The Philosophy of Science in a European Perspective

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New Directions in the Philosophy of Science

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Preface

This volume, the fifth in the series *The Philosophy of Science in a European Perspective*, collects selected articles from presentations delivered at the three events organised in 2012 by the European Science Foundation Research Networking Programme PSE (The Philosophy of Science in a European Perspective): (1) the conference "New directions in the philosophy of science" held on October 17–20 at the Bertinoro Conference Centre of the University of Bologna; (2) the workshop "Causation, dispositions and probabilities in physics and biology" that took place on November 22–24 at the University of Lausanne, and (3) the workshop "Philosophy and the sciences – old visions, new directions" held on November 30–December 1 at the University of Cambridge, on the premises of CRASSH (Centre for Research in the Arts, Social Sciences and Humanities).

The Bertinoro conference resulted from the synergy of the five teams of researchers belonging to PSE, namely: Team A: "Formal methods" (leader Stephan Hartmann, co-leader Thomas Müller); Team B: "Philosophy of the natural and life sciences" (leader Marcel Weber, co-leader Hanne Andersen); Team C: "Philosophy of the cultural and social sciences" (leader Wenceslao J. Gonzalez, co-leader Amparo Gomez); Team D: "Philosophy of the physical sciences" (leader Dennis Dieks, co-leader Guido Bacciagaluppi); and Team E: "History of the philosophy of science" (leader Thomas Uebel, co-leader Michael Stoeltzner). Each of these teams organised one main session and one junior session, all revolving around the central topic that imprinted the research carried out by PSE in its fifth year of activity, namely "New directions in the philosophy of science".

The Lausanne workshop originated from a project of Michael Esfeld, member of PSE's Steering Committee, in close cooperation with the leaders of Teams B and D. The papers read there aimed at investigating possible links between biology and physics in connection with the notions of causality and dispositions, taken in a probabilistic fashion. While such notions play an important role in biology, it is unclear whether the same holds for physics. It turns out that focussing on these notions can shed light on still unexplored relations between these two major fields of research in the natural sciences.

The Cambridge workshop linked the newly-established CamPoS (Cambridge Philosophy of Science) research group to PSE, and was locally organised by Huw Price in collaboration with PSE's chairperson Maria Carla Galavotti. The workshop focussed on the relationship between Cambridge and Vienna in twentieth century philosophy of science, with the hope that this relationship will again come to play a major role in European and world philosophy of science in the twenty-first century. Six mini-symposia, each hosting two speakers, were held at the workshop, plus two junior sessions comprising four papers each.

Since all three events pointed in some way or other to new trends in the philosophy of science, with special emphasis on research carried out in Europe, it was decided to arrange the contributions collected in this volume in five sections, corresponding to the five PSE teams, irrespective of whether they were delivered in Bertinoro, Lausanne or Cambridge. However, it does not seem out of place to recall to which of the three conferences they originally belonged. The names of the authors are listed here in the order in which their contributions appear in this volume. The Bertinoro conference hosted the papers of Thomas Müller, Liesbeth De Mol, Patrick Suppes, Raffaella Campaner, Jeroen Van Bouwel, C. Kenneth Waters, Pierre-Luc Germain, Wolfgang Spohn, Matti Sintonen, Daniel Andler, Tarja Knuuttila, David-Hillel Ruben, Katarzyna Paprzycka, Obdulia Torres González, Chiara Ambrosio, Christopher A. Fuchs, Guido Bacciagaluppi, F.A. Muller, Miklós Rédei, Michał Marczyk and Leszek Wroński, Pablo Acuña, Ronnie Hermens, Petr Švarný, Huw Price, Massimo Ferrari, Thomas Uebel, Matthias Neuber, Uljana Feest, Sean Crawford, Anastasios Brenner, and Cristina Chimisso. The Lausanne workshop hosted Mark Colyvan, Tim Räz, Jan Faye, Jan Baedke, Max Urchs, Raphael Scholl, Cristian Saborido, Andreas Bartels and Daniel Wohlfarth, Mario Hubert and Roland Poellinger, Claus Beisbart, Radin Dardashti, Luke Glynn, Karim Thébault, Mathias Frisch, Gábor Hofer-Szabó, Dustin Lazarovici, Tomasz Placek, and Dennis Dieks. The Cambridge workshop hosted the papers of Kerry McKenzie, Veli-Pekka Parkkinen, Tim Lewens, Maria Carla Galavotti, Henrik Rydenfelt, and Friedrich Stadler.

