

Nancy Cartwright's Philosophy of Science

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Nancy Cartwright's Philosophy of Science

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*Dedicated to the memory of
Daniela Bailer-Jones (1969–2006)*

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Christoph Schmidt-Petri

INTRODUCTION

In this chapter I discuss to what extent Nancy Cartwright's appeal to John Stuart Mill's use of "tendencies" to defend or motivate her central notion of "capacity" is justified. My observations are meant to shed some light on the relation between these two concepts rather than to criticize or defend either, and so I shall argue that the differences between Mill and Cartwright are more significant than Cartwright's writings suggest. This need not be seen as a fundamental problem for Cartwright, as she has a number of other, independent arguments to defend her claim that capacities should be taken to be the fundamental building blocks of the natural and social sciences; it simply shows that she should probably not appeal to Mill to support this claim. In any case, Mill's concept of "tendencies" is also problematic: It is not clear whether it squares well with his empiricist account of laws.

Cartwright refers to Mill in a large number of publications, most prominently in *Nature's Capacities and their Measurement* (see Cartwright 1989; 1994; 1999). There she literally takes "tendencies" and "capacities" to be synonymous:

Mill believed that the laws of political economy and the laws of mechanics alike are laws, not about what things do, but about what tendencies they have. . . . Substituting the word "capacity" for Mill's word "tendency", his claim is exactly what I aim to establish in this book . . . I suggest that the reader take my "capacity" and Mill's "tendency" to be synonymous until later in the book]. (Cartwright 1989: 170)¹

It might appear surprising that Cartwright appeals to the writings of Mill, for his—official—Humeanism is something she is vehemently arguing against. However, the apparent similarity of her views with those of Mill, as she reads him, gives them a historical dimension which supplements her arguments from the practice of contemporary science.

I look at Mill's use of "tendencies"; Anscombe and Geach's criticism of it, which Cartwright uses to support her reading of Mill; and then argue

that Mill's use of the language of "tendencies" is much less universal than most think. In fact he only uses "tendencies" in the particularly simple and relatively rare cases of what he calls the "mechanical" composition of causes. Furthermore, he is not realist about "capacities" as he himself uses this concept. Hence, I conclude, an appeal to Mill provides little support for "capacities" as a general and fundamental concept of the natural and social sciences.

MILL ON TENDENCIES

I first want to briefly outline why Mill uses "tendencies" in natural and social science. I think it is important to note that he does so for primarily methodological, that is, entirely practical rather than metaphysical reasons.

In the natural and social sciences, particularly in economics, but also in the moral sciences, Mill sees a multiplicity of causes giving rise to whatever phenomena we observe. Just like Cartwright, Mill believes that the world is "dappled" in the sense that there are very few occurrent regularities.² Hence there is little scope for a systematisation of our experiences just by regrouping phenomena under phenomenological laws by induction. In any case, these empirical laws would mostly be uninteresting, as they would generally be restricted in their range of applicability to the context in which they have arisen and hence not be stable enough for useful predictions. However, Mill also believes that experience shows that the phenomena are produced by relatively few causes. In the domain of economics, for instance, man's desire for wealth is by far the most important cause. By looking at just this desire we can relatively accurately predict what will happen in the markets, provided we manage to present a good description of the circumstances in which this cause operates. Two other causes also operate constantly in the economic realm by directly counteracting this desire for wealth, and these consequently always need to be taken into account when making predictions. These are man's laziness, in Mill's words, his 'aversion to labour' (Mill 1863: 52) as well as his myopic time preference, his 'desire of the present enjoyment of costly indulgences' (Mill 1863: 52). Mill, suggesting mechanical interaction, further notes that these 'accompany it always as a *drag*, or *impediment*' (Mill 1863, 53, italics added). Although other causes operate only occasionally, there will always be some that do.

