31 August – 5 September 2009, Les Treilles (France)

\textit{Science as it Could Have Been}

Discussing the Contingent / Inevitable Aspects of Scientific Practices

Supervisor: Léna Soler
l_soler@club-internet.fr

International Conference supported by \textit{Les Treilles Foundation}
www.les-treilles.com

Organization: Léna Soler and the PratiScienS group
(Rethinking Science from the Standpoint of Scientific Practices)
http://poincare.univ-nancy2.fr/Activites/?contentId=6163&languageId=1
Programme

Talks will be 45 minutes long and will be followed by a discussion

Tuesday 1 September 2009

Morning
Chairman: Ian Hacking

9h15 Opening, by Léna Soler

Catherine Allamel-Raffin & Jean-Luc Gangloff (Strasbourg, France)
Some Remarks about the Definitions of Contingentism and Inevitabilism

Catherine Dufour (Nancy, France)
Standard Quantum Mechanics versus Bohmian Mechanics: a Case for Contingentism?

Eran Tal (Toronto, Canada)
Making Time: Stability and Contingency in the Maintenance of the Standard Second

Afternoon
Chairman: Jean-Marc Lévy Leblond

Frédéric Wieber (Nancy, France)
On Questions Raised by the ‘Contingency-Inevitability’ Debate about the Status of Historical Explanation

Ronald Giere (Minnesota, USA)
Perspective, Contingency and History

Wednesday 2 September 2009

Morning
Chairman: Michel Bitbol

Andrew Pickering (Exeter, UK)
Science, Contingency and Ontology

Léna Soler (Nancy, France)
A General Structural Argument in Favor of the Contingency of Scientific Results

Hasok Chang (London, UK)
A Case for Scientific Pluralism

Afternoon
Chairman: Harry Collins

Mieke Boon (Twente, Pays-Bas)
Contingency and Conceptualization as the Fuel to Scientific Practices

Emiliano Trizio (Nancy, France)
Contingentism and Scientific Realism
Thursday 3 september 2009

Morning
Chairman: Jean-Michel Salanskis

Joseph Rouse (Middletown, USA)
Laws, Scientific Practice, and the Contingency/Inevitability Question

Harry Collins (Cardiff, UK)
What’s Special about Science?

Michel Bitbol (Paris, France)
About the Contingency of the (Quasi) Disappearance of Introspection in Psychology

Afternoon
Chairman: Ronald Giere

Yves Gingras (Montréal, Canada)
Necessity and Contingency in the Discovery of Electron Diffraction

Jean-Marc Lévy-Leblond (Nice, France)
What if Einstein had not been there? A Gedanken Experiment in Science History

Friday 4 september 2009

Morning
Chairman: Yves Gingras

Claude Rosental (Paris, France)
Is Logic Contingent? Reflections around the History a Logical Theorem

Jean-Michel Salanskis (Paris, France)
Inevitability, Inexorability, Framework

Ian Hacking (Toronto, Canada)
On the Contingency of Mathematics

Afternoon
Social activities
The PratiScienS group is supported by:

- Archives Henri Poincaré (Nancy, France)
- ANR (Agence Nationale de la Recherche)
- Région Lorraine
- MSH Lorraine (Maison des Sciences de l’Homme Lorraine – USR 3261)
- Nancy-Université
Abstracts

Catherine Allamel-Raffin & Jean-Luc Gangloff (Strasbourg, France)
catherine.allamelraffin@gersulp.u-strasbg.fr
jean-luc.gangloff@gersulp.u-strasbg.fr

Some Remarks about the Definitions of Contingentism and Inevitabilism
The debate between proponents of contingentism and those who advocate inevitabilism is relatively recent in philosophy of science. Its developments seem to be promising for the one who wishes to get rid of older debates such as the realism vs constructivism controversy. The aim of this paper is to discuss some issues related to the definitions of contingentism and inevitabilism, as they have been proposed by I. Hacking (1999, 2000) and L. Soler (2006, 2008).

