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Individualism and Holism in the Social Sciences*

Abstract: Harold Kincaid's *Individualism and the Unity of Science* is a subtle and nuanced analysis of the interlocking themes and issues surrounding the struggle between 'holists' and 'individualists' in the social sciences. Two major claims, one substantial and one methodological, emerge from this analysis. The substantial claim is a defense of a 'non-reductive unity' of the sciences. The methodological claim is that the disputes between reductionists and pluralists or between individualists and holists are empirical and not conceptual disputes. In this paper, I focus on what I take to be Kincaid's central theses.

1. Introduction

The appeal of reductionism has waxed and waned over the years. During the heyday of positivism, it was quite the rage. Recently, however, reductionism has fallen on hard times. The modern allure of reductionism can probably be traced back to the triumph of Newtonian mechanics. Physics appeared as a master science. It revealed the power of the mathematical method in reducing the complexities of material nature to regular laws. Newtonian mechanics served as the paradigm of a proper science. The 18th and 19th centuries saw one attempt after another to formulate an appropriate 'Newtonian' science of psychology, chemistry, biology, geology, sociology, politics and economics. In addition, physics appeared to deal with the most fundamental phenomena of nature so, in addition to serving as a methodological model, physics appeared to be the foundational science in a hierarchy. The appeal of this approach derives from the sense that, once a particular part of a complex system is reduced to order, there must be a simple set of rules and regularities in terms of which the whole becomes comprehensible. Once we have conceptualized nature as *one* system, we are driven to think that there must be *one* key to unlocking the secrets of that system. If the system exhibits hierarchical structure, as the natural and social world do, then if we can discover the laws of the fundamental units of the lowest level of the hierarchy, then we have the key to understanding the whole. Such are the intuitions that drive the reductionist program in all its manifestations.

In the social sciences, the reductionist wars take the form of battles between 'individualists' who argue that the key to understanding social phenomena lies in appealing to the properties and behaviors of individual agents and 'holists'

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who argue that some, if not all, social phenomena are irreducible. Harold Kincaid's *Individualism and the Unity of Science* (1997) is a subtle and nuanced analysis of the interlocking themes and issues surrounding this struggle. Two major claims, one substantial and one methodological, emerge from this analysis. The substantial claim is a defense of a 'non-reductive unity' of the sciences. The 'nonreductive unity' favored by Kincaid embraces four basic tenets: (1) The explanatory power of the special sciences cannot be reduced to that of some fundamental or basic science. (2) Explanatory independence is compatible with ontological dependence. (3) Scientific unity is achieved through 'integrative testing' of the special sciences against lower level counterparts, etc. So, in a pluralistic unity, there is room for both molecular biology and biochemistry, for sociology and psychology, etc. (4) The unifying glue that holds the package together is provided by 'interlevel theories' that connect the *sui generis* components from different levels of organization (Kincaid, 6). The key methodological point is that the disputes between reductionists and pluralists or between individualists and holists are *empirical* and not *conceptual* disputes.

In this paper, I focus on what I take to be Kincaid's central theses: (1) The dispute between individualists (reductionists) and holists (anti-reductionists) is an empirical not a conceptual dispute. (2) Approaches that are typically taken to be exemplars of individualism often, in fact, employ non-eliminable holistic assumptions. The force of Kincaid's arguments, whether we agree with them or not, brings into focus the complex factors that must be taken into consideration in providing a philosophical analysis. In the final section of the paper, I sketch a model for such analyses that emerges from a consideration of Kincaid's argumentative strategy.

2. The Empirical Character of the Debate between Individualism and Holism

Kincaid's fundamental methodological thesis is the claim that whether a given phenomenon can be wholly understood in terms of the actions of individuals is not something that can be decided by appeal to general criteria. Each case must be decided on its own merits. Kincaid's own pluralism attempts to steer a middle course between the two extremes. The issue of monism versus pluralism can be put in the following way. There are three fundamental alternatives for describing and explaining the social: we can (1) opt for Individualism, I; (2) opt for Holism, H; or, (3) opt for Pluralism, which involves some combination of I + H, where "I" and "H" can be taken to stand for individualistic and holistic factors respectively. The reductionist question is: can the H factors be completely eliminated in favor of the I factors? Kincaid argues that, in general, they cannot. He opts for pluralism.

