

On the Schiffer-Fodor Controversy over *Ceteris Paribus* Psychological Laws

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ABSTRACT. In *Mind* (1991), Schiffer argued that the standards of acceptability governing psychological statements cannot sustain the usual image of correspondence to facts. On the same journal (same issue), Fodor retorted that it is possible to give adequate truth conditions for functionally interpreted psychological *Ceteris Paribus* laws. In this paper, several responses to the questions raised in the Schiffer-Fodor debate are canvassed and shown to fail. Hence, it is claimed that the force of both positions appear to be still intact. Moreover, it is argued that, under a cognitive interpretation of causality, Schiffer's view offers a setting in which adequate sense can be made of c.p. laws.

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Over more than ten years ago, *Mind* published a debate between Schiffer and Fodor concerning the nomic force of psychological laws. Schiffer (1991) denied that there are any such things as true psychological laws. Fodor (1991) held that psychological laws are genuinely representative of states and proper-

ties to be found in the real world. One problem with Fodor's view is that psychological laws (and not only their folk versions) notoriously include *unspecified* scope limitations invoked by *ceteris paribus* (c.p.) conditions. This means that the laws in question are viewed as adequately secure and relatively safe but not altogether certain. Failures in prediction are allowed and explained away by assuming that the *cetera* were not *paria*. But if one adopts the traditional point of view according to which laws must be *strict* (they have the property of holding without exception), then psychological laws must rate as defective.

A variety of thoughtful commentaries have followed since, but the issue as originally addressed by Schiffer and Fodor still remains timely. One reason for re-evaluating the force of both positions is that a significant number of contributors to the debate are united, notwithstanding other differences, in shifting to issues that can be discussed independently of the prospects for the development of scientific psychology. Hence it happens that the controversy no longer focuses on the epistemological status of psychological laws as such; rather, it concerns the general question of the nomic significance of *special science* laws of which psychological laws are just an example (see for instance Pietrosky and Rey 1995). And since the question that is raised is whether a broad sense of lawfulness that encompasses both strict and c.p. laws can be provided, the original terms of the Schiffer-Fodor exchange tend to fade away.

It may be contended that Schiffer does start out by stating that he doesn't believe that there are any special science laws, but he develops his arguments bearing in mind the details of a *functional interpretation of psychological states*. Correspondingly, Fodor's rejoinder has its bite only under the assumption that the property of having intentional states is a property that many different physical systems can instantiate (1991, p. 33).

Other views relative to the debate are cast under the more general assumption that *all* laws (including basic science laws) are c.p. laws. Some authors analyze the way recognized laws are c.p. laws; then whatever case is made in favor of considering all laws as c.p. obviously applies to scientific psychology as well (Lange 1993; Cartwright 1989, Carrier 1988). Now, even if all laws were to be considered c.p., it is doubtful that Fodor would propose his truth definition of c.p. psychological laws as a truth definition for all science laws interpreted as c.p. (Fodor 1991, p. 22, n3). So the present point is analogous to my earlier point that the Schiffer-Fodor controversy really concerns the question of assigning legitimate purpose and content to functionally interpreted psychological laws.

It is a further question whether the concept of c.p. law has different meanings in different sciences. Much like Schurz (2001), I think it is not unreason-

able to suppose so. The fact that the objects of psychology and their properties and relations are not open to direct observation could affect our understanding of c.p. conditions in psychological laws. For instance, if we grant that scientific laws are compatible with facts and other laws only to a certain degree of approximation depending upon the precision of the experimental apparatus used, then the meaning of the c.p. conditions could be relative to the expected accuracy of experimental results in psychological research. On the other hand, we could take a less empirical stance and argue with Earman and Roberts (1999) that the very complexity of the conceptual perspective introduced to study *psychological phenomena* inevitably requires specific adjustments in both our nomic concepts and our standards of rationality in reasoning. But none of this is what Schiffer and Fodor are driving at.

Fodor thinks that psychological explanations are attempts, mostly successful, to tell truths about intentional states; but if c.p. psychological laws are to be laws, their truth must be grounded in fundamental physical structure. Schiffer thinks that psychological discourse is useful but it does not deal in genuine truth-apt assertions; hence his attempt to show that truth conditions of the kind that Fodor is looking for cannot be found. This is what I take to be the core of their discussion.

I said at the beginning that it still makes sense to analyze the concept of c.p. psychological law within the bounds that were set by Schiffer and Fodor. And this is what I propose to do. But in order to get a grip on the question, first, I will put forward a set of assumptions that act as common ground for their deliberations. Second, I will argue that the arguments raised against these assumptions are in some respects flawed. Then, I will give a closer look at both the Fodor and the Schiffer accounts and conclude that the force of both positions appears to be substantially intact. Last, I will argue that under a cognitive interpretation of causality, Schiffer's analysis suggests how we could gain an understanding of the status of c.p. laws.

1. The terms of the debate

As is well known, Fodor sees (folk) psychology as being construed by conceptualizations that appear in true intentional c.p. laws. Schiffer retorts that while there are truth conditions that concur to make strict laws laws, providing truth conditions for psychological or intentional c.p. laws proves to be impossible. To see this, he goes along with Fodor and reformulates c.p. sentences as true propositions that speak of functional states in non functional terms.

Hence, given an intentional A (interpreted as a functional state) and a behavioral state B , Schiffer tries out the following definition: the sentence $c.p.(A \rightarrow B)$ is true if, for every (physical) realizer R of A , the strict sentence

(1.1) there is a same level condition C such that $R \& C \rightarrow B$

is true. In other words A provides a non strict intentional sufficient condition for behavior B if any realizer of A has a same level *completer* C such that *together* they are nomologically sufficient for obtaining B . Completers are enabling conditions for the intentional cause to have its effect; the fact that completers might not always be instantiated explains why the laws in question are c.p.

From now on, if there is a realizer R of A such that (1.1) holds, I shall say that $c.p.(A \rightarrow B)$ is *reduced through* R . Moreover, (1.1) describes a *reduced version of a c.p. sentence concerning* A .

The arrow in (1.1) denotes causal sufficiency. Its meaning is not made precise but we can assume that what minimally is at stake is this: the consequent is a true prediction if its antecedent is true; so that if it happens that the antecedent is true, then the consequent can be *detached* for further use.

The problem, says Schiffer, is that for most functional explanations of intentional laws having the form $c.p.(A \rightarrow B)$, alternative physical mechanisms could equally explain the role played by A in obtaining the effect B . And it may happen that some physical realizer of A (perhaps constitutively) cannot *ever* have an adequate completer for B . If such is the case and according to his truth definition, the law $c.p.(A \rightarrow B)$ is false, even if it is successfully reduced by every other realizer of A . Moreover, since it would no longer be a question of a realizer having a completer that *sometimes* is not instantiated, prediction failures would be the rule without exception, thus seriously undermining Fodor's thesis that c.p. psychological generalizations may enjoy the privileged status of laws.