Michel Bitbol (Paris, France)
michelbitbol@orange.fr

About the Contingency of the (Quasi-)Disappearance of Introspection in Psychology
Even before its extensive use in psychology during the turn of the nineteenth and twentieth century, introspection was criticized for reasons of principle. Later on, after a short-lived burst of work in this field, introspection came under such intense attacks, from behaviorists as well as from its own ranks, that it (apparently) disappeared. Psychologists overtly discarded it, even though they were unable to dispense completely with it in practice. In recent years, a strong movement of renewal, and redefinition, of introspection has been witnessed. One may then raise several questions of epistemological relevance about this renewal. What changed between nineteenth century introspection and current introspection? Are the conditions for a successful study of first-person experience now fulfilled? Was the eclipse of introspection unavoidable?

Mieke Boon (Twente, Netherlands)
M.Boon@gw.utwente.nl

Contingency and Conceptualization as the Fuel to Scientific Practices
According to Soler (2008, 222), a realistically framed formulation of the contingency thesis is that “Reality seems capable of sustaining more than one account given of it” (Shapin, 1982, 194). The question whether this contingency thesis is correct, that is, whether science could have been different, is hard to answer. At face value, it is a historical rather than a philosophical question. Analyzing historical scientific texts (e.g., Newton, Sadi Carnot, Maxwell) from perspectives presented in the history of science literature (e.g. Crombie, Chang, Kwa), makes plausible that the development of science is ‘limited and afforded’ by many different aspects that ‘contingently’ arise in its intellectual environment (differently put, its cultural and societal environment). Scientific practices develop in a contingent, ongoing exchange and mutual interaction with its intellectual environment. Scientists in other scientific fields produce facts, phenomena and other ‘epistemic results’ that lead to new scientific puzzles and possible solutions (e.g. ‘epistemic results’ from geology and paleontology were crucial to Darwin’s conceptualization of evolution by ‘variation and
selection’ – cf. Kwa, 2009). Cultural development of new concepts allowed for new ways of conceptualizing scientific problems and new ways of explaining the world (e.g., Deism as an alternative of Theism in the 17th-18th century allowed for notions such as ‘laws of nature’ and ‘order, harmony or unity in nature’ – cf. Zilsel, 1942; while notions such as ‘history’, ‘transformation’, ‘irreversibility’, that arose in the 18th century paved the ground for new notions such as ‘evolution’ crucial to Darwin’s work – cf. Kwa). Also, the more or less independent development of mathematics ‘afforded and limited’ the development of the natural sciences (e.g., non-Euclidean geometry, analytic geometry, calculus, statistics).

Given these insights gained from the history of science, the idea that science is not contingent is hard to maintain. Science could very well have been different indeed, and reality seems capable of sustaining more than one account. Even more so, ‘reality’, such as our physical environment, is contingent as well due to the contingency of technological developments. Craftsmen produce instruments and apparatus which produce artificial phenomena that initiate new scientific quests (e.g. the steam engine which initiated Sadi Carnot’s scientific work, which at some point resulted in thermodynamics – cf. Boon 2009). Therefore, scientific developments seem to be contingent in many different respects.

This raises the question whether philosophical inquiries into contingency could be fruitful or productive, in particular as to a better, more fruitful understanding of scientific practice. Revealing the contingency of science has been an important strategy for undermining scientific realism as well as the idea that science and scientific truth is the crown of the human intellectual quest. Sometimes, such implications of the contingency thesis also seem to suggest that science is not successful. This suggestion is not always productive for scientific practices, nor is it for the societal role of science.

From this perspective, my inquiry into contingency aims at developing an understanding of science – in particular of how to account for the quality of scientific practices – that may be more productive to these practices and their societal roles. My general idea is that considering the contingency of science opens a space for rethinking our conception of science and of how to understand and evaluate its quality and success. My question then becomes, whether, by taking seriously the contingent nature of scientific developments, we could articulate in new ways what makes science valuable and successful. In other words, instead of taking contingency as an unfortunate aspect of science (as it forces us to accept that the supposed success of science does not lie in theories or models that are true about, or correspond to the world), I propose to investigate what could be gained as to our understanding of science when assuming that some of the mentioned causes of contingency fuel the development of science. My thesis is that the development of science is partly driven by such contingent causes -- new apparatus and instruments, new phenomena, new concepts, new epistemic results and tools infuse into scientific practices from outside, from its intellectual and technological environment, and ‘afford and limit’ its further development.