This volume ideally represents PSE's point of arrival after five years of activity starting in 2008. The other volumes in the same series are: *The Present Situation in the Philosophy of Science* (proceedings of the conference held in Vienna, 18–20 December 2008), edited by Friedrich Stadler, Dennis Dieks, Wenceslao J. Gonzalez, Stephan Hartmann, Thomas Uebel and Marcel Weber, published in 2010; *Explanation, Prediction, and Confirmation* (proceedings of the workshops held in 2009), edited by Dennis Dieks, Wenceslao J. Gonzalez, Stephan Hartmann, Thomas Uebel and Marcel Weber, published in 2011; *Probabilities, Laws, and Structures* (proceedings of the workshops held in 2010), edited by Dennis Dieks, Wenceslao J. Gonzalez, Stephan Hartmann, Michael Stoeltzner and Marcel Weber, published in 2012; and *New Challenges to Philosophy of Science* (proceedings of the activities held in 2011), edited by Hanne Andersen, Dennis Dieks, Wenceslao J. Gonzalez, Thomas Uebel and Gregory Wheeler, published in 2013.

Having directed the ESF programme PSE from beginning to end, and as the principal editor of this volume, I am proud to say that together with the others the present volume reflects the vitality and originality of European philosophy of science. It is widely recognised that during the last five years the world scenario of philosophy of science has become more balanced, with a significant number of research groups and important events taking place in Europe. Without a doubt, PSE's activities and publications played a major role in this development. On behalf of the European community of philosophers of science, I wish to express our deep gratitude to the European Science Foundation for having supported our research in this field.

Bologna, Italy Maria Carla Galavotti

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Part I
Formal Methods

Things in Possible Experiments: Case-Intensional Logic as a Framework for Tracing Things from Case to Case

Thomas Müller

1 Introduction

Science needs modality. Science is about finding out what the world is like and what there is – but it is also about finding out what the world could be like and what there might be. While this may be controversial when taken as a metaphysically loaded claim about some ultimate picture of reality, it is just a simple descriptive truth when one takes actually practiced science into account. That practice is often not just about writing down what happened where and when, but about studying the things involved in such happenings, and finding out what could possibly happen with them. A large amount of the vocabulary used in the sciences is dispositional in nature, and while this may be more easily visible in the so-called special sciences like biology, it is also true of fundamental physics. One does not even need to focus on linguistic issues to see the importance of modality. Think of experiment, a crucial ingredient in modern science: experiments consist in the manipulation of the course of nature in the interest of scientific insight – in active intervention on what is happening, in order to bring about something else. The very notion of an experiment presupposes an acknowledgment of different possible courses of events. Experiments also involve the prevention of unwanted disturbances – vibrations, electrical fields, or variations of temperature, depending on the case – and shielding these off can be difficult and costly; so it is important to know which disturbances are possible and what their effect would be.

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Given that we have a need for modal notions in science, it is interesting to ask which kinds of modality are involved, and how we can best understand them. In this paper we will focus on one aspect of these questions: we will look at the *representation of things in modal contexts* occurring in science. We proceed from the assumption, not to be argued for here, that formal methods of philosophical logic are often helpful in elucidating philosophical problems arising in philosophy of science, and consequently we approach our topic from a logical point of view. We will, however, keep our formalism minimal.

The paper is structured as follows. We begin by describing the use of possible experiments in science and in everyday life (Sect. 2). This will make salient the notion of tracing a thing from a given case to other possible cases. In Sect. 3 we describe this notion in a more formal way, exhibiting some consequences for standard systems of quantified modal logic. In Sect. 4, we introduce CIFOL, case-intensional first order logic, as a newly established formal framework that helps to elucidate the notion of tracing. We wrap up in Sect. 5.

2 Possible Experiments

It seems best to start with an example to introduce our general topic. We will look at possible experiments that could be conducted to find out an object's trait, while we ascribe that trait in any case. We start with an object's charge. Charge is a quantitative property of microscopic as well as ordinary objects. Much of the chemical behavior of atoms is explained by their being made up of charged particles, and when you carry a sufficient amount of charge, it makes your hair stand on end. Charge appears to be quite a fundamental property – it is surely objective, mind-independent and categorical if anything is. Charge does not appear to be a disposition of an object like fragility or solubility. But still, it has modal force, and that may even be one of its defining features.

Consider Coulomb's law, which links the force F acting on a test charge of q situated at a distance r from an object, to the charge Q of that object:

$$F = \frac{1}{4\pi\epsilon_0}\frac{Qq}{r^2}. \qquad (1)$$

There are various ways to read this equation. One sensible way to read it is as a criterion of when the object we are interested in, let us call it α, has a charge of Q. Read in that way, the equation explains our epistemic access to an object's charge via an experiment, and we actually conduct such experiments in some cases: if we put a test charge of known charge q at a distance r from α, we can use Coulomb's law to determine α's charge. (Of course, in an actual application of this recipe, we will need to make sure there are no unwanted interferences – that is what laboratories are for. Read as a law, (1) has the familiar feature of holding only *ceteris paribus*. Also, there will have to be some way of determining the resulting force F from *its* effect, e.g., via the acceleration of a particle of known mass. We will leave these additional complications out of consideration.)