The laws of the discipline of economics are deductively derived from putting these desires of man into an "economic" context. They state, in abstract, what would happen in the economic realm if no other causes were operative. In economics, experience shows that such theorising may already enable one to predict relatively efficiently a lot of the actual phenomena. Nonetheless, other "disturbing" causes are operative. Therefore, Mill says, if one wants to predict phenomena accurately one should not overconfidently predict *actual*

results, but only a “tendency” to the result: ‘a power acting with certain intensity in that direction’ (Mill 1863: 67).

The situation in the natural sciences is similar. Gravity always operates on every object; however, not every object actually falls to the ground as the law would seem to predict, considered just by itself. Other causes also operate on any individual object, which may offset the gravitational “pull” entirely. According to Mill, objects ‘have a tendency’ to fall even when, as described, they do not. He phrases the general point thus: ‘All laws of causation, *in consequence of their liability to be counteracted*, require to be stated in words affirmative of tendencies only, and not of actual results’ (Mill 1843: 445, italics added). Mill, then, uses the language of tendencies specifically when talking about causes that are impeded in their operation by other causes.

AN OBJECTION TO MILL

The usual interpretation, and the one Cartwright adopts, is to read Mill as making claims about tendencies of things to behave in some way. A tendency, in this sense, would be a feature of an object—a property, or a property of a property.³

However, this reading invites the following objection to Mill, the *locus classicus* of Anscombe and Geach (Anscombe & Geach 1961: 101). They argue that Mill’s use of “tendencies” as delineated above is incompatible with his “official” Humeanism about laws and causation. The reasoning is simple: Officially, Mill thinks that causation is nothing but constant conjunction of cause and effect. Of causal laws, it is then nonsense to say that they are “true” or that the effect of any cause is “fully realised”, as Mill does, if, actually, there is no constant conjunction. But this is exactly what happens in the case of interference. Given Humeanism, the absence of actual constant conjunction must mean that there is no law.⁴ But Mill, it is observed, does not go all that far. When he says that in such cases, there is “interference” and that the laws are nevertheless true, as they are actually about tendencies of things, which just happen not to be realised (or, counterfactually, in the absence of the interference would be realised), then, Anscombe and Geach contend, he is departing from his Humeanism, contrary to what he may believe. In fact, adopting tendencies is to subscribe to a rather more Aristotelian metaphysics.

Cartwright endorses this objection and how it forces Mill into accepting tendencies (and she also sees a further problem, which I discuss later). The problem I see with this argument is the following. Mill does not in fact claim that the relevant laws are laws *about* tendencies of objects to behave in a particular way.⁵ He merely says that these laws *require to be stated* in words affirmative of tendencies only. Mill’s language does not have the existential import both Anscombe and Geach as well as Cartwright see—he does not

say that there are such things or properties as “tendencies”.⁶ What Mill is after is a way of stating causal laws—that is, laws of constant conjunction—such that these laws are not falsified just because the causes do not operate one at a time but simultaneously. Mill’s point is merely verbal, or about the representation of laws, whereas Cartwright and Anscombe and Geach take him to make an assertion about the metaphysics, or the object, of laws.

The standard scenario may help to illustrate.⁷ Consider the case where some object is pulled in a northern direction by some force, and in an eastern direction by another force. Suppose further that these forces are of equal strength; as a result, the object moves northeast (this is a philosophically nontrivial fact of mechanics). What is uncontroversial here is that the object *actually* moves neither “just” north nor “just” east—it moves northeast. What is controversial is how to best analyse what is “really” going on.

Cartwright claims that Mill fails in his analysis of this case. According to Cartwright, Mill, because he does not want to engage in talk of tendencies in a substantial sense here (though he does so elsewhere), talks as if the body was in motion towards the east as well as towards the north (Cartwright 1989: 179). And this is, to all empirical appearances, just plain wrong, because the body moves in precisely one direction—northeast. The Millian stipulation of motion where there is none is not worthy of an empiricist, and certainly much less compatible with empiricism than the adoption of tendencies, which here just might not be realised. What would be accurate to say in this case is that the body has a tendency to move eastwards and a tendency to move northwards—but Mill does not say this. It turns out that Mill is right when, and only when, he is using tendencies. Hence, he really is giving up his Humeanism.