My take will be that (besides the use and development of technological and scientific instruments by which utterly new phenomena are produced) the use and development of concepts is at the heart of scientific practices. In a recent paper, Joe Rouse (2009) argues against Van Fraassen’s (1980) image of science which holds that the ultimate aim of science is empirically adequate theories. Rouse argues in favor of a revised image of science that places conceptual articulation at the heart of the scientific enterprise. Rouse summarized this revised image in a particularly telling quote:

“Conceptual articulation enables us to entertain and express previously unthinkable thoughts, and to understand and talk about previously unarticulated aspects of the world.”
In my view, this revised image of science entails that (1) conceptualizations stand between ‘pictures of the world’ (which are supposedly presented in our theories and models) and ‘reality’; (2) conceptualizations ‘afford and limit’ thinking about the world, and without them the world remains ‘unthinkable’; (3) scientific knowledge presents human conceptions of what the world is like rather than ‘pictures of a ready-made world’; and (4) these conceptions are human constructions that somehow fit to our experiences of, and reasoning about a world that is materially or physically independent of us. Moreover, this image acknowledges (or even embraces) the idea that (5) scientific development is contingent since some of the causes of its development (e.g., ideas, concepts, knowledge coming from the environment and which afford conceptual articulation) are contingent. Nevertheless, these contingent causes fuel the development of scientific practices and are partly responsible for its productiveness.

In my contribution, I aim to expand on this revised image of science and scientific practices (as the activity of conceptual articulation that makes the world thinkable), and will explore whether this revised image is fruitful to a better understanding of the quality and productiveness of scientific practices.

Hasok Chang (London, UK)
h.chang@ucl.ac.uk

A Case for Scientific Pluralism

I outline various arguments for normative scientific pluralism: it is beneficial to have multiple systems of knowledge in each area of science. I provide a different set of arguments for each of the various possible views on the aims of science. If the main aim of science is taken to be Truth, the chief argument for pluralism is based on the unpredictability of scientific development: since we do not know which line of inquiry will be ultimately successful, it makes sense to cultivate various lines. If the main aim of science is empirical adequacy or understanding, there are further arguments for pluralism because different systems of knowledge can contribute to the aim in different ways. If we consider that science has various aims, then there are even further pluralist arguments. I close by indicating how history and philosophy of science can help put scientific pluralism into practice, by assisting with the proliferation of systems of knowledge.

Harry Collins (Cardiff, UK)
collinshm@Cardiff.ac.uk

What’s Special about Science?

We do know that in the long term, one scientific position will come to dominate over its competitors. This may be because in the long term the world will support only one view or it may be because in the long term society will only support one view. Remember that it is also the case that views about art, fashion, religion etc come and go and some have more durability than others.

The potentially answerable questions are:

1. Do multiple views shut down quicker and with more finality in science than in non-sciences?
2. Why might the resolution of scientific controversies be different to the resolution of other types of controversy?
(3) Given that whatever happens in the long term, in the short term the world does support multiple views, how can one use science in the short term to make sound decisions in the faster moving policy sphere?

I will offer some suggestions in respect of questions 2 and 3.

Catherine Dufour (Nancy, France)
catherine.dufour@lpm.u-nancy.fr

**Standard Quantum Mechanics versus Bohmian Mechanics: a Case for Contingentism?**

Standard quantum mechanics (SQM) and Bohmian mechanics (BM) are two theories that are both able to describe quantum phenomena. They are observationally equivalent and logically consistent. Both can be considered as successful since both reproduces the same phenomena. However, today, SQM is commonly considered by a lot of physicists as the only possibility and BM is ignored or misrepresented by the physics establishment.