It seems sensible to say that for any charged object α, there should be some way of conducting an experiment of the mentioned type that will reveal α's charge. (Call this "modal verificationism" if you like. We will not argue for this doctrine's universal validity here, which would probably get us entangled in problems involving masking and similar effects. Never mind *that* for the time being – it seems sensible to work out the base case first.) Now, what is the link between a situation before us, in which we have identified an object α that we are interested in, and the mentioned merely possible experiment? It seems reasonable to spell out this link in terms of a counterfactual: if α, the thing before us, were tested in an experiment, the outcome would be such-and-such. (Again, leaving well-known problems aside.) In discussions of this approach, the focus is mostly on identifying the correct counterfactual case (or set of counterfactual cases) that needs to be considered. Certainly it will have to be one in which, contrary to what is happening to α in the case at hand, α is being subjected to an experiment of the mentioned type; the question then is what *else* also needs to change. (For example, I am in fact now writing this paper in a café. In a counterfactual situation in which I am now conducting the experiment, it must be assumed that I went to the lab instead of going to the café.)

In the identification of such a counterfactual case, one often invokes a notion of similarity of cases (or "possible worlds"), e.g., via a similarity ordering among cases. While many interesting issues are involved in developing the notion of similarity, we will not follow this line of investigation here. Rather, we will treat the more basic question of what we mean by re-identifying the object α, which we can identify directly (e.g., by pointing) in the case at hand, in some other possible case. It is certainly crucial that in another case such as the one involving the mentioned experiment, we keep our focus on the object, α, and do not switch to something else, since it is α's charge that we are interested in. It is also crucial that in following α, we treat it as a charged body, so that we incorporate equality of charge somehow into the adequacy condition for reidentification, and thereby make sure that we do not end up with an object that we may identify as α in *some* sense, but which will not do for identifying α's current charge.

Here is a more concrete example that brings out what is at stake. Consider my daughter's cat, Hannibal. I am interested in finding out his present mass, since I suspect that he is becoming a little chubby lately. Finding out his mass involves subjecting him to a sort of experiment, e.g., putting him on scales.[1] Now if it's really his exact *present* mass that I am interested in (suppose that I bet with my

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[1] Mach's definition of mass via a possible experiment, which does not involve a detour via an object's weight in a gravitational field, is analyzed by Bressan (1972), whose formal investigations are a main source of inspiration for the work presented here – see Sect. 4. Mach's definition is based on an experiment in which the object under consideration bumps into the unit mass at some specified speed. My daughter surely won't let *that* happen to her cat.

daughter that he is over 6 kg, and precision is required), I need to make sure that his mass does not change between the present case and a case in which he is put on the scales. Hannibal's present mass is not simply he mass of the cat, Hannibal, in a case in which he is put on scales. He was actually put on scales when he was a kitten, and weighed less than 2 kg then, but this obviously contributes nothing towards an answer to our question. Even starting out now, he might be put on scales after he has had some extra food, or shed some of his fur or otherwise lost some weight, which would lead to an incorrect answer. In order to get the right answer, it will not do to just follow the cat between the case at hand and some case with the cat on scales. Rather, I need to make sure that I am following the *massive object* that is identical to the cat right now, and subject *it* to the weighing experiment. What needs to be traced is the cat as a massive object, not the cat as a biological individual, since the property under consideration, mass, is one of massive objects and only derivatively one of biological individuals.

It is the same with charge. When we see the cat's hair stand on end and become interested in his current charge, it won't do to take him to the lab and bring some test particle close to him – chances are that in that case, his charge will already be quite different from what it was in the situation that is of interest. Some more elaborate scheme of tracing the charged object that is identical to the cat before me will have to be found.

Note that all of this does not mean that properties of the cat are "really nothing but" properties of the cat's matter. First, even in identifying the cat's mass or charge *now*, we do not need to trace the cat's current matter – that would be highly impractical, since a living cat constantly exchanges matter with his environment, and that matter quickly disperses all over the place, e.g., by diffusion. In the case of mass, it will be enough if we can trace the living cat and just make sure that the input/output mass balance is neutral,[2] and similarly for charge. Second, if we are interested in whether the cat has the property of responding to his name, we arguably need to trace the biological individual, the cat, to a case in which the appropriate experiment is conducted, i.e., in which he is called and responds – or doesn't.

It seems, then, that for various scientific and everyday purposes, it is important to understand how an object identified in a case at hand can be traced to other cases in which that object is involved in a possible experiment. In the remainder of this paper, we will look at formal means for the representation of such tracing.

3 Tracing in Standard Quantified Modal Logic

We have argued for the importance of the notion of tracing a thing between possible cases, or reidentifying a thing in another, merely possible case. From the formal-logical point of view that we are taking in this paper, this notion of tracing should be

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[2] This will obviously involve keeping the cat away from his food; depending on the precision required, ambient humidity and other factors may also have to be taken into account.

connected with the handling of variables and terms in a predicate logical framework that also allows one to talk about different possibilities. That is, we are entering the realm of quantified modal logic.