I think that Cartwright and as Anscombe and Geach overstate their case. It is quite possible to make sense of the above scenario without using “tendencies” in a deep sense.⁸ Mill is discussing the composition of causes: in particular, in what sciences we can rely on a “mechanical” composition of causes, as in Newtonian vector addition—and hence can rely on the deductive a priori method—and in what sciences “chemical” combinations of elements render such a neat deduction impossible and extensive testing inevitable (Mill 1843: Bk. III, Ch., IV, §1). Although Mill believes that the mechanical composition is the rule (Mill 1843: 373), he is aware that this principle ‘by no means prevails in all departments of the field of nature’ (Mill 1843: 371). As mentioned, two interesting cases are economics and mechanics, in which the causes do combine mechanically.

What I consider a relevant observation is that in the contested passage, Mill is talking specifically about those cases in which causes *do* combine mechanically, and have been *established* to combine mechanically. Of these only he says that

In this important class of cases of causation, one cause never, properly speaking, defeats or frustrates another; both have their full effect. If a

body is propelled in two directions by two forces, one tending to drive it to the north, and the other to the east, it is caused to move in a given time exactly as far in both directions as the two forces would separately have carried it; and is left precisely where it would have arrived if it had been acted upon first by one of the two forces, and afterwards by the other.

(Mill 1843: 371)

Cartwright interprets Mill's mention of both causes "having their full effect" as if both effects should thus be simultaneously realised in the sense of becoming individually visibly apparent (she changes the example slightly by talking about the more vivid concept of motion). Of course, if the causes did operate one after the other, it would have to be admitted that one *could* say that both effects were fully realised, in the strongest sense imaginable—there would first be a motion to the north, then to the east. And this is where Mill's argument starts: If and only if it is both the case that both effects are fully realised when the causes operate consecutively *and* the result of both causes acting simultaneously is exactly the same as when they do operate consecutively—and he only talks about cases where this is a testable, empirical and established matter of fact rather than a "counterfactual supposition"—*then* we are in the lucky circumstance of being able to derive this result deductively, that is to say, in these cases there is Composition of Causes. And it is only in such cases that Mill talks of "tendencies" and only when both causes actually operate simultaneously.⁹

Mill's point is, first of all, one about the most efficient method. We may use the comparatively convenient deductive method if it has been established that causes combine in a way that is amenable to such deductive reasoning.¹⁰ In those cases we can talk of the individual causes as having "tendencies".

The question now is whether to make sense of this phenomenon we must invoke the reality of tendencies, as I think is Cartwright's claim.¹¹ Mill's language might suggest this, yet the question is whether this really shows that Mill is a (closet) realist about tendencies. Mill's further examples are illustrative, as in all of them he describes how a consecutive operation of causes yields the same result as a simultaneous operation. For instance, he mentions a stream running into a reservoir that at the other end has a drain that simultaneously releases exactly as much water as is entering—the result of this is that the water level in the reservoir remains unchanged. He says that 'even if the two causes which are in joint action exactly annul one another, still the laws of both are fulfilled' (Mill 1843: 372). Although Mill does not specify which "laws" he is thinking of, he refers to the stream that 'tends to fill [the reservoir] higher and higher' and the drain which 'tends to empty it'.

But does this require one to take these laws to be laws about tendencies? I do not think so, for the simple reason that when Mill talks of

the consecutive operation of the causes, he does not use the “tendency” vocabulary. And surely if he doesn’t even use the word it would be far-fetched to claim that he is nonetheless talking about tendencies. He says that the two causes, if they acted, ‘would *produce* effects’ (Mill 1843: 372), not that they would “tend” to produce effects. In other words, Mill only states laws using the language of tendencies when he expects the law to be operating in a causal context in which the effect will not come to full realisation (i.e. will not produce the effect it would produce were no other causes operative) and when it is also the case that the final result is exactly the same as if all causes had operated in isolation, but one after the other.

