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How (not) to think about idealisation and *ceteris paribus*-laws

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Abstract *Semantic dispositionalism* is the theory that a speaker's meaning something by a given linguistic symbol is determined by her dispositions to use the symbol in a certain way. According to an objection by Kripke, further elaborated in Kusch (Analysis 65(2):156–163, 2005), semantic dispositionalism involves *ceteris paribus*-clauses and idealisations, such as unbounded memory, that deviate from standard scientific methodology. I argue that Kusch misrepresents both *ceteris paribus*-laws and idealisation, neither of which factually *approximate* the behaviour of agents or the course of events, but, rather, identify and isolate nature's component parts and processes. An analysis of current results in cognitive psychology vindicates the idealisations involved and certain counterfactual assumptions in science generally. In particular, results suggest that there can be causal continuity between the dispositional structure of actual objects and that of highly idealised objects. I conclude by suggesting that we can assimilate *ceteris paribus*-laws with disposition ascriptions insofar as they involve identical idealising assumptions.

Keywords Semantic dispositionalism · *Ceteris paribus*-laws · Idealisation · Dispositions · Kripke's Wittgenstein · Kripke · Fodor · Kusch

1 Introduction

According to reductive semantic dispositionalism, a speaker's meaning something by a given linguistic symbol is determined by the speaker's dispositions to use the symbol in a certain way, in particular by her dispositions to respond in a certain way to

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queries involving the symbol. Kusch (2005) uses a well-known example about what is involved in meaning plus by '+', drawn from Saul Kripke:

> It comes down to this: 'It is a fact about me that, when faced with the query "$57 + 68 = ?$", I am disposed, *ceteris paribus*, to answer "125." This shows that by the symbol "+" I mean the addition function' (Kusch 2005, p. 156; cf. Kripke 1982, p. 26).

The *ceteris paribus*-clause in this statement is intended to accommodate the fact that speakers are of course not always in the mood to volunteer answers to questions put to them. Moreover, they are also finite beings, whose actual capacities don't span the entire infinite set of ordered triples which constitutes the addition function, and they suffer multifarious other limitations that stem from the vagaries of physical existence. To cope with this 'problem of finitude', semantic dispositionalism needs to construe the *ceteris paribus*-clause in a way that invokes idealisations.

Kripke ironicises about the amount of science fiction and fantasy this appears to involve:

> (...) how should we flesh out the *ceteris paribus* clause? Perhaps something like: if my brain had been stuffed with sufficient extra matter to grasp large enough numbers, and if it were given enough capacity to perform such a large addition, and if my life (in a healthy state) were prolonged enough, then given an addition problem involving two large numbers, m and n, I would respond with their sum (...). But how can we have any confidence of this? How in the world can I tell what would happen if my brain were stuffed with extra brain matter, or if my life were prolonged by some magic elixir? Surely such speculation should be left to science fiction writers and futurologists (Kripke 1982, p. 27).

Kusch (2005) (and again in a recent monograph, Kusch 2006) endorses Kripke's misgivings about the viability of such an approach, and sets out to further explicate and justify them. The foil for his arguments is a brief discussion in Fodor (1990), where the latter does endorse—at least for logical vocabulary—a reductive dispositionalist reconstruction of meaning. Fodor has no qualms to hedge his explanation of what it is to mean *plus* by '+' with a *ceteris paribus* clause that involves the assumption of unbounded working memory, for he thinks that there is nothing wrong generally with assuming idealised counterfactual conditions in the specification of a law—science does it all the time (op. cit., pp. 94–95, 111). Supposing that appeal to the authority of science is a legitimate move in this context, the bone of contention between Kusch and Fodor is whether the latter's use of the *ceteris paribus*-clause and of idealisation sufficiently approximates that of the sciences.

This question carries wider interest than merely an answer to the arcane matter of whether I have finite or infinite adding dispositions, or indeed whether semantic dispositionalism is plausible for logical vocabulary. For it sheds useful light on the very notion of a *ceteris paribus*-law of nature and the practice of idealisation in science generally. I shall argue that Kusch misrepresents both *ceteris paribus*-laws and scientific idealisation, the function of neither of which is always to *approximate* the behaviour of agents or the course of events. A better way to think about idealisations and *ceteris paribus*-laws is as acts of theorising with the purpose of identifying and isolating nature's true component parts and processes—i.e. as attempts to "carve nature at the joints." A reconstruction of idealisation and *ceteris paribus*-laws along these lines vindicates semantic dispositionalism and counterfactual assumptions in science generally. Moreover, it assimilates *ceteris paribus*-laws with disposition-ascriptions, in so far as both of these involve similar idealising assumptions.

The plan of the paper is as follows: Section 2 begins with a brief presentation of Kusch's account of legitimate idealisation in science. I argue that none of the proposed hallmarks of proper idealisation are acceptable, because Kusch fundamentally misrepresents the function of idealisation. Section 3 finds a similar problem with Kusch's discussion of *ceteris paribus*-laws: his reconstruction of hedged law-like generalisations as referring to the action of single dispositions or forces suffers from the same defect of construing *ceteris paribus*-laws descriptively, as approximate representations of reality. In Sect. 4 I take a closer look at the current state of play in the science of mental arithmetic, to show that disposition-ascriptions are not approximations and that Kripke's qualms about too much "science fiction" in idealising to unbounded memory are misplaced. Section 5 continues the interpretation of the empirical data to argue that there can be causal continuity between the dispositional structure of actual objects and that of highly idealised objects. I close the paper with general remarks on where this leaves us with the question of the ontological reality of dispositions, and *ceteris paribus*-laws.

2 Legitimate vs. illegitimate idealisation

Jerry Fodor believes that

> If we did have unbounded memory, then, *ceteris paribus*, we would be able to compute the value of $m + n$ for arbitrary m and n (Fodor 1990, p. 95).