Fodor answers that psychological c.p. laws can still be grounded in true strict (low level) propositions once it is made clear what can or cannot count as realizers for a specific intentional state. He remarks that any genuine realizer for an intentional state A must also be an *A-realizer*, i.e. it must occur in *many* true reduced versions of c.p. laws concerning A ; for otherwise, it simply cannot qualify as being a realizer of A . Since the meaning of A , i.e. the *concept-of-A*, is the role it plays within a set of strict or c.p. laws involving A (called the *A-network*), any realizer of A which for want of adequate completers systematically fails to produce the effects predicted by the laws in the *A-network* cannot act as a possible physical substrate for the intentional con-

cept A . Hence Fodor suggests that the law $c.p.(A \rightarrow B)$ (in the A -network) is true only if (if and only if) for every realizer R , either one of the following conditions hold:

- (1.2) (a) $c.p.(A \rightarrow B)$ is reduced through R .
 (b) R is an A -realizer.

Thus, the truth of c.p. laws concerning an intentional state A can be maintained in the face of total absence of completers for some realizer R of A , if *many* c.p. laws in the A -network can be successfully reduced through R (condition (b)).

Now a certain number of authors do not directly object to (1.1) or (1.2). Rather, they take exception to several assumptions that Schiffer and Fodor share, if only for the sake of their argument about truth conditions for psychological c.p. laws. In the sequel, I shall consider the views which have led some commentators of the Schiffer-Fodor exchange to challenge one or more of the following:

Assumption 1. Psychological principles are about functional states and their truth is grounded in true lower level non functional laws.

The rationale is that “real” truth is independent of conceptualization and is thus a concern for lower levels of description where laws (presumably) lack conceptual depth.

Assumption 2. Laws tell truths about the real world.

Although the debate does not depend upon a particular concept of law, it is taken for granted that laws have truth conditions which, roughly speaking, sustain the usual image of correspondence to facts. Moreover, truth takes priority over the purely epistemic virtues of verification or confirmation.

Assumption 3. Scientific predictive inference is deductive.

To sustain this assumption is to construe the interconnections between scientific utterances as being from truth value bearing premises to a truth value bearing conclusion where the truth of the premises would justify the truth of the conclusion.

So my concern is to see whether there are good arguments in the literature on the Schiffer-Fodor debate to the effect that any of these assumptions are wrong.

2. Objections to Assumption 1

Mott (1992) thinks that the strategy outlined in *Assumption 1* will not lead to an understanding psychological c.p. laws. He starts by pointing out that even if the functionalist conception encoded in *Assumption 1* is purposefully left vague, it does have certain obligations. For instance, it is comprehensive of the following principles: for all intentional states A and D , for every behavioral state B and for every realizer R ,

- (P1) If $\text{c.p.}(A \rightarrow B)$ is true, then there is D such that $\text{c.p.}(A \& D \rightarrow \text{not-}B)$ is true.
- (P2) If $\text{c.p.}(A \rightarrow B)$ is true, then $\text{c.p.}(A \rightarrow \text{not-}B)$ is false.
- (P3) If R is a realizer for $A \& D$, then R is a realizer for D .

Mott's purpose is not to deny that there are genuine psychological c.p. laws. Instead, he has an argument to the effect that upon any understanding of *Assumption 1*, (P1)-(P3) are obviously true. Then, using Elementary Logic, he goes on to show that (1.1) is contradictory and (1.2) is practically vacuous. The relevant point here does not concern Mott's conclusion but the truth of (P1)-(P3): are these principles really uncontroversial?

I have some misgivings about (P3). Take, for instance, Minsky's classic example (1987) of a system having conflicting intentional states. Many parents have noticed that sleepy children who want to go on with their block construction *usually* resist sleep until they suddenly kick it down; then and only then do they go to sleep. So consider any child, say Bob, and let

A_1 = "Bob wants to go on with his block construction".

A_2 = "Bob wants to sleep".

B = "Bob destroys his own construction".

According to the picture sketched above, $A_1 \& A_2$ is a c.p. condition for B , i.e.

$$\text{c.p.}(A_1 \& A_2 \rightarrow B) \text{ is true.}$$

Now for another story: people who have observed small children at play know that while a child wants to go on with his construction (he wants it high), he also loves to see it topple down; so frequently in the process of construction, he kicks it down. According to this story, if

$A_4 =$ “Bob loves to destroy his own construction”,

then

c.p. ($A_1 \& A_4 \rightarrow B$) is true.

Let R_1 , R_2 and R_4 be realizers for A_1 , A_2 , A_4 respectively and suppose that R_3 realizes $A_1 \& A_2$ while R_5 realizes $A_1 \& A_4$. I submit that although $A_1 \& A_2$ and $A_1 \& A_4$ are different psychological states, they could have the same functional role: a state of “conflicting desires” that is a c.p. condition for B .¹ If this is right, R_5 could also be a realizer for $A_1 \& A_2$ while perhaps it is not a realizer for A_2 : it might lack something that all realizers of the “wanting to sleep” intentional state have in common.²

Of course, this is no *proof* that (P3) is wrong. The question ultimately is empirical. But my argument casts some strong doubts upon the idea that the complexity of intentional states is always neatly reflected in the complexity of the physical system supporting them.

Moreover, (P1) doesn’t appear to be right. For consider the question of instincts: a newborn baby’s wanting milk is a c.p. condition for his searching for a woman’s breast. It seems to me that this law can occasionally fail only for *physical and non intentional* reasons that are internal or external to the baby. Yet (P1) says that c.p. laws can always be overridden by contrasting intentions. On the other hand, instincts viewed as “intentions that are stronger than any other intentions” is a slippery subject; we need to have clear ideas about mental states individuation and their realizations before we can make any headway on this problem. So a better challenge to (P1) is the fact, already mentioned by Schiffer (1991, p. 4), that cognition failures apparently unexplainable in terms of psychological states are documented by neurophysiological research.

So the least that can be said is that Mott’s arguments against *Assumption 1* rests upon two dubious premises, viz. that every c.p. intentional law can be overridden by an intentional factor and that every realizer for a complex intentional state is a realizer for any of its intentional components.

¹ As Fodor says: “the question of what can count as a realizer and the question of what laws have to be in force tend to be very closely connected” (1991, pp. 25-26).

² This argument depends upon the assumption that no rationality constraints ought to be put on mental states or propositional attitudes. But many examples (aggressive but repressed people, terminal patients who believe and do not believe that their death is approaching, people who both want and do not want to retire, etc.) make it difficult to see how it could conceivably be required that the objects of all attitudes attributed to a person at one time be non contradictory.

Pietrosky and Rey (1995) also do without *Assumption 1*. They think that progress can be made in thinking about c.p. laws once it is established that their non reduced versions can be saved from vacuity. Their claim is that for c.p. $(A \rightarrow B)$ to be *non vacuous*, it is sufficient that the following conditions be satisfied:³

- (i) A and B are about properties that can occur in legitimate law-like statements,
- (ii) in every circumstance in which A obtains, either B obtains or else there is an *independent explaining factor C of not- B* ⁴ such that either C alone or C together with the statement $(A \rightarrow B)$ explain the failure of B ,
- (iii) there is a real circumstance in which A holds and where either B can be explained in terms of A and $(A \rightarrow B)$ or B 's failure can be explained by circumstances that satisfy (ii).