This is quite compatible with our everyday usage of “tendency”, where it is typically implied that the effect did not get realised. For instance, one would expect the assertion that people who start to go running several times a week “tend” to lose weight—rather than that they do lose weight—to be continued with an explanation of how the effect of running is in fact counteracted, for instance, by increased energy intake. And it is also understood that the running and the additional energy intake do not “interact” in special ways: Both activities have the same effect that they would have in the absence of the other cause, as a sufficiently long period of running followed by a sufficiently long period of additional energy intake would confirm.¹²

A “tendency” statement, on this reading, is thus a statement not about “undercover” goings-on, but about how causes combine, namely, that the composition is of the mechanical kind; that is, that it is a case of Composition of Causes. The regularity highlighted is not one about the stability of the mechanisms or “tendencies” that conjoin to produce the result, but about a feature of the conjunction itself, namely, that the conjunction of causes yields the same effect as if the causes had operated consecutively.

The point of this somewhat lengthy demonstration is this: An argument that runs from Mill’s use of “tendency” statements to the assumed stability of the “tendency” mechanism, and from then on maybe to a mechanical composition turns Mill’s approach on its head. It is precisely the regularity in the composition that is represented in the “tendency” statement. First comes the observation of mechanical composition or “stability” of causes then only the use of the “tendency” language.

This approach can only be faithful to Mill if in cases of “chemical” combination of causes he does not use the language of tendencies (though this by itself does clearly not establish my interpretation). This is indeed so. In these cases Mill says that

most of the uniformities to which the causes conformed when separate, cease altogether when they are conjoined; and we are not . . . able to foresee what result will follow from any new combination, until we have tried the specific experiment. (Mill 1843: 371)

Furthermore he says that concerning the

combinations of elements which constitute organized bodies; . . . the phenomena of life, which result from the juxtaposition of those parts in a certain manner, bear no analogy to any of the effects which would be produced by the action of the component substances considered as merely physical agents.

(Mill 1843: 371)

Again, this corresponds to ordinary language use of “tendency”. It would indeed seem odd to talk of “tendencies” in cases of “chemical” composition of causes. For instance, if in one experiment chemical elements *A* and *B* combine to *X*, but in another *A* and *B*, together with *C*, combine to *Y*, it does not seem accurate to say that in the latter case, *A* and *B* had a “tendency” to form *X*, which was in some sense “offset” or counteracted by the addition of *C*.¹³ The facts here seem most accurately stated without using “tendencies” altogether (even though a counterfactual of the form: “in the absence of *C*, *A* and *B* would have formed *X*” is true just as it would have been if *A* and *B* had had a tendency to form *X*, which was offset by *C*).

Mill, to conclude, uses the language of tendencies only in very specific cases. He is under no illusion that causes do not always combine mechanically, and, more importantly, that whether they do is itself to be determined empirically (not just counterfactually). The majority of the “interesting” causes in economics and physics may combine mechanically, but this itself needs to be established empirically. Hence it would be unwarranted to conclude that for Mill, “tendencies” are a fundamental and ontological building block of the sciences.

MORRISON ON CAPACITIES AND TENDENCIES

These observations need to be contrasted with an argument by Margaret Morrison, who also argues that Cartwright’s “capacities” are different from Millian tendencies (Morrison 1995). Her argument is that the former do not remain constant in the face of all interfering causes while the latter do (and that hence to invoke capacities rather than context-dependent causal laws seems unwarranted). Capacities do not always produce their characteristic effect (even when there is no capacity-modifying interaction), but tendencies in the Millian sense do universally make their characteristic contribution. Indeed, Morrison observes, Mill takes the leap of referring to tendencies even when they are not measurable because counteracted.