The construction of systems of quantified modal logic was approached syntactically by Barcan (1946), and later also semantically, most notably by Kripke (1959). Despite several refinements and much discussion, that latter framework can still serve to lay out the general idea behind most currently available systems of quantified modal logic. There are two main components. On the one hand, there is the handling of modality: the most common image is that of a set W of different possible worlds as modal alternatives – perhaps with an accessibility relation between them that grounds relational modal semantics, as in Kripke (1963). These possible worlds $w \in W$ provide a global view on modality; they correspond to complete alternative ways our world could be.[3] On the other hand, there is the handling of individuals: These are conceived of as inhabitants of the various possible worlds, so that each world w comes with its own domain of quantification, D_w. While there are important differences in the treatment of quantification in different systems, the common idea is that at a world w, a variable should have as its value some $d \in D_w$. (Arguments then arise, e.g., about the interrelation of the different D_w, or about the handling of reference failure.)

Based on this background, there are two main ideas for expressing the notion of tracing, or reidentifying, an object across different possible worlds. One idea, propounded by Kripke (1980), is that variables function as *rigid designators*, i.e., that they have the same value at each world. Another idea is to deny that different worlds can host the same individual, i.e., to deny so-called trans-world identity. On that approach, due to Lewis (1968), the worlds and domains need to be supplemented by a (perhaps context-dependent) *counterpart relation* that associates an inhabitant of a world w with its counterpart (or counterparts) in any given other world w'.[4] On both approaches, a metaphysical conviction settles details of the logical handling of variables.

In our view, both of these approaches are inadequate for capturing the phenomenon of tracing. Both either have a hard time handling the temporal aspect of modal alternatives in our examples, or make it difficult to represent the different, yet objectively grounded tracing principles of physics vs. biology that the examples point out.

The first problem is due to the image of possible worlds itself. In describing our examples, we were using the normal English idiom of possible cases, according to which necessity is truth in any case. Now "case" is not a technical term, and there are certainly useful applications of modal notions in which we consider world-spanning global possibilities, or possible worlds – e.g., when it comes to

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[3] Lewis (1986) famously proclaims that according to his doctrine of modal realism, every such way our world could be, is a way some world actually *is*. The theory of possible worlds is, however, independent of that doctrine.

[4] See Kracht and Kutz (2007) for a detailed exposition of the formal aspects.

models of cosmology. But cosmology is not a typical science: it has to do without experiments and to rely on observation instead. In everyday life and in sciences such as chemistry, biology and many branches of physics, we are much more interested in what is possible locally rather than globally. This is to be expected if science appeals to experiment, since we, the experimenters, bring about effects in the world locally, manipulating one thing (or a few things) at a time.

So, it seems necessary to allow for a more local notion of modality when trying to capture the notion of tracing – we do not normally trace objects from possible world to possible world, but from (local) case to (local) case. Such tracing has a temporal aspect. But the notion of rigid designation makes little sense when temporal cases are allowed. Certainly it needs to be possible to represent things as changing – but then, what does it mean that the value of a variable stays the same through different temporal cases, as rigid designation would demand? It seems that the image of a thing behind this approach is that of a bare particular, a mere peg to hang properties on that is devoid of any structure. We are not saying that it is not possible to build a quantified modal logic with rigid designator variables that handles the issue of temporal cases in some way – e.g., by representing change exclusively on the side of properties. But such a move seems awkward at best.

Counterpart theory seems to be better suited for tracing changing objects over time. After all, the counterpart relation can be *anything*. But that is exactly the problem with that framework. We would like to have a logic that, while allowing some sensible variation in tracing principles (as in the cat/massive object example of Sect. 2), also embodies at least some clear formal constraints on what can be a counterpart of what when moving from case to case.

We are not claiming that these problems provide decisive arguments against attempts of building quantified modal logic on a quantification theory involving rigid designation or counterpart theory. What we will do in the following, is rather to sketch an alternative, such that those interested in the issue may judge for themselves. This alternative, case-intensional first order logic (Belnap and Müller 2013a), based on Bressan (1972), avoids taking sides in metaphysical issues such as trans-world identity, and instead offers a metaphysically neutral framework that, on our view, really helps to elucidate the notion of tracing a thing from case to case.

4 Tracing in CIFOL: Case-Intensional First Order Logic

We will now introduce our preferred logical framework for tracing things across possible cases: case-intensional first order logic (CIFOL), described in detail in Belnap and Müller (2013a).[5] As mentioned, that system takes its main inspiration from the work of Bressan (1972), an Italian physicist who developed his complex

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[5] See also Belnap and Müller (2013b) for an extension to a system explicitly based on branching histories, which puts the general CIFOL machinery to work for a discussion of indeterminism.

modal-logical system ML^{ν} in order to better understand the modality involved in positing possible experiments in mechanics.[6] CIFOL is a first-order system of modal logic. Unlike Bressan's system, it does not allow for higher-order quantification over properties, relations or entities of higher types; its quantifiers are restricted to first-order entities, i.e., objects.