He defends the idealisation to unbounded memory with an argument by analogy: other idealisations such as perfectly elastic molecules, friction-less planes and rigid levers, rational agents, etc., are deployed with explanatory and predictive success throughout the physical and social sciences. In none of these cases do we know all the counterfactual truths entailed by the idealisation. Perfectly elastic collisions, for example, require a world in which there are no attractive forces between molecules, i.e. a world without electrical forces. We are nowhere near an understanding of what a universe without electrical forces might look like, and yet laws invoking elastic molecules are widely accepted as true laws of nature. Therefore, the fact that we don't know what would happen if brains really had unbounded memory is hardly an objection to idealising to unbounded working memory (ibid.).

Kusch chooses not to directly confront Fodor on the point he makes, but rather employs a further argument by analogy. He accepts the Ideal Gas Law ($PV = nRT$) as an instance of legitimate and successful scientific idealisation, and contrasts some of its features with Fodor's putative dispositionalist law. Idealising to unbounded working memory is not the same as idealising to point-sized molecules without attractive forces, because the Ideal Gas Law displays three features characteristic of all idealised laws, which the dispositionalist law lacks: (1) it is part of a system of laws, (2) the idealisations it deploys are an approximation of the real world, and (3) it allows 'de-idealisation' for predictive purposes (op. cit., p. 159). Thus, the Ideal Gas Law is an integral part of the Kinetic Molecular Theory of Gases. It represents an approximation of the behaviour of real gases, insofar as we can experimentally approximate the values predicted by the law. Finally, by 'de-idealising', or removing some of the ideal conditions over which the law is defined (e.g. by factoring in the electrical attraction between real molecules, as in van der Waals' equation), we can increase its predictive accuracy.

None of these three things are true, according to Kusch, of 'If we did have unbounded memory, then, *ceteris paribus*, we would be able to compute the value of $m + n$ for arbitrary m and n'. (1) This generalisation does not belong to any established body of theory (we're not even sure which science it might belong to). (2) The law's predictions cannot be experimentally approximated, for the addition function is defined over numbers so enormous, that they would dwarf even the number of atoms in the universe to the power of the number of atoms in the universe. Finally, (3) it is not clear for the same reason what a de-idealisation of the law would plausibly look like (ibid.).

Kusch's three criteria for idealised laws may be initially plausible, and yet they amount to an oversimplification and misunderstanding of the nature of idealisation. To begin with, Kusch is quite right to point out that the Ideal Gas Law is part of an established body of laws, whereas Fodor's putative semantic dispositionalist law does not seem to be. Yet, it would be far too stifling for scientific progress to reject a law-candidate on the sole ground that it involves a novel type of idealisation, or covers new ground. It may well be correct that all true laws of nature must be part of systems of laws,[1] however these systems of laws or bodies of theory cannot be expected to be known in their entirety at the time of discovery, or to always be discovered *en bloc*. In other words, any requirement that the relevant body of theory be actual—in the sense of being currently known and put forward by actual scientists—is too strong. We can hence dismiss Kusch's first criterion.

His second and third criterion, on the other hand, show that his notion of idealisation is exclusively of a type which some writers call 'Galilean idealisation' (McMullin 1985, p. 265). Galileo Galilee inaugurated experimental science by verifying his law of Free Fall through calculating its predictions for a perfectly round ball rolling down a perfectly smooth inclined plane, and then measuring the behaviour of hard bronze balls rolling down straight and smooth wooden grooves. Galileo's assumption was that the smoother and harder the ball and the plane on which it rolls—i.e. the more experimental conditions are "idealised" by reducing external causal factors such as friction—the more measurements will approach the values predicted by the law.

In his wake, the difference between the idealised and the actually observed came to be seen as one of *degree*, and idealisation as a kind of approximation: an act of theorising whereby we produce simple, approximately true theories or laws such that, were we to replace the idealised description of the conditions over which the law is defined with a more complex, realistic description, the resulting law would bring us

[1] Fodor seems to actually share that opinion, cf. Fodor (1990, p. 27).

closer to the truth. On this 'traditional' view of idealisation, we would expect generally that predictions derived from an idealised law converge with experimental results if (a) experimental conditions are improved to more closely resemble the initial conditions of the law (as in Galileo's case), or (b) the idealised law is relaxed, made more complex and thus more realistic ('de-idealised' as Kusch puts it) (Liu 1999, p. 239).[2]

Alas, things are not as straightforward as this. The central difficulty for the traditional view is vividly described in the following example by Laymon, who invites the reader to imagine a scenario in which

> . . . there are one hundred forces, f_1 through f_{100}, all aligned along a common axis, operating on some particle. And let the first ninety-nine be of equal magnitude but alternating direction, and the remaining force be twice the magnitude of the others. Now imagine that scientist S arrives on the scene, gradually learns of the existence of the forces in the sequence 1, 2, . . . f_i, f_{i+1}, and applies standard Newtonian theory, that the acceleration of the particle will be equal to the product of its mass and the vectorial sum of the forces. . . . The resultant summations can naturally be described as becoming increasingly more realistic and less idealised Laymon (1998, Sect. 3).

And yet, gradually discovering and taking into account the existence of forces $f_1 \ldots f_{99}$—in other words gradually increasing the complexity of the model and decreasing its degree of idealisation—will *not* lead to monotonic convergence with the experimental results concerning the true acceleration of the particle. Rather, 'convergence occurs all at once' when S has finally hit upon a fully adequate description of all forces present.

The example shows that we cannot let the acceptability of a given scientific idealisation hinge on Kusch's third criterion. It is not true in general that we must be able 'to increase the predictive accuracy of [our] theories by removing idealisations' (Kusch 2005, p. 159), e.g. by *adding* the effect of interfering causal factors, because the replacement of an idealised description in our law or theory with a more precise ("realistic") one does not in principle guarantee asymptotically more approximate predictions. Neither can we accept Kusch's second criterion: it is not true generally that all idealisations are approximations of observed phenomena, or indeed that they are intended as such. Scientific idealisations are, as Liu (2004a) notes, a rather colourful bunch, made for multiple purposes and applications to each of which corresponds its own kind of reasoning:

> . . . there are different types of idealisations, of which approximation production is only one type—e.g. those idealisations one depends on in finding generalisations in a collection of experimental data. But this is not the same kind of reasoning as the one which assumes that the universe contains only the sun and the earth or is without electromagnetic field. Nor are both . . . the same as the construction of lattice models for bulk matter and for quantum fields. From

[2] Cf. also Liu (2004a), Laymon (1985, 1987), Elgin and Sober (2002). Other writers call the process of de-idealisation, in other words the gradual relaxation of an idealising assumption, 'concretisation' (Nowak 1980; Cartwright 1989).