On another hand, despite the fact that they are based on a common mathematical structure, SQM and BM can be considered as different theories since they are conceptually incompatible with each other. This situation permits us to consider that the debate SQM versus BM is a kind of a school-case to discuss contingentism and inevitabilism theses.

In this paper, I will first discuss arguments usually invoked to sustain the contingency thesis: political, sociological and historical arguments. I will argue that most of these extra-scientific reasons used to explain the supremacy of SQM are hard to verify explicitly; moreover to admit that they played a role does not lead necessarily to a contingency thesis (since, in the same time, one can think that scientific factors have played the major role). So extra-scientific reasons that are, at the first glance, a case for contingentism, are in fact not entirely convincing as a proof of a contingentist thesis.

Then, I will analyse what is viewed at a first glance as arguments for the inevitabilism thesis: I will present and discuss the common scientific criticisms against BM advanced by physicists. I will argue that none of these objections provide a rigorous disproof of BM. Thus, the so-called ‘scientific’ reasons generally invoked in order to sustain the inevitabilism thesis are in fact a better case than extra-scientific reason for contingentism.

Ronald Giere (Minnesota, USA)
giere@umn.edu

**Perspective, Contingency and History**

I will first offer a general statement of contingentism and its negation, inevitabilism. I will then outline my arguments for perspectivism and a path from perspectivism to contingentism. I go on to look at two areas of inquiry outside of science studies where questions of contingency are investigated: evolutionary biology and counterfactual history. This exploration suggests some new ways of thinking about the question of contingency in science.
Necessity and Contingency in the Discovery of Electron Diffraction

I will open this talk with a quotation from Gaston Bachelard:

"Vue dans sa perspective de rationalité, la découverte n'est plus vraiment contingente. La contingence des découvertes scientifiques n'est souvent qu'une optique d'ignorant. Les découvertes scientifiques viennent ainsi surpréndre ceux qui ne font pas l'effort de comprendre, ceux qui ne bénéficient pas de la tension de recherches qui anime la cité scientifique". (Le Matérialisme rationnel, p. 7).

That is: « From the perspective of its rationality a discovery is no more really contingent. The contingency of scientific discoveries is often the view of the ignorant. Discoveries hence come as a surprise only to those who do not make the effort to understand, -- here the translation necessarily miss the nice euphony of comprendre and surprendre typical of Bachelard's play on words -- those who do not benefit from the tension that animates research in the scientific city ».

As usual with Bachelard, this polemic sentence contains, condensed in a few words, interesting ideas on the question of contingency and inevitability that he does not develop in any detail but which can serve as our departure point. Bachelard suggests that appeal to contingency is often a superficial conclusion of those who do not really understand that scientific facts are often closely related to theory or paradigms and are thus called for as necessary even when they are observed (though not really known) before the theory. For a rationalist like Bachelard, the more science is developed in a rational structure through abstract concepts the more its consequences are necessary and the less the part played by contingency. Behind the superficial events of historical developments there is a coherent meaning constructed by the rational agent trained in a “scientific city” (a discipline).

Talking about contingency can be slippery and generate deaf dialogues if one does not indicate the scale at which the investigation is made. For viewed at the most micro and individual level, a lot of things seem (and often are) contingent. We should thus always define the scale on which we look at science before discussing contingency. In that sense just saying that « science is contingent » is much too vague to be useful and serve as a basis of discussion. Likewise, the question of contingency is not a moral one dedicated to determining whether contingency should be feared, regretted or applauded. The question, as Lena Soler has made it clear in her contributions on the subject, is analytical. What exactly does it mean to say that a given aspect of science is contingent?

Beyond the question of scale, another important aspect of the contingency question is related to the fact that even when basic properties of the world are contingent in the sense of not logically necessary as Descartes would have loved, a great many consequences of a contingent existence are necessary once the existence of a given property of the world is admitted.

Hence it is obvious that the properties of what we call electrons are contingent and only an empirical analysis of them could tell us what they can do and cannot do. One can make predictions but they are not necessarily borne out by observation.