CIFOL's modality is based on a set Γ of cases, which do not have to be possible worlds, but may be temporal as well. For the basic system, nothing about Γ is assumed (except that it should not be trivial, i.e., it needs to have more than one member). Necessity is truth in all cases; formally:

$$\gamma \models \Box\phi \text{ iff for all } \gamma' \in \Gamma,\, \gamma' \models \phi.$$

Generally speaking, CIFOL is built on Carnap's method of extension and intension (Carnap 1947), which is applied universally to all parts of speech. Thus, each expression has an extension in each case, and an intension, which is the function from cases to the respective extensions. Formally:

$$ext_{\gamma}\xi = (int\,\xi)(\gamma); \quad int\,\xi = \lambda\gamma(ext_{\gamma}\xi).$$

For a sentence, ϕ, the extension in a case γ is a truth-value, **T** or **F**, so that instead of the "γ satisfies ϕ" locution, "$\gamma \models \phi$", we can also write "$ext_{\gamma}\phi = \mathbf{T}$". The intension of a sentence (a propositional intension) is then a function from the cases to truth values, representing the pattern of the extension of ϕ in case γ as γ is varied.

So much is standard in modal logic generally (even though it is often expressed differently). There are two features that set CIFOL apart and that allow for a useful analysis of tracing. First, the extension/intension method is applied to *all* terms, including variables (and even definite descriptions, but we will not go into that here). That is, a variable x has an intension, $int\,x$, and in each case γ, an extension, $ext_{\gamma}x$, that is a member of the extensional domain, D. Second, predication is generally intensional: whether a predication $\Theta(\alpha)$ is true or false in a case γ, need not be settled by the extension of α in γ alone. A predicate Θ therefore indicates, for each case γ, which *individual intensions* fall under it in that case. In contrast, standardly variables have just one constant extension (rigid designation), and predicates are extensional. There is another feature that strengthens CIFOL's expressive power: identity *is* extensional, i.e., whether two terms α and β are identical in a given case γ, only depends on these terms' extensions in that case, $ext_{\gamma}\alpha$ and $ext_{\gamma}\beta$. The semantic clause for identity statements is therefore:

$$\gamma \models \alpha = \beta \text{ iff } ext_{\gamma}\alpha = ext_{\gamma}\beta.$$

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[6] The expressive resources of ML^{ν} are comparable to those of Montague's more well-known IL (Montague 1973). One important advantage of Bressan's system is uniformity: ML^{ν} does not require explicit type conversions, in contradistinction to IL.

These technical choices allow for an interpretation of the formalism that is helpful for understanding tracing. The main idea is to effect a *Gestalt* switch with respect to the intensions of terms (individual intensions). In standard quantified modal logic, the *extension* of a term in a case is taken to indicate which *object* is designated by the term in that case. In CIFOL, on the other hand, the idea is that the object designated by a term corresponds to the term's *intension*.[7] A term's *extension* in a case is therefore not an object; in fact, no metaphysical interpretation of the extensions is needed at all. It is enough if they are there to do the technical work of grounding the truth or falsity of identity statements.[8] It is therefore a simple and metaphysically innocent thing to say, e.g., that the *cat*, Hannibal, *is identical*, in the case before us, to a *lump of matter*, without that being necessarily so. In fact, if the cat is identical to some lump of matter in one case, he will not be identical to that lump of matter once he has taken another breath and thereby exchanged some of his matter with his environment. No fancy doctrine of contingent identity is needed to model what is going on here: we simply have (extensional) identity in one case, but not in other cases.

It should now be clear that the tracing of an object across possible cases in CIFOL is effected, quite simply, by the intension of the term, which represents the object. (Or: which represents the object's extension varying from case to case.) This is already enough to show how CIFOL overcomes the problems that are caused by taking variables to be rigid designators in standard quantified modal logic: in CIFOL, variables have intensions, which may be regular object-intensions, and a change in an object from case to case can simply be modeled by a change of the corresponding extensions from case to case.

Now it may seem that by allowing for more generality in the notion of a case and in the handling of variables, we have in fact succumbed to the doctrine of counterparts after all – isn't the variation of extensions from case to case pretty much the same as a counterpart relation between the extensions? If there were no further constraints on the intensions representing objects, the two systems would indeed show some similarity (even though there would still be the important dissimilarity that CIFOL extensions do not represent things). CIFOL, however, provides crucial extra resources to help limit allowable means of tracing in a formally lucid way. These extra resources come as non-creative definitions that can be formulated within CIFOL, not as changes to CIFOL's logical system. (In our view, this counts

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[7] As variables are just terms, this interpretation accords with Quine's famous slogan that "to be is to be the value of a variable": the value of a variable is an intension, which represents an object.