Earman and Roberts (1999) identify within the wider context of the Sciences a problem similar to one that was originally brought up by Schiffer. If Schiffer is troubled by the force of *independent completers* in c.p. laws (1991, p. 5), Earman and Roberts are concerned about the influence of *independent explaining factors* in the failure of c.p. laws. Earman and Roberts' purpose is to show that Pietrosky and Rey's conditions are not sufficient to avoid vacuity and they achieve this by indicating a class of meaningless c.p. sentences which nonetheless satisfy (i)-(iii). To describe the generic member of this class, it is sufficient to suppose that A satisfies (i) and that there is a real circumstance in which A obtains; now, take *any* B satisfying (i) for which there is an independent explaining factor C such that it *alone* explains the failure of B . Then (ii) and (iii) are satisfied and c.p. $(A \rightarrow B)$ is non vacuous *even* if B and C are totally independent of the circumstances that verify A . But in that case, A has

³ These conditions, expressed in the language of Propositional Logic, deviate from the original ones couched in the more expressive language of First Order Logic. Since the following discussion does not hinge upon factors that are strictly speaking First Order, this simplified version will do.

⁴ According to Pietrosky and Rey (1995), C is an independent explaining factor of not- B if C has other-wise genuine explanatory power, i.e. it explains something else which is logically and causally independent of not- B .

nothing to do with B and B 's failure is explained solely in terms that are extrinsic to A , so $c.p.(A \rightarrow B)$ turns out to be vacuous.⁵

This last argument could be of serious concern for Psychology. For it implies that certain c.p. psychological statements satisfying Pietrosky and Rey's standards for non vacuity turn out to be void as *psychological* c.p. statements. To see that some c.p. sentence satisfying conditions (i)-(iii) could misguidedly be construed as a psychological statement, take A to express a proposition in the language of Psychology and suppose that C refers to a physical state which prevents a person from having Cancer (e.g. it inhibits the growth of new blood vessels or there is a strange protein X , etc.). This state could explain other facts that are not logically or causally connected with not having Cancer (e.g. it is involved in an explanation of vascular disease). Then, for a person – say Bob – $c.p.(A \rightarrow \text{Bob will get cancer})$ is non vacuous according to (i)-(iii). In particular, if A is the sentence “Bob is depressed”, then

$c.p.(\text{Bob is depressed} \rightarrow \text{Bob will get Cancer}),$

is deemed to express a non void *psychological* statement. Thus any appropriate physiological law that explains the failure of some physical property can be used to state meaningful c.p. sentences apparently endowed with bona fide *psychological content*. And this, among other things, fudges the whole issue about how the mental connects with the physical. So ultimately, if A in $c.p.(A \rightarrow B)$ is causally superfluous in achieving the effect B , the fact that A expresses psychological content is no reason for considering such a c.p. law a psychological one.

One remedy could be to eliminate from Pietrosky and Rey's condition (ii) the possibility that “ C alone explains the failure of B ” and rather require that

(ii) * A and C together and not singularly are causally sufficient for not- B .

But then (ii)* would rule out what Pietrosky and Rey call “catastrophic interferences”, i.e. factors that in themselves stifle the effect of the antecedent of a c.p. sentence (e.g. a stroke can explain why we do not always take steps to achieve our goals although we are, generally speaking, goal oriented). How-

⁵ Earman and Roberts (1999) provide the following example of a vacuous c.p. statement satisfying (i)-(iii): $c.p.(\text{spherical bodies conduct electricity})$. For, either a body is conductive or it has a molecular structure that alone explains its non-conductivity. For another proof of the existence of vacuous consequences of (i)-(iii), see Schurz (2001).

ever, eliminating catastrophic interferences from the class of factors that could interfere with the predictive power of c.p. psychological sentences is a high price to pay: there are numerous examples of putative psychological c.p. laws that must also take into account catastrophic explanations of exceptions to the law.⁶

On the other hand, any proposal that avoids specifying how independent factors can interfere with the predictive power of c.p. laws is open to the charge that the c.p. conditions could always be satisfied since it is never precisely clear what the possible interferences are. This in fact is the motivation behind Pietrosky and Rey's definition of non vacuity. Hence we have come full circle and have found more problems than solutions.

3. Objections to Assumption 2

Some contributors to the Schiffer-Fodor debate claim that laws concerning intentional contents cannot be evaluated as true or false with respect to the real actual world. The view that the c.p. psychological principles with which we predict and explain are not descriptions of reality *tout court* is closely associated with another one according to which there is no need to reduce Psychology to a nomic system dealing with the fundamental structure of the physical world. The contention is that unreduced c.p. psychological principles can be semantically defined once it is understood that c.p. laws are modal principles that characteristically involve states of affairs other than the actual one. This means that a truth definition for c.p. intentional sentences will introduce a semantic determinant: a set of hypothetical situations or "possible worlds" that satisfy certain *stipulations* with respect to which truth is to be defined. Accordingly, a c.p. psychological principle is taken to be true if and only if the corresponding strict principle is true in some specified class of situations.

This view, if taken literally, questions the serious representationality of (ordinary) psychological discourse. And when lawfulness is no longer construed as being comprehensive of unrestricted factual generality, it may concern matters of cognitive systematization.⁷ This in fact is the case here: there is a cognitive key and it is the introduction of possible but non actual situations in which psy-

⁶ Earman and Roberts (1999) first suggest that we could appeal to the condition that "C be relevant to A" and then dismiss this idea on the grounds that the concept of relevance is untractable.

⁷ We are not concerned here with the analysis of c.p. laws in terms of "capacities" or "dispositions". To see why c.p. laws do not appeal to dispositions see Drewery (2001). For the opposite point of view see Pietrosky and Rey (1995).

chological properties are instantiated. The idea is that by experimentation and abstraction we stipulate the conditions under which psychological regularities become apparent and emerge as laws. And the reason why these laws are subject to c.p. conditions is that the actual world may only approximate the worlds selected by our stipulations. Hence, proponents of possible worlds semantics may recognize that it is important to test the theories that psychology introduces for the purpose of explanation and prediction, but they emphasize that this can rarely be achieved by a direct comparison with the facts.

The explanatory potential of possible worlds is usually developed in connection with two sorts of stipulations: either it is required that a possible world be *ideal* or that it be *(most) normal*.

In case of the former kind of stipulation, a possible world is an imagined idealized world in which phenomena coinstantiated in the actual world are taken to exist in isolation and where disturbing factors that prevent a law from being fully operative are abstracted away. From this point of view, c.p. laws are laws that are true when there are no deviations from idealized circumstances.

In appealing to idealized circumstances it is admitted that in order to understand certain aspects of an intentional system, many other aspects have to be ignored. So ideality interpretations of psychological c.p. laws are not dissimilar to those provided in the context of the physical sciences, like studying forces in a perfect vacuum or laws on a frictionless surface. In fact, Pietrosky and Rey “think the need for c.p. laws stems from the need to idealize a complex world” (1995, p. 84) and since “the emergence of any theoretically interesting science requires considerable abstraction”, they propose to see c.p. laws as “a vehicle of such abstractions” (1995, p. 89). Most importantly, they do not see any intrinsic reason for thinking that idealizations about the mind are “somehow less legitimate than idealizations about any other region of the world” (1995, p. 107). Psychology, after all, would be doing nothing more than adopting the methods and procedures that are used in other authenticated scientific domains: “when formulating [any kind of] scientific laws, [this] same sort of strategy is followed” (Silverberg 1996, p. 219).