> the perspective of approximation production, we simply do not see anything in common in these different acts of idealisations. We must therefore begin afresh (Liu 2004a, p. 240).

The fresh beginning begins with the observation that some scientific idealisations are undertaken not with the aim to be descriptive of the evidence, but with the goal of "carving nature at its joints" (Liu 2004b, p. 366).

This is a crucial observation in our context. There is a type of scientific idealisation whose mode of reasoning is ontological and qualitative, rather than empirical and quantitative—it is a type of theorising whose goal is the separation (in the mind) of nature into what are assumed to be its true component parts and processes. Such a type of idealisation will often lead to statements which are inferior in their ability to approximate and predict the observed course of events, and yet are superior *qua* law-statements. Liu illustrates the idea with the two equations '$F = G(mM)/r^2 + a$' and '$F = G(mM)r^2 + b\ sin(\omega r)$'. If $|b| < |a|$ and ω is a frequency term, then the latter equation will provide a better *factual* approximation of the gravitational force between bodies m and M; and yet, the former will of course be closer to the actual *form* of Newton's Law of Gravity, and thus will be seen to better represent the underlying structure of reality *qua* law (Liu 2004b, p. 365).[3] One of the primary goals of data-modelling and curve-fitting techniques is precisely to find the right kind of compromise between closeness-of-fit to the data, and appropriately law-like hypotheses.

Recall now Fodor's putative law: 'If we did have unbounded memory, then, *ceteris paribus*, we would be able to compute the value of $m + n$ for arbitrary m and n'. It is quite plausible to think that its focus on the pure dispositional state of being able to add, and its attempt to separate that state from an important interfering factor that enters the fray when the subject actually attempts to add, is a paradigmatic act of isolating the true component parts and processes at work in nature (on a par with, say, decomposing a total force into its component vector forces). The relevant idealisation would thus seem to be a justified one. I shall discuss this question in more detail in a moment. For the time being it will suffice to note that in view of the above, faulting Fodor's law for its failure to approximate what happens as speakers attempt to add increasingly large numbers hardly constitutes a lethal objection to the practice of idealising to unbounded memory.

3 Acceptable vs. unacceptable *ceteris paribus*-laws

Kusch's criticism of the idealisations involved in Fodor's semantic dispositionalist law, if successful, should have been sufficient to bury it. Kusch envisages Fodor objecting, however, that the significant feature of his law is not so much idealisation, but rather the use of a *ceteris paribus*-clause. Given that *ceteris paribus*-generalisations are used widely in psychology and the special sciences at large, why should semantic dispositionalism not be allowed to avail itself of a similar hedging clause? After expressing a few customary doubts about *ceteris paribus*-laws (for more on which, see

[3] Assuming for the purposes of this example that we are indeed in a Newtonian universe. A similar, though perhaps less perspicuous, example could be constructed with the correct relativistic field equations.

infra), Kusch's strategy is, again, to establish a disanalogy between the scientifically acceptable use of the *ceteris paribus*-clause, and Fodor's unacceptable one. Thus, he argues that the role of the *ceteris paribus*-clause hedging, say, the Müller-Lyer Law in cognitive psychology, is not the same as in Fodor's semantic dispositionalist laws. In the process Kusch provides a general reconstruction of *ceteris paribus*-laws which is as interesting in its failures as in its successes.

Kusch endorses a view first mooted by Mott (1992) that hedged laws in cognitive science—and presumably, elsewhere as well—are nothing but 'descriptions of experiments that sometimes fail' (Kusch 2005, p. 161; cf. Mott 1992, p. 340). Experiments can fail because of (a) mistakes in the experimental set-up, (b) random interference, or (c) genuine exceptions that cause systematic and reproducible failures of the law—for instance, clearly identifiable groups of humans for which the Müller-Lyer law is not true. The standard scientific use of the *ceteris paribus*-clause, according to Kusch, is to protect laws from exceptions (a) and (b), but not from (c). The difference between (b) and (c) is essential: whereas in the case of (b), we have a small number of unusual failures due to an irregular range of chance factors which are not understood and not reproducible by the experimenter, (c) represents the type of massive, systematic, and reproducible breakdown of the law (which Kuhn would have labelled an 'anomaly') that requires us to revise the law (op. cit., p. 161).

Fodor's dispositionalist generalisations, by contrast, do not seem to be about established experiments in cognitive science. Moreover, a law-like generalisation such as

> "*Ceteris paribus*, when a human has the disposition to token "lo, a horse" then there is a horse in his or her vicinity" (Kusch 2005, p. 161; cf. Fodor 1990, pp. 89–136).

does encounter an infinite number of systematic and reproducible failures. After all, there are an infinite number of possible constellations of beliefs, whose presence would lead the believer to believe that there is a horse even when there is none in the vicinity. Generally, it seems that the *ceteris paribus*-laws of semantic dispositionalism encounter an infinite number of systematic failures, and stand in need of an infinite number of refinements.

Unfortunately, this reconstruction of the role of the *ceteris paribus*-clause is wide off the mark, which Kusch acknowledges. The problem is that most psychological laws are not cognitively impenetrable, i.e. they are susceptible to systematic and reproducible disruption from the subject's particular set of beliefs. In fact, like the purported laws of semantic dispositionalism, most of the *ceteris paribus*-generalisations of the social sciences suffer from a vast number of non-random exceptions. Perhaps this is because at the very heart of our explanation and prediction of human behaviour is the generalisation that 'if X desires A and believes that doing B is the best means for obtaining A, then X will try to do B' (Schiffer 1991, p. 2). It is immediately obvious that this hypothesis suffers from exceptions as numerous as the number of possible belief-desire combinations which could conceivably stop X from trying to do B, exceptions that are perfectly systematic and reproducible. Therefore criterion (c) would seem to rule out most *ceteris paribus*-laws in the special sciences, and it would not single out Fodor's.