In this talk, I will discuss these questions on the basis of a particular case study that of the discovery of the wave properties of the electron in the mid-1920s. In presenting the basic elements of that case, I will use them to try to clarify the meaning of contingency at various levels and which constraints may limit the spectrum of possible outcomes, including our very
though processes. I mean by these the fact that all humans trained to enter the dynamic of the scientific field -- I limit myself to that sub-set of humans simply because I do not want to open the door here to a discussion of the case of the so-called « primitive societies » and their supposedly « different logic » -- spontaneously apply schemes of thinking like the principle of sufficient reason and the principle of contradiction. Also implicit in all schemes of thinking are the Kantian antinomies: things are either continuous or discontinuous, finite or infinite, created or eternal, etc.

This talk will try to identify conceptual as well as technical reasons that contribute to explain why electron diffraction was observed in the mid-1920s and not before. If time permits, I will discuss the question whereas we could imagine a convincing history in which the electron was first conceived and detected as a wave before it was shown, roughly at the end of the 1890s, to be a particle through the work of physicists like Zeeman, Thomson and others.

This case study could help clarify the sense in which discoveries are “robust” and the extent to which the “contingency of science” is benign and lies more in the time sequence of discoveries and its associated possible irreversibilities (“lock-in”) than in any real “choice” of its intrinsic ontology.

Ian Hacking (Toronto, Canada)
ian.hacking@college-de-france.fr

On the Contingency of Mathematics

What counts as mathematics has varied enormously from the time of Eudoxus to the present. What we now call “mathematics” is the product of a highly contingent history. This has significant consequences for the philosophy of mathematics.

Jean-Marc Lévy-Leblond (Nice, France)
jmll@unice.fr

What if Einstein Had not Been There? A Gedanken Experiment in Science History

Suppose Einstein had not existed. How, in the early twentieth century, might our understanding of space-time physics have developed?

This paper proposes a reconstruction of history as it could have evolved, drawing on attested pre-Einsteinian works, and some little known post-Einsteinian ones, as well as introducing a few imaginary characters.

The virtual rise of Minkowskian chronogeometry will be reviewed, focusing on:

- the discovery of the inertia of energy and the subsequent construction of velocity-dependent formulas for energy and momentum (Von Dida 1909, Hunepierre 1909),
- the application of the Erlanger program to the construction of possible space-time structures and classification of chronogeometrical groups, from von Ignatowsky (1910, 1911), Franck and Rothe (1911,1912) to Blondevil and Prostov.

Some secondary sources will also be discussed (Zweistein, 1905).

Finally, the relevance of such an approach, on educational, epistemological and cultural grounds, will be highlighted.
Andrew Pickering (Exeter, UK)  
A.R.Pickering@exeter.ac.uk  

Science, Contingency and Ontology  
Extending the analysis that I developed in The Mangle of Practice, I argue for a contingentist understanding of the evolution of scientific culture while seeking to shift the argument from an epistemological to an ontological terrain. I discuss the ontological prejudices we transmit to our children, and seek to develop alternative ontological pictures that leave space for contingency. I first discuss a crystal world that can contingently split along a multiplicity of planes. I then develop a mangle-ish vision of the world as an endlessly lively place with which we engage via decentred, emergent and performative dances of agency. I argue that on this account contingency is not something to be feared or regretted; it is a necessary feature of how the world is. I also note that in science dances of agency have a peculiar structure in that they aim at their own extinction. This telos feeds back into our intuitions of necessity, but I argue that it does not, in fact, efface contingency. I end with the suggestion that this telos is itself contingent, that we are not compelled to structure our dances of agency around it, and that very different modes of being in the world are possible and even politically desirable.

Claude Rosental (Paris, France)  
claude.rosental@ehess.fr  

Is Logic Contingent? Reflections around the History a Logical Theorem  
To what extent can we say that logic, as a field of certified knowledge, is contingent? In order to address this issue, I will examine the process by which a logical theorem has been recently produced and certified by the scientific community.

