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## The (Hopefully) Last Stand of the Covering Law Theory – A Reply to Opp

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In his paper Karl-Dieter Opp heroically sets out to defend both the adequacy and sociological fruitfulness of the covering law account of explanation (the HO scheme). The attempt is bold, as he is not only defending the HO scheme as a theory of explanation but also as a scheme for finding and establishing causal relationships. In this reply I will argue that the defense is not successful; quite the contrary, it clearly demonstrates why mechanism-based reasoning is important in the social sciences. I also argue that this change in metatheoretical perspective has implications for thinking about the role of rational choice theory in sociology, which should not be seen as a foundational theory but rather as a version of common sense psychology that can be used for modeling purposes.

### *What is the HO scheme?*

Let us first take a look at Opp's position. What he takes and defends as the HO scheme is actually a combination of two separate theories. First, there is the theory of explanation attributed to Hempel and Oppenheim (Hempel 1965: 245-290). Second, there is a theory of causation, traditionally called the humean regularity theory of causation. While it is obvious from his text that Opp supports the HO theory of explanation<sup>1</sup>, my claim about the assumed theory of causation requires some justification. My argument is the following: The HO theory, in itself, says nothing about causation, and in the debates about scientific explanation, the absence of causal considerations have been

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<sup>1</sup> Opp claims that the HO scheme "is not in any sense a theory, as Hedström and Ylikoski (2010: 55) and others assert." This is nonsense. The HO theory is a philosophical theory about the nature of explanation and is discussed in the relevant literature as one.

identified as one of the key reasons for its failure.<sup>2</sup> However, Opp treats the HO theory as a scheme for evaluating and justifying causal claims. Thus there have to be some additional assumptions about causation. The text contains evidence of him supporting some sort of regularity view of causation. First, he defines laws as either deterministic or statistical general statements about the regular succession of events. Second, he opposes competing theories of causation, such as counterfactual and generative theories. Third, he denies that advocates of mechanisms have shown that there is a "difference between statements like 'X brings about Y' and 'X is regularly related to Y.' (Opp, xx)". Finally, he says that the statement "manipulating smoking changes cancer" is equivalent to "if smoking increases (or decreases), then cancer increases (or decreases)" (Opp, xx), thus reducing the manipulation theory of causation to the regularity view.

Neither the covering law theory of explanation nor the regularity theory of causation have fared well in philosophy of science debates over the last 50 years. While it would be false to say that a consensus exists on the nature of causation or explanation among philosophers of science, many think that neither of the theories Opp advocates are among the plausible candidates in their respective debates (Salmon 1989, 1998; Humphreys 1989; Ruben 1990; Woodward 2003; Craver 2007). This is reflected in the debates about mechanism-based explanation. For example, the criticism of the HO theory presented in Hedström & Ylikoski 2010 is basically a brief and selective summary of the standard arguments presented in the philosophical discussion. It is regrettable that Opp did not find the summaries convincing, and he should have looked at the more extended arguments found in the references. Given that these arguments have become philosophical commonplaces, I find it puzzling that Opp chooses to ignore them completely. He seems to assume that the HO theory is still the default position in the debate, as it was some 40 years ago. Thus he assumes that if he manages to criticize the mechanistic account sufficiently, his Hempel-inspired

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<sup>2</sup> This is compatible with the fact that Hempel himself thought that the model also applies to the case of causal explanation (Hempel 1965). Such explanations involve causal laws, but they are not explanatory because they involve causes, but rather because they satisfy the requirements of the HO scheme. Later philosophers have argued that causation plays a more fundamental role in explanation (Salmon 1989, 1998; Humphreys 1989; Woodward 2003).

account will be the one that benefits. My reading of the argumentative situation is quite different. The current debate in philosophy of science is not about whether we should replace Hempel's account, but what should be the replacement. The burden of proof lies on any supporter of the covering law theory wishes to be taken seriously: one must show that the accumulated evidence against the theory does not pose a serious challenge for the theory.