To deal with this problem, Kusch proposes a link between *ceteris paribus*-laws and disposition-ascriptions: we can, he says, think of a *ceteris paribus*-law as a description of the action of a *single* force or disposition, even if that disposition rarely or never acts alone. Moreover, even if we are unable to identify every possible source of interference, we can often make justified claims about the nature and existence of the disposition (Kusch 2005, p. 163; the same claim has been made by Lipton 1999 and many others). Although a given law may encounter systematic and reproducible failures, we can accept it if we are justified to believe that the law describes the action of a single isolated disposition or force. Having stated this view, Kusch rather abruptly comes to a conclusion: psychologists are rightly confident that the Müller-Lyer law describes existing dispositions, but we ought not feel equally confident about our disposition to add, 'for all the familiar reasons' (ibid.). These reasons are, the reader will have guessed, Kripke's qualms about too much "science fiction" in science: why, asks Kusch, 'should we be at all confident that my disposition to add is identical with the disposition regarding plus-queries that I would have if my brain were the size of a universe?' (ibid.) There can hardly be any causal continuity between my current rather modestly sized thinking organ, and such a stupendously fantastical entity.

Many a philosopher of a certain persuasion will fail to be swayed by Kusch's treatment of *ceteris paribus*-laws as disposition-ascriptions. To her, the best way to think of *ceteris paribus*-clauses is the one Kusch briefly mentions, but then passes over: the special sciences do not discover any genuine *ceteris paribus*-laws and the latter do not describe any dispositional properties, because *there is no such thing* as *a* ceteris paribus-*law of nature*. On this theory, most recently defended by Earman and Roberts (1999) and Earman et al. (2002), the only coherent way to understand law statements is the time-honoured one as universally quantified propositions describing strict nomic regularities between events or property instantiations. For we not only currently lack a satisfactory account of the contribution of the *ceteris paribus*-clause to the truth-conditions of a hypothesis, the vagueness of the clause also prevents *ceteris paribus*-laws from being confirmed by their instances, and renders them strictly untestable (Earman et al. 2002, p. 293). If this is correct, then Kripke's and Kusch's use of the *ceteris paribus*-clause is nothing but a fudge: given that no actual person is such that she realizes the requisite conditions, no one can add the enormous numbers involved; there would therefore be no (strict) law that could possibly be satisfied by anyone regarding those numbers. Regularities without instances ("empty regularities") are not regularities at all, however, let alone laws—and this is not helped by propping them up with the words 'everything else being equal.'

As a corollary of such scepticism about *ceteris paribus*-laws we can add scepticism about dispositions. A disposition is a property or state which provides for the possibility of another state or event, under specific conditions. It is therefore natural to analyse dispositions conditionally, and even staunch defenders of their ontological reality admit that dispositions are non-accidentally linked to the truth of certain counterfactual conditionals (Martin 1994; Mumford 1998; Mellor 2000). This raises a host of well-known and difficult questions, both semantic, metaphysical, and methodological: are the truth-makers of disposition-ascriptions the same as the truth-makers of those counterfactual conditionals? (If not, what are they?) Can dispositional properties exist without non-dispositional, or 'categorical', properties, which are the truth-makers of those counterfactuals and which "ground" dispositions? Can dispositions be causes, or do they require a 'causal basis' provided by those categorical properties? And finally, are dispositions required to account for scientific practice? Dispositions are not directly observable; are they nevertheless the proper *relata* of scientific laws, and if so, how are we to harmonise their epistemology with their metaphysics?

Sceptics about dispositions are, unsurprisingly, often also sceptics about *ceteris paribus*-laws. They tend to be philosophers of an empiricist bent, with a preference for a Humean view of laws as statements summarising strict but contingent regularities between causally inert particulars (events or property instantiations), and a pronounced dislike of necessary connections, modal properties, capacities, and powers. In a Humean world, there is no need for *ceteris paribus*-laws that are not reducible to strict laws, because the *ceteris paribus*-clause can be seen as simply shorthand for the indeterminate and largely unknown set of further nomic regularities interfering at any moment with a given regularity. The world, after all, is a messy place, and the '*ceteris paribus*'-phrase is a handy heuristic device. Alternatively, we can see the *ceteris paribus*-clause in front of a statement pragmatically, as 'an elliptical and imprecise expression of a large and unwieldy body of information', namely of the totality of the evidence in favour of the statement (Earman et al. 2002, p. 296). On such a position, the generalisations of the social sciences are not really laws—though they are none the worse for it.

What are we to make of these scepticisms? They are by no means mandatory. Philosophers who do believe in *ceteris paribus*-laws often also believe in dispositions: they postulate a world inhabited by active particulars, intrinsic causal powers, capacities, and *de re* modalities. Most of the empiricists' so-called 'categorical' properties are, they argue, in reality dispositional, and fundamental dispositional properties such as charge, etc., are "ungrounded" (Mellor 1974, p. 171; Mumford 2006). Moreover, laws of nature if conceived as regularities between occurrent properties or events are highly limited in scope. The world is a "dappled" world, and any regularities we happen to observe are merely the result of the fortuitous arrangement of causal powers and dispositions into temporarily stable structures—'dispositional arrays' (Martin and Heil 1997) or 'nomological machines' (Cartwright 1999).

There is thus no such thing as a *strict* law of nature with universal scope, and if laws are to be construed as such we should have to admit that they are all "lying" (Cartwright 1983). Given that laws of nature cannot lie, any successful metaphysical account must allow that laws relate not inert particulars, but dispositions. *Non-strict* laws fit comfortably with this world picture: dispositions are underlying states that remain constant across the varied changes that are their visible manifestations, and they are therefore ideal candidates for the true subject matter of *ceteris paribus*-laws. For by construing laws as referring to 'stable dispositions that may be widely present even if only rarely directly manifested' (Lipton 1999, p. 164), we best explain how some law-like generalisations—say, Kepler's First Law that 'Planets travel in ellipses'—can be true although they have few or no instances at all.