I take that those who endorse the view that a sentence c.p. $(A \rightarrow B)$ concerning an intentional A is true when

(3.1) in *ideal* conditions, if A is true, then B is true,

do not mean to imply that the truth of alleged c.p. laws is a matter of preference. Ideal states are mental variants of situations we face but, given a certain context, the features that deserve to be given priority and those that do not is

a question that is settled even if there were no psychologists or cognitive scientists capable of envisaging the solution to that problem. It follows that “truth in ideal conditions” generates an *absolute* concept of *truth*: truth in some sense approximates what is ideally true.

Alternatively, it is possible – as Schurz (2001) and in some respects Silverberg (1996) have done – to endorse the idea that psychological c.p. laws encode expectations. Expectations appear to rely on qualitative judgements that express what is *normally* the case without ruling out the possibility of exceptions. Possible worlds, now, are taken to be *normal* worlds; they are instrumental in interpreting c.p. claims as being about what happens on the average indeed in the *most likely* among a range of normal circumstances. Truth, here, is a *comparative* concept since c.p.($A \rightarrow B$) concerning an intentional A is true when

(3.2) in any of the *most normal* conditions if A is true, then B is true.

Most normal circumstances are usually understood as being most commonly occurring circumstances. Sometimes, most normal worlds are like the worlds that Lewis (1973) conceived of for the Logic of Counterfactuals, i.e. worlds that are *most similar* to the actual world except perhaps for the fact that some lawful generalization holds unrestrictedly. This kind of strategy appears to be reasonable since counterfactuals are usually thought to be part of the explanation of what scientific laws are.

It is often said that ideal conditions are not normal conditions precisely because the former can be rare and remote from the conditions that are to be found in the actual world while prevailing conditions, by definition, often are the actual conditions. Yet, while it is true that ideal worlds are factually distant from actuality, they are not per se impossible and hence they might belong to the range of normal worlds that have to be taken into consideration. Take for example the statement

(3.3) c.p.(children seek protection in adults).

It happens that the child Bob has been brutalized by an adult, so Bob represents an exception to (3.3). What would a children-ideal world be like? one which abstracts from the personal history of the children that live in it? If so, there is no place for Bob’s (or any other child’s) dramatic experience in that world. But that ideal world is also a children-normal world since, normally, children are not brutalized by adults. Only it is not the *most* normal one (not the most common nor the closest to the actual world) because it does not sat-

isfy other claims which concern the non-idealized part of the actual world. This suggests that what distinguishes truth condition (3.2) from truth condition (3.1) is the appeal to a preference relation over possible circumstances where it is understood that preferred worlds are *more normal* than less preferred worlds and where one must look for the maximally preferred ones.

Now, what is the nature of this preference relation? If normal circumstances are prevailing circumstances, then most normal circumstances are most prevailing circumstances among a range of alternative outcomes. Thus the preference relation appears to be probability based and normality interpretations of psychological c.p. laws simply turn out to be probabilistic interpretations in disguise. This probabilistic foundation is not made explicit because the probabilities involved in c.p. laws are not very interesting: a most normal world always has extreme probability, i.e. probability close to 1. Accordingly, condition (3.2) translates into “ $A \rightarrow B$ has sufficiently high probability”.

Once this is clear, it remains to be seen whether it is appropriate to interpret the *ceteris paribus* condition in psychological laws as discounting possible deviations from idealized (or alternatively most normal) circumstances. For instance, if the predictive success of (folk) psychological theory is considered to be the result of appropriate idealizations of psychological systems, one still needs to explain how such processes can qualify as being genuinely scientific. The traditional view about how idealizations work in the discovery of nomological explanations is that because the phenomenal world consists of a superposition of states and because we do not have sufficient epistemic resources to process them all contemporarily, we factor out the different components of the complexity and stipulate that these are displayed separately. But this cognitive process of separating and detaching coinstantiated elements is considered to be a respectable way of doing science only if certain methodological requirements are satisfied. Among them the existence of standards in terms of which idealized claims can be judged to be weak or strong, i.e. the extent to which they are or are not capable of truly explaining what goes on in this complicated world. It is not easy to define the conditions under which such theoretical co-ordination of phenomena is successful but there are some describable conditions under which the appeal to idealized laws does not fall short of the ideals of scientific explanation.

For one, idealization is valuable when we have ways of describing the departure of ideal cases from actual ones (e.g. the way ideal gas differs from actual gas). Once ideal conditions are not left vague, it is possible to grasp which properties not included in the ideal descriptions might influence the elements singled out by the law and thus explain possible counterinstances to the law.

Usually there are theories concerning the factors that prevent the ideal law from being instantiated and consequently the law becomes integrated in a larger theoretical framework. So having ways of measuring the distance that separates ideality from actuality is instrumental in judging when an idealized claim merits endorsement.

By the same token, empirical adequacy is generally considered to be an asset of every true theory: if a theory gets the facts wrong, then it is false. Appreciating empirical adequacy is not a straightforward task, but nonetheless empirical control still takes priority over other methods of evaluation. So, idealized scientific claims must also, in some sense, be capable of empirical tests. Usually by different combinations of theory and experimental design, ideal conditions are approximated and idealized claims are then confirmed (e.g. Galileo ‘s “piano inclinato” to confirm the law of uniformly accelerated motion) or disconfirmed.

Now the question arises: can c.p. psychological claims interpreted as idealized claims meet the above requirements? Take Schiffer’s example

(3.4) c.p.(if a person wants something, then she’ll take steps to get it).

What are the ideal circumstances of a person wanting something like? Must she have no other beliefs or desires? (She shouldn’t be wanting to wear a red hat?) Or must there be no beliefs and desires that conflict with subsequent action? (She shouldn’t be afraid of competition?) There is a multitude of diverse psychological factors that the law in question must neglect and we need a way of discerning between genuine possible interferences and harmless coincidence of other mental states. The point is that idealization is useful if it is possible to study properties in relative isolation; but many psychological properties appear to overlap (this is true over and above the difficulty of “carving nature at its joints”) and forcing clear boundaries could prevent genuine understanding.

Similarly, it is far from clear how one is to construct an experimental situation which approximates the ideal situation in which an intentional content is causally relevant to behavior. The problem is that we have to induce people to manufacture mental variants of real psychological situations in which some features stay exactly the same and others slip away. We can construct external situations which we think should induce approximately ideal psychological states, but how can we have the certainty that these actually obtain.⁸

⁸ Mott (1992) thinks that the general problem of constructing an experimental situation in which people’s intentions are *manipulated* is practically unsolvable. According to him, this problem is particularly acute for what he calls left-intentional laws (when the intentions are confined to the antecedent of the law) which connect intentions with behavior.

Because of the elusive character of psychological idealizations, it is difficult to endorse the view that psychological laws interpreted as idealized laws play the same kind of methodological role that idealized laws play in the various areas of Science.