Quite clearly Opp does not do this. Our claim in Hedström & Ylikoski 2010 was that the HO theory fails as a theory of explanation. I have found nothing in Opp's paper that would challenge this claim. Consider his answer to the problem of irrelevant information, a well-known problem for the covering law theory. From early on, critics of Hempel's account have pointed out that explanations can be constructed that fulfill all the requirements of the HO scheme but which do not seem to be explanatory. For example, explaining John's failure to get pregnant by citing his regular eating of contraceptive pills would be – counterintuitively – a valid explanation according to the HO scheme. For philosophers of science, counterexamples like this signify something being wrong with the HO theory. They agree with Opp that scientists rarely provide silly explanations like these. However, they sharply disagree with him about the significance of this observation. Opp thinks that everything is in order, because there is an additional criterion to be used: "the laws are to be applied that are used in the social sciences" (Opp, xx). I fail to see how this solves the problems with the HO-theory. The challenge for a theory of explanation is to identify the features of explanations that make them explanatory, and Opp's suggested criterion does nothing like that. Thus it is still legitimate to think that the easy way in which counterexamples can be generated for the HO theory suggests that it has missed something essential about explanation. This is precisely what we claim in Hedström & Ylikoski 2010.

The problem of irrelevant information is not the only serious problem the HO theory faces. The philosophical literature is full of similar challenges. Here are some examples. First are the problems justifying the two central assumptions of the theory – that explanations are deductive arguments and

that explanations must refer to laws. Neither seems to be necessary for simple cases of causal explanation (Salmon 1989; Ruben 1990, Ylikoski 2005). Second, while the theory is presented as a general theory of explanation, it has had serious problems with explaining both probabilistic events and laws (Salmon 1989). The problem with explaining laws was recognized by Hempel (1965) himself, and in the explanation of probabilistic events, ideas like the model of inductive-statistical explanation seem to create more problems than solve them. (For example, there is no generally accepted version of inductive logic.) Third, the theory does not seem to get the direction of explanation right: after all; it sounds implausible to explain Karl-Dieter's length by appealing to the length of his shadow – the explanation should go the other way around – but this is what the theory allows. Thus the HO theory denies the plausible assumption that explanation is an asymmetric relation (Salmon 1989). Fourth, it is widely recognized that the HO theory is unable to make a sensible distinction between explanation and prediction (Salmon 1989). For Hempel, explaining a thing is to make it expected, but this perspective has trouble with cases where reliable diagnostic criteria allow reliable prediction but we do not understand why, and also with cases where we can explain (after the fact) what happened but are not able to predict the outcome beforehand. Both kinds of cases are common in the sciences, which suggests that Hempel's idea about the similarity between explanation and prediction is mistaken. Fifth, the theory faces a thorny problem of epistemic access: given that we are often ignorant of the relevant laws and deductive arguments, how do we manage to make judgements about explanatory relations? In the HO theory it is a mystery how ordinary explanations (even in science) provide understanding, as the crucial elements of the explanation are epistemically inaccessible to ordinary users of such explanations (Woodward 2003: 23-24). Sixth, the notion of "law" still remains a mere placeholder for a better-defined concept, despite the fact that most of the heavy lifting in the theory is done by it (Woodward 2003). Finally, it seems that covering laws that could be used in explanations are quite rare in the non-

physical sciences, and that those appearing like they could are not truly explanatory, but rather things to be explained (Cummins 2000).

These are all well-known problems with the HO theory, and they do not go away simply by ignoring them. Thus it is fair to say that Opp fails in his defense of the HO theory. Is he more successful in his criticism of the mechanism-based view of explanation?

### *Deduction and generalizations in explanation*

Opp's implicit assumption seems to be that the main point of the mechanism-based account of explanation is to deny that explanations are deductive arguments and that generalizations have a role in the justification of causal claims. This is a misunderstanding: the mechanisms-based approach is not based on a negation of covering law theory, but rather on a set of positive ideas about explanation. I will not repeat here what has been clearly said elsewhere (Craver 2007, Hedström & Ylikoski 2010, Ylikoski 2011), but I do want to show how Opp has misunderstood the arguments about the role of deduction and generalizations.