I do not propose to adjudicate here in this standoff between worldviews, or to discuss the quality of the many arguments on both sides.[4] I shall, rather, aim for a more circumscribed objective. Kusch himself does not argue via the 'abstract' route of denying the existence of dispositions or hedged laws. Rather, he attempts to refute semantic dispositionalism by showing that it fails his normative criteria for scientifically legitimate idealisation, *ceteris paribus*-laws, and disposition-ascriptions. I have argued that his proposed constraints on idealised laws fail to capture the nature of idealisation, and that his initial reconstruction of the *ceteris paribus*-clause does not show what's illegitimate about Fodor's use of it. We are thus left with Kusch's constraints on *ceteris paribus*-laws *qua* ascriptions of single dispositions.

They are interesting constraints, because they raise important questions. Kusch's grounds for refusing to attribute the adding disposition to people are exactly the same as those for which he rejected laws idealising to unbound working memory: we cannot accept that the dispositions of a finite cognitive agent could be identical to those of an idealised one. As Kusch puts it, 'the Müller-Lyer Law does not apply to humans with brains the size of universes. In such extreme cases, psychologists would probably say that the disposition itself is lost' (Kusch 2005, p. 163). The general principle justifying this claim seems to be, therefore, that there cannot be causal continuity between the dispositional structure of actual objects and the dispositional structure of highly idealised objects.

Whether this is a universal principle is worth investigating, for if true it would have immediate consequences for scientific methodology. One of the most puzzling features of scientific practice is, precisely, that unrealistic, i.e. descriptively *false*, laws and models can be used to provide understanding of the way actual mechanisms and causal processes work. In this sense, it seems that idealistic models can help us to construct a better picture of reality than more empirically correct ones would. How could this be if Kusch's principle were true? I shall argue that if idealisations are not to be thought of as approximations of actual behaviour, then neither are disposition-ascriptions. Kripke's and Kusch's qualms about the science fiction implicit in saying that people are disposed to add are caused by this misunderstanding. The relevant "fiction" is in fact the most plausible picture of reality, given the evidence we currently have.

In the remainder of this paper I will develop my argument by taking a look at the science of mental arithmetic, a closer one than is usual in philosophical discussions of Kripke (1982). The upshot will be a scientific picture of people's arithmetic capacities, which is entirely consistent with the hypothesis that they are adding, and indeed that they are *disposed* to add. Although no knock-down argument for the ontological reality of dispositions, the argument will show that there can be causal continuity between the dispositional structure of actual objects and the dispositional structure of highly idealised objects, and that disposition-ascriptions—no matter how "fantastical"—are therefore not to be rejected on the ground that they fail to approximate the actual course of events.

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[4] Nor would I like my rough sketch to suggest that any actual philosopher holds all or a significant subset of the above views at the same time. It is quite possible, in particular, to accept *ceteris paribus*-laws without making appeal to dispositions (Lange 2002), or conversely to postulate dispositions without need for *ceteris paribus*-laws (Cartwright 2002).

4 Mental arithmetic

Carnap in his seminal (1936) pointed out that whether an object has a given disposition does not depend on whether it has had or will have any opportunity to manifest its disposition: glasses can be fragile without ever having broken; a burnt match may be said to have been non-water soluble, even if prior to its burning it never came into contact with water. We would even be correct to say that matches are not water soluble if none of the other wooden objects on this planet had ever come into contact with water, either; and also if there were no water on this planet (cf. Carnap 1936, p. 445). Patently, disposition-ascriptions can be very theoretical, and they cannot straightforwardly be read off our observations of the course of events in the actual world. They, too, "carve nature at the joints," but do not necessarily approximate our observations of it.

Nowhere is this better illustrated than in cognitive psychology. It is a foundational assumption of contemporary research in cognitive psychology that human behaviour is the net result of the effects of a multitude of causally interacting substructures and subsystems, many of which are located in the brain. In particular, performance in mental arithmetic tasks of the type envisaged by Kripke is currently assumed to depend on a number of processes and subsystems, such as a long term-memory for the storage of learned arithmetic facts, and a short-term working memory (see e.g. Groen and Parkman 1972; Ashcraft 1982; Brainerd 1983; Lemaire et al. 1996). 'Working memory', in turn, is a cognitive construct of the general resource responsible for the organisation and maintenance of information in the brain. It 'makes conscious thought possible', through the activation and inhibition of information newly accessed from perception or retrieved from long-term memory (Morrison 2005, p. 470). One authoritative current model hypothesises working memory to consist of four functionally discrete components, a central executive, visiospatial sketchpad, phonological loop, and episodic buffer (Baddeley 1986, 2000).

Thus, an agent's 'disposition to add', if it exists, would be a *global* state that is the net causal effect of the properties exhibited by each of the subsystems involved in and necessary to support the performance of mental arithmetic. As a result, 'adding dispositions' even more so than other dispositions cannot be directly read off an agent's outward behaviour. Now, we may suspect that Kusch's and Kripke's modal intuitions in this case are based on the following simple idea: one cannot take one piece of a complicated puzzle of interlocked systems and subsystems, and magnify it out of all proportion without destroying the whole structure of which it is a part. After all, one cannot increase by many orders of magnitude the power of one part of a car engine—say of the ignition system—and expect the overall performance of the engine to increase by many orders of magnitude, too. If the spark plugs heat up to a million degrees, the engine doesn't go faster, it melts.