[8] There is one special extension, denoted $*$, that is used to signal non-existence, as in failed definite descriptions; it is also useful to treat $*$ as a term always denoting $*$. In a temporal interpretation of CIFOL cases, it may be useful to think of the extensions as stages of objects, or as tropes. No matter which way; the important thing is *not* to think of these extensions as objects themselves. Technically, all that matters is the cardinality of the extensional domain D. It doesn't even matter whether the domains are taken to be case-relative or not. We work with the simpler choice of just one global extensional domain, D.

towards the usefulness of CIFOL as a *logic*.) The two main definitions are due to Bressan (1972), but CIFOL allows for some simplifications. The idea is to formulate constraints on those properties that can sensibly be taken to correspond to natural sortal properties (or substance sortals), by singling out certain first-order definable conditions on those properties.[9]

The first definition describes modal constancy: a property is *modally constant* iff an intension that falls under it in some case, falls under it in all cases. (Only a cat can be a cat; if something is possibly a cat, it is necessarily a cat.) This can be expressed as a condition on a predicate, Θ, as follows

$$\Box\forall x[\Diamond\Theta x \rightarrow \Box\Theta x].$$

The second condition is *modal separation*: it prescribes individual intensions falling under a sortal to be properly individuated, so that overlap of things of the same sort is precluded. (No two cats in exactly the same place.) Since we are dealing with sortals whose instances are contingent beings that can fail to exist in a case, we need to add a clause involving the existence predicate, E, so as not to forbid that two different things falling under the same sortal should fail to exist in the same case.[10] As a condition on a predicate, Θ, modal separation reads as follows:

$$\Box\forall x\forall y\,[(\Theta x \wedge \Theta y \wedge Ex \wedge Ey \wedge x = y) \rightarrow \Box(x = y)]$$

Putting these two definitions together, we can define a tracing property, or a *CIFOL sortal*, to be any predicate Θ that is both modally constant and modally separated.[11]

With all of this machinery in place, we can now explain CIFOL's approach to tracing things from case to case. As shown in detail in Belnap and Müller (2013a), a CIFOL sortal allows one to identify a thing falling under it, from the sortal and an extension in just one case. And typical natural sortals, such as "cat" or "lump of matter", which specify persistence conditions and a modal profile for the things falling under them, have the formal properties of CIFOL sortals.

Let us look at an example. No matter how you identify him in the case at hand (e.g., as my favourite pet, the black thing on the couch, or the best hunter on the block – all of which are case-relative descriptions), you can trace the cat, Hannibal, through all possible cases just by following the intension falling under the sortal, cat, that is identical to the extension of the identifying term in the given case. There

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[9] The aim is emphatically not to provide necessary and sufficient conditions for a property to be a natural sortal property – that seems hopeless, as it is a task belonging to science and metaphysics, not to logic.

[10] Existence can be defined in terms of the special term, $*$, that signifies non-existence: $Ex \leftrightarrow x \neq *$. See footnote 8 above; for details, see Belnap and Müller (2013a).

[11] Bressan (1972) calls such predicates "quasi-absolute", the "quasi" having to do with allowing for non-existence in a case. See also the previous note.

can be just one such cat-intension (by modal separation), and the property of being a cat applies to that intension in any case (modal constancy).

Note that it is not the case that the term, “my favorite pet”, falls under the sortal, cat, in the present case – that term has an intension that varies from case to case in a way that violates the persistence conditions of cats. It is true that all of my favorite pets have been and probably will be cats – but my favorite pet was first this cat and then that, and for a while I didn’t have any favorite pet; furthermore, who knows whether attempts of turning me into a dog person will not be effective, or whether I won’t settle for frogs in the end. “My favorite pet” does not designate a proper thing of any sort, its intension is a gerrymandered mess. But still, in the present case, the extension of “my favorite pet” is equal to the extension of “Hannibal”, and the latter is a name of a cat, falling under the sortal, “cat”. No matter how I identify him, given the sortal, I can trace the cat from case to case and find out about his properties in other cases.

So much for cats; the next step is to see how to link that with our discussion of possible experiments from Sect. 2. There it appeared crucial that we could trace the cat under different sortals as well – e.g., we could trace him as a massive object rather than as a biological individual. From a CIFOL point of view, we can say the following: there are other CIFOL sortals, “massiv object” and “lump of matter” among them, that also fulfill modal constancy and modal separation. In the case at hand, they do not apply to the term “Hannibal”, which is after all a name for a cat: its intension falls under the sortal “cat”, not “lump of matter” or such, and these sortals prescribe vastly different persistence conditions. (See our remarks in Sect. 2 about how hard it would be to trace the lump of matter that is a cat’s matter in one case, from one moment to the next.) These other sortals do however apply extensionally, meaning: there is a massive object, represented by an intension falling under the CIFOL-sortal “massive object”, that is, in the case at hand, identical to the cat before us – but that object is not identical to the cat in all cases. In fact, these objects come apart once the cat has taken one bite from his bowl. It is the same with the charged object identical to the cat, or with his matter: given a case, some identifying term gives us an extension, and that extension is enough, given an appropriate sortal, to identify an object of the respective sort, represented by an intension. In order to find out the cat’s mass now, you have to trace the massive object now identical to the cat, to some case in which an experiment is done from which you can read off the mass; it will not do to trace the cat. Note that both objects are readily available without any fancy stories about supervenience, metaphysical overlap or contingent identity. It is not necessary to introduce any special extensions for biological individuals either – the extensions can all be taken to be perfectly physical, whatever that means. (In fact, it seems better to abstain from any verdicts about their metaphysical character, since such a verdict is not needed in order to explain their systematic place in the framework.) The answer to the question about the cat’s mass is provided via a possible experiment – but that experiment needs to be done on the appropriate massive object, not on the biological individual, the cat.