Kincaid takes as his primary foil the individualist thesis that "we can understand everything we want to know about the social world entirely in terms of the actions of individuals" (Kincaid, 1). Kincaid argues that this thesis is false. In the social sciences, "individualism ... frequently fails because it presupposes

rather than eliminates background social structure" (Kincaid, 7). In order to assess this claim, we need to get a handle on exactly what the thesis of 'individualism' amounts to. Unfortunately, as Kincaid's analysis brilliantly shows, this is no easy task. He distinguishes four generic forms of 'individualism', some of which come in alternative versions.

> First, there is *ontological individualism.* This doctrine comes in two forms:
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> OI-1: social structures do not exist separately from individuals;
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> OI-2: social structures do not *act independently* of individuals (Kincaid, 13–14).
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> Second, there is *theoretical individualism*:
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> TI: all social explanations can be reduced to theories about individuals (Kincaid, 14).
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> Third, there is *explanatory individualism.* This principle has three versions:
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> ExI-1: full explanation requires reference solely to individuals;
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> ExI-2: full explanation requires *some* reference to individuals;
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> ExI-3: purely individualist theories suffice to fully explain (Kincaid, 14).
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> This last version, ExI-3, is compatible with the thesis that holist accounts *can* be explanatory as well, but ExI-1 and ExI-2 rule this out! Finally, there is *evidential individualism.* This principle has two versions:
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> EvI-1: all evidence is 'evidence about' individuals in some sense;
>
> EvI-2: no social account is well confirmed until we have evidence about individuals, particularly individualistic mechanisms (Kincaid, 14).

These views are related to one another in complex ways and Kincaid's subtle analysis shows that those who seek to wade into the fray need to be more careful and sensitive than most have been in the past.

John Watkins (1973) characterizes individualism in terms of two theses: W-1: The ultimate constituents of the social world are individuals; W-2: Social events are brought about by people. Kincaid characterizes W-1 as an 'exhaustion' principle and W-2 as a 'determination' principle. Their force, he argues, is that the social *supervenes* on the individual. So, one can endorse a form of 'individualism' without thereby committing oneself to reductionism.

Just how plausible are W-1 and W-2, Kincaid asks? He claims that both W-1 and W-2 are *empirical* guesses about what the best theories now or in the future will do. What, exactly, theories and explanations in the social sciences are, or should be, designed to do are issues addressed more fully in Kincaid (1996).

There are three reasons to think that explanatory reductions, in general, will fail. First, social events and processes are likely to be multiply realizable. Second, individual actions have alternative descriptions depending on context. Third, "any workable individualist social theory will in all likelihood presuppose social facts" (Kincaid, 33).

In effect, the first and second reasons are a reflection of the fact that maps from the individual level (I) to the social level (S) are many-many. Multiple realizability amounts to the claim that the maps R: S $\rightarrow$ I are 1-many. Alternative descriptivity means that the maps H: I $\rightarrow$ S are 1-many as well. If reduction requires lawlike co-extensionality between predicates at the individual and social level, these reasons are telling. If not, then they are not. Kincaid considers and rejects some attempts to defuse the anti-reductionist force of the first two reasons.

Some have argued that multiple realizability can be overcome by using disjunctive clauses to effect the reduction. So, if holist property h can be realized as i_1 or i_2, one can 'artificially' create a disjunctive property i_3 (= 'i_1 or i_2') and the multiple realizability apparently disappears. The problem, Kincaid notes, is that such ploys provide only 'accidental' co-extensionality. When disjunctive property terms are substituted for holist terms, the resulting expressions have the same truth value, in fact, as the original but they do not support the same counterfactuals. Kincaid takes reductionism to require the original and reduced expression not only have the same truth value but support the same counterfactuals as well (Kincaid, 38f). Rosenberg (1994) makes a similar point in connection with the alleged reducibility of genetics to molecular biology.

