement vrai » et « constitutif du concept d'objet » (Voir Reichenbach, The theory of relativity and a priori knowledge, p. 48). Le philosophe de Berlin entend réduire le concept à la deuxième signification seulement, afin de pouvoir caractériser adéquatement le statut des « principes de coordination », qui bien que constitutifs pour la théorie physique dans laquelle ils apparaissent, ne sont pas pour autant à l'abri d'une révision lors d'un progrès ultérieur de la science. Constitutif mais révisable : voilà ce que signifie a priori pour Reichenbach en 1920.

À la suite d'une correspondance avec Moritz Schlick, à la fin de l'année 1920, Reichenbach se laisse convaincre par son influent aîné que ses « principes de coordinations » ne sont rien d'autre que de simples conventions. Dans ses ouvrages ultérieurs (Reichenbach, Hans, Axiomatization of the theory of relativity, Berkeley (Calif.), Etats-Unis, 1969 ; et Reichenbach, Hans, The philosophy of space & time, New York, Etats-Unis, Dover publ, 1957.) Reichenbach développera une conception des théories beaucoup plus proche de la doxa empiriste logique, une forme d'empirisme conventionnaliste. Témoin de ce changement, dans le classique La Philosophie de l'espace et du temps, Reichenbach rebaptise ses principes de coordinations des « définitions coordinatives » pour mieux marquer ce caractère conventionnel.

Contrairement aux interprétations néo-kantiennes qui sont habituellement faites du texte de 1920, nous voudrions montrer qu'il peut au contraire servir à fonder un empirisme inédit qui intégrerait le développement historique comme élément de justification des connaissances. Il faudra cependant pour cela expliquer pourquoi les « principes de coordination » ne sont pas de simples conventions, ainsi que Reichenbach l'accorda à Schlick, mais jouent au contraire le rôle de l'élément manquant dans la justification des hypothèses physiques « par l'histoire ». Ce faisant, nous nous écarterons aussi de l'interprétation qu'en donne Michael Friedman dans Dynamics of Reason (Friedman, Michael, Dynamics of reason: the 1999 Kant Lectures at Stanford University, Stanford, Calif., CSLI Publications, 2001.).

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*L'empirisme modal*

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Le réalisme scientifique est la position suivant laquelle les théories scientifiques décrivent correctement la réalité. A l'inverse selon certaines positions empiristes, notamment l'empirisme constructif, nous devrions seulement accepter que nos théories sont empiriquement adéquates, c'est à dire qu'elles « sauvent les phénomènes ». Le réalisme structural a été proposé comme une solution de compromis entre ces deux positions à même de répondre aux arguments des deux camps. Il s'agit d'affirmer que les théories décrivent correctement sinon la nature de la réalité, au moins sa structure relationnelle. L'objet de cette intervention est de proposer une position originale dans ce débat, proche du réalisme structural : l'empirisme modal. Elle consiste à affirmer que nos théories décrivent correctement les relations de nécessité physique entre observations possibles.

Tout comme l'empirisme constructif, l'empirisme modal affirme que nos théories scientifiques sont empiriquement adéquates, mais cette notion d'adéquation empirique demande à être précisée. Après avoir envisagé plusieurs possibilités, on optera pour la définition suivante : une théorie est empiriquement adéquate si ses modèles font de bonnes prédictions chaque fois qu'ils s'appliquent à des situations concrètes. Il conviendra bien sûr d'élucider ce que revêt la notion d'applicabilité, mais même une fois cet aspect clarifié, l'adéquation empirique peut être comprise d'au moins deux manières. Selon une première compréhension, les modèles de la théorie font de bonnes prédictions pour toutes les situations actuelles auxquelles ils s'appliquent (il peut éventuellement s'agir de toutes les situations passées, présentes ou futures, observées ou non). Selon une seconde compréhension, ils font de de bonnes prédictions pour toutes les situations possibles auxquelles ils s'appliqueraient.

Cette deuxième position suppose un engagement envers les modalités naturelles : il existe une nécessité dans le monde contraignant les situations possibles et leurs déroulements. C'est ce qui la distingue des autres positions empiristes et la rapproche d'un réalisme. Elle se distingue toutefois également d'un réalisme pur en ce qu'elle ne se prononce pas quant au contenu des théories qui n'est pas directement observable : tout ce qu'elle affirme, c'est que nos théories rendent compte correctement des rapports de nécessité entre observations possibles. C'est cette position que j'appelle l'empirisme modale.

Il reste à situer cette position dans le débat sur le réalisme scientifique en la confrontant aux différents arguments. Je défendrai l'idée qu'elle conserve les atouts de l'empirisme, mais permet mieux que lui de répondre aux arguments réalistes.

L'empirisme peut être motivé par la sous-détermination des théories par l'expérience : plusieurs théories alternatives peuvent en principe rendre compte des mêmes phénomènes en postulant des entités différentes, et alors nous ne pouvons savoir laquelle est vraie. L'empirisme modal n'est pas victime de cette difficulté dans la mesure où il est agnostique sur l'existence des objets inobservables postulés par nos théories. De plus si deux théories diffèrent quant aux relations de nécessité entre observations qu'elles impliquent, il est en principe possible de les départager en mettant en œuvre une expérience appropriée.

Un autre argument est celui de l'induction pessimiste. Il n'existe pas de continuité quant à l'ontologie postulée par les théories successives lors des changements théoriques, ce qui met en péril le réalisme à propos de nos théories actuelles, puisqu'elles seront probablement remplacées à l'avenir par de nouvelles théories. De nouveau l'empirisme modal conserve les atouts empiristes : on peut considérer que les lois d'observation impliquées par les anciennes théories aujourd'hui abandonnées (par exemple la loi de la chute des corps de la physique classique) sont toujours valables dans leur domaine d'application, et sont conservées d'une théorie à l'autre, y compris si on les conçoit comme correspondant à des rapports de nécessité naturelle.

Côté réaliste, il est généralement avancé que le réalisme scientifique est la meilleure explication au succès prédictif des théories scientifiques, voire la seule explication qui n'en fasse pas un miracle, notamment quand ces théories font de nouvelles prédictions inattendues. L'empirisme, en affirmant seulement que nos théories sont empiriquement adéquates, se contenterait d'affirmer ce succès au lieu de l'expliquer. L'engagement de l'empirisme modal envers l'existence de rapports de nécessité dans le monde lui permet de répondre à cet argument, puisque ces rapports de nécessité peuvent expliquer le succès empirique des sciences, en particulier si les nouvelles prédictions inattendues sont interprétées en terme d'actualisation de situations possibles.

Un autre argument contre l'empirisme met en avant l'indispensabilité des termes théoriques, en particulier dispositionnels, qui ne sont pas réductibles à leurs manifestations observables mais jouent un rôle crucial dans les inférences scientifiques. Cette indispensabilité nous demanderait un engagement plus fort envers ces termes théoriques. Cependant les termes dispositionnels pourraient être identifiés à des relations modales entre observations plutôt qu'à des propriétés naturelles, et en effet, celles-ci ne se réduisent pas à leurs manifestations actuelles.

La difficulté la plus sérieuse reste que la position semble reposer sur une distinction entre observable et inobservable qui peut être critiquée. Quelques pistes seront envisagées pour y répondre.

Pour finir l'empirisme modal sera comparé aux différentes versions de réalisme structural, qui prétendent également répondre aux arguments du débat sur le réalisme. Le réalisme structural épistémique affirme que nous pouvons seulement connaître les relations entre des objets inaccessibles de la réalité. Cependant les relations y sont traditionnellement conçues comme purement logico-mathématiques, ce qui soulève la charge de la trivialité (objection de Newman). Le réalisme structural ontique parle, pour éviter cette objection, de relations modales, à l'instar de l'empirisme modal. Mais il diffère de ce dernier en ce qu'il se veut une position métaphysique sur la nature de la réalité. A ce titre, l'empirisme modal pourrait mieux rendre compte de l'ancrage empirique des théories scientifiques.

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*An evolutionary and mechanistic explanation of moral intuitions*

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Despite the wide use of the notion of moral intuition in the philosophical and psychological literature, its functioning and psychological features remain a matter of debate. We still lack a general explanation of why humans have intuitions in given circumstances, but not in others; is there a general pattern or a mechanism that could explain or predict the occurrence of moral intuitions? In this talk, I will answer this question with a novel evolutionary account of the capacity to experience moral intuitions. This account (developed in collaboration with Chloë FitzGerald [1]) captures the core phenomenological and mechanistic features of intuitions, highlights the eliciting conditions for intuitions, and explains why they are very resistant even in the face of evidence of their interpersonal variability and even when we face good reasons for questioning them, or discounting their force. I will conclude with some remarks on the normative consequences of this account.

In this talk, I'll argue that one important individuation criterion for intuitions is their affective component: an intuitive evaluation feel more compelling than an ordinary evaluation and this feeling tends to shut down or bias further reflection. I'll call it the feeling of rightness (FOR). The FOR belongs to the particular class of 'metacognitive feelings' which are vivid affective experiences about our own thought processes and cognitive capacities. The FOR is about the correctness of an evaluation that arises quickly and fluently in one's mind.

I'll hypothesize that the FOR is produced by the activity of a simple task-specific psychological system, whose mechanistic functioning and biological function can be singled out. The FOR plays a role at the interface between Type 1 (spontaneous and automatic) and Type 2 (slow and rational) thinking [2]. The FOR favors spontaneous Type 1 responses and discourages or biases Type 2 thinking [3].

Here is an evolutionary explanation for why the FOR favors Type 1 responses. The evolution of the capacity for Type 2 thinking created a problem for early humans. Abstract thinking and reasoning allowed humans to communicate with each other, to cooperate in large groups and to develop sophisticated tools and techniques [4]. However, it is sometimes more adaptive to make a decision based on one's 'gut feelings', or Type 1 processes, rather than based on careful deliberation [5]. Humans often need to make quick decisions about complex problems based on incomplete knowledge. In these circumstances, they may sometimes be better off abandoning rational weighting of all relevant cues, relying instead on evolved task-specific systems that react to the most important information and ignore the rest. Thus, with the development of Type 2 thinking, a need to prevent the excessive interference of rational deliberation in the generally adaptive operation of evolved Type 1 task-solving systems emerged. This need led to the co-evolution of a further system: the feeling of rightness system (hereafter FORs). The evolutionary function of the FORs is to help humans select optimally among competing evaluative responses that are simultaneously present in their minds. The FOR makes the responses produced by evolved task-specific systems more appealing. It thereby facilitates decision-making and biases it in favor of systems that have been selected for solving the task at hand, or a similar task.

At the mechanistic level (see figure below), as with any evolved system, the FORs responds to certain stimuli and produces distinct task-specific operations: the stimuli processed are conflicting evaluative responses; the operation performed is to give priority to the response that arises most spontaneously in the subject's mind because it is a reliable indicator for the activity of an adaptive task-solving system. Phenomenologically, the FORs expresses itself as an FOR, which pushes us to think that the spontaneous evaluation is correct.

This explanation of moral intuitions makes an important distinction between the phenomenological aspect (the FOR) and the mechanistic aspect (FORs) of intuitions. This distinction enables to single out the eliciting factors for intuitions: the experience of intuition arises during Type 2 mental activity and only against a background of contradictory evaluations produced by different task-solving systems. Thus, any evaluation that feels intuitive has been challenged by a conflicting evaluation while a Type 2 process took place. Thanks to this account of intuitions, it is possible to explain why a spontaneous Type 1 evaluation sometimes feels intuitive and sometimes not. It is also possible to predict when people are more likely to experience moral intuitions (e.g. when they discover conflicting moral views), and to explain why intuitions remain so compelling and resistant (i.e. we are typically convinced that our intuitions are right and should be universally held) in the face of reasons to discount them.

To summarize, mechanistically, moral intuitions are a two-component phenomenon involving a challenged evaluation and a feeling of rightness (FOR). The FOR is produced by a psychological system (the FORs) whose eliciting factors are conflicting evaluations. The FORs prioritizes the response that comes quickly and fluently to the subject's mind. It thus facilitates optimal decision-making, preventing excessive interference by rational deliberation.

I'll conclude with a short analysis of the normative implications of this account. Moral intuitions' identity criteria are independent of moral appropriateness. Therefore, intuitions may reinforce evaluations that can easily be justified (i.e. intuitive condemnation of jihadists' attacks) as well as evaluations that can hardly be justified (i.e. intuitive reluctance against Angela Merkel's open immigration policies). Thus intuitions are poor devices for discovering the right and the wrong.

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*The patterns of life, and a new response to the species problem*

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Traditionally, the Tree of Life (TOL) hypothesis has been the only way to represent the history of life in the light of the synthetic theory of evolution (STE). The TOL has also been considered the only way to represent the relations among species. The picture of the TOL is an inclusive hierarchical pattern, where the only possible relations between branches are divergences and taxa are in the branches terminals. This means that, according to the traditional view, a single rooted and dichotomously branching representation of the relationships between all life forms is the best representation of the history of species and all levels above them.

Nowadays, the validity of the TOL hypothesis is under dispute (Doolittle and Bapteste 2007; Franklin-Hall 2010; O'Malley and Dupré 2010; Soucy, Huang and Gogarten 2015). For instance, Cheryl Andam, David Williams and Johann Gogarten (2010) claim that the complexity and the diversity of the living world cannot be reduced to a single evolutionary process of vertical inheritance. Although vertical inheritance is true for microorganisms, many biological processes that depart from a simple furcating path generate biological diversity, metabolic innovations, and units of selection. In this sense, horizontal gene transfer is an important mechanism in the evolution of prokaryotes and even of unicellular eukaryotes; however, it is not recognized as an evolutionary mechanism by STE. Furthermore, several authors deny the capability of the hypothesis of TOL to give an account for the history of life and, on this basis, propose an alternative pattern known as Web of Life (WOL) (Doolittle and Bapteste 2007; Soucy, Huang and Gogarten 2015).

On this basis, in this paper we will argue for two related but independent assertions: (i) the interpretation of the history of life supplied by the TOL does not solve the species problem since it privileges the genetic level and, consequently, some biological processes over others; and (ii) the multiple definitions of species and the different patterns that may explain the history of life are not mutually exclusive, but can be integrated as simultaneously valid from the perspective supplied by a Kantian rooted ontological pluralism.

According to Marc Ereshefsky (2010), at least three approaches can be invoked to define a species and more than a dozen different concepts account for different aspects of biodiversity. In this paper we will analyze these approaches in the light of the knowledge, provided by molecular biology, about the relationships among the different "lineages" of prokaryotic life forms. For example, the biological species concept has an excessive pretension regarding its explanatory power. In agreement with this concept, the synthetic theory of evolution has privileged certain evolution mechanisms: natural selection, gene flow, mutation and genetic drive. But these mechanisms and the biological species concept cannot account for the evolutionary history of prokaryotes because they are not the only evolutionary mechanisms to explain microevolution (Doolittle and Bapteste

2007; Soucy, Huang and Gogarten 2015). This fact has relevant consequences regarding the conception of the history of life: whereas the traditional mechanisms naturally lead to a picture of tree, the possibility of horizontal genetic transference promotes the view of a web.

Nevertheless, instead of advocating for the replacement of the TOL view with the WOL view, we will argue for the coexistence of different conceptions of species and of both ways of conceptualizing the history of life. We propose the following question: Is it necessary to opt for a definition of species? Or, by contrast, diversity can be retained with no need to choose one of them as the privileged one? We will suggest that a Kantian rooted pluralist realism supplies a valid interpretive framework to the problem of species and of the patters that explain the history of all life forms.

According to the Kantian rooted pluralist realism of Olimpia Lombardi and Ana Rosa Pérez Ransanz (2012; see applications to chemistry in Lombardi and Labarca 2005, 2006; Lombardi 2014), scientific knowledge is knowledge of phenomena. The subject of knowledge is no longer a passive spectator: he imposes its own categorical and conceptual schemes to the independent reality to constitute the ontology. The objects to which we have cognitive access are objects in the Kantian sense: they result from the synthesis between our schemes and the noumenal reality, independent of us. But, unlike the Kantian doctrine, from the pluralist realism here considered, it is accepted that there are categorical and conceptual schemes that are alternative and not converging nor reducible to a single scheme. This thesis of conceptual relativity naturally leads to an ontological pluralism that assumes the possibility of the coexistence of different conceptions of the world, with their own different ontologies, sometimes incompatible with each other, but appropriate in certain contexts according to certain interests and goals.