Joseph Rouse (Middletown, USA)  
jrouse@wesleyan.edu  

Laws, Scientific Practice, and the Contingency/Inevitability Question  
The paper has two parts. The first part extends prior queries about how to interpret the nested counterfactual question of whether different investigations of the same scientific subject matter, if similarly successful, must converge upon the same (or compatible) results. Some construals of the counterfactual make the answer trivial (by defining success, sameness of subject matter, or other constitutive notions in ways that settle the question in a peremptory way). I begin by considering some further ways in which the contingency question might be reformulated through a shift of attention from scientific knowledge to scientific practice. Often this shift inverts the question by showing how even convergence upon similar results might nevertheless result in very different sciences. Among the possibilities I take up are a shift from a retrospective focus on results to a prospective focus on research; a consideration of different conceptions of scientific significance; a reflection upon the relative importance of high-level theoretical claims versus the instruments, skills, models, and experimental practices through which those claims are articulated and deployed; consideration of the extent and manner in which scientific practice extends beyond the laboratory; and the effect of acknowledging limits to the empirical success of current scientific understanding. The section concludes by posing the history of 20th C. reductionist research programs in the life sciences.
as a promising site for posing the contingency question in a more empirically tractable way. The partial retrospective vindication of earlier embryologists’ and German geneticists’ criticisms of classical genetics and the neo-Darwinian synthesis provides some empirical accessibility for the question of whether there could have been a significantly different historical trajectory that successfully dispensed with the classical gene concept.

The second part of the paper asks how the contingency issue is affected by serious consideration of the role of laws and nomological necessity in science. Many oft-cited scientific defenders of strongly inevitabilist positions (e.g., Weinberg, Glashow) explicitly base their view on the role of laws. The canonical framing of the issue nevertheless circumvents questions about laws and necessity by talking about “inevitability,” partly because the literature on nomological necessity has been troubled, with some prominent dissent over whether “laws” play an important role in scientific understanding. I argue that Marc Lange’s work on laws provides a more adequate conception of laws and nomological necessity, and shows how that conception is integral to scientific practice and understanding, even in fields (such as the life sciences and medicine) often thought to proceed without appeals to law. I then use Lange’s work to pose further issues about contingency. Lange commits us to one form of inevitabilism, since his work shows that different counter-factual histories could not legitimately agree in their “sub-nomic” claims while disagreeing about the laws. Yet Lange’s account of laws as indexed to disciplinary aims may open a space for contingency in the heartland of inevitabilism. The different ranges of counterfactual invariance in the laws in different sciences, and the different patterns of inductive salience that constitute empirically successful inductive strategies, may suggest that the sciences as they actually have been already demonstrate the possibility of alternative, empirically successful scientific practices.

Jean-Michel Salanskis (Paris, France)

In the first place, my paper deals with the inevitability issue in the context of mathematics.

Following that line, I discuss Cavaillès’ conception of mathematical becoming as autonomous, necessary and unpredictable. I analyze his way of making such claims, and how the mathematician Paul Levy answered him at a famous session of the French ‘Société des agrégés’, where Cavaillès was giving a talk together with Albert Lautman. Paul Levy argues very strongly in favour of inevitability, thinking he challenges Cavaillès, and takes the case of the invention of Lebesgue integral (by Lebesgue) as an example. I examine in a critical way Paul Levy’s position.

Then, I use the recent experience of non-standard methods in mathematical analysis for the epistemological debate about inevitability. Beginning with a quotation from Abraham Robinson, which seems to assert contingency, I evoke the search for the right framework for infinitesimal arguments, and more generally the ‘framework function’ in mathematics. I come to discuss the case of the ‘duck theory’, which maybe be analyzed as a technical result depending on some non-standard framework, and therefore in some sense avoidable. Meanwhile, I also discuss the relationship of any non-constructive mathematical framework with the core of strictly inevitable constructive mathematics.

In the second place, I offer general remarks concerning the landscape of the inevitability debate: 1) I try and formulate some implicit presupposition about historical human practice
which, maybe, underlies the inevitability thesis, connecting it with Hannah Arendt’s conception of action; 2) I reflect on the ‘put up or shut up’ argument; 3) I wonder whether we owe our inevitability thesis to Galileo.