I endorse Morrison’s arguments. However, once it is realised that Mill uses the language of tendencies only when he has previously established that the causes produce the same effect when operating simultaneously as

when operating consecutively, to reproach Mill for not following his empiricist teachings seems overly strong. Mill is not appealing to counterfactuals without empirical basis. But more importantly, the limited universality of capacities with respect to tendencies appears no longer as a problem for Cartwright: Mill simply never talks of tendencies when causes do not combine in the “correct” way. He therefore only artificially achieves the “universality” of tendencies which in fact is itself limited to particularly fortuitous scenarios. Hence although Morrison’s arguments are valid for those cases in which Mill talks of tendencies, because he does not do so universally, her case against Cartwright is itself less universal than it seems. However, Morrison’s general observations that capacity claims are of limited universality is nevertheless strengthened if my observations are correct.¹⁴

MILL AND THE REALISM OF CAPACITIES

“Capacity” in the sense that Cartwright uses the word is a technical term that overlaps but does not correspond one-to-one with its usage in ordinary language.¹⁵ What exactly the metaphysical import is of saying that *X* has the capacity to ϕ , in Cartwright’s sense, is therefore sometimes difficult to discern, and Cartwright has been criticised for being too vague about this quite crucial concept (which in ordinary language gets used quite indiscriminately).¹⁶ In fact, Cartwright even says that ‘I . . . have no metaphysical views about dispositions versus capacities versus powers. I choose the word “capacity” since it is less often used by others; hence it carries fewer presuppositions with it’ (Cartwright 2002: 3).¹⁷

What matters for my purposes is that Mill clearly did not endorse the realism of capacities in one of the senses it is used by Cartwright. Hence, to the extent that Cartwright’s and Mill’s conceptions of capacities coincide, there is a clear case that Mill cannot be adduced to support Cartwright’s realism about capacities. Though Cartwright never says that what Mill calls “capacities” corresponds to what she calls thus—she restricts her arguments to Millian “tendencies”—and clearly Mill thinks these are very different concepts, capacities à la Mill bear enough resemblance to capacities à la Cartwright to justify the following quotations.

Mill says: ‘[A] capacity is not a real thing existing in the objects, it is but a name for our conviction that [these objects] will act in a particular manner when certain new circumstances arise’ (Mill 1843: 337). He presents an example:

Putting a coat of white paint upon a wall does not merely produce in those who see it done, the sensation of white, it confers on the wall the permanent property of giving that kind of sensation. . . .

(Mill 1843: 337)

He therefore agrees that the wall has acquired a permanent property. But he continues: ‘no one now supposes the property to be a substantive entity “inherent” in the object’ (Mill 1843: 337). Another example Mill uses is gunpowder. Gunpowder is in a “state of preparation” which conjoined with its lighting will result in an explosion. But this property of gunpowder is reducible to a purely physical description as it “consists in a certain collocation of its particles relatively to one another” (Mill 1843: 337).

Although in these cases it is arguable whether Mill’s sense of “capacity” corresponds to Cartwright’s, the following will suffice to drive the point home. Mill talks about the interaction of gravitational and magnetic forces, an example Cartwright repeatedly uses.

The earth causes the fall of heavy bodies, and it also, in its capacity of a great magnet, causes the phenomena of the magnetic needle . . . The purpose to which the phraseology of Properties and Powers is specially adapted, is the expression of this sort of cases . . . it is usual to say that each different sort of effect is produced by a different property of the cause. Thus we distinguish the attractive or gravitative property of the earth, and its magnetic property; the gravitative, luminiferous, and caloric properties of the sun . . . (Mill 1843: 345)

However, Mill continues by saying that

These are mere phrases, which explain nothing, and add nothing to our knowledge of the subject; but considered as abstract names denoting the connexion between the different effects produced and the object which produces them, they are very powerful instruments of abridgment. (Mill 1843: 345)

For Mill, then, talk of “capacities” may be pragmatically useful, but in his opinion such “capacities” will always be reducible to more primitive physical facts.

CONCLUSION

I have argued that Cartwright’s appeal to Mill’s writings provides relatively little support for her conception of capacities. This, of course, must not be taken to constitute an argument against her own views, which I did not discuss in any detail. My observations are not likely to damage her approach as such; if right, they simply show that it has less support from the ultra-empiricist Mill than might have otherwise have been supposed.