From the interpretive framework given by this ontological pluralism, we will propose to consider other evolutionary mechanisms, such as horizontal gene transfer, as views underlying different but equally legitimate conceptions of species. Moreover, we will consider the WOL as a pattern that deserves to be taken into account but, at the same time, does not override the explanatory power of the TOL, since the two patterns are both genuinely valid interpretive frameworks. In summary, we will propose a Kantian rooted ontological plurality in the three main subject matters considered: the definitions of species, the relevant mechanisms for speciation, and the patterns that represent the history of life.

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*Functions in biology and in technology: in defence of a unified account*

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*Abstract*

In this paper I will present arguments for a unique account of functions in biology and in technology. The biological-artificial system distinction seems to be motivated by a principle of empirical sciences to avoid making reference to unclear teleological notions such as designer. Intuitively, it appears that keeping the designer in the functional analyses of man-made systems is less problematic. However, even this can lead to difficulties in the attributing functions. Should we take into account designer's intentions or user's intentions? What about failure in design that lead to systems with useful and unforeseen applications? Furthermore, the biological-artificial system distinction is conceptually and empirically untenable. Developments in synthetic biology provide concrete examples of artificial biological systems (Hutchison et al. 2016). I will argue that systemic account in

line with Cummins (1975) is either able to accommodate the desiderata for functional analyses as resumed by Wouters (2005) and more recently by Garson (2016) or to respond to their lack with the advantage of unifying accounts of function in biology and in technology.

_Keywords_: Function, biological functions, functions in technology, systemic account, unified account

*Extended abstract*

The notion of function plays a central role in biological explanations. In engineering, it is used in design and in analysis of technologies. The different uses of the term in these disciplines would suggest that there are different kinds of functions. Furthermore, even in the domain of biology, different analyses can attribute different functions to the same feature of the same organism.

For instance, Aristotle, contrary to Plato, thought that the heart was the seat of sensations and intelligence and the role of the brain was important but limited to the cooling of the heart (Gross 1995). After a basic course of biology, one would dissociate the sensations from functions of the heart and rather say that the function of a heart and even an artificial one is to pump blood. But is it? A study of heart–brain system in a patient with an artificial heart suggests that the heart affects the emotional and cognitive processes (Couto et al. 2014).

Aristotelian teleological explanations had the advantage of being applicable to both biological organisms and artefacts. However, the teleological notions are not satisfactory for empirical science, such as biology. Darwin's theory of evolution by natural selection removed teleology[4] and a designer from the explanations concerning biological systems, but did not affect functional attributions based on intentions in artificially created systems.

The distinction between functions in biology and in technology would be acceptable if one believed that there is a fundamental difference between living systems and artificial (man-made) systems. A consequence of such a supposed distinction is the multiplicity of the meanings of 'function' – some for biology, others for artefacts. Even if it seems that taking intentions into account for functional ascriptions in man-made systems is not problematic, it is not clear whose intentions should count. An obvious choice would be the designer's intentions. However, there are cases of failed design but successful use. The problem with user's intentions is that there may be several users of the same system. To avoid these problems, I would suggest to follow the example of biology and to remove intentions from functional ascriptions.

A further argument for a unified account of functions in biology and in technology is that the biological-artificial system distinction is not tenable. A simple conceptual analysis is sufficient to show that these two notions are independent and their extensions may overlap. Therefore, it is possible for a system to be both biological and man-made. Furthermore, developments in synthetic biology provide concrete examples of such systems (Hutchison et al. 2016).

The systemic account proposed by Cummins (1975) provides an analysis of functions applicable to both biological and artificial systems (and even combinations of those). The seeming shortcomings of this analysis are surmountable with suitable definition of the studied system and its dispositions (actual or hypothetical activities). Furthermore, with precisions of systems and dispositions relative to environments, theories based on propensity Bigelow and Pargetter (1987) can be expressed in the systemic framework and the difficulties of aetiological theories (vestigial traits, past selection vs. actual contribution) can be accounted for.

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4 Justin Garson (2016, Section 3.2, p.32) points out that the claim that teleology was removed from biology by Darwin is 'not quite correct'.

*Who is afraid of scientific imperialism?*

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*Short Abstract*

In recent years, many authors debated about the justifiability of scientific imperialism, the systematic application of a discipline's findings and methods to model and explain phenomena investigated by other disciplines. To date, however, widespread disagreement remains regarding both the identification and the normative evaluation of scientific imperialism. In this paper, I aim to remedy this situation by articulating an informative characterization of scientific imperialism and by providing a normative evaluation of the most prominent criteria proposed to ground opposition to it. I shall argue that these criteria provide an informative basis for assessing some instances of scientific imperialism, but do not yield cogent reasons to think that scientific imperialism is inherently disputable and should be resisted. I then highlight this result's implications for the ongoing philosophical debate about the justifiability of scientific imperialism.

Keywords: Scientific Imperialism; Scientific Change; Disciplinary Autonomy; Disunity of Science; Epistemic Justification; Pragmatic Justification.

*Extended Abstract*

Over the last few decades, there has been significant philosophical discussion of so-called scientific imperialism (henceforth, SI), the systematic application of a discipline's findings and methods to model and explain phenomena investigated by other disciplines (e.g. Clarke and Walsh, 2009 and 2013, Cartwright, 1999, Dupreé 1995 and 2001, Mäki, 2009 and 2013). The involved authors provided increasingly sophisticated conceptualizations of this notion and debated at length about the justifiability of specific instances of SI. There are at least two reasons why SI deserves detailed philosophical scrutiny. First, SI contributions target a wide range of both natural and social disciplines, and have potentially widespread implications for modelling and theorizing in these disciplines (e.g. Lazear, 2000, on the impact of economists' SI contributions on other decision sciences). And second, SI contributions raise pressing epistemic and pragmatic concerns, which bear on issues of great societal and political relevance (e.g. Dupré, 2001, on the political implications of evolutionary psychologists' SI contributions, and Vincent, 2013, on the implications of neuroscientists' SI contributions for widely held conceptions of moral and legal responsibility). To date, however, widespread disagreement remains regarding both the identification and the normative evaluation of SI (e.g. XXX on calls for and against so-called neuroscience imperialism). In this paper, I aim to remedy this situation by articulating an informative characterization of SI and by providing a normative evaluation of the most prominent criteria proposed to ground opposition to SI.

The paper is organized as follows. In Section 2, I explicate the notion of SI and distinguish it from various forms of non-imperialistic cross-disciplinary interaction. In Sections 3-6, I identify and appraise four influential criteria proposed to ground a normative critique of SI. I shall consider in turn: the objection from disciplinary autonomy (e.g. Aizawa and Gillet, 2011, Fodor, 1974), which opposes SI contributions on the alleged ground that these contributions reduce or threaten the relative autonomy of the imperialized disciplines from the imperializing ones; the objection from the disunity of science (e.g. Cartwright, 1999, Dupre?, 1983), which holds that the modelling and explanatory differences between the imperializing and the targeted disciplines undermine the prospects of SI interactions between such disciplines (e.g. Dupre?, 1995, for putative examples of failed economics imperialism); the objection from counterfactual scientific progress (e.g. Clarke and Walsh, 2009 and 2013), which opposes SI contributions insofar as these contributions preclude the targeted disciplines from progressing in the way they would have progressed in the absence of SI contributions; and the objection from cumulative constraints, which subordinates the justifiability of SI to the satisfaction of a series of evaluative constraints (e.g. Mäki, 2009 and 2013, on ontological, axiological, institutional, and epistemological constraints).

I shall argue that these criteria provide an informative basis for assessing some instances of SI, but do not yield cogent reasons to think that SI is inherently disputable and should be resisted. If correct, this result has at least three implications of general interest for the ongoing debate about the justifiability of SI. First, the critics of SI should provide more convincing reasons for their opposition to SI and ground their calls against SI on more plausible empirical and normative presuppositions. Second, what is objectionable (if anything) about some SI contributions is not their imperialistic character, but rather the empirical and/or normative flaws in their presuppositions (e.g. Longino, 2002, for some illustrations) and the unwarranted societal and/or pragmatic implications some derive from such contributions (e.g. Rolin, 2015, on various cases of epistemically unjustified marginalization of dissent). And third, the justifiability of SI contributions is best judged in terms of specific case studies rather than in terms of general evaluative criteria that abstract away from the modelling and explanatory practices of the examined disciplines.

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*Poincaré on the role of beauty in science*

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While Poincaré's views on the aim of science have received significant attention, his views on the aesthetics of science are still unexplored. In this paper I offer the first systematic analysis of Poincaré's understanding of beauty in scientific theories and show how this account fits with his views on the aim of science.

Aesthetic judgements are integral part of scientific practise. Scientists employ aesthetic judgements in the selection of phenomena to study, the construction of hypotheses, the evaluation of theories and in deciding their epistemic commitments towards a theory. For Poincaré beauty is an important motivation for the study of nature. He argues that "[t]he scientist does not study nature because it is useful to do so. He studies it because he takes pleasure in it, and he takes pleasure in it because it is beautiful" (Poincaré 2001, 368). Poincaré clarifies that beauty equates to the harmonious order that our theories reveal and not to the beauty that 'strikes the senses'. Poincaré concerns himself with the beauty found in our theories that unveil unity and harmony in the phenomena. According to him the aim of science is to offer us understanding of the underlying relations between phenomena, of the harmony in nature. It is in this underlying harmony or unity that our theories uncover that we find beauty. I argue that for Poincaré beauty is an aesthetic property that reduces to the elegance and unity of scientific theories. I reconstruct the argument for simplicity and the argument for unity in order to show their relationship and the overall consistency of Poincaré's position.

Starting with the argument for simplicity, I address the following three questions: (1) how is simplicity defined; (2) how is it used; and (3) how is it justified. I argue that Poincaré is mainly concerned with the mathematical elegance of scientific theories. When it comes to the applicability of simplicity, I argue that Poincaré takes simplicity to play a purely methodological, not epistemic, role. That is, simplicity does not lead us to true theories; rather, it aids our decision-making. Aesthetic values guide our choice in the construction and selection of hypotheses (ibid., 99), but these are not regarded as objective properties. Whilst simplicity plays a methodological role it does not have epistemic significance, it leads to convenient not true theories. To support this claim, I point to a historically motivated argument, offered by Poincaré, which shows that the development of science sheds doubt that nature in itself is simple (ibid., 99-100). He argues that simplicity is not linked to truthlikeness, it is not a guide to the true nature of reality, but is a condition of our making.

Turning to unity, Poincaré identifies is with the grasp of the harmony between the phenomena that scientific theories give us, which ultimately provides us with understanding of these phenomena. I argue that the harmony our theories reveal cannot be understood in either objectivist of projectivist terms. It is not an objective feature of the world outside our mental capacities; however, it is not a merely subjective feature projected on nature by us. Appealing to a form of intersubjective validity, Poincaré argues that unity is part and parcel of our intellectual capacities and an ideal we follow in our enquiries (ibid., 396-397).

I argue that for Poincaré there is a sharp distinction between unity and simplicity. We presuppose unity as a regulative ideal (ibid., 112). Simplicity has purely instrumental value insofar as it promotes the ultimate goal of science, that of unification. Unity, on the other hand, is considered the goal of science and an ideal we follow in our enquiries because it gives us understanding. For Poincaré, beauty is to be found in the harmony our theories reveal. It is to be found in the hidden relations that our theories uncover and the unification that they give us in showing how different, apparently disconnected phenomena, unite. It is this harmony that Poincaré takes to give us understanding.

With these elements of Poincaré's argument in place, I turn to the question of justification: how are simplicity and unity justified as aesthetic principles guiding scientific enquiry? I argue that while for Poincaré simplicity is justified because it is linked to the goal of unification, unification is taken to be the ultimate goal of science. It is in revealing 'hidden kinships' and 'real relations' in the phenomena that Poincaré finds the aim of science accomplished.

To further defend my account of Poincaré's aesthetics of science, I analyse the link between aesthetic judgement and utility. I turn to Poincaré's account of creativity in scientific discovery. It is here that Poincaré makes the important link between utility and aesthetic judgement. He argues that it is our aesthetic sensibility that guides the selection of useful and fertile hypotheses during the creative process. Poincaré argues that the useful ideas are the ones that trigger the scientists' aesthetic sensibility. It is in this context that he appeals to simplicity and harmony. The aesthetic sensibility, Poincaré argues "plays the part of the delicate sieve" which checks the result blindly generated by the mind and selects only the most elegant and beautiful combinations produced (Poincaré ibid., 397). For Poincaré the aesthetic sensibility selects the theories that best suit our aesthetic requirements, but he also claims that "[t]he useful combinations are precisely the most beautiful" (ibid.).

I argue that Poincaré's account offers new ways of thinking about aesthetics judgement in science. By reducing aesthetic judgements ultimately to being judgements about the unity and simplicity of scientific theories, Poincaré offers an interesting reductivist account of aesthetic properties. I show how this view of beauty of scientific theories and creativity are compatible with his views that the aim of science is the construction of a convenient system of relations that aims to describe the structure of nature.

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## *Does epistemic pluralism foster scientific progress?*

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In his Is Water H20? Evidence, Realism and Pluralism (2012), Hasok Chang brought forward an argument for epistemic pluralism according to which the cultivation of multiple systems of practice in a field of research is supposed to be favorable for progress within that field. The term "system of practice" in this context denotes a set of epistemic activities performed to achieve certain scientific aims. Maintaining a plurality of such systems has, according to Chang, two kinds of benefits: The first are benefits of tolerance: By maintaining several systems of practice, we avoid the risk of following a single, potentially erroneous line of inquiry. Also, by allowing for more than one system of practice, we are in a better position to satisfy different scientific aims and epistemic values. The second type of benefits results from the interaction between different systems of practice. Putting different systems in contact to each other leads to mutual stimulation. Below the line, pluralism facilitates scientific progress and guarantees a maximal yield of knowledge.

I will begin my talk by outlining the details of Chang's argument for pluralism. I will also briefly review some of its criticisms (Chalmers 2013, Mauskopf 2013, Klein 2014, Kusch 2015). I will show that these criticisms mainly attack Chang's way of defending pluralism and not so much pluralism per se. They argue that Chang's historical case study (the chemical revolution in the eighteenth century), and the way in which Chang interprets the case, does not provide sufficient support for pluralism.

I will then go on and try to defend Chang's plea for pluralism based on a systematic argument for pluralism that does not depend on the interpretation of a specific historical case study. The argument is not mine. It was developed in an almost forgotten debate about the benefits of pluralism in science in the 1960s and early 1970s. It is Paul Feyerabend's idea of anomaly import between incommensurable theories (see Bschir 2015).

Feyerabend developed his idea of anomaly import against the background of a critique of Kuhn's notion of normal science on the one hand, and a critique of the monistic model of theory testing of the empiricist tradition on the other. According to the monistic model of theory testing, a theory can be tested against empirical facts in isolation and independently of alternative theories. The crucial point of Feyerabend's approach consists in the claim that a potential mismatch between a theory and certain observational facts can be made apparent only if those facts are described from the perspective of an alternative theory. Anomalies can be imported, as it were, into existing theoretical frameworks by considering alternatives. Hence the availability of alternatives is of crucial importance for the detection of deficiencies and anomalies in well-established and well-confirmed theories. As a consequence, the proliferation of theoretical approaches has a catalytic effect on scientific progress.

Feyerabend also claimed that anomaly import works best between incommensurable, i.e. logically incomparable theories. Because of the problematic character of the notion of incommensurability, Feyerabend's pluralism based on the idea of anomaly import has been heavily criticized (see for instance Worrall 1978). However, by taking into account the fact that Feyerabend's pluralism was initially presented as an extension of Popper's critical rationalism, and that Popper had already developed the basic idea of a pluralistic test-model without reference to incommensurability, I will argue that Feyerabend's extension of Popper's framework does not necessarily presuppose incommensurability. (Feyerabend initially admitted that his idea of theoretical pluralism was based on Popper's critical rationalism. He later withdrew his confession.)