Appealing to normal circumstances apparently solves the problem of disposing of an empirical base. Proofs in real life situations are obtainable because (most) normal circumstances, by definition, frequently occur.⁹ But normality interpretations face other problems and the first one has to do with the meaning of “normality”. If we understand (3.4) as a generalization holding in most normal circumstances, then it is awkward but we must recognize that it’s more normal to have *everyone* actively seeking to reach their goals than having many that do and some that don’t. For, most normal worlds concerning people with desires are worlds in which the generalization contained in (3.4) doesn’t fail. It’s pretty clear that normal, here, cannot be given its intuitive sense.

If we switch to the counterfactual, Lewis-type interpretation of normality which relies on a preference relation based on the imprecise notion of a world being “factually most similar to the actual world”, we encounter another problem familiar to philosophers and logicians interested in Modality. It concerns the meaning that is to be assigned to this preference relation. Lewis (1973) rejected the idea of providing context independent similarity conditions: any precise definition of comparative similarity was, according to him, bound to fail. And Silverberg (1996, p. 221) agrees: there must be such a preference relation for otherwise we simply could not proceed in reasoning about the world; but since this relation is not theoretically explainable, it must be transcendental in the Kantian sense. Hence, his conclusion that using an undefined similarity relation on worlds to give truth conditions for c.p. statements is a perfectly legitimate strategy. But, saying that there is a preference relation among possible worlds which provides inescapable preconditions for efficient prediction in psychology is not enough to warrant the position that c.p. psychological principles are true laws and not, for instance, astute pragmatic principles. Indeed, the way in which an a-priori preference relation could interact with empirically constrained evidence to yield genuine laws is left unexplained.¹⁰

⁹ In fact, Horgan and Tienson (1996) claim that physical and psychological laws have different c.p. qualifications. The former are taken to refer to ideal worlds, while the latter refer to the kind of circumstances that we can find in the actual world.

¹⁰ Joseph (1980) and Pietrosky and Rey (1995) reject the counterfactual reading of laws of physics interpreted as c.p. Whether counterfactual interpretations of c.p. psychological laws are legitimate is a problem that is substantially left open.

Alternatively, as we said above, c.p. laws can be interpreted as normic laws, i.e. laws that are reliable because true in a significant number of circumstances (Schurz 2001). Here normality is *implicitly* seen as a statistical or a probability concept which perhaps incorporates only certain features of a probability distribution, viz. those connected with a high probability criteria for acceptance. On this view normality comes in degrees and c.p. conditions express maximal normality conditions that can be couched in the vocabulary of probability theory. Hence, saying that

c.p.(responsible people keep their promises)

is the same as saying that the objective probability of responsible people keeping their promises is sufficiently high. So including a c.p. condition in a statement serves the purpose of making clear that the statement (without the c.p. clause) is believed *only to a certain degree*: it cannot be fully endorsed but nonetheless it has high probability.

Now, in spite of the fact that a full fledged probability analysis might not be required, it is difficult to see how to set the boundaries of the evidential corpus (mental events and behavior) from which high probabilities should be extracted.¹¹ Undoubtedly a probabilistic approach has the significant advantage of relying on an empirical base but in the case of psychology it is far from clear in what part of the universe some kind of statistical analysis should be performed. Vast portions of psychological experience offer such imperfect data that a careful probabilistic analysis – even one that doesn't require the full precision of computing with numbers – is not always justified. Psychology is a realm of experience which is not particularly congenial to measurement. Rescher (1987), for instance, has pointed out that when the evidence consists of testimony (e.g. in perception), the use of probability is controversial: any concrete application of probabilistic reasoning in these matters requires assumptions about the way in which the problem itself is construed. Davidson (1976) also has argued that the application of probability in Psychology does not rest upon sufficiently neutral epistemological grounds. All in all, it is difficult to see how a high probability characterization of normality could yield a cogent explication of c.p. psychological laws.

¹¹ Here the problem of probability is seen as being relative to *non reduced* psychological statements. Schiffer also criticizes probability interpretations of c.p. laws but he reasons in a functionalist perspective. He notes that “there won't be an objective chance of causing an event *B* that all [intentional] *M* share”, his point being that there can't be an objective probability that will fit each and every *realization* of *M*.

4. Objections to Assumption 3

Critics of *Assumption 2* hold that c.p. psychological statements are to be assessed as being true or false only relatively to circumstances characterized either abstractly or statistically. But if c.p. statements are meaningful in either one of these senses, the question arises as to how they should be *used* in prediction or explanation. In other words, the philosophical stance according to which mind is governed by principles that represent ideal causal relations or statistical tendencies must be paralleled by the existence of a logical device that will efficiently yield appropriate inferences. Clearly such an inferential mechanism cannot admit of a deductive schematization for the characteristic goal of deduction is truth preservation and a logic of inferences from c.p. laws cannot obviously preserve truth. So, critics of *Assumption 2* are also committed to the thesis that inference in scientific (and *a fortiori* in folk) psychology is not deductive, i.e. they must object to *Assumption 3*.

The following discussion does not concern the views of those like Hempel who hold that scientific inference is not deductive because *provisos* (which, according to Hempel 1988, p. 27, are *not* c.p. conditions) must be included in any *application* of the universal laws of nature. As Earman and Roberts (1999) have stressed, there is a difference between maintaining that laws are universally true but that they must be *applied* with qualifications and seriously challenging the idea that only plain and straightforward truth is nomically relevant. It is the latter philosophical position that is relevant here, the target of the discussion now being whether there are non deductive logics that can go hand in hand with the conception that psychological principles are not *simply* true. More specifically, I shall examine the view (Silverberg 1996, Schurz 2001) that the rules underlying much of our psychological thinking carry the kind of defeasible quality that has motivated the development of *Nonmonotonic Logic*.

The study of nonmonotonic inference was begun by researchers in Artificial Intelligence interested in rules of proof that violate a characteristic property of deduction: the cumulativeness of inferences. The term “nonmonotonic” indicates that a logic thus qualified is equipped with a mechanism for retracting a conclusion when contrasting evidence is made available. Hence, nonmonotonicity is a syntactic property which does not define only one kind of inference pattern; it does, however, apply appropriately to reasoning on the basis of generalizations that admit of exceptions (see for instance Fischer Servi 2001).

A variety of nonmonotonic logics have been set forth in the Artificial Intelligence literature and some in fact bear a strong relation to the kind of Conditional Logic that has inspired the normic interpretation of c.p. laws (section

3). It deserves note that these systems called *conditionally based nonmonotonic inferential logics* define a class of nonmonotonic inferential relations whose semantical correlates use a comparative normality relation over possible worlds (see Shoham 1987, Kraus, Lehman and Magidor 1990 as well as Lehman 1992). Moreover, Adams (1986) and Geffner (1992) have shown that assertions true in most normal worlds and high probability conditionals (interpreted as conditional probability) sanction the same nonmonotonic inferences. Furthermore, a conditionally based approach that captures what can also be construed as reasoning about ideal situations can be found in Fischer Servi (1996). Hence there are ways borrowed from Artificial Intelligence investigations in Logic that explain the rules by which statements are established and retracted when idealized or normic generalizations are used to predict the possible outcomes of a factual situation.