Let us begin with the role of deduction in explanation. The central claim of the mechanism advocates is not that explanations cannot be reconstructed as deductive arguments. Instead, they argue that explanations are not explanations because they are deductive arguments. Opp seems to have missed the difference between these two claims. For example, Ylikoski (2005, 2009) argues that deductive reconstruction of explanations is advisable, but that these explanations are not explanatory *because* they are deductive. People had been giving explanations long before the invention of formal logic; there are plenty of nonpropositional explanations (for example, using diagrams and pictorial representations); and most propositional explanations do not have a deductive structure (although they might have some deductive passages). So as a descriptive claim, the thesis that all explanations are deductive arguments is not very plausible. However, deductivism can be taken as a practical suggestion for improving explanatory practice. Explanations are

therefore not deductive arguments, but they can be reconstructed as such (Ylikoski 2005). The idea is that an (even partial) attempt at deductive reconstruction can improve explanatory practice as it forces us to be explicit both about background assumptions and about the intended *explanandum*. This is probably the main reason some people are still intuitively attracted to the deductive ideal. Naturally, deductive reconstruction is not a foolproof procedure (Ylikoski 2009). For example, if the explanation is based on circular reasoning it will satisfy the requirement of a deductive relation trivially. Similarly, deductive reconstruction does not help if the explanatory theory is fudged with filler terms and other placeholders that hide rather than open the crucial black boxes. To deal with these, one needs more substantial ideas about explanation. This has, in fact, been one of the main motivations for developing mechanism-based accounts of explanation.

Similarly, Opp's multiple arguments in defense of causal generalizations completely miss their target. The defenders of mechanisms do not argue for the foolish position that general causal claims are impossible or that they are irrelevant to the justification of causal claims. To the contrary, the mechanism-based account presupposes causal generalizations. However, Hedström & Ylikoski 2010 (see also Ylikoski 2011) argue that these are not universal generalizations about the constant conjunctions of events assumed by Opp, but more like domain-restricted invariances that allow for contrafactual inferences about the effects of potential interventions described by James Woodward's theory (Woodward 2003: 239-314).

This is important because the traditional view – supported by Opp and Hempel – has failed to provide a satisfactory account of the nature of laws and their role in explanation. While Opp attempts to reduce invariances to laws, they do not satisfy the criteria he himself sets for laws. Invariances usually hold only for a limited range of possible interventions (and changes in background factors); they can refer to particular objects, places and times, and they might have exceptions. In short, they might hold only in a certain domain and collapse outside of it. This is good news for the social sciences (and other sciences outside of fundamental physics): the

generalizations satisfying the traditional criteria of lawhood are quite rare, and mostly not very useful from the point of view of explanation. However, there is one sense in which the idea of invariance is more demanding; i.e. invariance is a modal notion, and it makes a claim that the invariant relationship between cause and effect variables will hold under a set of possible interventions. The idea is that this captures what is essential from the point of view of explanation, whereas most of the traditional attributes are merely superficial. Of course, Opp is free to call these generalizations "laws," but then he must recognize that they are quite different from the generalization his own account is based on.

In the context of the justification of causal claims, Opp completely misses the main motivation for talking about mechanisms in this context. The advocates of mechanism have not denied that all causal claims are generalizable (in some form). Nor have they denied that we can appeal to generalizations when we are justifying causal claims. The advocates of mechanisms have typically argued that knowledge about mechanisms plays an important role in justifying claims that certain generalizations are indeed *causal* generalizations. So the issue is not whether generalizations are involved in the justification of causal claims, but rather how do we know that the generalizations used are truly causal. A singular causal claim can only be justified by a generalization if that generalization is itself causal. The constant conjunction view of causation advocated by Opp is notorious for its inability to distinguish real causal relations from mere correlations, and thus is not a very good candidate solution to this problem. This failure is the main reason why people have begun to discuss things like mechanisms and causal manipulation.

I conclude that little that is useful emerges from Opp's criticisms of the mechanism-based theory of explanation. Whatever problems mechanism-based thinking in analytical sociology have, Opp does not identify them. This is probably because he assumes that the mechanistic approach amounts to a denial of the HO theory. As I have suggested above, this is not the main point. The goal is to replace the HO theory, not just deny it. There is no reason to repeat what has been said elsewhere

(Hedström 2005; Hedström & Ylikoski 2010, 2011; Ylikoski 2011, 2012), so I will finish this reply by presenting some observations about the manner in which the mechanism-based approach can help in rethinking rational choice theory in sociology.

*Rational choice theory as a general theory of action?*

How explanations are conceived is not just an esoteric metatheoretical question, it has real implications for thinking about sociological theory. A good example of this is Opp's defense of rational choice theory as a foundation of sociological theory. If the covering law theory is abandoned and replaced with a more plausible mechanism-based account, much of Opp's theoretical vision for sociology will lose most of its appeal. I cannot provide a full account of rational choice theory in sociology, but I hope the following observations will help in seeing how metatheoretical ideas about explanation have real implications for sociological theory.