A second proposal is to reject the requirement that reductions require biconditional connections between T_h and T_i. If this *were not* a requirement, then multiple realizability would not pose a problem. Kincaid does not offer a knockdown argument against this, but he rejects a proposal along these lines offered by Mellor (1982). Mellor argues that the reducing relationships can be approximations and that such approximations are readily available for the various special sciences. Kincaid rejects both claims.

A third proposal is to argue that reduction does not require any bridge laws at all. Kincaid rejects this as a conflation of theoretical reducibility and explanatory reducibility (Kincaid, 39). So, as far as he can see, and I take his case to be a strong one, the first two reasons for having doubts about the viability of purely individualistic explanations of social phenomena are telling.

The third reason for having doubts rested on the allegation that so-called individualist accounts often rest on or presuppose social facts. For example, some theories about the behavior of individuals appeal to preferences. But where do these preferences come from? Are they innate or do they have social explanations? Of course, for limited purposes, we can ignore these questions and treat the preferences as given. However, in the individualism-holism debate such questions cannot be ignored because how we answer them determines whether we have 'full explanations' at the lower level or not. If preferences do have social explanations, then they are effects that are functions of social contexts. This, in turn, suggests that social information is crucial to the implementation

and application of what are supposedly 'fully individualistic' theories. Now this raises another question about completeness or full explanation. In one sense, no explanation or set of explanations, no matter how comprehensive, can be full or complete. All explanations, to appropriate a remark by Wittgenstein, come to an end at some point in the given. This point may be a 'floating point' but it cannot be overcome by appealing to further explanations without introducing a new stopping point in its stead. So, all theories are saddled with concepts that they employ but do not further analyze or explain. In the present context, the question is whether or not the best 'final theory' will or will not include essential reference to irreducibly social factors. Kincaid's claim is that this is an empirical question, one that cannot be decided *a priori* on conceptual grounds. Three examples give this worry some bite.

Consider, for example, the theory of 'organizational ecology'. It uses population genetic models of organizations and the competition for resources. This approach introduces natural selection models to account for the evolution and development of organizations. But, natural selection models are 'multiply realizable'. So, no reduction to purely individualist level is achieved by these approaches. In general, Kincaid concludes, "social theories will be irreducible when they describe selection processes at the institutional level" (Kincaid, 19–20).

Consider, as another example, economic theories of the firm (Kincaid, 20). If firms are treated as black boxes (as individual preferences were in the first example) we have another example of a concept—at the social level—whose ultimate reducibility to the purely individual is a matter of faith. Whether it is achieved or not is an empirical question and not a result forced by any conceptual analysis. Finally, consider rational choice theory (Kincaid, 20). Gary Becker's account rests on individual preferences for social goods that seem to call for holistic or social explanations (Becker 1976; 1981). Rational choice theory, far from reducing social facts to individual facts, presupposes them. In general, "rational choice accounts ... generally rely on a background of social institutions and process".

The main point is that whether such reductions will succeed or not is an empirical question to be decided on a case by case basis. On the surface, this result might be seen to threaten the unity of the sciences by leaving it open whether any general reduction program will work. But, once we accept the possibility of science as a 'non-reductive unity', this piecemeal approach does not threaten to lead to a disunity of the sciences (cf. Kincaid, 66).

3. The Non-Reducible Supervenience of the Social on the Individual

Kincaid argues that social facts supervene on individual facts but are not reducible to them. It follows that purely lower level accounts are incomplete (Kincaid, 70). The gist of the problem is that supervenience, by itself, only guarantees that one kind of fact is 'fixed' by other kinds of fact. But, reducibil-

ity requires a number of other constraints as well. Whether these other constraints are satisfied in particular cases is an empirical question that needs to be settled on a case by case basis (Kincaid, 74). Jaegwon Kim's arguments are the foil here (Kim 1993). How, Kim asks, can higher level structures exhibit any 'real' causal efficacy if, indeed, they are composed of and 'fixed' by lower level structures and processes? Kincaid's defense rests on an appeal to natural kinds. Without delving into the mysteries of what we should understand these kinds to be, we can attest that they serve as the "categories that play a central role in explanation" (Kincaid, 75). So, if higher level structures are genuinely causal, there must be higher level 'natural kinds'. If lower level structures are genuinely causal as well, there must also be lower level 'natural kinds'. Whether the higher level causal relations are epiphenomenal or not turns on whether the higher level kinds can be 'reduced' to the lower level kinds. But, in the light of the triumvirate of reasons for thinking that reductions, in general, will fail, in particular, in the light of multi-realizability, we can grant that higher level structures are *token* identical with lower level structures without assuming that *higher order kinds* are identifiable with *lower level kinds*. They may or may not be, but a *general* acceptance of supervenience does not commit us to a general acceptance of *type-type* identifications. If higher order kinds are not reducible to lower level kinds then the fact that they supervene on lower level tokens does not establish that higher level causal links are impotent.