Despite the fact that this essay is not per se an essay in logic, the concept of conditionally based nonmonotonic inference needs to be further explained. Consider a conditionally inspired nonmonotonic inference relation $A \sim B$ as asserting that “the truth of A is a *soft* reason to believe B ”, i.e. A does not guarantee but strongly supports B . Roughly, the way revisability of inference works is this: if $A \sim B$ and it happens that $A \& \text{not-}B$, then the fact that the truth of A is a soft condition for B is still in force but it is not acted upon. This is possible because the concept $A \sim B$ is intrinsically *relational*. It is intended as an inseparable expression so that the conclusion B cannot be *detached* from the premise A : it cannot be asserted on its own even if it happens that A is true. Strictly speaking, the sentence B is not even a conclusion; conclusions are such because they are *guaranteed* by their premises but B , here, is strongly supported and not fully justified by the truth of A . To put it differently, the inferential relation $A \sim B$ does not transfer truth from A to B , so B cannot acquire the same status as A ; B can be qualified only relative to A .

An inquiring psychologist who interprets the law c.p.($A \rightarrow B$) as asserting the (conditionally based) nonmonotonic inference relation $A \sim B$ cannot *adopt* B when A is true. He can only act upon the information that “ B is softly embedded in A ”; he is not able to draw (not even provisionally) the conclusion B in an algorithmic way. What he has is a set of “coherence” rules with which he derives instantiations of c.p. psychological sentences from instantiations of other c.p. psychological sentences obtained either inferentially or axiomatically, but he has no instructions for determining when to believe a particular conclusion if its premise is true.

But on a conceptual level, reasoning from evidence to prediction does not really require that we think of “c.p. predictions” as being only tentatively em-

bedded in their premises. When we use (putative) c.p. laws to predict or explain, fallibility is temporarily *ignored* since predictions are detached and acted upon. Actually, “c.p. predictions” are treated provisionally as *full beliefs*, i.e. as possessing a *non*-relational property which can be lost if conflicts arise. This is the whole point about a law being c.p.: contrasting evidence is considered to be evidence against full belief in a prediction licensed by a c.p. law; it is not regarded as conclusive evidence against belief in the law itself. So, predictions in c.p. laws are not chained to the evidence they are embedded in. They can be asserted on their own once it is recognized – via a c.p. clause – that they are subject to correction. To put it more neatly: correction of detachable predictions is the price we pay for discounting fallibility in the use of c.p. laws.

If this analysis of inference from c.p. laws is correct, then, clearly, conditionally based nonmonotonic logics do not provide an adequate foundation for the kind of defeasible (folk) scientific reasoning that occurs when psychological c.p. laws are interpreted as idealized or normic laws.

Conditionally based nonmonotonic formalisms suffer from other defects: they have trouble accommodating irrelevance (e.g. how to infer from c.p.(angry people are aggressive) that c.p.(angry people with red hats are aggressive)) and conflicts (c.p.(angry people are aggressive), c.p.(shy people are not aggressive) and (Bob is angry and shy) causes paralysis unless *priorities* are assigned to the different psychological properties). Other nonmonotonic formalisms, not conditionally based, although successful in facing some of the above problems turn out to be computationally untractable. In sum, it is open to doubt that Nonmonotonic Logic, as it has been developed so far, will provide an appropriate theory concerning the logical status of psychological c.p. laws.

5. Objections to the specifics of the Schiffer-Fodor exchange

I said at the beginning that my first concern was to examine some arguments that call into question the very terms in which the Schiffer-Fodor exchange was framed. This has been done. Now, it remains to be seen whether some of the complaints that have been put forward against the specifics of the controversy are justified. Let us start with those philosophers who think that Fodor’s rejoinder to Schiffer’s challenge is inadequate. Warfield (1993), Silverberg (1996), Earman and Roberts (1999) and Mott (1992) all agree that (1.2), Fodor’s truth conditions for c.p. laws (section 1), are dangerously incomplete. For consider any realization R of A such that there are many laws in the A -net-

work that can be reduced through R , i.e. R is an A -realizer; consider now *any* X such that $\text{c.p.}(A \rightarrow X)$ cannot ever be reduced through R . Then by (1.2) $\text{c.p.}(A \rightarrow X)$ is (vacuously) true. The problem arises because according to (1.2 (b)) the existence of A -realizers is enough to guarantee the truth of any c.p. statement concerning A , be it meaningful, absurd or even contradictory. To evade this objection Silverberg (1996) suggests that

- (5.1) If $\text{c.p.}(A \rightarrow B)$ cannot be reduced through a realization R of A , then for *sufficiently many* S , where S is a realizer of A , $\text{c.p.}(A \rightarrow B)$ is reduced through S .

be substituted to condition (1.2 (b)).

There is nothing intrinsically wrong with (5.1) but I think that the original objection to (1.2) is misguided. What has not been sufficiently appreciated is that Fodor starts out with an *unanalyzed* notion of law network (1991, p. 31) and defines what it means for *a member of the law network* to be true. Hence it is obvious that the nomic character of a statement is not to be found *solely* in the truth of that statement. Presumably, truth is only a part of lawfulness (there are many true non nomic statements, e.g. accidentally true generalizations are not nomic) and conversely lawfulness is not a part of what a statement says.

Now, there are many disputes concerning the interpretation of the term “nomological” or the justification of its use, but one could observe that factors such as explanatory power, integration with other well founded assertions, capacity to suggest new lawlike statements, repeated confirmation by experience are repeatedly on call. However, Fodor explicitly says that he doesn’t want to address the issue of what are the necessary and sufficient conditions a statement must satisfy in order to be nomologically necessary. He thinks that responding to this problem is a respectable and inspiring task but not a particularly breathtaking one (1991, p. 32). But Fodor does think that it is interesting and proper to investigate “how hedged [laws] differ from strict ones” (1991, p. 22). And once it is understood that conditions (1.2) apply to c.p. *laws*, the objection that they could license as true many undesirable c.p. statements loses its force. For one can simply reply that, under any understanding of what laws are, no such statements would belong to a law network in the first place.

Silverberg (1996) and Warfield (1993) are worried by the fact that truth conditions (1.2) appeal to the vague quantifier “many”: it appears in fact in the definition of R being an A -realizer. Although the concept of an A -realizer is characterized in a neat and intuitive sense, it is true that it is not made precise.

But, as Earman and Roberts (1995) have observed, it is possible that in the future, research or a tacit consensus in the scientific community will explain what has been left vague. On the other hand and independently of the value of this last argument, I don't think that the demand for more precision really has to be met. It is the privilege (and the fun) of functional psychology to say interesting things about mental principles of organization of a causal kind without having to provide scientifically accurate descriptions from the standpoint of the underlying fundamental physical structure. To be sure, the term "*A*-realizer" belongs to the functionalist vocabulary but the "many" laws the realizers of *A* are involved in concern lower level descriptions. My point being that from the higher point of view it is not necessary to satisfy the standards of rigor that are recognized as adequate for the lower level of description. And it seems to me that Fodor's truth functional account of c.p. laws does not fall short of the standards of precision we are accustomed to in functional psychology.¹²

Now as I said, there is nothing in conditions (1.2) that refers to lawfulness, so we are left with the conditions that can make c.p. laws true. Schiffer, on the other hand, holds that (ordinary) psychological discourse serves certain legitimate purposes but not one of them is to state truths. And he goes on as far as saying that true laws cannot emerge in anything that goes beyond the interaction of fundamental physical forces. Physics would then be the only science capable of discovering the "basic rules of the game".