I conclude that the idea of anomaly import (sine incommensurability) provides a refined explanation for why the interaction between different systems of practice can foster progress in a field of inquiry. The idea of anomaly import therefore lends systematic support to Chang's epistemic pluralism, a kind of support that is not affected by criticisms that attack Chang's historical analysis. Although Chang does refer to Feyerabend as a source of inspiration, and although his own line of reasoning based on the notion of interaction between systems of practice bears much similarity with Feyerabend's approach, Chang never explicitly makes use of Feyerabend's argumentative strategy. Incorporating anomaly import as a mechanism for the precipitation of anomalies enhances Chang's case for pluralism by showing how pluralism can foster scientific progress.

**Vendredi matinée 01 juillet / Friday morning July 01**
Salle / Room 321

*Determinism, epistemic holes and truncated spacetimes*

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Manchak (2016a) suggested the following condition for ruling out spacetimes in which some form of artificial hole has been made: epistemic hole freeness (ehf):

spacetime <M, g> has an epistemic hole iff there are two future inextendible timelike curves y and y' with the same past endpoint and which satisfy [a condition] such that I?[y] is a proper subset of I?[y'].

One obtains two variants of (ehf) by putting in place of [a condition] that [ehf(g)] both curves are geodesics, or that [ehf(f)] both curves have finite total acceleration. Such a spacetime has epistemic hole in the sense that there are two observers, starting from the same point, and one of them is in significantly worse epistemic position than the other one: the privileged observer has access to everything another observer does, plus some more.

Epistemic hole freeness has numerous advantages over other forms of conditions which rule out holes in spacetime, such as inexendbility or various variants of hole freeness: it operates withing single spacetime. Hence it does not require comparison of a g>ven spacetime with some other spacetime, which seems to assume distinction between physically reasonable and unreasonable spacetimes. It also g>ves intuitively correct verdict in a large class of examples. Moreover, any spacetime which is ehf(f) is not nakedly singular. In light of this, one may be tempted to take ehf(f) or ehf(g) as necessary condition for the spacetime to be physically reasonable.

Let us make two observations about epistemic hole freeness.

First, an advantage of epistemic hole freeness is that (in contrast with previous conditions for being hole free) epistemic hole freeness g>ves a correct verdict in case of some non-globally hyperbolic spacetimes. To see that is is the case, consider anti-de Sitter spacetime (<M, g>) and anti-de Sitter spacetime with point q removed (<M\{q}, g'>). For any achronal set S, D(S) is empty in both of these spacetimes, so isometric mapping which transforms in some way D(S) will not be able to distinguish between these spacetimes: either it will remain empty, or it will become non-empty in both cases alike (depending on the details of the definition).

On the other hand, it is easy to see that with a point q removed from the bulk of <M, g> one can easily find two curves y and y' with the same past endpoint p such that I?[y] is a proper subset of I?[y']: for y, take the timelike geodesic between p and q in <M, g> and consider its counterpart (by map P, which is identity map on M restricted to M\{q}) in <M\{q}, g'>. For y', consider timelike geodesic between p and some point q 0 in the intersection of neigborhood of p and I + (p), and its image by P. y is incomplete, and I?(y) is fully contained in the I?(q'), so it is contained in I?(y'). So even though anti-de Sitter spacetime satisfies ehf(g) (as Manchak (2016a) shows), anti-de Sitter spacetime with point removed does not satisfy ehf(g).

Second, slightly generalized epistemic hole freeness is sufficient to impose a form of determinism. Consider the following requirement (which is a time-symmetric version of ehf):

reverse condition (rc):

spacetime <M, g> has past epistemic hole iff there are two past inextendible timelike curves y and y 0 with the same future endpoint and which satisfy [a condition] such that I+[y] is a proper subset of I+[y'].

Spacetimes such as Taub-NUT violate orig>nal version of epistemic hole freeness. Any of the non-isometric extensions of polarized Gowdy spacetime (see Chru?ciel and Isenberg (1993)), in which time orientation is chosen such that the extension is made to the past satisfies epistemic hole freeness, but not the reverse condition; similarly in case of extendible Bianchi IX solutions discussed in Ringström (2009). One can easily see that by noting that in all known such cases have globally hyperbolic reg>on with two classes of incomplete geodesics, each of which gets extended through the Cauchy horizon and completed in one of the non-globally hyperbolic extensions. To find a witness for an epistemic hole in an extension of such a spacetime it is sufficient to take one of the geodesics which remain incomplete in the extended spacetime (so a geodesics which does not get extended across the horizon) as y, and one of the geodesics which get completed in the extension as y'. Demanding epistemic hole freeness, then, removes a number of indeterministic examples from the set of physically reasonable spacetimes. Again, epistemic hole freeness contrasts with previous versions of hole freeness (according to which extendible maximal globally hyperbolic spacetimes do not have holes).

Could epistemic hole freeness also be a sufficient condition for a spacetime to be physically reasonable? There are some intuitively physically unreasonable spacetimes, on which unphysical cutting and pasting have been performed, which do satisfy both ehf(g) and ehf(f).

Let <M, g> be two-dimensional Minkowski spacetime, and consider three different truncations:

1. <M1, g1>, where M1 is M with spacelike slice without edge S and I+(S) removed; the metric is g restricted to M1.

2. <M2, g2>, where M2 is M with a straight null line S and I+(S) removed; the metric is g restricted to M2.

3. <M3, g3>, where M3 is M with zigzagg>ng achronal (but not acausal) slice S and I+(S) removed. That is, consider two hypersurfaces Z for t = 1 and Z' for t = 0, and a null curve d intersecting t = 1 hypersurface at point x and t = 0 at point y. Our slice S is then: (??, x] subset of hypersurface Z ("from the left" to x), then (zigzag) d from x to y, and [y, +?) subset of hypersurface Z 0 (from y "to the right"), and the metric is g restricted to M3.

Intuitively these spacetimes seem to be physically unreasonable: there is no physical reason for spacetime to abruptly come to an end in this way.

In cases <M2, g2> and <M3, g3> there is an epistemic hole (both in the ehf(g) and ehf(f) sense): consider two curves, y which goes to some p on the null segment of S, and y' which goes to some r on S, such that r is in J+(p). Then I?[y'] is a proper subset of I?[y].

But <M1, g1'> does not have an epistemic hole (neither in the ehf(g) nor ehf(f) sense). Consider two curves y1 and y2 which would intersect S if the truncation has not been performed. Either they would intersect S at the same p in S or at two distinct points. If they would intersect S at p, then I?[y1] = I?[y2] in the truncated spacetime. And if they would intersect S at two distinct points, then symmetric difference of I?[y1] and I?[y2] is non-empty. Thus: whether a truncated Minkowski spacetime counts as having an epistemic hole or not is a matter of how the truncation has been performed.

What do our examples show? It is known that there are (see Manchak (2016a)) examples of spacetimes which are inextendible and hole free (in the sense that D(S) cannot be enlarged), but intuitively do have some kind of a hole; inextendibility and hole freeness are thus insufficient to rule out all artificial examples. I constructed examples of spacetimes (extendible and having a hole according to some versions of the hole freeness condition) which are intuitively are physically unreasonable, but do not have epistemic hole. If one would like to use epistemic hole freeness as the sole condition for being a physically unreasonable spacetime, some truncated spacetimes would be classified as physically reasonable. Hence, epistemic hole freeness considered on its own is insufficient to rule out all artificial holes in spacetime.

A natural way of reacting to these examples is to provide an additional condition C and declare that physically reasonable spacetime satisfies epistemic hole freeness, reverse condition and C. There are few intuitive candidates for C, but I will argue that none of them does the job.

First, truncated Minkowski spacetime is extendible: maybe epistemic hole freeness should apply only to inextendible spacetimes? C would then be inexendibility. Note that in such case one would lose some conceptual of advantages

of epistemic hole freeness: to apply inextendibility one needs to work between spacetime <N, h> and its extension <N', h'>, making use of the (problematic) class of all physically reasonable spacetimes. But even if one is ready to swallow that, of course all <Mi, gi> (i=1,2,3) can be made inextendible by taking a smooth positive function O: Mi -> R going to infinity as the boundary of truncated spacetime is approached, and consider spacetimes <Mi, (O squared)gi> (for i = 1,2,3). Spacetimes <M2, (O squared)g2> and <M3, (O squared)g3> still do have an epistemic hole, and <M1, (O squared) g1> still are free from epistemic holes.

Second, note that all causal geodesics in spacetimes <Mi, gi> are incomplete. One may be tempted to take C to be existence of at least one complete (in the standard sense that generalized affine parameter of the curve has unbounded range) inextendible timelike curve. Consider however (a) a Big Crunch scenario, in which expansion of the universe reverses and the universe ends with a black hole singularity, and (b) globally hyperbolic reg>on of Misner spacetime. In such spacetimes all causal geodesics are future incomplete, and a modified condition would imply that they are not physically reasonable.

Third, C could be 'blowup-or-completeness': any inextendible past (future) directed timelike curve y is either complete, or some curvature component is unbounded along y. This certainly gets rid of truncated examples, in which there is no blowup along future incomplete geodesics (indeed, curvature scalars are vanishing in the example I explicitly discussed); and it does allow for Big Crunch type scenario. In a sense this amounts to inextendibility in disguise: if curvature blows up along any future directed timelike curve in spacetime <M, g>, <M, g> is inextendible. And if not, a (possibly non-unique extension) can be found. Note that even though ehf is satisfied in globally hyperbolic region of Misner spacetime, 'blowup-or-completeness' is not, and so even globally hyperbolic region of Misner spacetime would not be physically reasonable under this condition. Such a condition would equate existence of physically reasonable incomplete timelike curves with curvature blowup, which (at least) does not follow from known singularity theorems. And to what extent blowup can capture singular behaviour is not clear (Curiel (1998)). So it seems likely that such a condition is too strong for the purposes of delineating physically reasonable spacetimes from physically unreasonable ones. And even if it were not too strong, finding a salient motivation for this condition is another matter.


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*Exploring indeterminism through the hypothetical nature of randomness within quantum physics*


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Randomness is a janus concept, characterizing either the absence of determination in a process, or by metonymy, the plurality of the possible outcomes of a process, what can be called process-randomness [1] and outcomes-randomness respectively. Thus, one can distinguish indeterministic randomness from apparent randomness in Nature, that does not necessarily presuppose or imply an indeterministic nature of Nature. For an assumption of randomness does not necessarily contravene the assumption of determinism. It could appear contingent, typically in games of chance, where the probabilities are seen as a useful tool, only valid at a level conditioned by the limited knowledge of the parameters of the problem. On the contrary, quantum physics (QP) is often presented either as truly indeterministic or as dealing with true randomness. In this paper, I explore the possibility of settling indeterminism within an assumption of randomness and identify two different strategies in QP, subject to a common limitation, that reveals their virtues with regard to randomness, both from the point of view of physics and philosophy.

In the search of an indeterministic origin to randomness, we are not directly concerned by outcomes-randomness, essentially because, there, randomness refers to a mathematical concept that characterizes the link between the elements of a sequence and does not require a priori that each physical entity by itself has a random character. This can also be understood from the impossibility to certify randomness in the observed sequences, neither by statistical tests nor by Kolmogorov complexity. According to the process sense of randomness, either Nature is subject to pure speculation about its random nature completely disconnected from mathematics or Nature is described by a probabilistic theory [1]. In the latter case, randomness takes the process sense and appears as an underlying assumption, which in most of the cases is not directly related to the random properties of the sequence produced by the random process that is considered. We argue that it is notably the case in QP, where the randomness assumption alone is not sufficient to predict the amount of randomness in the outcomes of an experiment, typically for random numbers generation. More fundamentally, the assumption of randomness is not sufficient to certify that there is no underlying deterministic process rendering contingent the use of probabilities [2], hence the need to find its foundation elsewhere.

The main strategy identified to establish indeterminism within QP takes an indirect road consisting in the demonstration of the impossibility of supposed deterministic alternatives to the probabilistic QP's description. In that regard, the test concerns alternative theories that make the quantum probabilities a consequence of an ignorance of some hidden parameters and thus enforce the idea of the contingent nature of the assumption of randomness in QP. Initiated by the work of Bell [3], this strategy has been followed in numerous works giving rise to a long sequence, still extending, of no-go theorems. The closing of different loopholes, more and more combined [4], increasingly limits the range of possible alternatives to QP. We distinguish this strategy from a direct one, that tries to capture the origin of randomness within QP [2], thus to legitimate the probabilistic description, making it a necessity [5]. In several works, this direct way consists in a derivation of the Born rule [6].

Both strategies will inevitably encounter the obstacle of superdeterminism, which guarantees the possibility of determinism, based on the possible connection of any pairs of events through a common past event [4]. Nevertheless this limitation does not affect the scope of the results obtained within both strategies, on the contrary it reveals their virtues. The effective closure of loopholes or the potential necessity of the probabilistic description allows us to define explicit constraints for alternative theories to QP [4,5]. Randomness amplification and randomness expansion are without an equivalent in deterministic theories and QP gives now effective procedures that guarantee, at the cost of an minimal assumption of randomness, random numbers generation [7]. Finally, the direct strategy opens the way to a promising interpretation of indeterminism based on an assumption of randomness of a probabilistic description proved to be necessary.

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## *Causation and time reversal*

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*1. Overview.*

The question "What would the world be like if run backwards in time?" is ambiguous: does a 'backwards-in-time' world involve an inversion of cause and effect? In discussions of an analogue of this issue in the case of time-reversal symmetric physical theories, it is common for people to understand time reversal 'causally', as holding that whatever can happen forwards in time can happen backwards in time. This causal interpretation of time reversal is problematic as it appears to be incompatible with the asymmetry of cause and effect, and hence has been taken by many to motivate eliminativism about causation in physics, and at the 'fundamental level' more generally. In what follows, I argue that such worries are misplaced on two grounds. First, a 'causal' interpretation of time reversal is poorly-motivated. I argue that time reversal should be understood 'non-causally', such that a world 'run backwards' is not a genuine possibility — pairs of worlds that are the time-reverse of each other contain the same causal relations. Second, I show that even on a causal interpretation of time reversal, a time reversal symmetric theory is compatible with causation.

*2. The Directionality Argument.*

Bertrand Russell (1913) cites time symmetric features of the law of gravitation (taken by Russell as an exemplar of physical laws) as incompatible with both the asymmetry and time asymmetry of causation. This prima facie incompatibility has been presented as an argument in the recent literature under the name 'the Directionality Argument', with Field (2003, p. 436), Ney (2009, p. 747), Norton (2009, pp. 481–2) and Frisch (2012, p. 320) each noting the role of time reversal invariance in the anti-causal argument, of which only Norton explicitly claims a time-reversal invariance is incompatible with causation.

*3. What does time reversal reverse?*

To assess the compatibility of time-reversal invariance and causation, I focus on whether time reversal ought to be taken to invert causal relations, giving us two options.

*Causal time reversal.* Time reversal involves inverting causal relations, taking causes to effects and vice versa.

*Non-causal time reversal.* Time reversal does not invert causal relations; the distinction between cause and effect remains invariant under time reversal.

I show that these sit best with distinct ontologies of temporal relations. First, causal time reversal implies a 'B-theory' of time - an ontology of time-directed relations, such that two worlds may differ solely with respect to the direction of time. Second, non-causal time reversal implies a 'C-theory' of time - an ontology of undirected temporal relations, such that no two worlds may differ solely with respect to the direction of time. On the C theory, inverting every 'earlier than' relation gets back the same possible world. Only on the C theory does time reversal amount to a redescription of a single causal structure.

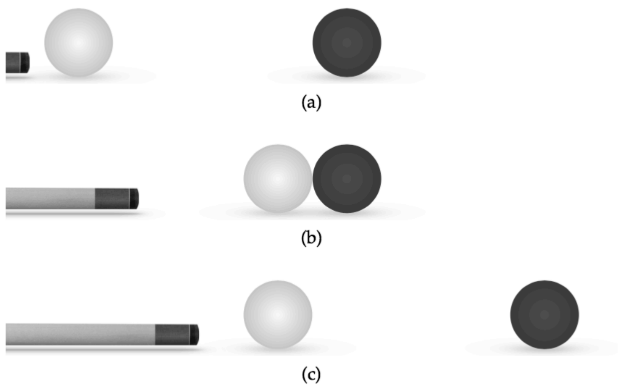

Figure 2: *The time asymmetric collision of two realistic snooker balls of equal mass on a frictional snooker table.*

[FIGURE 1 a-c.]