There are two opposing views. (1) Danto (1973), Rosenberg (1985), and Stich (1985) all argue that individualist explanations are best and fully explanatory *even though* reduction is not feasible. (2) Dennett (1989), Garfinkel (1981), Putnam (1981) and Wimsatt (1976) argue for the opposite tack: lower level supervenient structures explain nothing about the higher level structures they constitute. If either of these claims were true, they would threaten Kincaid's picture of a non-reductive unity. To show that these claims can be defused, Kincaid appeals to the pragmatic conception of explanation. On this view, an explanation is a polyadic relationship which takes into account not only the explanans and explanandum but the interests of the inquirer and the context in which the call for an explanation arises. Garfinkel (1981), and van Fraassen (1980) present versions of this model. With this conception in hand, one can see how supervenient explanations may, on the one hand, provide adequate answers to *some* questions—i.e., those that deal with the components of higher level structures, without being able, on the other hand, to answer *all* questions about higher level structures. This will be true if (as we assume) there are relevant higher level *kinds* which cannot be 'reduced' to lower level kinds (Kincaid, 81).

Kincaid's defense of his pluralistic unity rests on two key assumptions: (1) that there are 'important' questions that higher level theories can answer but lower level theories cannot, and (2) that higher level theories are genuinely explanatory (Kincaid, 82). As examples Kincaid cites the examples of understanding persons (which seem to rely on irreducible psychological concepts such as 'rationality') and our understanding of ideologies (which appeal to social structures and evolutionary theory). *Prima facie*, understanding persons and ideologies would seem to qualify as 'important questions' that, if Kincaid is

right, cannot be adequately dealt with by lower level theories. Indeed, if 'context sensitivity' and 'reliance on higher order presuppositions' is granted then lower level theories make non-eliminable reference to these structures.

The arguments by Putnam and Garfinkel to the effect that lower level theories do not explain at all, appeal to the fact that micro-level explanations offer irrelevant details that macro-level accounts can ignore. Thus, in Putnam's example of why a square peg does or does not fit into a round hole, appeal to the micro-structure of the objects involved seems unnecessary. Kincaid objects, however, that this assumes that the micro-level accounts are supposed to provide micro-level types to match the macro-level types that, in this case, seem to doing all the explanatory work—viz., circularity and squareness. Of course, as we have just seen, multiple realizability precludes this. But, Putnam and Garfinkel are too precipitous in denying any explanatory power to the lower level accounts. If we look at the tokens and not the types, then the micro-level accounts are (or can be) explanations.

Since, on Kincaid's view, questions of reducibility and explanatory power have to be dealt with on a case by case basis, no general argument is forthcoming. As an illustration, Kincaid considers

the case of neo-classical economics (Kincaid, chapter 6). Neoclassical economics, despite its shortcomings, is often defended as providing the 'best explanation' of a wide variety of economic phenomena. Kincaid argues that the inadequacies are not outweighed by the explanatory power (Kincaid, 91). Explanations appealing to neo-classical economics rest on appeals to inferences to the best explanation. Inference to the best explanation (IBE) is often claimed to be a 'foundational principle'. Kincaid rejects that characterization and he rejects the claim that neo-classical economic explanations *always* provide the best explanation of economic phenomena.