The question is whether this *prima facie* plausible intuition is applicable to our case. Does it arise from a detailed knowledge of the relevant facts? Psychologists have long noted variations in the response times of different groups of individuals to simple arithmetic problems such as "$4+3=?$", and have proposed two different models to account for them. Thus, the 'fact-retrieval' or 'associative' model (Ashcraft and Fierman 1982) stipulates that adults simply retrieve the correct answer directly from their long-term memory, which contains a representation of a set of arithmetical facts in the form of a network of learned associations between number combinations and solutions. Children, on the other hand, generally take longer to respond than adults, which the 'minimum addend model' (Groen and Parkman 1972) explains by hypothesising that they typically resort to less efficient non-retrieval procedures, such as counting. The model stipulates, more specifically, that in small children computing "$m + n = k$" (where $m \geq n$) is a counting process, which consists in setting a counter to the value of the larger addend m and then increasing that counter by one n times (op. cit., p. 329)—a process heavy in working memory requirements.[5] This picture has subsequently been complicated by difficulties in fully explaining the data on the so-called 'problem size effect', i.e. the fact that subjects' solution times generally increase with the sum of the problems. For example, adults take significantly less time to calculate "ties" such as '$5 + 5$', which suggests that like children, they also make use of non-retrieval procedures (LeFevre et al. 1996, p. 217). Short-term working memory has thus turned out to be crucially involved in the mental arithmetic of both adults and children. But working memory is patently a limited cognitive resource. Numerous dual-task studies show that subjects' response latencies to a primary task increase substantially when they are required to concurrently perform certain secondary tasks, strongly suggesting that working memory is a scarce resource for which there is competition in the brain (Holyoak and Morrison 2005, p. 459). (An everyday example of such competition is responsible for car drivers' inferior reaction times to traffic events while talking over a mobile phone).

This points to a different possible explanation of the difference between adult and child performance, namely a handicap of children with respect to the efficiency of their short-term working memory. The latter hypothesis is supported by data which show that improvements in short-term memory functioning and efficiency are more important sources of developmental changes in arithmetic performance, than improvements in the accuracy of arithmetical processing (Brainerd 1983). This is further corroborated by recent studies which suggest that as children's arithmetic skills develop their left inferior parietal cortex becomes increasingly functionally specialised, accompanied by decreased dependence on memory and attentional resources (Rivera et al. 2005). Younger subjects' brains are less efficient and less functionally optimised than adults', requiring more working memory and attentional resources to complete the task of calculating (op. cit., p. 1786).

The science of mental arithmetic is still in its infancy, however, as the practitioners themselves readily admit. In particular, the nature and role of working memory is still not fully understood, and the problem-size effect is still not entirely explained.

[5] Both theories correctly predict that response times increase monotonically with problem size: in adults, the associative links would be weaker for larger problems, which have been encountered less frequently during learning and require a longer search through the representation; in children, more increments of the counter would be necessary. However, response times should increase as a function of k in adults, and as a function of n in children (Brainerd 1983, p. 8121). This is *roughly* consistent with observation, which shows a different 'problem-size effect' for the two groups: for children younger than 10, response times increase linearly with the size of the smaller addend, whereas the response latencies of adults increase less drastically and form curvilinear functions, such as the square of the sum or the products of the addends (LeFevre et al. 1996, p. 217). But see *infra.*

Response times also differ between individuals and between cultures, which suggests other components to the explanation, such as variability in the type of non-retrieval procedures used, and experiential differences (LeFevre et al. 1996; Penner-Wilger et al. 2002). For the purposes of my present argument, though, it suffices to take stock of the modular nature of the models deployed, and of the interactions that are hypothesised between those functionally specialised modules: Rivera et al. note that while there was a significant negative correlation between reaction time and age, they found no correlation between age and accuracy (op. cit., p. 1781). Consequently, the difference in overall arithmetic performance between children and adults is not due to children's inferior arithmetic understanding or processing (or fewer arithmetic facts stored in long-term memory), but is attributable to the inferior efficiency of their working memory.

Here we do have a case, therefore, where increasing the performance of the ignition system actually does make the car go faster! Another conclusion to be drawn from these results is that we must nevertheless clearly distinguish between the ignition and the other parts of the engine—in other words between working memory as a *general* cognitive resource on the one hand, and the systems and subsystems involved in arithmetical processing proper, on the other. For although working memory capacity is correlated with arithmetic performance, the evidence shows that the former is not to be identified with arithmetic capacity, nor is it a proper part of those components of our cognitive architecture that are responsible for arithmetic understanding *itself*. Indeed, it is common to now distinguish between three different types of arithmetical understanding, factual, procedural (or strategic), and *conceptual* (Bisanz and LeFevre 1990), each with different working memory requirements.[6]

Does this mean that we could *really* blow up working memory capacity beyond all proportions, and still expect everything else to remain as before? Well, who is to know—and more pertinently, why would we need to know all of that? What the empirical evidence suggests is that working memory and the structure implementing conceptual arithmetical understanding are functionally separate subsystems such that a change in one does not imply a change in the other. What the evidence does *not* suggest is all else of what would happen if working memory were to actually dramatically increase. Fodor's objection to Kripke (which Kusch passes over) was precisely that in order for a law to be scientifically acceptable, we do not have to know all the causal consequences if the conditions described in the antecedent were to actually obtain; we only need to know the counterfactual(s) that the law supports. If a law of nature says that 'All As are followed by Bs', then it must be true that if 'A' were to obtain, then 'B' would obtain; but our law must not, to be considered true, specify a whole

[6] The distinction between the systems responsible for our arithmetical capacities on the one hand, and those other subsystems involved in actual arithmetical performance on the other, echoes of course Noam Chomsky's well-known performance/competence dichotomy in linguistics: 'Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-communication, who knows its (the speech community's) language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of this language in actual performance' (Chomsky 1965, p. 3). The idea itself is not new, de Saussure already emphasised the difference between *langue* and *parole*. If Chomsky is right then to understand the cognitive capacities underlying adding *per se*, we should similarly abstract away from arithmetically irrelevant conditions such as memory limitations, shifts of attention and interest, and errors—essentially describing an idealised cognitive agent.

possible A-world. And so it is generally considered true that in the absence of disturbing causes, the rate of wage varies inversely with the supply of labour, although our economic theory does not explain what a world in which such laws actually remained undisturbed might look like.