*4. Does time reversal symmetry eliminate causation?*

Using examples of time symmetric and time asymmetric processes, I compare the I argue that the C theory provides a stronger account of both causal relations and time reversal than does the B theory. Here, I focus briefly on the time-asymmetric example. Figure 1 depicts the time asymmetric collision of two realistic snooker balls of equal mass on a frictional snooker table. In the conventional 1a–1c description, the cue strikes the cue ball, setting it in motion, and the cue ball then collides with the red ball, transferring most of its momentum to the red ball. The red ball then loses momentum due to the frictional force of the baize on the snooker table until it is at rest, as depicted in 1c. The 1a–1c description contains a number of causal terms, e.g. implying: the cue movement causes the cue ball's movement; the cue ball's movement causes the red ball's movement; the baize causes the red ball to lose momentum. In the unconventional 1c–1a description, an anomalous series of causal processes is implied. Firstly, heat in the baize together with incoming air molecules conspire to set the red ball in motion. Secondly, the red ball's motion in synchrony with inverse, concentrating soundwaves jointly impart a gain in momentum in the collision of the red ball into the cue ball. Finally, the cue ball's momentum is absorbed in a collision with the cue.

*5. Answers.*

On the C theory both the forwards and backwards descriptions pick out the same causal structure. On the B theory, the two descriptions pick out distinct causal structures. Crucially, I argue that: (1) the C theory's non-causal account of time reversal is clearly preferable given consideration of time asymmetric causal processes; (2) on neither the causal nor non-causal readings of time reversal is time reversal invariance incompatible with causation.

*First*, as a candidate causal process, the 1c–1a is highly unsatisfactory. Two issues in particular stand out: (a) there are several points that imply a violation of the Causal Markov Condition; (b) the snooker player apparently loses her agential control over the balls' motion. Only on the B theory is this a candidate causal process distinct from 1a–1c.

*Second*, the most direct way for causation and time reversal symmetry to be incompatible is for a time reversal invariant theory to entail, for some pair of events x and y, that if x is a cause of y, then (by time reversal symmetry) y is also a cause of x (and vice versa). By holding TR-twins to represent distinct worlds, the B theory avoids incompatibility since the distinct causal relations expressed by the TR-twins hold between distinct sets of events in different possible worlds. On the C theory, incompatibility is also avoided on the grounds that time reversal does not invert causal relations. Moreover, the non-causal account of time reversal entails this conclusion regardless of whether the relevant physics it time reversal invariant.

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Symposium:

*Technique, styles et évolution dans les études d'épistémologie historique des sciences de la vie*

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*Introduction générale*

La compréhension des causes et des modalités du changement scientifique constitue l'un des problèmes fondateurs de l'épistémologie historique. Cela est vrai, pour ainsi dire, des deux côtés de l'Atlantique. Si, d'une part, l'histoire des sciences et l'épistémologie anglo-saxonnes ont été profondément marquées par les travaux de Kuhn sur les changements de paradigme, d'autre part, ce qu'on pourrait appeler « la tradition française d'épistémologie historique » a hérité de Bachelard l'intérêt pour la dialectique des sciences (conçue comme un processus incessant de refondation du savoir à l'aulne des connaissances nouvellement acquises). Plus généralement, la question de la transformation du savoir scientifique est transversale aux différentes orientations de l'histoire des sciences. Histoire internaliste ou histoire externaliste, histoire présentiste ou histoire historiciste, histoire philosophique ou histoire historienne, voire sociologique : quels que soient les termes de l'opposition, la question de savoir pourquoi et comment les connaissances scientifiques s'accumulent et se remplacent se pose sans cesse. Néanmoins, de telles oppositions méthodologiques semblent incapables de saisir tous les enjeux de la question du changement scientifique.

La dichotomie entre histoire interne et externe des sciences, par exemple, réduit l'explication des transformations des sciences soit aux facteurs sociaux ou idéologiques, soit à une évolution logique immune à toute influence extérieure. À l'égard de cette dichotomie, certaines interprétations anglo-saxonnes de l'épistémologie historique risquent précisément de la réduire à une histoire interne des sciences, qui ne s'occupe que de la dimension théorique ou discursive de la connaissance, en tant qu'elle est une histoire de la formation et de la transformation des concepts et des normes des énoncés scientifiques. De manière analogue, la dichotomie entre continuité et discontinuité risque d'obscurcir la relation étroite qui, tout au long de l'histoire des sciences, a lié conservation et progrès, persistance et changement soudain. Comme le remarque Michel Foucault dans L'Archéologie du savoir, si l'attention des historiens des idées, de la philosophie ou des sciences s'est progressivement tournée vers des phénomènes de rupture, une telle pensée de la rupture doit coexister avec des orientations qui privilégient la continuité des grands processus historiques, dans le contexte d'un réseau d'échelles spatiales et temporelles.

Le symposium que nous souhaitons organiser a pour but d'analyser les implications de ces deux dichotomies pour l'épistémologie historique du changement scientifique. Pour ce faire, nous nous concentrerons sur quelques aspects particuliers d'une telle question. Premièrement, nous mobiliserons le concept de « style de raisonnement scientifique », formulé par Ian Hacking, afin d'en jauger les potentialités en tant que modèle historiographique pour l'étude du changement scientifique. Deuxièmement, nous examinerons l'importance des sciences du vivant comme pierre de touche méthodologique pour l'épistémologie historique, aussi bien dans le cas de Hacking que dans celui de Georges Canguilhem. Troisièmement, nous mettrons en avant le lien étroit qui, du point de vue de l'épistémologie historique, existe entre changement théorique et changement technique, en nous concentrant encore une fois sur les sciences du vivant, et tout particulièrement sur la théorie de l'évolution.

*Sciences et changement, une question de style: Hacking et le modèle historiographique des sciences de la vie* (**Matteo Vagelli**)

La question du changement dans les sciences est devenue, de plus en plus dans les dernières décennies, un problème de méthode pour l'histoire et la philosophie des sciences. On est passé de la question du contexte, des conditions et de la justification du changement scientifique, à la question concernant la possibilité de localiser, tracer, comprendre et évaluer les évènements sur le "rouleau" de l'histoire de la théorie aussi bien que de la technique. Autrement dit, l'interrogatif plus pressant, même aujourd'hui, semble être: comment, à travers quel concept analytique et en se plaçant sur quel niveau d'analyse, peut-on saisir au juste la nature et la portée du changement scientifique?

Pour répondre à cette question, le philosophe canadien Ian Hacking a développé, depuis les années 1980, une méthodologie dite des « styles de raisonnement scientifiques », qui, à maints égards, est à l'origine de la réinvention du terme d'épistémologie historique dans les domaines anglo-saxons. Mais qu'est que ça veut dire décrire le changement dans les sciences en termes de styles de raisonnement scientifiques plutôt qu'en termes de paradigmes ou schémas conceptuels ? Celle de style est une catégorie qui se veut à mi-chemin entre plusieurs alternatives. D'abord entre continuité et discontinuité, parce que les styles peuvent avoir des commencements souvent soudains, mais, s'ils développent ce qui Hacking appelle des "techniques d'autostabilisation" suffisantes, ils peuvent survivre au passage des époques et des cadres théoriques différents. En ce sens, un style scientifique se situe au croisement entre l'histoire des concepts et l'histoire des techniques et il est censé porter l'attention sur le changement aussi bien des modes de raisonner que de manières d'intervenir dans le monde. Dans le sillage de A. C. Crombie, Hacking se réfère à l'évolutionnisme darwinien comme le style de raisonnement proprement historique. De ce point de vue, il est alors intéressant de remarquer que tous les styles sont parfois décrits par Hacking comme des organismes vivants, c'est à dire, comme des unités plus ou moins closes dont la survie est assurée principalement par les techniques d'autostabilisation[1].

Cette notion de style a été inspirée par toute une série de références importantes, parmi lesquelles il y a certainement le Foucault de l'archéologie. Dans cette présentation je tâche cependant de rapprocher certains des aspects de cette méthodologie des styles, et notamment l'utilisation d'un vocabulaire tiré des sciences de la vie, à l'historiographie des sciences de Canguilhem. Point focal de l'analyse sera le parallèle entre les techniques d'autostabilisation caractérisant les styles et les mécanismes d'autorégulation qui, selon Canguilhem, définissent la nature d'un organisme[2]. Ensuite, dans une deuxième partie de la présentation, j'examinerai d'une façon critique les derniers développements chez Hacking d'une "écologie des styles", approche écologique qu'il voit comme faisant partie d'une histoire cognitive de la raison et des facultés mentales[3]. Contrairement à la critique de Kusch, qui, a cru voir dans les métaphores biologisantes et vitalistes une récupération du modèle de l'histoire des idées, je tenterai de montrer que l'utilisation de ce vocabulaire n'implique nécessairement une philosophie des sciences naturalisée, ni un évolutionnisme épistémologique en historiographie[4]. A ce propos, je distinguerai, dans la troisième et dernière partie de mon intervention, la méthodologie des styles scientifiques de l'application, faite par Stephen Toulmin, du principe de l'évolution et de la sélection naturelle à l'histoire des sciences[5].

*The road to structure: changement théorique et styles de raisonnement scientifique dans la biologie de l'évolution* (**Nicola Bertoldi**)

L'une des grandes orientations de la philosophie des sciences depuis le Cercle de Vienne a consisté en la reconstruction formelle de théories scientifiques à l'aide d'un système de notation symbolique emprunté tantôt à la logique du premier ordre (c'est le cas de la dénommée « conception syntaxique des théories scientifiques »[6]), tantôt à d'autres domaines des mathématiques, tels que la théorie des ensembles et la topologie (on parle, à ce propos, de « conception sémantique »[7]). C'est dans une telle optique « formaliste » que des philosophes et des biologistes tels que Joseph Woodger, Conrad Waddington, Morton Beckner, Michael Ruse, David Hull, Mary Williams, Alexander Rosenberg et Paul Thompson[8] ont essayé de doter la biologie d'un cadre théorique formel le plus global possible. Toute conception formaliste des théories biologiques se heurte toutefois à deux problèmes majeurs. D'une part, les notations utilisées par les philosophes sont rarement maîtrisées par les biologistes[9]. D'autre part, toute entreprise de formalisation est confrontée à son incapacité de prendre en considération l'utilisation que les agents (dans ce cas, les biologistes) font des théories en question, ce qui est essentiel pour la détermination de leur contenu[10]. C'est à de tels obstacle qu'il est possible d'imputer la distance qui sépare toujours la philosophie et la biologie, malgré l'importance grandissante de la théorisation dans les sciences du vivant[11].

Le but de cette communication est de jeter les fondements d'une conception alternative des théories scientifiques, et tout particulièrement de la théorie de l'évolution. Une telle conception consistera à rendre compte à la fois du contenu et de la forme d'une théorie en replaçant ces derniers dans une optique historicoépistémologique. Il s'agira d'analyser l'histoire de la dite théorie comme processus dynamique de changement structurel. L'analyse philosophique la plus connue du changement structurel des théories scientifiques étant celle de Thomas Kuhn, nous tâcherons cependant d'en proposer une autre, mieux adaptée à la théorie de l'évolution. Pour ce faire, nous prendrons en considération à la fois l'histoire des différentes reconstructions formelles de la théorie de l'évolution et l'histoire d'une lignée particulière de théories biologiques, celle qui a contribué à la constitution de la génétique des populations sur la base du Théorème fondamental de la sélection naturelle de Ronald Fisher[12]. Dans une tentative de mettre en dialogue l'histoire de la biologie et l'histoire de la philosophie des sciences, nous utiliserons comme référence théorique l'analyse que Ian Hacking a fait du concept de « style de raisonnement », ainsi que son programme de fonder une « ontologie historique »[13].

*Le rapport entre la science et la technique dans les travaux d'histoire épistémologique de Georges Canguilhem* (**Ivan Moya Diez**)

Nous nous proposons d'examiner les conséquences méthodologiques de la conception de la technique de Canguilhem dans sa démarche d'histoire épistémologique des sciences de la vie[14]. Le rapport entre science et technique est l'une des problématiques principales des premiers écrits de George Canguilhem. La technique y est reconnue comme une activité créatrice originale qui ne se réduit pas à une mise en action de la connaissance. En ce sens, Canguilhem va renverser la lecture mécaniste de la philosophie cartésienne pour critiquer le principe positiviste comtien qui réduit la technique à une pure application de la science[15]. Ce principe sera le cible principal de sa célèbre thèse sur le normal et le pathologique[16]: la science n'y apparaît que comme une explication rétrospective des problèmes et des solutions techniques posées par le vivant en débat avec son milieu. Le rapport d'antériorité de la science à la technique en résultant inversé. Il s'agira par la suite de résoudre cet antagonisme initial entre science et technique à l'égard de la vie[17].

A cet égard, les études de Canguilhem sur le concept de milieu[18] permettent de mieux comprendre le rapport entre science et technique. Premièrement, Canguilhem y analyse l'importation du concept de milieu de la mécanique à la naissante biologie. Grâce à cette importation, ce concept s'est constitué comme garantie de scientificité pour élaborer différentes théories sur les rapports d'adaptation de l'organisme au milieu chez Lamarck, Darwin, ou les neo-lamarckiens. D'après Canguilhem, le principe positiviste « savoir pour prévoir, prévoir pour pouvoir » se fonde sur la rupture entre le vivant et son milieu, et en ce sens, il serait doublement débiteur du mécanisme cartésien et des conceptions déterministes du milieu propres des théories de l'évolution. La primauté de la science sur la technique qui en résulte, entraînerait une résignation du vivant face à l'ordre du monde[19]. Le déterminisme du vivant par rapport au milieu et ainsi solidaire de la réduction de l'agir au savoir.

Pour renverser le rapport de subordination du vivant au milieu, Canguilhem se sert des usages du concept du milieu dans d'autres disciplines. D'une part, les travaux au XIXe siècle de l'école française de géographie humaine reconnaissent dans tout vivant un facteur géographique en tant qu'il est créateur de son propre milieu en fonction de ses capacités et de ses valeurs vitaux. Dans le cas de l'homme, c'est principalement à travers l'action technique qu'il transforme son milieu. D'autre part, c'est notamment la distinction entre milieu géographie et milieu du comportement dans la Gestalttheorie qui permet à Canguilhem de réarticuler le rapport du vivant au milieu à travers l'action technique.

Finalement, le rapport entre la science et la technique n'est pas antagonique, mais, au contraire, la valeur de la science va résider dans sa capacité de transformer la réalité à travers des procédures de vérification, c'est-à-dire à travers l'action technique[20]. En ce sens, la valeur du concept et des énoncés scientifiques n'est pas donnée par les rapports logiques qu'ils établissent dans un système théorique. Au contraire, leur valeur est de servir comme un outil explicatif. Par conséquent, ce sont les vérifications techniques et les effets pragmatiques des énoncés scientifiques -aussi bien à l'intérieur qu'à l'extérieur de la science- qui permettent de comprendre les transformations de la science.

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Séance plénière / Plenary session:

*How Einstein did not discover*

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*Abstract*

Einstein was a maker of change in science. What powered his discoveries? Was it asking naïve questions, stubbornly? Was it a mischievous urge to break rules? Was it the destructive power of operational thinking? It was none of these. They are myths that have arisen through our need to find a simple trick underlying his achievements. Rather, Einstein made his discoveries through lengthy, mundane investigations. They brought moments of disappointment, frustration and even despair. Einstein persisted with tenacity and discipline. Then there were also moments of transcendent insight. However these were rare moments and were only possible because of the painstaking preparatory work. I will illustrate these ideas with the examples of Einstein's 1905 discoveries of special relativity and the light quantum.