In analyzing intentional principles, Schiffer makes a distinction between folk psychology and its more sophisticated version, Cognitive Science. In the case of folk psychology, what Fodor takes to be c.p. laws, Schiffer sees as expressing what to expect in our experience. His view is that expectations are the vehicles of acquired folk psychological concepts and a rough picture of how these are formed could be this. People have *specific* problems, e.g. Ann is thirsty, Odile wants to be with her lover and thinks he is in Lyon. Absolutely secure solutions are seldom available since the problems manifest themselves in a complex, constantly changing world and sorting out all features of the problem situation is computationally too expensive. So people try to reach a balance between paralysis and the search for a reliable solution by looking for what is "contextually appropriate" and "pragmatically relevant" given the context of the problem. Schiffer says that the outcome of such a procedure is a matter of *subjective* preference which can, in first approximation, be expressed

¹² For instance compare the degree of precision in Fodor's definition (1.2) and in Schiffer's definition of a realization (1991, p. 6).

in probabilistic terms. Hence c.p. sentences do not express propositions; they testify to the fact that the construction of a subjective pragmatically constrained probabilistic function has become a part of a person's internal world.

Schiffer is aware that the use of probability theory in connection with folk psychology can be seriously questioned (1991, p. 12, n6). Actually, experimental psychology throws considerable doubt on the idea that we have a fairly reliable intuitive access to probabilistic reasoning.¹³ One could suggest that encoding expectations is really a matter of (subjective) *qualitative* rather than quantitative estimation and evaluation. Alternatively, one could argue that "making believe" that expectations are factual claims is a cognitive feature of our mind.¹⁴ At any rate, Schiffer's main point still holds: folk psychology yields insights as to the working of a *subjective process* that does not provide a nomic system concerning a non-physical aspect of the world.

When shifting from an intuitive to a more scientific perspective, the line of argument changes for now Schiffer must explain the predictive success of *general* psychological principles. He still holds that psychological discourse only has an instrumental role. Observed regularities in cognitive psychology, he says, cannot lead to declarative knowledge because what is deemed to be a c.p. psychological law is really a description of a mechanism, i.e. a description of the *modus operandi* of a system. Mechanisms are programs and so on Schiffer's view, c.p. sentences are descriptions of what programs do. Programs are dynamic "objects", they cannot be true or false; either they are instantiated and they work or else there are some reasons why they are prevented from reaching their prescribed goals. And this is where the c.p. conditions come in. So when Silverberg (1996) says that, even if c.p. sentences only are descriptions of how things work, Schiffer still has to face the conceptual problem he has raised as to how one is to make sense of c.p. conditions, he is not appreciating the fact that c.p. conditions always implicitly accompany the assertion that a program will do what it is supposed to do. Obstacles to the full execution of a program are frequent and including c.p. conditions is a way of referring to malfunctioning of all sorts. The fact that there might be a discrepancy between the observed and the correct system behavior does not require a *conceptual ex-*

¹³ A. Tversky and D. Kahneman report that experimental subjects commit fallacies when asked to rank hypotheses with respect to probability (1983).

¹⁴ Belief revision systems (see for instance, Gardenfors 1988) attempt to describe this way of handling information. These systems formalize the presystematic view that the subject, in his deliberations, can ignore the possibility that fallible assumptions are false. Hence fallible assumptions are treated as full beliefs that are removable when contrasting evidence shows them to be wrong.

planation: by retracting enough assumptions about correctly behaving components of the system, one can remove the conflict between the intended and the observable behavior of that system.

It remains to be seen why descriptions of programs could be mistaken for laws, if only c.p. First, programs are *general* because they serve to solve a class of similar problems. Second, programs have *explanatory power*: they say *how* things are brought about. Last, they have *predictive value*: you can reliably predict the outcome of a run. These are some of the virtues that programs share with the contents of nomic statements.

I have examined many objections that have been raised against the arguments Schiffer and Fodor have advanced in their controversy over the meaning of psychological c.p. laws. I have found these objections to be inadequate. Both positions are compelling; so more than a decade later, it seems to me that the original terms of their exchange still stand.

6. An alternative between Fodor and Schiffer

By way of conclusion, let me sketch a quick picture of what I take to be at stake in Fodor and Schiffer's competing schemes. This, in turn, will suggest a possible strategy for tackling the problem of giving meaning and content to c.p. laws.

First, let us consider Fodor's views: he thinks of laws as describing causal relations in an objective and realistic sense; hence, causal relations hold among real conditions that are independent of the minds that acquire knowledge about them. In the case of Psychology, laws may instantiate statements of the type:

(6.1) x 's mental states cause x 's actions

where the causal relation is taken in an *ontic* sense: it is dependent upon the ontological causal relation between the lower physical states which realize the higher level mental states. It follows that psycho-physical reduction sanctioned by Functionalism puts Fodor in a position to resume (in a contemporary way) a standard philosophical stance, viz. Determinism.

Schiffer, however, objects to (6.1): he offers as a substitute

(6.2) x 's mental processes explain x 's actions;

although he does not give a full account of explanation, he does discuss some of its characteristic properties. One of them is to require that certain circum-

stances *typically cause* other circumstances (Schiffer 1991, 14). Now the question is: does Schiffer's concept of explanation – which we recall is not law invoking – carry with it some negative assumption about the reality of these typically instantiated causal relations? Let me describe the problem in different terms. Schiffer sees explanations as descriptions of how mechanisms work and in that sense, explanations are neither true or false: either they work or they don't; but does he deny that statements saying that such and such physical systems realize such and such mechanisms have truth values? Are the causal relations exhibited by physically realized mechanisms *not* real? If he doesn't think so, there would be grounds for endorsing the existence of Special Sciences laws; for descriptions of physically realized mechanisms would count as good candidates for true c.p. causal laws.

Note that Schiffer's main point against the possibility of giving truth conditions for c.p. laws in the Special Sciences seems to be independent of the prospects for the development of a particular theory on causality. So he might have an argument to the effect that realizations of mechanisms (mostly) behave according to their underlying program without there being bona fide c.p. true causal statements expressing just those facts; and bearing in mind the difficulty of passing from the language of programs and inputs/outputs to the corresponding true assertions about them (as seen in Logic Programming), this view cannot be excluded from the outset. But if descriptions of causal relations implicitly embedded in programs do not yield (c.p.) true statements, then it would be a sort of "category mistake" to interpret (typically instantiated) causality as being ontological rather than *cognitive*.¹⁵

If that is so, i.e. if the concept of causality required by Schiffer is cognitive, then it remains to be seen whether we are bound to hold that there are no such things as c.p. laws. I submit that these can be construed as describing the way mechanisms regularly operate, as causal statements which are qualified to be *cognitively acceptable (justified)*. The basic intuition is to interpret c.p. laws as propositions that, given a fixed level of cognition, a subject can assert beyond any reasonable doubt.