5 Conclusion

We started out by following one specific use of modality in science: the link of an object's trait in a given case to a (merely possible) case in which an experiment is performed that attests to that trait. Charge or mass are generally considered to be unproblematic, categorical properties; we stuck to these in our examples so as not to become entangled in discussions about dispositions before any useful formal work could be done. We argued that the notion of tracing a thing from case to case is useful for framing the issue of possible experiments and the identity of the things involved in them.

In Sect. 3 we gave a brief overview of the standard approaches to tracing things, as represented in standard systems of quantified modal logic, and argued that they do not provide adequate elucidation of the notion of tracing. In the main Sect. 4, we presented, mostly informally, our own logical approach to the tracing of things, using the resources of the recent framework of case-intensional first order logic (Belnap and Müller 2013a). In that framework, one can specify necessary conditions for a property to be a sortal, and such sortal properties allow for tracing a thing from case to case. CIFOL's use of the method of extension and intension, together with case-relative identity, makes it possible to explain how we can trace the thing before us in a given case *both* as a cat and as a massive objects. Both means of tracing are important for some of our scientific purposes, and both find their appropriate formalization in the CIFOL framework.

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The Proof Is in the Process: A Preamble for a Philosophy of Computer-Assisted Mathematics

Liesbeth De Mol

> *Mechanization tends to emphasize practice rather than theory, deeds rather than words, explicit answers rather than existence statements, definitions that are formalized rather than behavioristic, local rather than global phenomena, the limited rather than the infinite, the concrete rather than the abstract, and one could almost say, the scientific rather than the artistic.*
>
> Lehmer (1966)

1 Introduction

Is the computer really affecting mathematics in some fundamental way? Despite the historical connection between mathematics and computers, research within philosophy, history and sociology of mathematics on this question has remained relatively limited.

The main philosophical issues discussed within this context are mostly related to the challenge posed by computer-assisted mathematics to more traditional accounts within the philosophy of mathematics, accounts which view mathematics as an a priori, non-empirical and purely deductive science that generates absolute knowledge through the progressive accumulation of theorems. Computer-assisted proofs of important theorems like the four color theorem by Appel and Haken or the use of "computer experiments" to e.g. give support to important mathematical conjectures *seem* to challenge the very idea of an infallible and a priori mathematics. In this sense, studies of CaM fit in well with the growing emphasis in recent years on

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mathematical practices.[1] However, not all authors agree on the role of the computer here. In fact it has been argued before that, if experiments exist at all in mathematics, the computer is not (Baker 2008, p. 343) "*an essential feature of experimental mathematics. There is experimental mathematics that makes no use of computers*."

In Avigad (2008) it is argued that some of the typical questions within the philosophical literature on computer-assisted mathematics are too vague. Examples of such questions are (*id*., pp. 3–4):

- In what sense do calculations and simulations provide "evidence" for mathematical hypotheses? Is it rational to act on such evidence?
- Does formal verification yield absolute, or near absolute, certainty? Is it worth the effort?

Instead such questions should be formulated "*in such a way that it is clear what types of analytic methods can have a bearing on the answers*." The task of the philosopher is then to study how these "*pre-theoretic [questions] push us to extent the traditional philosophy of mathematics in two ways: first, to develop theories of mathematical evidence, and second to develop theories of mathematical understanding*" (*id*., p. 5). Hence, we should study such pre-theoretic questions in their proper philosophical context. Furthermore, since "*none of the core issues are specific to the use of the computer* per se [. . .] *issues regarding the use of computers in mathematics are best understood in a broader epistemological context*" Avigad draws two important methodological conclusions from this:

> Ask not what the use of computers in mathematics can do for philosophy; ask what philosophy can do for the use of computers in mathematics [. . .]
>
> What we need now is *not* a philosophy of computers in mathematics; what we need is simply a better philosophy of mathematics.

This paper nicely sums up the general tenor of some of the recent philosophical literature on computer-assisted mathematics: the object under study are issues within the philosophy of mathematics which already have a tradition and, even though the computer raises some questions that challenge more traditional accounts of philosophy of mathematics, these issues are not really essential to the use of the computer.

Even though this approach of studying CaM in a broader philosophical framework is valuable, its insistence on viewing computer-assisted mathematics as something which doesn't really change anything fundamental and merely serves existing debates, runs the risk of underestimating the actual effect on practices of CaM.