For a draft paper, see

http://www.pitt.edu/~jdnorton/homepage/cv.html#Einstein_discover

*Une ontologie dispositionnelle du changement*
*Analyse dispositionnelle de la vitesse et extension à d'autres formes de changements*

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Le monde décrit par la physique – incluant ses applications à d'autres sciences telles que la chimie ou la biologie – est en changement permanent. Les objets matériels changent de position, et ce changement est mesuré par une vitesse, dont le changement est lui-même mesuré par une accélération. Une substance va par exemple changer d'un état solide à un état liquide. Une pression sanguine va changer de valeur selon différents paramètres. La question qui se pose au métaphysicien est la suivante : quelle ontologie du changement est compatible avec nos théories scientifiques usuelles ?

Il peut sembler cohérent de voir ces changements comme l'expression de propriétés dispositionnelles. Les dispositions sont des entités qui peuvent exister sans être manifestées ; un example de disposition est la fragilité d'un verre, vue comme une disposition à se briser, et qui existe même lorsque le verre ne se brise pas. De même, un objet doté d'une certaine vitesse semble être doté d'une disposition à poursuivre son déplacement en ligne droite (une tendance qui peut être contrecarrée par des forces newtoniennes s'appliquant à l'objet) ; une substance chimique va avoir une disposition à se vaporiser dans un certain milieu ; une pression sanguine va avoir une disposition à augmenter ou diminuer dans certaines circonstances.

Notre présentation examinera la cohérence d'une ontologie dispositionnelle du changement. Nous examinerons en particulier cette question dans le cadre de l'ontologie formelle BFO (Basic Formal Ontology). Cette dernière se situe dans un cadre philosophique réaliste admettant à la fois l'existence des particuliers (les entités individuelles qui peuplent le monde, telles que cette chaise, Victor Hugo ou la tour Eiffel) et des universaux (qui représentent tout ce que tous les particuliers d'un regroupement donné - par exemple, le type « félin » ou « mammifère » - ont en commun, et prétendent expliquer la structure, l'ordre et la régularité dans le monde). Röhl & Jansen (2011), ont proposé un modèle formel de la notion de disposition dans ce cadre, qui associe à chaque disposition un type de réalisation et un type de déclencheur, le déclencheur étant un processus spécifique qui me mener à la réalisation de la disposition. Ils commencent ainsi à définir ces entités et leurs relations au niveau des particuliers, puis les formalisent au niveau des universaux. Par exemple, dans le cas de la fragilité, le déclencheur pourra être une forme de tension subite par le verre, et la réalisation pourra être la brisure ou la fêlure du verre.

Nous adapterons ce modèle à plusieurs cas de changement, en examinant notamment si une ontologie de la vitesse comme disposition est tenable. Deux écoles sur le statut ontologique de la vitesse ont été défendues dans la littérature philosophique : le réductionnisme, une école héritée de Bertrand Russell (Meyer, 2002; Easwaran, 2013) ; et le primitivisme (Tooley, 1988; Bigelow and Pargetter, 1990; Carroll, 2002). Les premiers pensent que la vitesse d'un objet dépend de sa position dans le temps, et acceptent la définition classique de la vitesse comme dérivée première de la position dans le temps. Pour les seconds, en revanche, la vitesse est une propriété au-delà des positions de l'objet. Considérons par exemple une expérience de pensée dans laquelle les objets se déplacent de manière totalement aléatoire dans l'espace, d'un point à l'autre, et sans transition (Tooley, 1988) ; mais où la trajectoire d'un objet formerait par hasard une courbe continue pendant un certain intervalle de temps. Selon les primitivistes, cet objet n'aurait pas de vitesse, bien que sa position admette une dérivée première par rapport au temps.

Ce débat entre réductivisme et primitivisme est lié à la question du rôle causal de la vitesse (Lange, 2005). En effet, la vitesse peut être vue comme ayant un rôle causal sur les trajectoires futures : un objet qui est à la position r0 at t0 sera proche de la position r0+v0.delta_t à un temps t0+delta_t parce que, à t0, il a une vitesse v0. Plus précisément, on peut interpréter la seconde loi de Newton du mouvement comme affirmant qu'une trajectoire d'un objet dans un intervalle]t0, t0+delta_t[ peut être expliquée causalement par la masse de l'objet, les forces qui s'exercent sur cet objet à tout instant dans l'intervalle ]t0, t0+delta_t[, et certaines conditions initiales qui constituent l'état de l'objet : la position de l'objet r0, ainsi que la vitesse de l'objet v0.

Nous montrerons que dans le cadre formel de BFO, deux types de vitesse peuvent être introduites : le vitesse d'un processus de mouvement (qui décrit la dérivée première de la position de l'objet en mouvement), et la vitesse d'un objet, vue comme une disposition. L'exemple de l'objet se déplaçant de manière aléatoire ci-dessus montre que ces deux types de vitesse sont conceptuellement indépendants : un tel objet n'a pas de disposition à se déplacer en ligne droite, mais on peut calculer la dérivée par rapport au temps de sa position. Ceci est corroboré par l'exemple du pendule de Newton, dans lequel une première boule vient frapper une seconde, qui transmet immédiatement son énergie à une troisième tout en restant immobile : dans un tel cas, la seconde boule a une disposition à se déplacer, mais sa position reste inchangée ; l'objet a donc une vitesse non-nulle au sens dispositionnel, mais la vitesse de son mouvement est nulle.

Nous étendrons par la suite ces considérations afin d'examiner pour quelles autres propriétés une ontologie dispositionnelle du changement est tenable.

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*La résistance au changement dans la modélisation mathématique en finance : l'exemple de la représentation brownienne de 1950 à 2000*

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Alors qu'en médecine la confrontation des données fournies par le syndrome clinique avec celles qui sont fournies par le système biologique permet de serrer au plus près le diagnostic, il n'en a pas été de même pour la science financière : l'accumulation de résultats statistiques violant l'hypothèse brownienne, couplée avec des difficultés opérationnelles importantes pour la mise en œuvre pratique du modèle de Black-Scholes-Merton (BSM) n'a pas permis de modifier la représentation brownienne qui a perduré plus de cinquante ans malgré l'étendue de ces invalidations. Une représentation probabiliste (le mouvement brownien) s'est ainsi imposée sans fondements statistiques, agissant dans la pensée des acteurs professionnels comme une prénotion dans le sens de Durkheim (1895), c'est-à-dire une « représentation schématique et sommaire (...) formée par la pratique et pour elle », si ce n'est qu'ici cette prénotion fut issue de la science elle-même et non d'une réflexion antérieure à la science, et produisit en quelque sorte une épistémologie spontanée. La question qui vient alors à l'esprit est celle de l'explication de la persistance de cette représentation brownienne qui fondait l'usage de la formule de BSM en en validant son postulat initial : comment expliquer une telle durée de vie ?

Pour aborder cette question et tenter d'y apporter une réponse, nous allons tester pour la science financière les différentes dynamiques de la connaissance scientifique. Ces dynamiques sont toutes des manières particulières de décrire les rapports entretenus entre la connaissance d'un phénomène (ici la représentation d'une dynamique boursière) et ce phénomène (ici les fluctuations boursières). Pour cela, en effectuant un survol des différentes théories de la connaissance scientifique, nous chercherons à faire parler ces représentations de la science dans la science financière, afin de pouvoir réfléchir sur la domination de la représentation brownienne en finance et l'étendue des applications du modèle de Black-Scholes-Merton.

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*Traitement des données et simulation numérique : quelle différence ?*

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Dans cet exposé, nous confrontons deux outils conçus dans le contexte du tournant computationnel des sciences, qui est la conjugaison historique de l'avènement des méthodes numériques et de l'introduction de l'ordinateur. Ces deux outils sont le traitement des données et la simulation numérique. Ils ont ceci en commun de fournir de la connaissance à partir d'un ensemble de données de départ suivant des processus algorithmiques définis dans un programme informatique. Mais, tandis que le terme "traitement des données" apparaît généralement dans un contexte d'observation, le lien entre simulations et observation est beaucoup moins clair. Notre but est de répondre à la question suivante : comment deux pratiques apparemment si proches — au point d'être difficiles à distinguer — peuvent-elles néanmoins aboutir à des résultats aux statuts si éloignés ? Nous espérons ainsi contribuer à clarifier ce que sont les simulations, car on ne trouve pas de définition consensuelle à leur sujet (Hartmann 1996, Guala 2002, Humphreys 2004, Parker 2009).

Nous partons de la ressemblance qui existe entre traitement des données et simulations :

1 ) les deux partent de données initiales…

2 )…lesquelles sont transformées suivant des procédures algorithmiques…

3 )…qui formalisent des modèles physiques de phénomènes.

En première analyse, la différence n'apparaît que dans le contexte d'utilisation. Principalement, alors que les données traitées interviennent dans un contexte observationnel ou expérimental, les simulations interviennent dans un contexte de travail théorique. Plus précisément, le traitement des données sert à passer de données brutes tirées de mesures instrumentales à des données corrigées de tout bruit et artefact instrumental, ou encore à des données reconstruites dans un autre espace mathématique (par exemple en scanner ou en IRM), afin d'en faciliter l'analyse. La simulation numérique, quant à elle, est le processus computationnel allant des données initiales sur le système cible aux sorties de simulation, afin de prédire ou d'expliquer le comportement de ce système.

Nous expliquons ensuite pourquoi cela n'est pas une raison suffisante pour considérer que les données traitées, contrairement aux sorties de simulation, ont un caractère d'observation. Nous soutenons que le contexte dans lequel est utilisé un outil épistémique n'est pas à même de déterminer la nature (observationnelle ou théorique) des connaissances que l'on peut en tirer. Autrement, il suffirait d'être dans un contexte d'observation pour observer. Or, même si les philosophes ne s'accordent pas forcément sur ce qu'observer veut dire, ils partagent l'idée qu'il existe des éléments normatifs dans l'observation qui sont garants d'une certaine autorité épistémique. Si traitement des données et simulations doivent être séparés du point de vue de leur rapport à l'observation, cela doit donc être sur la base d'éléments constitutifs — et non sur des contextes d'utilisation — différents.

Dès lors, nous partons à la recherche d'une différence constitutive entre traitement des données et simulations. Parmi les trois étapes communes au traitement des données et aux simulations qui sont répertoriées plus haut, c'est la troisième qui requiert à notre avis une analyse attentive. En effet, le fait que, dans les deux cas, des algorithmes mettent en œuvre des procédures de calcul qui formalisent des modèles physiques de phénomène nous pousse à nous interroger sur le type de phénomènes modélisés dans l'un et l'autre cas. Deux pistes peuvent ainsi être suivies pour différencier le traitement des données et les simulations du point de vue des modèles formels qui fondent les opérations mathématiques.

Notre première piste consiste à établir une distinction sur les modèles physiques qui sous-tendent les calculs, selon qu'ils portent sur le système cible ou non. Dans une simulation, partant de données initiales sur le système cible, le calcul modifie ces données à partir de modèles qui portent aussi sur le système cible. Dans cette hypothèse, le traitement des données se distinguerait par le type de modèles dont il serait fait usage, qui porteraient sur le dispositif expérimental, et non sur le système cible. On parlerait de traitement des données si notre image initiale de galaxie était modifiée non pas suivant des modèles qui décrivent les galaxies (leurs équations d'état ou d'évolution, la manière dont elles se forment, etc.) mais suivant des modèles qui portent sur la mise en image. Par exemple, afin de déflouter une image et rendre interprétable les données, le traitement repose sur un modèle du détecteur. Cela permet, pourrait-on dire, de réaliser des observations à partir de données traitées alors que, dans la simulation, les modifications faites sur les données initiales portent les traces des hypothèses théoriques sous-jacentes qui dénaturent en quelque sorte ces données.

La deuxième piste repose sur l'hypothèse selon laquelle les hypothèses formalisées pour le traitement des données prennent la forme d'équations d'état, non dynamiques, tandis que les simulations mettent en œuvre des hypothèses dynamiques. L'observation étant attachée à un lieu et à un temps précis, la simulation aurait pour résultat de « délocaliser » la représentation du phénomène, ce qui irait à l'encontre des attentes habituelles de l'observation. Prétendre que le résultat d'une simulation peut relever de l'observation serait alors aussi étrange que d'affirmer que l'on observe une future éclipse du soleil alors que la terre, la lune et le soleil ne sont pas encore alignés mais que l'on peut prédire, même de manière très sûre, une prochaine éclipse, ou que l'on observe un futur papillon alors que l'on a une chenille sous les yeux.

Ces deux pistes seront donc explorées pour les confronter à plusieurs études de cas en imagerie médicale et en astrophysique. Nous espérons ainsi les mettre à l'épreuve de la pratique pour déterminer si en effet une règle systématique permet d'établir une distinction de fond entre traitement des données et simulations.


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*A property-cluster kind approach to life*

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Science is plagued with concepts that are absolutely central and yet puzzling in different ways. In particular, some of these central concepts are incredibly hard to define. Biology's most important term, 'life', is one interesting case. Despite the remarkable amount of biological knowledge gathered so far, and old and fundamental issue around life remains unsettled: What is life? What is a living system? What is the difference between living and non-living systems? There hardly is a more intrinsically philosophical issue than this one, constantly renewed and challenged by developments in biological sciences.

The boundaries of life are certainly vague. As a matter of fact, the "lifeness" of several currently existing entities (for instance, viruses or prions) has been disputed -with no consensus to the date- because they lack one or many valuable features of paradigmatic living things. But the problem of the definition of life not only involves these current "grey cases". Indeed, it is also related to the problem of the origins of life on Earth. The most supported theories of the origins of life hold that the transition from inorganic chemistry to life must have been a gradual and continuous process. In light of this, it seems that a definition of life couldn't help being extremely arbitrary, since there is no categorically different point at which we could say that life started (Lederberg 1960). On the other hand, recent biological fields that are centred on lifeness, such as artificial life (that seeks to create life-like processes without a specific material instantiation), synthetic biology (that aims to reproduce the phenomena of life in the laboratory) and astrobiology (the search for life outside our terrestrial environment) challenge the most classical conceptions of life, but also offer exciting areas to "test" possible answers to the topic.

Several criteria have been proposed to capture what every living system consist in (Schrödinger 1944, Mayr 1997, Luisi 1998, Koshland 2002, Dupré & O'Malley 2009, Ruiz-Mirazo et al. 2010, Benner 2010). Some of them are tightly interconnected or even partially overlapping. The most popular are: (a) a minimal cellular organization, (b) metabolism, the ability to engage in biochemical reactions through which free energy and nutrients from the environment are transformed to elaborate compounds needed for the system and decreasing the system's entropy, (c) autonomy, or the general capacity to self-organization and self-maintenance of organization via metabolic processes along the period of the system's life, (d) a certain spatial boundedness that delimitates the system's environment from an external one, a requisite for a successful metabolism, (e) the capacity to reproduce and bring about a system capable of the same processes, (f) the ability to react to external stimuli, defending itself against injuries, (g) a carbon-based molecular constitution, and (h) evolvability, the capacity to undergo Darwinian evolution due its reproduction with heritable errors that may result in fitness differences.

These traditional criteria face two sorts of problems. First, there are known counter-examples to several of them, and it is highly plausible to conceive of counter-examples to the rest. For instance, a single rabbit –paradigmatic case of living- cannot reproduce on its own, whereas crystals –clear case of non-living- are said to replicate. Second, some criteria have been argued to be non-operational in the search for new forms of life. Especially, the Darwinian criterion is considered unpractical since it requires long-term and statistical observations. In a nutshell, these are not necessary nor sufficient conditions: not every living system meets all of these criteria while non-living systems meet some. Due to this, some authors have concluded that life cannot be defined and, moreover, that a proper definition of life depends on there being a theory of life (Cleland 2012) we currently lack (and one that perhaps could never be achieved).