In the covering law account all the premises of the deductive argument have the same status, so it does not provide the resources to distinguish those assumptions whose falsity is irrelevant to a given explanation from those whose falsity matters. This easily leads to the kind of instrumentalism that many rational choice theorists represent: it does not matter if the explanation contains false assumptions – or that the highly stylized *explanandum* has no counterpart in empirical reality – as long as the explanation has a deductive structure where the favorite premises of the theorist play a central role. And if many 'elegant' explanations can be constructed where the same premises figure, the theorist calls that set of premises a 'powerful' and 'unifying' theory.<sup>3</sup> A good example of this is the way Opp represents his 'wide rational choice theory.'

This approach to explanation seems arbitrary and it does not allow for a sensible way of dealing with unrealistic assumptions. For realists about explanation, the key issue is whether explanation

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<sup>3</sup> It is worth noting that while covering law theorists like Opp are quite keen on making judgments about "explanatory power", but they never provide systematic account of what kind of quality it is. In contrast to the competing account (Ylikoski & Kuorikoski 2010), the HO theory does not seem to have the resources to articulate the different dimensions of explanatory goodness and to show why they are epistemically valuable.



gets the crucial causal dependencies right. It is not enough that the model “saves the phenomena,” it should represent the essential features of the actual causal process responsible for the observed phenomenon (Hedström & Ylikoski 2010, 2011). For this reason the “as if”-attitude displayed so often by rational choice theorists is not acceptable. Ultimately the theoretical assumptions should be both empirically valid and compatible with the results of other disciplines.

This realist attitude has implications for rational choice theory in sociology. Contrary to what Opp claims, rational choice theory has not been rejected by analytical sociologists (Hedström & Ylikoski 2010) simply because it contains “false assumptions.” Analytical sociologists have not rejected rational choice theory – in fact, quite many continue to employ it (see Demeulenaere 2011) – but what has been suggested is the reinterpretation of its place in sociological theory. The advantage of the assumptions of rational choice theory is that they can be used for modeling social processes, and this has generated a lot of interesting research in the social sciences. However, these benefits of rational choice theory can be enjoyed without assuming that it has a foundational role in the social sciences or that rational choice theory is the only legitimate framework for modeling social phenomena.

As an explanatory scheme, rational choice theory (in its many variations) is just one variant of common sense psychology that employs intentional concepts (Hedström & Ylikoski 2011). When rational choice theory actually explains individual behavior, it does it by identifying the beliefs and desires that actually behind a person’s behavior. The talk about preferences is just a convenient way to abstract away from the messy psychological processes related to desires and other behavioral dispositions (Freese 2009: 98-99). While the abstract scheme of beliefs and preferences is useful for some purposes, there is no reason to arbitrarily restrict the available representations of intentional action to those that have their origin in normative decision theory.

The conceptual scheme of beliefs and preferences is quite flexible, but this vocabulary cannot accommodate all psychological phenomena that might be of sociological interest. All kinds of

psychological factors such as emotions, habits, and expressive motives might be important in explaining social processes, and it would seem foolish to exclude such factors from consideration only because they cannot be plausibly expressed in rational choice vocabulary. Similarly, we should be open to findings from the cognitive sciences. Things like stereotype threat (Schmader, Johns & Forbes 2008) or automatic processes (Wilson 2002) might have great sociological significance in particular applications. Naturally, all of the details of our cognition are not relevant from the point of view of understanding social mechanisms – quite often something like the simple desire-belief-opportunity scheme is sufficient for sociological purposes (Hedström 2005) – but sometimes they are. Too strong an adherence to a particular version of common sense psychology might blinker sociological research.

The argument here is not that rational choice theory is the wrong foundational theory for sociology. The point I am trying to make is that sociology does not need a foundational theory of action. It makes no sense to assume that there should be some unique or privileged version of intentional psychology – the sociological theory of action – that would serve all sociological purposes. So what I deny, is the explanatory privilege of rational choice theory. Rationality is not an excuse for misrepresenting relevant causal facts. Nor it is a good reason to block the development of sociological theorizing. If the idea of a general sociological theory of action make sociologists neglect the findings of the sciences of cognition, this is not only unappealing but also positively harmful.