A complementary approach which does take the practice of computer-assisted mathematics more seriously seems necessary in order to get a more balanced account of the impact of the computer on (the philosophy of) mathematics. This

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[1] See for example van Kerkhove and van Bendegem (2008).

has already been argued to some extent by van Kerkhove and van Bendegem (2008) where it is stated that we *should* account for the practices underpinning formal proofs, including the use of experimental methods (*id.*, p. 434):

> [I]t is clear that already today mathematicians rely on computers to warrant mathematical results, and work with conjectures that are only probable to a certain degree. Every so often, we get a glimpse of what is happening back stage, but what seems to be really required is not merely the idea that the front can only work if the whole of the theatre is taken into account, but also that, in order to understand what is happening front stage, an insight and understanding of the whole is required. If not, a deus ex machina will be permanently needed.

But what does it mean to take the machine more seriously? What does it mean to give an account of "the whole theatre"? Several approaches are possible but the one I propose is one which includes a study of the technical details underpinning a practice and, as such, is bottom-up. Within this approach the computer is regarded as a real medium in the sense of people like Friedrich Kittler and Martin Carlé (Carlé and Georgaki 2013):

> The entire impact of a technically informed media theory, from matters of the vowel alphabet all the way to the realm of digital signal processing, brings about one insight: that far more than ideas, it is the 'instrumentality' of thought or the means of communication which establish a dominant regime of knowledge, thus shaping historical reality and its associate notion of truth. Media are no tools. Far more than 'things at our disposal' they constitute the interaction of thinking and perception–mainly unconsciously.

An implication of this point of view is that our mathematical knowledge is really shaped by the machine. The problems that result from its usage *must* thus be regarded as specific to the use of the computer per se. More concretely this view results in a methodology that does not shy away from the "gory" details of the (history of) computer-assisted mathematics and takes the conditions, imposed by the computer on mathematics, more seriously. On the basis of an extensive analysis of CaM, the purpose of this approach is to detect which issues *are* inherent to the use of the computer and, on their basis, to detect how the practice of mathematics is or is not affected by the computer. Such an approach is sensitive to historical fluctuations and does not aim at providing a once-and-for all given answer to the question of the impact of the computer on mathematics.

2 Human-Computer Interactions, Time-Sensitivity and Internalization

In what follows I will focus on experimental mathematics and will thus not consider issues like on-line communities of collaborating mathematicians, the impact of typesetting software on mathematics, etc.

Experimental mathematics is understood here in the sense of number theorist and computer pioneer Derrick H. Lehmer. Lehmer identifies two "schools of thought" in mathematics (Lehmer 1966, p. 745):

> The most popular school now-a-days favors the extension of existing methods of proof to more general situations. This procedure tends to weaken hypothesis rather than to strengthen conclusions. It favors the proliferation of existence theorems and is psychologically comforting in that one is less likely to run across theorems one cannot prove. Under this regime mathematics would become an expanding universe of generality and abstraction, spreading out over a multi-dimensional featureless landscape in which every stone becomes a nugget by definition. Fortunately, there is a second school of thought. This school favors *exploration* [m.i.] as a means of discovery. [B]y more or less elaborate expeditions into the dark mathematical world one sometimes glimpses outlines of what appear to be mountains and one tries to beat a new path. [N]ew methods, not old ones are needed, but are wanting. Besides the frequent lack of success, the exploration procedure has other difficulties. One of these is distraction. One can find a small world of its own under every overturned stone.

For Lehmer it is exactly this possibility of exploration that opens up the path of "*mathematics [as] an experimental science*".

In my previous research I made several detailed case-studies throughout the history of computer-assisted "experimental mathematics" to understand on a more concrete basis the impact of the computer on mathematics. These studies show very clearly the significance of technological advances in computer science (hardware, software and theoretical) for the way experiments are set-up, the types of methods that are developed and the way they are interpreted (see e.g. Bullynck and De Mol 2010; De Mol 2011): in fact, the short history of computer-assisted experimental mathematics itself already underwent important changes due to e.g. increase in computing speed, more efficient read and write operations, developments in programming etc. It are exactly these technological changes that are specific to the use of the computer and allow to trace characteristics of practices of experimental mathematics that come to the fore because of these technological conditions. These characteristics allow to partially explain the increasing popularity of so-called experimental mathematics (see Sect. 2.3).

2.1 Mathematician-Computer Interactions

If there is one characteristic inherent to the use of the computer per se, it is the interaction with the machine. Of course, there is a long history in mathematics of interactions between mathematicians and non-human instruments. The most frequently used is the pen-and-paper method: writing on a piece of paper, a blackboard etc.[2] Figure 1 illustrates the interactive feedback process of such writing practices. Evidently, such interactions are processual – one does not just have one interaction with the piece of paper but many while developing e.g. some idea for a proof or writing a result down to be communicated to the mathematical community.

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[2] In this paper I do not consider earlier uses of mechanical devices within mathematics (for instance, Hartree's differential analyzer). These were much less frequently used than digital computers. A comparative study of such devices would be very interesting. It might be that some of the characteristics studied in this paper might also apply to some extent to these earlier devices.