In this paper, I propose a property-cluster kind approach to life in order to skip the obstacles in the pursuing of a classical definition. Though the topic of natural kinds (in biology) usually concerns biological taxa (e. g., species), the notion of natural kind turns out to be relevant for this issue as well. Living systems can be conceived of as a natural kind whose members have at least some properties in common, from a naturalist (non-conventionalist) standpoint about natural kinds. This proposal, I argue, has several benefits. First, as this is not a classical definition, it is revisable in the light of new empirical evidence. New properties of life could be discovered and added to our knowledge of the cluster. Second, and for this reason, it does not blind the search for extra-terrestrial life, because it does not compel to look for objects exhibiting a fix set of properties. Instead, the cluster suggests several clues to be pursued. Third, the cluster-kind conception is compatible with the "gradualist" theories of the origins of life, because it allows for vague boundaries even in present-day forms of life. Forth, it succeeds in accommodating controversial known cases, such as viruses, with the non-controversial ones altogether in a single scheme. Certainly, whether such a conception of life plays a substantive role in biological explanations can be called into question (Lange 1996), and this would not be a trivial concern. I reply, however, that this is not the issue at stake. On the contrary, I see the ultimate problem to be biology's object of study, and I argue that only this conception can make sense of the heterogeneity of things assumed to be a matter of biology (for instance, it explains the fact that viruses are fully and properly accounted for in biological terms). Thus, far from constituting a sterile analytical construal, the proposal rather entails a certain pragmatic element that places it close to actual scientific practice.

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### *Understanding biological stability: an organicist perspective*

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Since the sixties, Biology has been profoundly influenced by the theoretical paradigm at work in Molecular Biology according to which biological structures and functions, and ultimately the whole organism, are determined by genetic information and the molecular mechanisms through which such information is expressed.

Often referred to under the name of 'genetic program', this idea conveys a specific conception of biological stability and variation. On the one hand, genes are presented as the main factor of biological stability (i.e. maintenance and reoccurrence of biological properties over time), both at the individual and the evolutionary scale, because biological organisms result from the expression of the information contained in DNA, a highly stable and replicable molecule. On the other hand, genetic variation is seen as accidental, but playing a crucial evolutionary role, because it generates the differences upon which natural selection operates. Correlatively, genetic variation within the individual organism is not supposed to be functional, and is mainly understood as potentially deleterious noise.

In recent years, several leading philosophers and scientists have claimed that this paradigm is challenged by experimental evidence. Well-established experimental results (e.g. epigenetic mechanisms such as DNA methylation and alternative splicing) reveal a high degree of variation in the results of gene expression, so that protein synthesis is underdetermined; this is even more true of the functional organisation of the organism as a whole. Moreover, according to some authors, experimental evidence shows that gene expression is not only variable but also intrinsically stochastic. One of the crucial implications of these results is that biological organisms exhibit widespread morphological and functional variation even in the absence of genetic differences. As a consequence, the idea that the stability of DNA accounts for the stability of biological phenomena is challenged. Today, the recognition of ubiquitous biological variation leads a number of authors to acknowledge that variation is a constitutive dimension of individual organisms. Within philosophy of biology, in particular, a prominent proposal is the so-called "process ontology", which puts heavy emphasis on variation and change rather than on stability. In this framework, biological entities (as, for instance, the genome) are better understood as being in constant change – as processes – rather than (stable) "things". This shift in the conception prompted a rapid evolution of experimental technologies that enable to investigate variation at the single-cell (or even single-molecule) scale. These technologies, in turn, allow accumulating more and more observations of unforeseen variation at all levels of description.

Emphasising variation and change in organisms, however, raises the question "Why are they stable?". What does explain their stability if their development and functioning are poorly determined by genetic information?

In spite of their massive variability, biological phenomena do exhibit indeed significant degrees of stability that call for an explanation, at both the ontogenetic and phylogenetic scale.

In this presentation, I will address the prospects of an organicist perspective on biological stability. Broadly speaking, organicism puts organisms in the foreground of biological science, as the level of description involved in any relevant explanation of biological phenomena. In particular, one of the main tenets of organicism is the idea that organisms are organised systems, and biology should aim at understanding the principles of organisation.

The main goal of the presentation will be to examine to what extent the concept of organization can play a pivotal role in explaining biological stability.

I will focus on a characterisation of biological organisation that relies on the interplay between processes and constraints. Processes refer to all those changes that occur in biological systems and involve the alteration, consumption, production and/or constitution of entities in open thermodynamic conditions. Constraints, in turn, refer to entities that, while acting upon these processes, exhibit some kind of conservation and can be said to remain unaffected by them. Organisation is specifically understood as the mutual dependence (in technical terms: a 'closure') of constraints, such that each constraint controls the thermodynamic flow so as to maintain the conditions of existence of the others: as a result, the whole system is able to self-maintain.

The central hypothesis is that the capacity of collective self-maintenance due to organisational closure might constitute the fundamental factor of biological stability over time. On the one hand, each functional constraint subject to closure stabilises target processes and reactions, and avoid that they undergo deleterious variation, which would undermine the overall functioning of the organism. On the other hand, the conservation of each constraint holds only at a given time scale ?, which means that, at longer time scales, they must be regenerated or repaired. If it were not the case, their role in stabilizing processes and reactions would be altered, and would eventually cease. Because of closure, the maintenance of each constraint is (among other things) dependent on the activity of other organised constraints. Accordingly, the maintenance of each constitutive constraint beyond ? depends on the organisational closure.

In the final part, I will discuss two issues. First, the organicist perspective on biological stability modifies the understanding of the role of DNA. Interpreted as a constraint, DNA plays a crucial role in determining stability, although this role cannot be dissociated from its mutual dependences with the whole set of constraints subject to organizational closure. Second, the relation between organization and stability should be integrated with an account of how biological systems vary. Understanding how biological organization can ground stability and, at the same time, enable, promote and preserve functional variation is a central challenge for a sound organicist perspective in biology.

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### *How function changed science*

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Current debates in biology signal a renewed interest in the nature and status of function. Much of the fervor concerns the extent to which the notion of function incorporates or implies the related notions of purpose, design, intentionality, and agency. Such notions are generally eschewed by scientists as they are typically conceived as amounting to a teleological account of nature, as exemplified by Aristotle's appeal to final causality (Neander, 1991 b). While the early modern period sought concertedly to rid the scientific landscape of all teleological elements, resistance was met in the biological sciences by the challenge posed by living nature. Immanuel Kant (1724-1804) in his Critique of the Power of Judgment and Opus Postumum drew attention to the unique nature of an organism as a self-organizing natural product that exhibits reciprocal causality, i.e., its parts exist for the sake of the whole and the whole exists for the sake of its parts (Kant, KU, 5: 372-74). As such, Kant asserts, we view an organism as if it were a natural purpose (Naturzweck), an end of nature, equipped with a function – though only in a regulative and not a constitutive sense. Kant stresses that the feature of organisms that renders them distinct from other organized beings, is that the source of the organization is not external to the organism but rather internal. This source is the self which the organism generates in the activity of sustaining and regulating its existence (Keller, 2007). It is for this reason, Kant maintains, that organic beings are not entirely reducible to merely mechanistic phenomena, and thus are underdetermined by the laws of physics.

Kant's characterization of organic beings profoundly influenced the study of organic nature in his time and ultimately resulted in the demarcation of a new domain in the scientific disciplines of the eighteenth century – biology . The establishment of this new discipline to deal with the unique subject matter of this domain largely derived from the recognition of the irreducible notion of function that an understanding of organic beings required. (Quarfood , 2006; Van den Berg, 2014). Kant's account of the nature of organisms and the new study it engendered has, however, produced more complexity than it has resolved. Specifically, it has left philosophers and scientists bewildered as to what status the notion of natural purpose has in scientific explanation and to what extent it is indicative of genuine purposiveness and functionality in nature. Kant explicitly states that the notions of purpose, function and final end are not to be taken as indicative of a constitutive teleology pertaining to nature itself, but merely as regulative principles for guiding our investigation of organic beings. This framework is operative in the current study of biology which claims to adopt a teleonomic rather than a teleological approach (Pittendrigh, 1958; de Laguna, 1962; Mayr, 1974; Reese, 1994; Toepfer 2012), which stresses the merely apparent purposefulness manifested by organisms. Teleonomy also emphasizes that any apparent goal-directedness or function ought to be understood as having its source in natural laws, as opposed to human or divine activity. Teleonomy has also been associated with a programmatic or computational conception of purposiveness, distinguishing it even more definitively from robust Aristotelian teleology.

Very recent debates in genomics concerning "junk DNA," however, suggest that the notions of purposiveness and function still require substantial scrutiny before we can adequately employ these notions in scientific study. Even in their present, refined versions, significantly facilitated by Kant's novel conceptual framework, much disagreement persists among biologists, in particular microbiologists, with respect to the kind of function that may be attributed to certain kinds of DNA that have, for forty years, been thought to be "junk," i.e., lacking a function. The controversy has been generated by ENCODE investigators who claim to have disproved the long-held belief that our genome essentially consists of structural components that are devoid of information that can be viewed as in any way functional (ENCODE Project Consortium et al. 2012). Those who question the validity of this claim have drawn attention to the fact that a resolution of the controversy requires a closer analysis of the proper use of the term function (Doolittle, Brunet, Linquist and Gregory, 2014). The problem, it seems, is that these disputes often involve the conflation of two quite distinct conceptions of function: a) causal role functionality (CR), and b) selected effect functionality (SE). The former is a much weaker conception of functionality and ultimately denotes simply what something does. The latter is conceived as a more robust conception of functionality that appeals to the effects of natural selection. Conflating these distinct notions of function, some claim, amounts to viewing all processes and aspects in organic nature as the product of adaptation and natural selection, amounting to panadaptationism (Doolittle et al. 2014).

I argue that the resistance that some microbiologists manifest to the attribution of such ubiquitous SE functionality to organic nature (instead of acknowledging the weaker CR functionality in some instances) suggests that SE functionality is conceived as an ontologically real notion of function manifested by organic beings. Consequently, the notion of function, even in the teleonomic sense employed in current microbiology, seems to acknowledge the Kantian notion of the irreducibly biological element that demarcates organic nature as a unique field of scientific inquiry. I contend that, despite the radical development that the notion of function has undergone, it has not been completely deflated. Specifically, I argue that an appreciation of some key distinctions pertaining to the notion of natural purpose that Kant had introduced to explain the nature of organisms as self-organizing beings may contribute to a clarification, if not a resolution, to the important current "Junk DNA debate" in microbiology. The concept of a self-organizing being was definitive in demarcating the domain of biological inquiry, and entailed a conception of function (natural purpose) that proved to be ineliminable from this form of inquiry, not only in Kant's own time but also, as I shall show, in ours.

*Scientific realism, approximate truth and the argument from underdetermination*

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Scientific realism is the view that we are justified in taking our best scientific theories to be approximately true, not only in what they say about the observable part of the world, but also in what they say about the unobservable aspects of it. Realists argue that, if our best scientific theories were not approximately true, their predictive success would be a miracle. But anti-realists reply that, since theories are underdetermined by the evidence, the fact that a theory is predictively successful can never warrant the belief that it is true, even if we grant that predictive success is truth-indicative in a very strong sense. The argument from underdetermination rests on the assumption that, for any theory T, there is another theory T' which makes exactly the same predictions about the observable part of the world but is nevertheless inconsistent with T. Moreover, empiricists assume that, unless a theory T is more predictively successful than a theory T', we cannot be justified in believing that P(T is true) > P(T' is true). Finally, the argument rests on the assumption that, if a proposition A is inconsistent with a proposition B and we have no reason to suppose that P(A) > P(B) or that P(A) < P(B), then it would be irrational to believe that A is true or that B is true. I show that, if those assumptions are true, then indeed the no miracle argument is refuted.

Realists typically reply by arguing that at least one of the premises is false and, while I think they are probably right about that, I also think that kind of response misses what is really problematic with the argument from under-determination. I want to argue that, even if all the premises of that argument were true, it would still pose no threat to scientific realism. Indeed, scientific realism is the view that we are justified in taking our best scientific theories to be approximately true, not that we are justified in taking them to be true. But the argument from underdetermination, as it has traditionally been formulated, only shows that we are not justified in taking our best scientific theories to be true, which realists should be happy to concede since they never claimed otherwise. When the argument is reformulated in terms of approximate truth, as it should be if we take seriously the way in which realists state their view, it no longer is valid.

Indeed, the original argument rested on the fact that, if a theory T is inconsistent with a theory T', the proposition that T is true is inconsistent with the proposition that T' is true. If, as the first premise of the argument from underdetermination asserts, for any theory T, there is another theory T' which is empirically equivalent to but inconsistent with T, it follows from the second premise of the argument that we are neither justified in believing that P(T is true) > P(T' is true) nor that P(T' is true) > P(T is true). In turn, by the last premise of the argument, this implies that we are not justified in believing that T is true no matter how predictively successful it is, because the proposition that T is true is inconsistent with the proposition that T' is true. But I argue that, from the fact that a theory T is inconsistent with a theory T', it does not follow that the proposition that T is approximately true is inconsistent with the proposition that T' is approximately true. Hence, when the argument from underdetermination is reformulated in terms of approximate truth, the last step is no longer valid. Thus, if we take seriously the fact that any plausible version of scientific realism must be cashed out in terms of approximate truth, the argument from underdetermination fails. I conclude by outlining some ways in which, beyond what I have said about the argument from underdetermination, the debate about scientific realism could be illuminated if philosophers of science, including realists, took that fact more seriously.

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*How to deal with truth in pluralism within philosophy of sciences: boundaries and scopes*

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Pluralism has gained supporters in the last decades, particularly in philosophy of science. According to some authors, a pluralistic approach would provide the key to understand the development of scientific practice and work out certain classical philosophical problems. Indeed, pluralism takes the actual and multiple scientific practice and history of science as the spearhead against monism, reductionism and fundamentalism. It shows that the existence of universal laws or the belief in a single scientific system are wrong and misguided assumptions that do not fit in with the dappled and multicolor picture of science. For that reason, pluralism has found a comfortable place and has grown in the realms of special philosophies of sciences. Nevertheless, it is not possible to identify pluralism with a single and coherent set of thesis: some authors hold a merely epistemological or methodological pluralism (Chang 2004, 2012), whereas others assume a stronger one committed to the idea of a plural reality –generally an internal or constituted reality (Dupré 1993, Cartwright 1999, Lombardi and Perez-Ransanz 2008, 2012).

The notion of truth has an undeniable value in philosophy of science. Some issues involved in discussions regarding scientific realism, in particular semantic and epistemic realism, are useful to understand the question of truth in pluralism and with this, its boundaries and scopes. Within pluralistic approach there is a widely spread dictum according to which pluralism has to leave behind every idea of truth in the sense of correspondence. In accordance with the fundamental intuition of a correspondence theory, truth depends on the fact that a state of things showed by the world satisfies the assert of a statement. Hasok Chang's pluralism, for instance, claims that monism would imply a notion of truth as correspondence and certain ontological commitment since it assumes that the world is one and, consequently, truth about the world is one. Thus, not only the task of science must be looking for truth but there is also one answer to every scientific question. However, Chang seems to hold the contrary implication as well: correspondence notion of truth implies monism so that every reasonable pluralistic position cannot assume a correspondence notion of truth. Instead, a coherence notion of truth seems to be in line with a genuine pluralism. Chang offers several arguments for supporting his pluralism but the defense is based fundamentally on his rejection of monism: the search for truth, as it is one of the most firm motivations for monism, constitutes a restriction for scientific practice because it cuts short multiple alternative lines of research. Thus, a pluralistic point of view would be a preferred choice for diversifying the scientific research.

However, some authors have gone one step further within pluralism. Nancy Cartwright (1999) argues we live in a dappled world; our best scientific theories describe the world adequately, but natural laws apply only to very special cases or shielded environments. Hence a scientific theory is only true in its particular domain that cannot be universal. Her nomological pluralism claims nature is governed in different domains by different systems of laws not necessarily related to each other in any systematic or uniform way. That is why natural laws describing this world are a patchwork. Cartwright's pluralism is semantically (and ontologically) stronger than Chang's since it not only gives up fundamentalism and universalism but it also holds a strong realism where natural laws are literally true. Therefore, it assumes a correspondence notion of truth being at odds with the Chang's dictum. Also, Rudolf Carnap advocated a pluralism approach and a correspondence notion of truth in his famous paper "Empiricism, Semantics and Ontology" (1956). Conforming to Carnap, questions about existence of entities or natural laws have to be made within a linguistic framework: it is only in relation to a linguistic framework that questions of existence make sense, since to be real means to be an element of a linguistic system. External questions related to the existence of the "thing world" itself cannot be answered because they are formulated in a wrong way. This kind of carnapian pluralism has mutatis mutandis been development and carried on by Hillary Putnam and its internal realism (1981) and by Olimpia Lombardi and Ana Rosa Perez Ransanz (2008, 2012), who stand up for an ontological pluralism of Kantian roots. Even though Putnam's internal realism maintains a notion of truth based on "idealized rational acceptability" (closer than Chang), Olimpia's and Ransanz's pluralism, following Kant's philosophy, holds a correspondence notion of truth which makes sense within conceptual scheme that constitutes reality.