In the third place, I attempt to say something about physics. I compare the idea of transcendental inevitability with the idea of inevitable results. And I suggest that the ‘framework function’ also plays for physics, which makes more reasonable and modest to refrain from any inevitabilist commitment.

To finish with: I don’t know at the moment which parts of this paper I’m actually going to expose, and in which order exactly. My apologies for English mistakes, also.

Léna Soler (Nancy, France)
l_soler@club-internet.fr

A General Structural Argument in Favor of the Contingency of Scientific Results

The aim of the talk is to fight against the inevitabilist instinct which, I think, is deeply active in each of us in a form or another – although depending on the individual sensibilities, different kinds of scientific ingredients might work as the most powerful attractors of inevitability.

Such a fight seems to me important for at least two reasons. First because under careful examination, behind the instinct, no strong argument seems able to support inevitabilism. At the end of the day, a non trivial form of contingentism appears more plausible. Second, because when analyzing what lies behind inevitabilist intuitions, we find very fundamental and pivotal commitments which seem inherent to the very idea of what we value as science and knowledge.

In the present talk I will try, inspired by Andrew Pickering’s work (1984, 1995) and some more circumscribed writings of Ian Hacking (1992), to articulate what I take to be the strongest and the most plausible general argument in favor of the contingency of scientific results.

The argument is grounded in an analysis of the way human knowledge is built. It is structurally akin to a Duhem-Quine thesis, because based on the holistic working of the dynamical elaboration and production of reliable scientific achievements. But a Duhem-Quine thesis of an “extended” kind (Hacking 1992), since the argument aims to take into account the multiple heterogeneous ingredients involved in scientific practices and the process of their evolving interactions through time. In that perspective, the holistic units involved in the argument will not be restricted, as in the classical presentations of the Duhem-Quine thesis, to sets of coherent propositions. The corresponding scientific “symbioses” (to borrow a word often used by Pickering and Hacking) will involve items of various heterogeneous types (possibly including material and instrumental resources, know-how and professional skills, local norms and standards guiding the production of results, short-term concrete feasibility…). Correlatively, the relations between these items, and in particular the kinds of ‘glue’ involved in their ‘fit together’, will not be restricted to logical coherence, but will be much more diversified.

In the framework of such a symbiotic, holistic and interactionist scheme of scientific development, the argument will be articulated relying on a particular historical episode, often picked out under the heading of the ‘discovery of weak neutral currents’ (WNC) in the 70s. This is a well-documented episode ([Pickering 1984]; [Galison 1983]…) which, for reasons
that will be articulated, is especially interesting with respect to the discussion of the contingency issue. As far as this historical example is concerned, the target of the contingency/inevitability issue is directed on a scientific ingredient that usually works as a strong attractor of inevitabilist intuitions, namely the epistemic kind of ‘experimental facts’. But beyond this specific historical case and the particular epistemic kind it exemplifies, the argument could, I think, be accommodated and applied to other ingredients of scientific practices that are candidate to inevitabilist intuitions.

The argument in favor of contingency passes through the establishment of the plausibility of the two following points.

(a) Multiple plays, at many different levels, are not only in-principle possible but moreover actually manifested all along the process of the co-maturations, mutual adjustments, co-constitutions and sometimes co-stabilizations of the various ingredients of scientific practices (in Pickering’s words: the “plasticity” of scientific practices and emergent scientific symbioses).