To spell this out, let me start by stating the obvious: the Special Sciences deal with intrinsically complex phenomena. Complexity here is both physical

¹⁵ It deserves to be noted that Schiffer does speak of pragmatic concerns in connection with the concept of explanation (Schiffer 1991, p. 14) but he does not connect them with the concept of causality which inheres to it. For instance, he takes for granted that causality supports counterfactuals; it is not clear whether this should be so, when causality is taken in a cognitive or epistemic sense.

and conceptual. The problem with a physically complex system is that it cannot be thought of as a *context-free* sum of components: too many of them are involved to make the system *understandable*. So we resort to *context-dependent* larger parts and give up descriptive and predictive precision without giving up the belief that larger units have their own characteristic principles of organization. Clearly, when analyzing complex phenomena, context dependence of the parts is a reason why descriptions of regularities cannot be taken to be unconditionally true. And that takes care of c.p. conditions.

But physical complexity is paralleled by conceptual complexity. Working out the processes of a physically complex system by breaking it down into interacting context dependent parts requires *conceptualizations*:¹⁶ these provide powerful means for securing types of data, as well as their governing principles, that could not have been discovered without them. Thus the insight we gain into the modus operandi of a system is moulded by a conceptual perspective, i.e. by the way the world presents itself to the inquiring mind. It stands to reason that if concepts are crucial in understanding operational complexity, the key notion cannot be “truth, if only c.p.” but more likely “justification (acceptance) relative to a cognitive level of appraisal”. And relative justification requires, among other things, recognition of the context-dependent nature of phenomena at the cognitive level in which it is processed.

Do we have a viable definition of cognitive acceptance so as to substitute, in the concept of causal law, truth with it and obtain thereby a characterization of a c.p. law? Certainly, the notion of relative acceptance is in a more vulnerable position than the notion of unconditioned acceptance which is truth. But, when *causal* statements are the source of concern, it is debatable whether defining the first notion is a *much* harder problem than defining the second one. To be sure, the fact that often truth values are not forthcoming does not by itself undermine a realist interpretation of Fundamental Physics. By the same token, the fact that what is acceptable according to a conceptual order is not often secured should not be taken as an obstacle to the view that c.p. laws describe objective properties and relations concerning the constituents of a cognitively complex world.

There is no doubt that this general strategy stands for a better discussion. But the relevant point, for my purpose, is that it is not at all obvious that there must be radically more serious misgivings about c.p. laws than about strict causal laws.

¹⁶ Idealization and conceptualization are not the same. For one, idealizations serve to escape from a messy world; conceptualizations, on the other hand, are part and parcel of it. In general, one can say that idealizations are neat but brittle, while conceptualizations are fuzzy but flexible.

REFERENCES

- ADAMS, E.W. 1986: "On the Logic of High Probability", *Journal of Philosophical Logic*, 15, pp. 255-279.
- CARRIER, M. 1988: "In Defense of Psychological Laws", *International Studies in the Philosophy of Science*, 12, pp. 217-232.
- CARTWRIGHT, N. 1989: *Nature's Capacities And Their Measurements*, Oxford: Oxford University Press.
- DAVIDSON, D. 1976: "Hempel on Explaining Action", reprinted in D. Davidson, *Essays on Actions And Events*, 1980, Oxford: Clarendon Press.
- DREWERY, A. 2001: "Dispositions and *Ceteris Paribus* Laws", *British Journal for the Philosophy of Science*, 52, pp. 723-733.
- EARMAN, J. and ROBERTS, J. 1999: "*Ceteris Paribus*, There Is No Problem of Provisos", *Synthese*, 118, pp. 439-478.
- FISCHER SERVI, G. 1996: *Intuitionism and Models of Cognition*, N.Y.: GiRo Press.
- , G. 2001: *Quando l'eccezione è la regola*, Milano: McGraw-Hill.
- FODOR, J.A. 1991: "You Can Fool Some of the People All of the Time, Everything Else Being Equal; Hedged Laws and Psychological Explanations", *Mind*, 100, 397, pp. 19-34.
- GARDENFORS, P. 1988: *Knowledge in Flux: Modelling the Dynamics of Epistemic States*, Cambridge (Mass.): MIT Press.
- GEFFNER, H. 1992: *Default Reasoning: Causal and Conditional Theories*, Cambridge (Mass.): MIT Press.
- HEMPEL, C. 1988: "Provisos: A Problem Concerning the Inferential Function of Scientific Theories", in eds. A. Grunbaum and W. Salmon 1988, *The Limitations of Deduction*, Berkeley: University of California Press.
- HORGAN, T. and TIENSON, J. 1996: *Connectionism and The Philosophy of Psychology*, Cambridge (Mass.): MIT Press.
- JOSEPH, G. 1980: "The Many Sciences and the One World", *Journal of Philosophy*, 77, pp. 773-790.
- KRAUS, S., LEHMAN, D. and MAGIDOR, M. 1990: "Preferential Models and Cumulative Logics", *Artificial Intelligence*, 44, pp. 167-207.
- LANGE, M. 1993: "Natural Laws and the Problem of Provisos", *Erkenntnis*, 38, pp. 233-248.
- LEHMAN, D. 1989: "What does a Conditional Knowledge Base entail?", in *Proceedings of the First International Conference on Principles of Knowledge Representation and Reasoning*, Toronto: Morgan Kaufmann.
- LEWIS, D.K. 1973: *Counterfactuals*, Oxford: Basil Blackwell.
- MINSKY, M. 1987: *The Society of Mind*, N.Y.: Simon and Schuster.
- MOTT, P. 1992: "Fodor and *Ceteris Paribus* Laws", *Mind*, 101, pp. 335-346.

- PIETROSKY, P. and REY, G. 1995: "When Other Things Aren't Equal: Saving Ceteris Paribus Laws from Vacuity", *British Journal for the Philosophy of Science*, 46, pp. 81-110.
- RESCHER, N. 1987: *Forbidden Knowledge*, Dordrecht: D. Reidel.
- SCHIFFER, S. 1991: "Ceteris Paribus Laws", *Mind*, 100, 397, pp. 1-17.
- SCHURZ, G. 2001: "Pietrosky and Rey on Ceteris Paribus Laws", *British Journal for the Philosophy of Science*, 52, pp. 359-370.
- SHOHAM, Y. 1988: *Reasoning about Change: Time And Causation from The Standpoint of Artificial Intelligence*, Cambridge (Mass.): MIT Press.
- SILVERBERG, A. 1996: "Psychological Laws and Nonmonotonic Logic", *Erkenntnis*, 44, pp. 199-224.
- TVERSKY, A. and KAHNEMAN, D. 1983: "Extensional Versus Intuitive Reasoning: the Conjunction Fallacy in Probability Judgment", *Psychological Review*, 90, pp. 293-315.
- WARFIELD, T.A. 1993: "Folk Psychological Ceteris Paribus Laws", *Philosophical Studies*, 71, pp. 99-112.