However, boundaries and scopes of pluralism are unclear and fuzzy. Must pluralism in philosophy of science only be epistemic? Must it assume a coherence notion of truth and reject realism? To put them in another way, may pluralism be realistic and hold a correspondence notion of truth? And if it were so, what kind of correspondence are we talking about if it always depends on conceptual scheme?

In this work, we will try to shed some light on these questions. On the one hand, we shall argue against the Chang's dictum: pluralism is not intrinsically and unavoidably engaged with a coherence notion of truth neither with instrumentalism. Thus, a correspondence notion of truth under a realistic pluralism may be advocated. We think it would be philosophically fruit-bearing for pluralism because it allows going beyond Chang's constraint. On the other hand, we shall discuss the idea of an ontological pluralism and a correspondence notion of truth in relation to a linguistic framework or conceptual scheme. We suspect that classical discussion between the metaphysical realism and pluralistic realism comes from certain misunderstandings about what it is meant by "reality" and by "correspondence". Indeed, it seems that while the metaphysical realism strategy claims to endorse Carnaps external sense, the pluralistic one is to talk about reality and correspondence but always within Carnap internal sense. That is why we wonder if the discussion is necessarily developed on the basis of these unrecognized misunderstandings or if it is possible to defend a pluralistic position without “deflating” reality subordinating it to a constitutional framework.


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*The epistemology of no-go theorems*


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When scientists extend their theories or develop new ones they sometimes encounter so-called no-go theorems or impossibility results. These results usually have had two effects on the field. Either, the no-go result effectively stopped that research programme or one or more of the assumptions involved in the derivation were questioned. Further research would then usually show that the assumptions were too strong and should be replaced with something else. Although methodologically obvious, that second option was recognized only much later and for independent reasons. Given the role no-go theorems play in the development of science it is surprising that they have not received more attention within the philosophy of science literature.

In this paper I address some more general features of no-go theorems and try to address the question how no-go results should be interpreted. The way they should be interpreted differs significantly from how they have been interpreted in the history of physics. More specifically, I will argue that no-go theorems should not be understood as implying the impossibility of a desired result, and therefore do not play the methodological role they purportedly do, but that they should be understood as a rigorous way to outline the methodological pathways in obtaining the desired result.

A no-go theorem is usually presented as an impossibility result. Given certain assumptions one has to accept that a certain goal is not possible. However, if you are committed to that goal, you will use the no-go result as a modus tollens argument against one of the assumptions involved. However, this, as I will argue, is a too simplified view of the general structure of no-go theorems. No-go results in physics have a much richer structure, which effects the interpretation of the no-go result. The paper consists of two parts:

In the first part of the paper I will give a detailed historical case study of the development of a class of no-go theorems, regarding the impossibility to combine internal and external symmetries in particle physics. This case nicely illustrates the many aspects of no-go theorems. Furthermore, it is an exemplar of how the no-go result was misunderstood and where the more general view would provide the right interpretational framework.

In the second part I give an abstract and systematic analysis of no-go theorems and discuss the methodological implications one can draw from them. I define a no-go result as an inconsistency that arises between a derived consequence of physical assumptions P within a mathematical structure M stated within a framework F, and a goal G or physical background assumption B. An analysis of this more general structure allows one to more carefully differentiate the methodological implication one should draw from the result. For this purpose I assess the empirical and theoretical accessibility of each element involved in the above definition. It is shown that the assumptions with the least amount of justification are the mathematical assumptions rather than the physical assumptions that are usually considered. Furthermore, the mathematical assumptions usually are empirically less or not at all accessible. I will then discuss the consequences this has for the interpretation of no-go results more generally.

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**Vendredi après-midi 01 juillet / Friday afternoon July 01**
Salle / Room 321

*Théories finitistes des transitions de phase, émergence et idéalisations infinies*

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Le phénomène de transitions de phases, tel que la solidification de l'eau ou la transition ferro-paramagnétique, fait l'objet d'intenses discussions en philosophie des sciences. Selon certains (Batterman 2011, Jones 2006, Liu 1999, Mainwood 2006) ce phénomène met en échec la réduction de la thermodynamique à la physique statistique et doit être considéré comme un phénomène émergent. Cette position a été jusqu'ici critiquée, soit en proposant une analyse critique de la définition des transitions de phase (Callender 2001), soit en montrant que la notion d'émergence dans ces discussions est compatible avec une notion de réduction (Butterfield 2011, Butterfield & Nazim 2012). Dans cette intervention, je propose d'examiner une nouvelle critique proposée par Menon et Callender (2013) et qui, à ma connaissance, n'a pas encore fait l'objet de discussions en philosophie. Cette critique s'appuie sur le développement de nouvelles approches théoriques en physique statistique pour étudier les transitions de phase. Mon but est d'examiner cette critique et de montrer les nouvelles questions qu'elles soulèvent. Pour cela, après avoir rappelé l'argument central des conceptions émergentistes des transitions de phase, je présente les nouvelles théories auxquelles Menon et Callender font référence. Ensuite, (i) j'explicite en quoi ces nouvelles théories permettent de formuler une objection sérieuse à la conception émergentiste, et (ii) je montre que la critique de Menon et Callender suppose d'analyser avec soin en quels sens et dans quelle mesure ces nouvelles théories décrivent et expliquent le phénomène de transitions de phase.

Dans le débat philosophique actuel, l'argument principal en faveur d'une conception émergentiste des transitions de phase  repose sur un résultat théorique (Griffiths 1972) : la physique statistique ne peut rendre compte d'une transition de phase que dans la limite où le nombre de constituants du système tend vers l'*infini*. Autrement dit, « l'existence d'une transition de phase nécessite un système infini. Aucune transition de phase n'a lieu dans un système avec un nombre fini de degrés de liberté » (Kadanoff 2000, p. 238). Pourtant, les systèmes réels ne sont composés que d'un nombre *fini* de constituants. Ainsi, selon Batterman (2011, p. 1033) et d'autres philosophes, il n'est pas possible d'expliquer qu'un système réel composé d'un nombre fini de constituants exhibe une transition de phase et, par conséquent, les transitions de phases doivent être considérées comme des phénomènes émergents.

Menon et Callender renouvellent le débat actuel sur les transitions de phase en montrant que de nouvelles approches théoriques en physique statistique n'utilisent pas la limite infinie pour étudier les transitions de phase: « il y a déjà plusieurs approches pour décrire les transitions de phases dans les systèmes finis. […] C'est un domaine de recherche toujours en cours mais dont les résultats montrent déjà l'existence de transitions de phase thermodynamique pour des systèmes finis. » (2013, p. 18). Dans cet exposé, je me concentrerai sur deux approches théoriques. Une première (théorie 1) consiste à étudier les transitions de phase dans l'ensemble microcanonique (Chomaz, et al. 2001). Une deuxième (théorie 2) consiste à décrire les transitions de phase à l'aide de fonctions mathématiques complexes (Borrmann et al. 2000).

Dans un premier temps, je commencerai par expliciter ce qui est, selon moi, l'élément central de la critique de Menon et Callender mais que les deux auteurs ne discutent pourtant pas explicitement. Ces théories finitistes mettent en défaut un présupposé majeur de la thèse émergentiste, à savoir que l'explication du phénomène de transitions de phase en physique statistique requiert une idéalisation infinie *inéliminable* (Jones 2006, p. 4), à savoir que le système est composé d'un nombre infini de constituants. Le développement de ces approches finitistes montrent, au contraire, que l'on peut tout à fait éliminer cette idéalisation infinie. Ces théories finitistes ne sont pas en contradiction avec le résultat de Griffiths (1972). Elles montrent cependant que l'on peut proposer de nouvelles définitions pour les transitions de phases qui s'affranchissent des hypothèses de ce résultat d'impossibilité. En l'occurence, la théorie finitiste 1 étudie un système dans l'ensemble microcanonique alors que le résultat de le résultat de Griffiths est valable dans l'ensemble canonique. De même, la théorie finitiste 2 change la représentation mathématique de l'énergie libre du système, en la définissant comme une fonction complexe alors que le résultat de Griffiths portent sur des fonctions à valeurs réelles. Plus généralement, le développement de ces théories finitistes illustrent que les considérations métaphysiques à propos des théories scientifiques tirées de résultats d'impossibilité peuvent être remises en cause par le changement scientifique.

La force de la critique de Mellon et Callender dépend de la viabilité et de la généralité de ces nouvelles théories. Dans un deuxième temps, je propose ainsi d'examiner dans quelle mesure ces nouvelles approches permettent effectivement de décrire, expliquer et prédire les transitions de phase. Pour cela, je commencerai pas examiner la manière dont ces approches sont confirmées, à l'aide de données empiriques ou bien de simulations numériques. Ensuite, je propose de clarifier en quels sens les phénomènes étudiés par ces nouvelles approches finitistes peuvent être légitiment qualifiés de "transitions de phase". C'est une question qui se pose expressément puisque ces théories conduisent à de *nouvelles* définitions pour les transitions de phase. Par conséquent, j'examinerai si, et en quel sens sens, ces nouvelles définitions sont compatibles avec la définition traditionnelle des transitions, éventuellement équivalentes à cette définition ou même plus générales que celle-ci. Pour cela, je proposerai une analyse comparative, à la fois des nouvelle approches théoriques entre elles, mais aussi des nouvelles approches par rapport à l'approche traditionnelle.


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*De l' « utilité négative » de la philosophie d'Ernst Cassirer : application à l'argument EPR*


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Dans une lettre de 1937, Einstein résume à Cassirer l'argument EPR, qui vise à démontrer l' « incomplétude » de la théorie quantique en exploitant une propriété spécifique à son formalisme tensoriel, la corrélation des états de deux sous-systèmes d'un même système quantique global. Mon intention ici n'est pas de discuter cet argument en tant que tel (dont la structure est alambiquée, et qui a par ailleurs été l'objet d'une littérature surabondante), mais de chercher à reconstituer la réception que Cassirer aurait pu en avoir. À ma connaissance en effet, il n'en existe pas de trace, ni dans sa correspondance, ni dans son ouvrage de 1936 consacré à la théorie quantique (exceptée une courte remarque de Margenau dans la préface de la traduction anglaise, où il affirme que Cassirer aurait rejeté la complétion de la théorique quantique par des variables cachées - l'argument EPR ne proposant pas directement de variables cachées, mais ayant été utilisé par les défenseurs des théories à variables cachées). Je montrerai, sur la base de ce dernier ouvrage, que Cassirer aurait sans doute rejeté l'argument EPR, ce qui confèrerait à sa philosophie une « utilité négative » au sens de Kant, en tant qu'elle servirait à « corriger » ou « rectifier » notre connaissance.

La version de l'argument exposée dans la lettre résume celle de l'article : deux « points matériels » (particules) interagissent puis sont séparés ; la connaissance de la fonction d'onde globale , ainsi qu'une mesure au point 1, permet d'en déduire (sans la mesurer) la fonction d'état caractérisant le point 2. Par ailleurs, il est possible de faire une autre mesure au point 1, ce qui impliquerait une autre fonction d'état pour (supposément) le même point 2, et contredirait donc l'exigence de « coordination univoque » entre la théorie et la réalité physique. Or, je soutiendrai qu'il y a au moins deux grandes raisons pour lesquelles Cassirer aurait rejeté l'argument.

La première est qu'il n'aurait pas souscrit à ses prémisses, non pas tant celle du fameux « critère de réalité EPR » que celles (explicite et implicite, respectivement) de « localité » et de « séparabilité », sur lesquelles repose fondamentalement l'argument. La « localité » devrait plus exactement être appelée causalité locale (ou relativiste : c'est l'impossibilité de propager une influence causale entre des points séparés par un intervalle de type espace). Mais c'est la prémisse de séparabilité (qui considère des points spatialement séparés comme ayant des existences individuelles), sur laquelle repose celle de localité, qui est la plus fondamentale. Il vaut la peine de remarquer que, dans les versions plus simples de l'argument données par Einstein, les présupposés de localité et de séparabilité demeurent essentiels (on pourrait objecter qu'il a également donné une version qui ne fait pas appel à ces présupposés pour montrer l'incomplétude de la théorie quantique, mais ce dernier exemple (le « baril de poudre ») ne concerne (tout comme le chat de Schrödinger) que l'incomplétude de la description quantique de systèmes macroscopiques, et demeure controversé aujourd'hui. En ce qui concerne du moins des particules quantiques (des photons dans les expériences d'Aspect), la « non séparabilité locale » a été établie comme un fait expérimental qui interdit toute complétion de la théorie quantique par des variables cachées : si la question de la complétude de la théorie quantique demeure donc ouverte, un raisonnement type EPR sur des systèmes quantiques en interaction est inapplicable car ses prémisses sont réfutées par le fait). Or Cassirer n'aurait certainement pas souscrit à ces présupposés, le principe d'individuation de la théorie (quantique ou autre) devant toujours être, à ses yeux, son "terminus ad quem" et non son "terminus a quo" basé sur des intuitions immédiates, conformément à sa re-conceptualisation de la philosophie transcendantale de Kant.

De plus, le but de l'argument EPR (établir l'incomplétude de la théorie quantique) obéit au réquisit (fondamental pour Einstein) d'univocité de la coordination entre la théorie et la réalité physique. Or, bien qu'en 1920 Cassirer souscrive encore à cette exigence, en 1936 il l'a abandonnée, comme en témoigne son adoption du principe de complémentarité de Bohr, qui rejoint un principe fondamental de sa philosophie systématique (la pluralité des manières de penser la réalité). Du reste cette exigence, en tant qu'elle présuppose une vérité correspondance entre la théorie et la réalité (une « théorie copie », comme dirait Cassirer), et, en termes kantiens, l' « idéal de la complète détermination » (spatio-temporelle, dans le cas de la théorie quantique), contredit déjà des thèmes constants de sa philosophie (la lutte contre l'ontologisation des symboles, ou de la réalité en soi), présents dès 1910. Ici, comme précédemment, c'est l'effort toujours renouvelé pour repenser le concept d'objet de la science (conformément, comme je le soutiendrai, à un des sens de l' « objet transcendantal » de Kant), en se concentrant sur ses seules conditions d'accessibilité (théoriques surtout, dans son cas), qui permet à Cassirer de mettre en évidence, et de dépasser, les présupposés auxquels de nombreux physiciens restent encore attachés.

En conclusion, Cassirer n'aurait donc sans doute pas souscrit à l'argument EPR, ni dans sa forme originale (la localité-séparabilité implique l'incomplétude), ni dans sa forme contraposée (la complétude implique la non localité-séparabilité), ce qui lui aurait permis d'éviter le (faux) « dilemme » dans lequel nous enferme l'argument, comme le dit Fine. On comprend ainsi l'utilité négative de la philosophie de Cassirer, qui consiste, selon la « méthode transcendantale » analytique de Kant, à critiquer les présupposés dont font usage les physiciens, contre tout réalisme naïf. Une telle utilité négative, sans aller jusqu'à la « rationalité transhistorique prospective » de Friedman (que l'on pourrait assimiler à l' « utilité positive » de Kant, qui « élargit » notre connaissance), facilite le passage d'un paradigme à l'autre (plus exactement, facilite l'abandon de l'ancien paradigme, sans pour autant suggérer le nouveau). Mais cette pertinence de la philosophie cassirérienne a aussi son revers : son inconsistance croissante, sinon au sens d'une contradiction de ses thèses (comme entre la complémentarité et le caractère constitutif de l'a priori), du moins de leur affaiblissement (l'a priori de Cassirer finissant par se réduire, en tant qu'exigence de légalité, à la définition même de la physique).