(b) Scientific achievements are genuinely path-dependent: what is done and decided at a point of the history of science matters in essential ways with respect to what is done and decided subsequently. For pragmatic reasons, practitioners have to decide at a point (say t), most of the time in situations where many doubts and obscurities subsist. At first sight this seems harmless with respect to the subsequent destiny (the possible reversibility at t’) of the decisions taken at t (for example: yes, there are WNC: a new phenomenon has been experimentally identified). What is important, according to the common fallibilist rhetoric, is the fact that what has been once stabilized is not a dogma and remains open to subsequent criticism and possible refutation ‘if needed’. But under examination, the determined experimental answer which, after a ‘sufficiently deep’ inquiry, is indeed crystallized at a given ‘conclusive moment’ and acquires the status of ‘the right answer’ (until further orders of course), is not at all indifferent with respect to the ulterior refutation/corroboration of the ‘taken-as-established-at-t’ phenomenon under scrutiny. Once a determined answer has been stabilized, if it matters, it will be used as such, detached of its initial justification, in multiple new investigations and will be involved in multiple new holistic equilibria. There will be a sort of ‘cascade process’, through which the ingredient will become more and more entrenched. So that if, at a subsequent time t’, ‘something does not fit in our science’, it will not be legitimate to reason ‘all thing being equals’, as if scientists were in the same position, with respect to the ‘refutation/corroboration’ of this taken-as-(of course provisory)-established-at-t phenomenon, as if an opposite answer (no WNC) had been given at t. Since the support for one ingredient is always given by a multiplicity of other co-present ones that mutually reinforce each other; since the decisions to modify this or that cluster of local ingredients are not independent of the degree of entrenchment of this ingredient; and since this degree of entrenchment in turn depends on the anterior historical decisions about its solidity/fragility, the path – the historical order of scientific actions, decisions and acquisitions – constitutively matters.

Fighting with the inevitabilist instinct and considering multiple prototypical kinds of inevitabilist replies all along the road, I will try to argue that all this gives credits to a non trivial form of contingency of scientific achievements, but does not questions the solidity of the co-stabilizations indeed achieved in the actual history of science.

The moral is that we should learn to make sense of the counter-intuitive idea of an alternative science which would be at the same time: (a) robust, efficient and progressive in the same sense we think our science is; but (b) radically different from ours at all levels, including experimental facts and commitments of practitioners about what is supposed to exist in the
world. I will attempt to make suggestions about the implications of all that with respect to very fundamental requirements that can be said to be ‘quasi-analytically’ attached to our present ideas and ideals about the scientific enterprise, such as the demand of unicity so deeply attached to our conception of sound knowledge.

A. Pickering

P. Galison

I. Hacking

Eran Tal (Toronto, Canada)
eran.tal@utoronto.ca

*Making Time: Stability and Contingency in the Maintenance of the Standard Second*

The standard second is currently the most precisely realized measurement unit in the *Système International d'Unités* (SI). Calculated from the results of about 300 atomic clocks around the globe, the stability of the standard second sets a practical higher limit on the precision of any dimensioned measurement in the physical sciences. This makes the standard second an important test-case for the stability of quantitative results in physics. In this paper I explore the sources and limits of the stability of the standard second and examine the relationship between stability and contingency.

Emiliano Trizio (Nancy, France)
emilianotrizio@hotmail.com

*Contingentism and Scientific Realism*

After a general analysis of the contingency issue and of the relevant definitions, I will argue that contingentism implies a basic multiplicity thesis, that is the claim that, given a certain subject matter, different and incompatible successful accounts of it are possible. Subsequently, I will analyze different ways in which the multiplicity thesis can be spelt out, leading to two distinct contingentist scenarios, one based on the notion of underdetermination and the other on the notion of robust fit. This analysis will allow developing some considerations on the plausibility of contingentism and on its possible consequences for scientific realism. In particular, I will consider structural realism as a representative of moderate versions of realism whose main feature consists in taking seriously the challenge of past theoretical change, and I will try to argue that contingentism constitutes a serious challenge also for these more refined realist positions.
Frédéric Wieber (Nancy, France)
frederic.wieber@wanadoo.fr

On Questions Raised by the ‘Contingency-Inevitability’ Debate about the Status of Historical Explanation

In this talk, I will argue that the contingency thesis is related with the new modes of making history of science that emerged around 1980 as part of the practical turn in science studies. Since the project of these new modes of making history of science is, in particular, to import in the study of science the methods and explanations of general history, I will propose a reading of some works in epistemology of history and in historiography which discuss the status of historical explanation. This reading will try to analyze the relations, in history, between explanation and contingency (of historical facts, of historical factors).