In any case, even if I wholly shared Cartwright’s reading of Mill (and I made clear that I don’t), there still is room for interpretation of Mill’s views. Even if one accepts my reconstruction of the conditions for his usage of the

“tendencies” language, Mill is less clear about what “tendencies” are than one might have hoped. This would nevertheless provide relatively little in the way of empiricist credentials, as Cartwright’s reading of Mill—inspired by the Aristotelian context of Anscombe and Geach’s criticisms—focuses on aspects of Mill’s writings, where it is not clear that he himself is faithful to his own empiricist teachings.

NOTES

1. Later the synonymy is relativised: “‘Capacity’ is reserved for a special subset of these [tendencies]—those tendencies which are tendencies to cause or to bring about something’ (Cartwright 1989: 226; see also Cartwright 1998: 45, 48; 1999b: 4).
2. This, of course, is not intended to be a complete description of Cartwright’s position. For a discussion of the sense in which she sees the world as “dappled” (see Lipton [2002] and Cartwright’s reply [Cartwright 2002]).
3. Cartwright says explicitly that she discusses claims *about* tendencies (Cartwright 1989: 178), in order to distinguish such from “tendency laws”—these being laws of irregular correlation only.

In interpreting Mill’s language of tendencies in his moral philosophy, the second of these interpretations is prominent: Urmson (1953) has claimed that only *types* of actions have tendencies to *P*, these being a “more often than not” correlation between its tokens and the effect *P*; this is also endorsed by Quinton (1973). Champlin & Walker (1973) instead argue that a *token* action has a tendency to *P* if, among its many effects, most of them *P*. In all of these, a “tendency” is nothing beyond some type of correlation. These readings must, however, be wrong: Mill clearly uses “tendencies” to avoid having to talk about exceptions altogether, rather than to model them, as Cartwright realizes.

4. But see Mackie (1980: Ch.3, 75 in particular).
5. At the very least it is accurate to claim that these formulations do not show that he is committed to such tendencies. But there is no better evidence in Mill for the claim that he is so committed.
6. My objection might also apply to Hausman’s reading of Mill: ‘Tendencies are the causal powers underlying the genuine regularities . . .’ (Hausman 1992: 127).
7. See Creary (1981); Cartwright (1980), 1983; Gibson (1986); Psillos (this volume).
8. But this is not to rescue Mill from all problems with his notion of tendency. In particular it does not show that Mill’s use of “tendency” is in the end compatible with a constant conjunction view of causation.
9. The common criticism that Mill does not provide sufficiently detailed rules of composition for tendencies or even neglects this problem is therefore somewhat besides the point. See most prominently Hausman (2002: §§ 4, 5) who objects that ‘To speak, as Mill does, of a deductive method, is misleading because the law governing the conjoint operation of causes cannot be deduced from the laws governing the component causes separately’ (Hausman 2002: 302). Such objections ignore the fact that Mill only ever talks about tendencies when these “laws” *are* well known. Mill does not need, as Hausman claims, *assumptions* of “additivity”, “compositionality”, or “some sort of persistence or non-interaction” (Hausman 2002: 303), nor is it true that ‘Mill has no answer to those who doubt whether causal laws of complex phenomena such

as economies can be deduced from the laws of the separate causes' (Hausman 2002: 304). But it certainly is true that Mill generally provides not enough detail about how he conceives of the operation of "tendencies".

10. To my knowledge Mill does not explain how stable across different contexts he thinks this observation will be.
11. 'What makes capacity claims true are facts about capacities' (Cartwright 1999: 54).
12. The claim is not that this is in fact so (it is not) but that this is what the typical utterer of such a "tendency" statement would want to express. The testing of such claims may in practice also be done differently.
13. It may seem accurate if X and C combined to Y. But then the composition would not be "chemical" in the relevant sense but "mechanical". Note that actual chemical reactions extremely rarely take place in this "chemical" way; in fact, on some level of analysis, they might never.
14. As Cartwright does not agree with Morrison that capacity claims are less than universal, her reply to the present objection might similarly be to give up Mill's even further reduced endorsement (Cartwright 1995).
15. This is sometimes not realised (e.g., Glennan 1997).
16. Cartwright does give a precise definition in her 1998 encyclopaedia entry on "capacities", but this is restricted to its use in economic methodology (see, e.g., Psillos this volume: §6.2).
17. Though in various places Cartwright contrasts capacities with dispositions (e.g., Cartwright 1999: §3.4).