From this perspective, most of the unification of social scientific knowledge produced by rational choice theory is more or less illusory. Many kinds of models can be built using the wide rational choice theory framework, and most analytical narratives provided by social scientists can be translated into the abstract belief-preference language of rational choice theory. This does not imply that rational choice theory is a powerful explanatory theory. It merely shows that rational choice theory uses a flexible vocabulary. Thus, Opp (2011: 213) cannot merely state that "rational choice

theory only claims that human behavior is governed by costs and benefits; there need not be calculation”. He should also give us an account of *how* the costs and benefits govern human behavior in the different applications of the theory. Without this the theory provides no substantial unification in terms of theory of action, only a highly abstract verbal way to describe the choices the agents face. This is one of the drawbacks of the HO theory: it makes it too easy to give explanations based on black boxes as it does not require an articulation of underlying mechanisms.

Contrary to what Opp suggests, rational choice theory is not best conceived as a collection of empirical generalizations that serve as the theoretical foundation of social scientific theories. It is hard to see the key assumptions of wide rational choice theory as substantial empirical claims. The principle ”preferences are determinants of action” (Opp, xx), for example, sound more like a definition than a substantial causal claim. Similarly ”social action is determined by what the actors think is best for them” (Opp, xx) is more like a principle for reconstructing an actor’s beliefs and preferences rather than an empirical finding in social psychology. The wide rational choice theory seems to amount to a conceptually thin – but flexible – scheme for providing intentional explanations. I see no reason to give it any kind of theoretical privilege. Thus when sociologists use wide rational choice theory in their analytical narratives, it is just one variant of common sense psychology that can often be replaced without losing anything empirically important.

There is an alternative way to conceive of rational choice theory. As Lovett (2006) argues, many economists and political scientists do not treat it as a unified, monolithic, or universal theory of social phenomena. Rather than a foundational theory of action, they take it as a set of conceptual tools for constructing models. In this view rational choice theory does not make substantial assumptions about psychological processes that generate human behavior, it only assumes that an agent’s preferences can be (approximately) presented utility by functions. As Lovett say:

Since utility functions are simply defined merely as those functions we expect social actors to act as if they are attempting to maximize, an intentional explanation of social phenomena

grounded on utility maximization would indeed be (almost) perfectly circular. But this is not the role utility functions play in well-constructed game-theoretic rational choice models. They merely summarize in a concise mathematical expression our expectations regarding their behavior insofar as it is relevant for the social phenomenon we are interested in understanding. (Lovett 2006: 264)

The point Lovett makes here is that when used in this way, rational choice theory is not doing any real explanatory work, it only provides a modeling framework. The purpose of these models is to determine how stable patterns of social behavior might arise given some configuration of structural constraints, not to explain individual behavior.

By themselves, these models are only conceptual explorations of how mechanisms built upon certain assumptions would behave if certain assumptions hold. If the models are to be used to explain real empirical observation – much of the formal work in game theory does not address any recognizable empirical facts – some extra work is needed. For the results of the model to be extrapolated to the real world, they must be robust (Lehtinen & Kuorikoski 2007, Kuorikoski, Lehtinen & Marchionni 2010). Thus models that are highly sensitive to detailed modeling assumptions are not good candidates for extrapolation: the model has to identify the dependencies that can be expected to hold in more complicated situations as well. If the explanatory dependence is highly dependent on some unrealistic assumptions of the model, the model will not permit an extrapolation to possible causal mechanisms that are relevant in the real world. Tractability should not trump causal realism.

As it turns out, many rational choice models do not have the right kind of robustness, and these models cannot be taken seriously as models of real world phenomena. They might have some interesting formal properties, and they might be useful for some heuristic purposes, but they are not the kinds of explanatory accounts that empirically oriented social scientists are looking for. Luckily for rational choice theory, all rational choice models do not fail the robustness test. However,

usually the successful models only give rise to qualitative claims about real world phenomena. What is interesting about this exportable content of formal models is that often it can be expressed without commitment to the restrictive assumptions of rational choice theory. Quite often a very simple DBO scheme (Hedström 2005) is sufficient for this purpose. Thus when rational choice formalism is finally given an interpretation in terms of common sense psychology, the vocabulary of rational choice theory has no special privilege and the restrictive rational choice theory assumptions play no real explanatory role.