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*La notion de précision expérimentale dans les ajustements des constantes de la physique*

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L'« ajustement des constantes de la physique » est une activité qui remonte à l'année 1929, avec les travaux du physicien américain Raymond Birge (1887-1980), qui produisit une synthèse critique des mesures de précision des constantes physiques effectuées par ses contemporains. Selon les théories physiques acceptées à un instant donné, ces constantes sont reliées les unes aux autres au travers d'un réseau surdéterminé d'équations. Pourtant, les valeurs mesurées ne respectent pas toujours les équations du réseau : c'est la trace de la présence d'« erreurs » de mesure. Birge réalisa donc qu'il ne fallait pas considérer les constantes indépendamment les unes des autres, mais qu'il pouvait modifier – « ajuster » – les valeurs mesurées de façon à dériver un ensemble unifié et cohérent de valeurs, leurs « valeurs probables ». L'initiative personnelle de Birge fut reprise par différents scientifiques durant les quatre décennies qui suivirent, menant ainsi à une tradition d'ajustements qui se structura progressivement jusqu'à devenir institutionnalisée et centralisée au sein du CODATA ("Committee on Data for Science and Technology"), un organisme international qui se charge désormais de publier tous les quatre ans une liste de valeurs recommandées des constantes de la physique.

La démarche d'ajustement réunit trois ingrédients essentiels. En premier lieu, elle est adossée aux théories physiques acceptées à un moment donné, qui permettent d'établir le réseau d'équation par lequel sont reliées les constantes. L'ajustement n'est alors valable qu'à la condition que les théories employées soit valides ; en retour, l'ajustement peut lui-même amener à s'interroger sur la validité des théories en question et permet de ce fait un potentiel test des théories. En second lieu, la démarche d'ajustement hérite d'une méthode statistique classique, l'« ajustement aux moindres carrés » proposée par Legendre et Gauss au début du XIXe siècle, et qui, appliquée aux ajustements des constantes, consiste grossièrement à effectuer une moyenne généralisée des différentes valeurs obtenues pour chaque constante. Enfin, en troisième lieu, la démarche d'ajustement met en jeu une réflexion indispensable sur la précision des mesures. Les ajustements font intervenir de façon structurelle un estimateur quantitatif de la précision des mesures, que l'on appelle aujourd'hui « incertitude de mesure ». Le site du CODATA indique par exemple :

Masse de l'électron : me = 9,109 383 56(11).10^(?31) kg

L'incertitude de mesure sur les deux dernières décimales, exprimée par la valeur entre parenthèses, indique la largeur de l'intervalle de valeurs dans lequel les scientifiques considèrent raisonnable de penser que la masse de l'électron se trouve.

La présentation que je propose vise à m'interroger sur la signification qui est accordée à la notion en apparence intuitive de « précision » d'une mesure dans le domaine de la physique de précision, en m'appuyant sur l'analyse des comptes-rendus d'ajustement, depuis ceux de Birge en 1929 jusqu'aux travaux du CODATA les plus récents. Je montre que l'appréhension de ce concept est en fait multiple et, en particulier, fait émerger une tension entre incertitude de mesure et erreur de mesure. Le problème philosophique essentiel qui se pose à un expérimentateur qui souhaite évaluer la précision d'une mesure réside dans le fait que cette démarche l'amène à chercher à quantifier l'étendue de ce qu'il ne sait pas – une tâche que l'on pourrait tenir pour impossible par définition. En réponse à ce problème, une approche contemporaine de la mesure qui progresse chez les scientifiques interprète l'incertitude de mesure comme l'expression d'un degré de croyance subjectif de l'expérimentateur, et ce indépendamment de l'erreur effective (inconnue) de mesure, c'est-à-dire de l'écart entre le résultat obtenu et la valeur dite « vraie » (inconnue) de la constante mesurée. Cependant, cette interprétation est à son tour problématique car, en faisant disparaître tout attachement à l'erreur de mesure, et en ramenant l'évaluation d'une mesure à une description d'un état subjectif de croyance, elle fait disparaître le caractère normatif de la mesure, et avec lui, son idéal d'objectivité. L'incertitude de mesure ne peut être tout au plus que ce que l'expérimentateur croit être la précision du résultat. Si un autre scientifique souhaite faire usage du résultat de mesure que lui fournit le premier expérimentateur, il entre lui-même dans un rapport de confiance – dans un état de dépendance épistémique – avec son fournisseur. De fait, les ajustements des constantes de la physique nous rappellent justement à quel point les notions d'erreur de mesure et de précision ont un caractère social, qui s'exprime tout particulièrement lorsqu'on confronte les résultats de mesure obtenus par différentes personnes, dans différents laboratoires, avec différentes méthodes de mesure, et à différents instants. Or, on qualifie souvent l'incertitude de mesure d'évaluation de la qualité d'un résultat, ou d'évaluation de la confiance que l'on accorde à un résultat. Je défends qu'en regard de l'interprétation contemporaine du concept, ces deux acceptions sont impropres et ne recouvrent que partiellement la façon dont les incertitudes de mesure sont exploitées par les scientifiques dans leurs pratiques. Dans la continuité, je rappelle l'importance d'une seconde notion, qui se rattache elle aussi à l'idée commune de « précision », mais d'une façon différente, à savoir l'« exactitude » d'une mesure, comprise comme l'« étroitesse de l'accord entre une valeur mesurée et [la] valeur vraie d'une [grandeur] » (Vocabulaire international de métrologie, 2012), c'est-à-dire comme une évaluation de l'erreur commise. S'il va de soi qu'on ne peut jamais garantir à un instant donné l'exactitude d'un résultat, pour les raisons mentionnées précédemment, celle-ci demeure un objectif important, et une notion qui conserve un sens et un intérêt, à condition de la considérer dans un processus continu et à jamais inachevé de perfectionnement des résultats de mesure, dont le moteur est la correction des erreurs de mesure. De fait, plus que de chercher à obtenir de « bonnes » valeurs des constantes physiques, les ajustements visent à permettre l'identification des erreurs de mesure tout en fournissant au passage un test des théories physiques mobilisées. L'exactitude de mesure est alors une conception tournée vers le futur, que l'on est amené à comprendre en rapport avec une dynamique de progrès scientifique – une dynamique de correction.

## Index


*A*

**Accorinti, Hernan**, hernanaccorinti@gmail.com, Universidad de Buenos Aires · 96, 130

**Allori, Valia**, vallori@niu.edu, Northern Illinois University · 32

**Andler, Daniel**, daniel.andler@gmail.com, Université Paris-Sorbonne & Ecole normale supérieure · 15

**Arcangeli, Margherita**, Margherita.Arcangeli@etu.unige.ch, University of Geneva - Swiss Center for Affective Sciences · 56

**Ardourel, Vincent**, vincent.ardourel@gmail.com, Université catholique de Louvain · 133

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**Barberousse, Anouk**, Anouk.Barberousse@paris-sorbonne.fr, Université Paris 4 · 15

**Barton, Adrien**, adrien.barton@gmail.com, Université de Sherbrooke · 123

**Bary, Sophie**, sophie.bary@gmail.com, Muséum National d'Histoire Naturelle · 15

**Basilico, Brenda**, brendavbasilico@gmail.com, Université de Lille 3 · 18

**Baulu Mac Willie, Mireille**, mireillebm@hotmail.ca, Université Sainte-Anne · 28

**Bedessem, Baptiste**, baptiste.bedessemp@gmail.com, Laboratoire Philosophie, Pratiques et Langages · 44

**Bertoldi, Nicola**, nicola.bertoldi87@gmail.com, IHPST, Université Paris 1 Panthéon-Sorbonne · 119

**Bittencourt, Wellington**, biowell@hotmail.com, Université fédérale de Bahia · 63

**Bschir, Karim**, bschir@phil.gess.ethz.ch, ETH Zurich · 112

**Butterfield, Jeremy**, jb56@cam.ac.uk, Trinity College · 63

*C*

**Casini, Lorenzo**, lorenzodotcasini@gmail.com, University of Geneva · 58

**Chavalarias, David**, chavalarias@gmail.com, CNRS et Institut des systèmes complexes, Paris · 63

**Clavien, Christine**, Christine.Clavien@unige.ch, iEH2 - Institut Éthique Histoire Humanités · 106

**Córdoba, Mariana**, mariana.cordoba.revah@gmail.com, Conicet – Universidad de Buenos Aires · 107, 130

**Cordovil, João L**, jlcordovil2@hotmail.com, Center for Philosophy of Sciences of the University of Lisbon · 33

**Cozic, Mikael**, mikael.cozic@ens.fr, Université Paris-Est, IUF & IHPST · 23

*D*

**Dardashti, Radin**, radin@gmx.de, Université de Genève, MCMP · 132

**Dieli, Anna Maria**, annamariadieli@gmail.com, Université Paris 1 Panthéon-Sorbonne · 84

**Doboszewski, Juliusz**, jdoboszewski@gmail.com, Jagiellonian University / Université de Genève · 114

**Doerig, Adrien**, adrien.doerig@gmail.com, EPFL · 69

**Dupin, Aurore**, aurore.dupin@tum.de, Technische Universität München · 35

*E*

**Egg, Matthias**, matthias.egg@philo.unibe.ch, University of Bern · 31

**El Skaf, Rawad**, rawadskaff@gmail.com, Université Paris 1 IHPST · 55

**Esfeld, Michael**, michael-andreas.esfeld@unil.ch, University of Lausanne · 59

*F*

**Fahrbach, Ludwig**, ludwig.fahrbach@gmail.com, University of Essen · 75
**Farr, Matt**, mail@mattfarr.co.uk, University of Queensland · 117
**Faugère, Elsa**, elsa.faugere@avignon.inra.fr, INRA, UR 767 · 15
**Felline, Laura**, lfelline@uniroma3.it, Universita Roma Tre · 70
**Ferrando, Tiziano**, tiziano.ferrando@unil.ch, Université de Lausanne · 73
**Ferreira Ruiz, María José**, mariaferreiraruiz@gmail.com, University of Buenos Aires · 126
**Ferreira, Anthony**, a.a.c.ferreira@laposte.net, Institut de recherches philosophiques / U.Paris Ouest Nanterre la Défense · 37
**Ferry, Juliette**, julietteferry2@gmail.com, Université Paris-Sorbonne · 38
**Fisler, Marie**, marie.fisler@gmail.com, LabEx Comod · 63
**Fletcher, Samuel**, scfletch@umn.edu, University of Minnesota, Twin Cities · 61
**Fumagalli, Roberto**, R.Fumagalli@lse.ac.uk, University of Bayreuth, London School of Economics · 110

*G*

**Gayon, Jean**, jean.gayon@gmail.com, Université Paris 1-Panthéon Sorbonne · 35
**Gerville-Réache, Léo**, Leo.gerville-reache@u-bordeaux.fr, Université de Bordeaux - IMB · 42
**Ghica, Felicia**, ioanafelicia.ghica@gmail.com, Université de Lausanne · 20
**Giovannetti, Gabriel**, giovannettigabriel@gmail.com, CEPERC - UMR 7304 · 103
**Giroux, Elodie**, elodie.giroux@univ-lyon3.fr, Université Jean Moulin Lyon 3 · 43
**Grégis, Fabien**, fabien.gregis@etu.univ-paris-diderot.fr, Université Paris Diderot · 135

*H*

**Heesen, Remco**, rheesen@andrew.cmu.edu, Carnegie Mellon University · 51
**Held, Carsten**, carsten.held@uni-erfurt.de, Universität Erfurt · 93
**Henschen, Tobias**, tobias.henschen@uni-konstanz.de, University of Konstanz · 24
**Herzog Michael**, michael.herzog@epfl.ch, EPFL · 69
**Hladky, Michal**, michal.hladky@unige.ch, Université de Genève · 108
**Hooker, Clifford**, Cliff.Hooker@newcastle.edu.au, The University of Newcastle · 83
**Hubert, Mario**, Mario.Hubert@unil.ch, Université de Lausanne · 77
**Huneman, Philippe**, philippe.huneman@gmail.com, IHPST, CNRS · 63

*I*

**Israel-Jost,Vincent**, visraeljost@gmail.com, Université Catholique de Louvain · 124
**Ivanova, Milena**, milena.ivanova.phs@gmail.com, Ludwig Maximilian University of Munich · 111

*J*

**Jebeile Julie**, julie.jebeile@gmail.com, C.E.A. Saclay · 124

*K*

**Kaiser, Marie I.**, kaiser.m@uni-koeln.de, University of Cologne · 80
**Kao, Molly**, molly.kao@gmail.com, University of Western Ontario · 74
**Kirman, Alan**, alan.kirman@univ-amu.fr, Aix Marseille University and EHESS · 23
**Kostic, Daniel**, daniel.kostic@gmail.com, Institute for Philosophy, Faculty of Philosophy-University of Belgrade/IHPST-CNRS · 71

*L*

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**Lemoine, Philippe**, philippe.h.lemoine@gmail.com, Cornell University · 130
**Lequin, Mathilde**, mathildelequin@gmail.com, Université Toulouse Jean Jaurès, ERRAPHIS · 86
**Lipko, Paula**, lipkopaula@gmail.com, Conicet – Universidad de Buenos Aires · 107
**Lopez, Cristian**, lopez.cristian1987@gmail.com, Conicet – Universidad de Buenos Aires · 130
**Lopez, Olga**, olopez@yachaytech.edu.ec, Yachay Tech University · 102
**Love, Alan**, aclove@umn.edu, University of Minnesota · 80

*M*

**Malecka, Magdalena**, malecka.magdalena@gmail.com, Helsinki University · 88
**Marasoiu, Andrei**, aim3gd@virginia.edu, University of Virginia · 48
**Martínez González, Juan Camilo**, olimac62@hotmail.com, CONICET-Universidad de Buenos Aires · 96
**Matarese, Vera**, vera.matarese@gmail.com, University of Hong Kong · 60
**McFarland, Andrew**, andrewlmcfarland@gmail.com, North Carolina State University · 93
**Merlin, Francesca**, francesca.merlin@univ-paris1.fr, IHPST, Paris · 99
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**Mossio, Matteo**, matteo.mossio@univ-paris1.fr, IHPST (CNRS/Paris1/ENS) · 127
**Moya Diez, Ivan**, ivanmd@gmail.com, ISJPS, PhiCo, Université Paris 1 Panthéon-Sorbonne · 119

*N*

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**Norton, John**, jdnorton@pitt.edu, University of Pittsburgh · 122

*O*

**Oldofredi, Andrea**, andrea.oldofredi@unil.ch, Université de Lausanne · 78
**Onelli, Corinna**, corinna.onelli@gmail.com, Indipendent / London · 53

*P*

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*R*

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**Rial-Sebbag, Emmanuelle**, emmanuelle.rial@univ-tlse3.fr, UMR 1027 Inserm / Université Paul Sabatier Toulouse 3 · 99
**Rivelli, Luca**, luca.rivelli@gmail.com, University Paris 1 Panthéon-Sorbonne and University of Padova · 90
**Romano, Davide**, davide.romano@unil.ch, University of Lausanne · 76
**Ruphy, Stphanie**, stephanie.ruphy@wanadoo.fr, Université Grenoble Alpes · 70
**Ruyant, Quentin**, q_ruy@yahoo.fr, Université de Rennes 1 / Université de Louvain-la-Neuve · 104

*S*

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**Schmitt, Eglantine**, eglantine.schmitt@utc.fr, Université de technologie de Compiègne · 50
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**Stamenkovic, Philippe**, philippe.stamenkovic@polytechnique.org, Université Paris Diderot · 134

*T*

**Theurer, Kari**, Kari.Theurer@trincoll.edu, Trinity College · 49
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*W*

**Walter, Christian**, christian.walter@msh-paris.fr, Fondation Maison des sciences de l'homme · 124
**Weirich, Paul**, weirichp@missouri.edu, University of Missouri · 27
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**Wilks, Anna Frammartino**, anna.wilks@acadiau.ca, Acadia University · 128
**Wüthrich, Christian**, christian.wuthrich@unige.ch, University of Geneva · 95

*Y*

**Yapi, Ignace**, yapiaci@yahoo.fr, Université de Bouaké (Côte d'Ivoire) · 36