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Reply to Christoph Schmidt-Petri

Christoph Schmidt-Petri's defense of J. S. Mill's empiricism in the face of his talk of tendency laws blends historical and philosophical analysis neatly into one, and I find it convincing when he says that I am quite possibly wrong to suppose Mill at one with me in endorsing tendencies. Rather, Schmidt-Petri argues that, for Mill, to talk about tendency laws is not to endorse the existence of tendencies but rather to point to the fact that regularities exhibit a certain pattern: What regularly follows when a number of factors co-occur is the "sum", in some sense, of what would happen were they to occur consecutively.

I suspect Schmidt-Petri is right in rejecting tendencies on Mill's behalf. The position he defends is consistent with Mill's empiricism, and it fits the texts. But as Schmidt-Petri points out, I do defend tendencies. That's because I do not believe that there are regularities of the kind Mill needs for his account, because what regularities there are in physics and political economy do not involve only factors that can be admitted in an empiricist ontology.

Suppose though that I am wrong. We do not need to refer to interferences, triggers, shields, nomological machines, or the like to state the relevant regularities that will, as Schmidt-Petri argues, save Mill's empiricism. Still reference will not, I should like to point out, save what is called the "Mill-Ramsey-Lewis" view of laws. In this view, laws are those true regularities that best balance breadth of coverage and simplicity. I think it is worth here recalling an old point from *How The Laws of Physics Lie*: Because we want "true" regularities, to secure any predictive or explanatory power, we shall have to sacrifice simplicity entirely—and once we have done that, the empiricist sense of laws as the simplest regularities will turn out to be a sham.

Consider Mill's case of vector addition of forces, as we might do it in elementary mechanics today. I suppose, for the sake of argument, that there is a true regular association expressed by " $f_t = ma$ ": The total force on an object is always equal to its mass times its acceleration. I take it that we want to be able to use this to explain or predict—on a scientific basis—actual accelerations. That requires, for a given object, a law (a regularity law!) that links that object's circumstances to the total force on it. Now even if only gravity were at stake, that law would be incredibly complex, as it must have a term

for every tiny piece of every material object in the universe. Of course we do not do this in a real scientific explanation or prediction. Instead we idealize to some short description that will yield accurate enough predictions for the purposes at hand. But, as Craig Callenden has pointed out in describing my view, that would leave us with an unpleasant trade-off: Either we never explain what really happens or we admit an indefinite number of excruciatingly complex laws.¹ This latter is a problem for the Mill–Ramsey–Lewis view; however, as none of these incredibly complex descriptions are apt to recur even once, let alone often. So, with the exception of $f_t = ma$, the laws we need to explain or predict what really happens will not be regularities after all.

Of course we know what these very complex laws will look like. As Schmidt-Petri reminds us, Mill says they will be a sum of terms describing what would have happened if each cause *‘had* operated in isolation’ (Schmidt-Petri this volume: 8). To say that is to describe a common pattern among these very complicated “laws” that we need for explanation and prediction, not to reduce them to a handful of simple ones. Also note what a strange tactic we must take to identify these terms: Via subjunctive conditionals, and worse, conditionals that are never instantiated even once, let alone regularly. So we don’t find any empiricist regularities here either—not that it would have helped with the original problem anyway. The lesson I want to draw is that we must not take Schmidt-Petri’s probably successful defense of Mill’s empiricism to double as a defense of the contemporary view of laws named in part after Mill.

NOTES

1. Personal correspondence, University of California-San Diego Seminar, January 2005.