Suppose that *ceteris paribus*-laws are indeed descriptions of the action of a disposition in a world where that disposition rarely manifests alone. Suppose also that often we are indeed 'rightly confident about the nature and existence of the disposition even though we are unable to list every possible source of interference' (Kusch 2005, p. 162; Lipton 1999; and others). What are the grounds on which we could then *deny* that adding dispositions exist? Kusch's is a simple scope argument: psychological laws are true only of humans with roughly their current brain size and processing power. But is there a justification for this limitation in scope once we accept the idea that there are dispositions which never manifest alone—in other words, dispositions which are never found isolated in actual humans, so that their display is permanently interfered with by other dispositions? It seems that if a law were to describe the causal role of this one disposition in isolation, then it would necessarily outstrip actuality. Kusch does not provide any independent support for his scope argument. In the next section, I shall argue that it looks rather arbitrary in view of a standard interpretation of the empirical data concerning mental arithmetic.

5 Dispositions, actual and idealised

It is scientifically perfectly acceptable to claim that people have a disposition to add—or at least, to hypothesise that what people are doing is *adding*. True, no extant study investigates directly the prima facie trivial question whether subjects when asked to add are indeed adding, subtracting, multiplying, etc. The assumption that healthy adult speakers will latch on to the meaning of the experimenter and attempt to compute the intended function is taken for granted by psychologists, and self-reports are taken at face value without Kripke-style scepticism. Moreover, studies usually focus on small problem-sizes, and the analysis of error rates in calculations with large multi-digit addends is often relegated to a later stage, when we have fully understood the process in the case of single- and two-digit additions.

However, the facts as revealed in studies designed for other purposes together with very anodyne curve fitting leave little room but for the hypothesis that people are *adding*. We know that mental arithmetic with small numbers is a skill well-honed in most people: (LeFevre et al. 1996, p. 219), note that in a study with 1,600 trials, young adults' error rates in performing single-digit two-operand additions were 1%, and hence too small to be analysed. Yet, as we look at small three-operand problems (of the form $x + y - z =?$), error rates rise to 18%, and they increase further to 28% in the case of double-digit numbers. (For double-digit multiplication and division problems of the form $x \times y \div z$, we even see 30% errors, and 35% refusals to attempt the calculation) (Robinson and Ninowski 2003, appendix).

Combining the data from LeFevre and Robinson, we thus have Fig. 1.

The question what happens as we increase even further the problem-size and difficulty is a straightforward task of extrapolation beyond the available data, in other

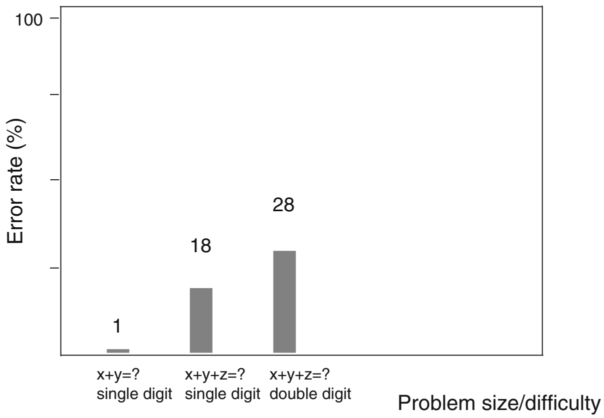

Fig. 1 *Source*: Robinson and Ninowski (2003, appendix), LeFevre et al. (1996, p. 219)

words of curve fitting. The correlation between problem-size and error suggests that as problem size increases without bounds, error rates and/or refusals to attempt the problem are likely to approach 100%. (Whether the regression line should be rectilinear or asymptotical is underdetermined by the data).

How are we to explain the rise in error rates? We could explain it by hypothesising à la Kripke that the subjects are not really computing the addition function at all, contrary to their self-reports. Rather, they could be computing any one of a range of functions which happen to yield the same values as addition for *small* arguments, but then deviate from it. Thus, we would be explaining away the errors by postulating that they are in fact correct results, and that the function actually computed by the subjects is much more complex than the addition function. Alternatively, we could hypothesise à la Kusch a limitation in scope, and suggest that subjects are computing a function which coincides with the addition-function, but is only defined for a small subset of the values for which the addition-function is defined, beyond which lie the "enormous numbers" with respect to which we have no dispositions at all (Kusch 2005, p. 157; cf. Van Inwagen 1992). V. Inwagen even sees the possibility of such an explanation as reason to declare that 'there is no such thing as addition' (ibid.). This is not without initial appeal. After all, if no *actual* cognitive agent can be said to have a disposition to add enormous numbers, must we not conclude that no actual cognitive agent is *stricto senso* adding…? In both cases, we ascribe not the disposition to add, but another disposition (not Fodor's *ceteris paribus* law, but another).

If dressed up as a scientific hypothesis, however, this type of view ought to strike us now as extremely contrived. Surely, a *better explanation* of the data and therefore a more plausible hypothesis is that subjects are indeed computing the addition function (and have a corresponding disposition to do so). Rising error rates, surely, are due to the increased impact on performance of limitations on working memory capacity. Given our present understanding of working memory—in particular the fact that working memory is an independent but limited resource, and the data showing a correlation between arithmetic performance and working memory efficiency—this is the best available hypothesis.

True, arithmetical errors are not always symmetrically distributed, and the analysis of error patterns does play an important part in understanding the role of non-retrieval procedures chosen by the subject (LeFevre et al. 1996). In other words, depending on the aim and method of the study, we do not always idealise away error. However, neither LeFevre nor any other researchers looking at error patterns have found that they give them reason to suspect that subjects are not in fact attempting to add, but computing some *other* function. That is a mere logical possibility due to the underdetermination of theory by data, and hence old news: for any curve fitted to evidence there will always be an infinity of alternative curves fitting the data equally well. The point is that we have no good theoretical reasons to take into account most of the infinitely many alternatives.