Thus it is possible to accept, and endorse, without a commitment to Opp's vision of foundational rational choice theory, those rational choice models whose robust results do not depend on unrealistic rational choice assumptions. This is good news. Given the empirical challenges to the assumptions of the theory, sociological rational choice theorists have been forced into defensive moves. First, they have moved further and further away from the *homo economicus* core of rational choice theory by broadening the content of the theory (Kronenberg & Kalter 2012). The problem here is that as the core theory becomes less rigorous, it also provides less theory-guidance and constraints. Finding a rational choice model that fits a particular phenomenon becomes rather easy, as there are no real constraints on preferences and beliefs that can be attributed to the individuals in question. In addition, a real danger exist of the analytical strengths of the standard rational choice theory model being lost. A second set of defensive moves concerns metatheory, meaning that rational choice theories feel tempted to adopt an instrumentalist attitude that would allow them to claim that it does not really matter whether the explanatory assumptions are true, that the precision of the models is more important than their empirical adequacy, and that concerns about derivational unification override those of getting the crucial causal details right.

These unproductive defensive moves are not necessary. We only need to update our metatheory about explanation and give up the idea about the foundational role of rational choice theory. In the more flexible realist account, social scientists can take an instrumentalist attitude towards rational

choice theory rather than towards the explanatory goals of science. In this view, rational choice theory is treated as one possible scheme for building models about social processes. Social scientists are therefore free to endorse and accept those models that do not fail the robustness test. However, they are also free to explore alternative modeling frameworks – such as rapidly developing agent-based simulation (Macy & Flache 2009) – under the same rules. This view recognizes that social scientists are bound to use some version of common sense psychology in their theorizing, but no version of it is given a privileged position, and the pursuit of formal unification does not restrict the way in which insights from cognitive and psychological theories can be incorporated into the accounts of human action.

#### *Unification and middle-range theories*

The unification of knowledge should be about developing a comprehensive and consistent understanding of the causal structure of the world. Given the world's complexity, the idea of a unified foundational theory should be treated with great suspicion. It is better to focus on theories of a more limited scope that are compatible with knowledge that is produced by other fields. This is the vision of middle-range theorizing that analytical sociologists advocate (Hedström & Ylikoski 2010). Contrary to what Opp (xx) claims, these sociologists do not subscribe to everything ever called middle-range theory, nor do they assume that these theories are limited to specific subject matter, as he also suggests (Opp, xx).

The idea advocated by Hedström & Ylikoski (2010) is that a mechanism-based account of scientific knowledge is embedded in mechanism schemes and not in empirical generalizations, as with more traditional empiricist accounts. The suggestion is that middle-range theories are about these mechanism schemes. These mechanism schemes are more or less general in the sense that they can be employed and adapted to particular situations and explanatory tasks. So although empirical data, research methods, and substantial theories differ from one subfield of sociology to another, the

general ideas about possible causal mechanisms are something these fields could share, and thereby benefit from each other's work. If these points are taken into account, it is clear that most of the points Opp makes against middle-range theories are based on misunderstanding.

I believe that the idea of mechanism-based integration is important in a highly specialized and fragmented discipline such as sociology. It helps to build bridges between the different subfields of sociology, but also with other social sciences. This view also provides a fruitful way to think about the growth of social scientific knowledge: knowledge accumulates as the understanding of individual mechanisms grows and new mechanisms are found (Ylikoski 2011). This vision of knowledge does not *require* that all social scientific mechanisms be ultimately organized into a grand unified deductive theory. It is only assumed that the elements of the mechanical toolbox are mutually compatible and combinable with each other. Formal unification is valued, but only if it brings along a substantial integration of how we understand the ways in which the world works. It is my impression that pursuing the rational choice theory foundations for sociology does not provide such substantial integration. It cannot therefore be presented as a real alternative to the vision of middle-range theorizing.

### *Conclusion*

Opp in his paper advocates a comparative approach to the evaluation of methodologies and theories. I fully agree. However, it is important for this approach that the bookkeeping is taken seriously. It is crucial to keep track of the problems of one's own approach and of the merits of the competing approaches. In my judgement, Opp's paper fails on both counts: he does not take seriously the arguments that have accumulated against the HO theory, and he fails to acknowledge many of the advantages of the mechanisms-based approach. For example, it helps to rethink the position of rational choice theory in sociology without losing its real accomplishments. Of course, the mechanisms-based approach to social sciences is far from ready. It requires further conceptual

clarification and, most importantly, good exemplars of mechanisms-based theorizing. However, I am confident that the HO theory does not have much to contribute to either.

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