Kusch's main quarrel with Fodor's putative *ceteris paribus* law was that it amounts to ascribing a disposition to people which the latter fail to manifest for any but the smallest operands. Therefore, the ascription of the disposition does not approximate actual behaviour very well: as the numbers increase in size, people increasingly fail to hit upon the sum of the operands. This is correct, but it is hardly a good theoretical reason for limiting the scope of Fodor's law, or for attributing a truncated version of the adding disposition. Given our background knowledge concerning working memory and simplicity considerations, it is a better explanation that as numbers are becoming so big to be almost impossible to add, subjects are still *trying*, but due to memory limits now *failing*, to compute $f(x, y) = x + y$, than that they are successfully computing something different and more complex. (Such as $f(x, y) = x + y$ for $x, y \leq \alpha$, and "no answer" for $x, y \geq \alpha$, where 'α' is some 'enormous number'). In other words, even when faced with enormously large numbers, people are still shooting for their sum.

Approximation to the actual data is not everything in scientific theorising, and by endorsing a law-like generalisation such as Fodor's over any of Kripke's or Kusch's alternatives, we are effectively trading goodness-of-fit for greater simplicity and the goal of "carving nature at its joints." We thus find ourselves preferring an explanation of people's arithmetic behaviour in terms of adding, because like Liu above we find that the corresponding generalisation better resembles the form of a law. If we reason in terms of dispositions, we conclude that ascription of the adding disposition does a better job at disentangling the dispositional properties exhibited by the mesh of subsystems and components making up the cognitive architecture of the agent, and at identifying the causal contribution of an isolated disposition—namely of our arithmetical understanding per se. Although it may not be true that *all* the dispositions of an idealised cognitive agent would remain identical to those we observe in an actual one, it is quite consistent with both the evidence and general scientific methodology to hypothesise that at least *one* of them, namely the adding disposition, would.

Kusch's principle that there is necessarily a lack of causal continuity between actual systems and idealised ones is therefore not universally true. To use another car analogy by Alexander Bird, two vehicles may be fitted with engines of vastly different powers, even if the fact that the more powerful one has been fitted with a speed-limiter means that it never achieves any higher speeds than the vehicle with the weaker engine. The empirical evidence clearly suggests that our modest short-term memory is a speed-limiter for our mathematical abilities. Who is to know how fast we could go without this limitation?

6 Conclusion

I have argued that Kusch's defence and development of Kripkean doubts about semantic dispositionalism goes off the rails on multiple occasions. Laws idealising to unbounded memory cannot be dismissed because they make predictions that cannot be experimentally approximated, or because their predictions do not monotonically approach observations as we 'de-idealise' them. For idealisations such as conceiving the universe as a closed two-body system, the market as unperturbed by weather or politics, or indeed a brain with unlimited working memory, are not about getting a description of reality approximately right by leaving out a few details. They are an attempt at identifying the true causal structure of reality, at "carving nature at its joints".

Ceteris paribus-laws, similarly, cannot be faulted if they encounter systematic and reproducible exceptions—most *ceteris paribus* laws do. If, however, we are to conceive of them as descriptions of the action of a single disposition or force, as Kusch recommends we do, then we must again recognise that the function of hedged laws so construed is not simply to approximate reality. Disposition-ascriptions are, I argued, acts of theorising that like idealisations do not stand in an approximative relationship to what is directly observed. They, too, carve nature at its joints, because most dispositions rarely manifest alone, and some never do—although they are there!

Kusch nevertheless thinks that attributing to people the disposition to add any two numbers, no matter how large, is going a step too far. He doesn't accept that the arithmetic dispositions of an infinite cognitive agent could be identical to those of a finite one—implying that there cannot be causal continuity between the dispositional structure of actual objects and the dispositional structure of highly idealised ones. If this were true, it would render a large amount of scientific idealising irrelevant to the causal structure of actually existing things. I argued that it is not, by taking a look at current research into mental arithmetic.

The evidence from trials suggests that the brain supports a number of functionally specialised modules. In particular, we see a functional distinction between working memory as general cognitive resource, and arithmetical processing and understanding. There is a correlation between working memory efficiency and response latency on the one hand, and problem size and response latency on the other, from which we can conclude that better working memory would certainly make us calculate *faster*—and hence enable us to add larger numbers. Moreover, we can also see that modifications to working memory, as during normal development, don't imply changes to arithmetical understanding.

Is this sufficient reason to say that infinite working memory would enable us to calculate infinitely large numbers? To suggest that it is, I employed an inference to the best explanation. We see a very strong correlation between problem size and error rates. The evidence also shows a correlation between arithmetic performance and working memory efficiency, and we conclude that rising error rates are due to limits on working memory efficiency. Any other explanation, certainly any explanation to come out of Kusch's or Kripke's discussion, would run counter to standard simplicity considerations.

We are justified, then, to claim that 'If we did have unbounded memory, then, *ceteris paribus*, we would be able to compute the value of $m + n$ for arbitrary m and n.' Kripke's qualms about "science fiction" are misplaced, because our acceptance of this claim does not commit us to being able to specify what would actually happen if working memory were to be so increased (or indeed what else would *need* to happen for it to be so increased). But this ought not be surprising: I can be justified in believing that if Hillary Clinton is elected President, then she will raise taxes, without being able to specify everything else that will happen if she is elected President (or what else would *need* to happen for her to be elected President).

We are, however, equally not committed by this claim to the existence of dispositions, or indeed to *ceteris paribus*-laws of nature. The debate between realists/anti-realists about dispositions and between sceptics and believers in *ceteris paribus*-laws is, as I have shown, complex and multi-level. There are without doubt ways to re-interpret the above *ceteris paribus*-statement without recourse to dispositional properties, and without granting it nomic status. Nevertheless, I hope my discussion has shown that the ascription of an adding-disposition and a corresponding *ceteris paribu*s-law would be consistent with current science. Moreover, it suggests that we arrive at *ceteris paribus*-laws and disposition-ascriptions via the same type of inference at work in all idealisations, namely one moving away from approximation of observations to the isolation of underlying causes and processes. The degree to which this analysis removes one possible Kripke-style doubt about the legitimacy of *ceteris paribus*-laws and dispositions, is also the degree to which it is an argument in favour of *ceteris paribus*-laws, and dispositions.

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