By a "foundational principle", Kincaid means that (1) the principle in question must be a primitive strategy, i.e., an unjustified justifier; (2) the principle must be purely formal; and (3) the principle must be sufficient, i.e., given the data and the competing hypotheses, the outcome of the application of the principle must be indefeasible (the outcome cannot be overridden by appeal to other principles (Kincaid, 95). But, Kincaid argues, inferences to the best explanation "rest on substantive, contingent, and often implicit assumptions to do their work" (Kincaid, 92). As such they cannot be foundational in the required sense. In order to assess this claim, we need some view about what constitutes "explanatory power". This is a contentious issue in its own right. Kincaid considers two accounts. (1) Unification (a la Kitcher) and (2) the 'ability to cite causes'. Both, Kincaid argues, appeal to empirical, substantive claims. The argument he gives is as follows (Kincaid, 97–8):

(1) Explanatory power is cashed out either in terms of the power to unify or the power to cite causes.

(2) IBE as inference to the most unifying theory is just the principle: choose the theory that is best confirmed.

(3) Therefore, IBE as IBUE (Inference to the Best Unifying Explanation) is *not* a special principle at all.

(4) IBE as IBCE (Inference to the best Causal Explanation) is neither formal nor sufficient.

(5) Therefore, IBE as IBCE is not foundational.

The crucial assumptions are (2) and (4) and Kincaid provides arguments for them. The argument for premise (2) goes like this: IBUE can be understood in a number of ways. First, what does unification amount to? If it is cashed out in other epistemic terms (Harman 1965; less ad hoc, more plausible: Howson/Urbach 1993, best supported by the data) then it is not primitive. In fact, Kincaid suggests, on any such reading the concept is empty. This sounds right to me. I have always wondered what the big deal about IBE is. Kincaid's analysis suggests that, on some standard accounts, it is not doing much of anything.

If unification means cohering with a set of beliefs or 'fitting' with the 'most comprehensive argument strategy' (as Kitcher 1989 suggests), then IBE is not trivial but it is neither formal nor sufficient (Kincaid, 98). IBE would be defeasible on such a reading because if a hypothesis is an IBE if and only if it best coheres with a set of beliefs B we have about some empirical system, then we need to presume that the empirical system we are trying to account for is, in fact, best characterized by B.

The example Kincaid suggests is this. We have a set of basic beliefs about Darwinian selection systems, which we can label "D's". We come across a population P whose dynamics we want to account for. Suppose there are two competing hypotheses, NS (Natural Selection) and RD (Random Drift). The IBE that singles out NS on the grounds that NS best coheres with D will only be the best explanation of the dynamics of P if, in fact, the population P is adequately characterized as a D-system. Of course, we can expand what it means to be a D-system by including alternative mechanisms such as random drift but this does not affect Kincaid's point. The point is that the principle of choosing between H_1 and H_2 (where these are competing hypotheses to explain the behavior of some system S) on the basis of IBUE depends for its application in particular instances on *empirical* assumptions about the system S. IBUE, so construed, is neither formal nor non-defeasible.

It is certainly not formal unless the characterizations of the system S are taken to be part of the *data*, in which case, applying IBUE to a system S under one description might very well yield different results from applying IBUE to the same system under a different description. Given such a case, one might argue that what we have shown is not that the principle IBUE is defeasible but rather that our descriptions or characterizations of systems are defeasible. Consider, for example, *modus ponens*, or MP. Surely, MP is a formal principle if any is. But, applying Kincaid's test, it might seem not to be. Suppose someone reasons as follows: "If Tully is a Greek, then Tully is mortal. Tully is a Greek. Therefore, Tully is mortal." We then point out that Tully is not a Greek but a Roman. On this re-interpretation of the empirical data, the inference is not justified. Well,

what do we say? We do not conclude that MP is not a formal principle. We say, rather, that the argument as originally presented was unsound. So it appears that the *application* of formal logical principles to particular cases can go astray without our concluding that the inference principles employed are themselves *non-formal.* What is supposed to be different about the IBUE case?

Consider the construal of IBE as IBCE. Kincaid argues that so construed, its applicability hinges on assumptions about the nature of causation and assumptions about what causal variables are or are not relevant in a given case (Kincaid, 99). These are clearly substantive assumptions and if they are presumed *not* to be part of the data to be explained, then the application of IBCE rests on substantive assumptions and is not a purely formal principle either. But, the same worry about the non-formality of IBUE can be run through here as well.

What are we to say about these cases? One might argue that the counter-example appeal to the principles of logic is not legitimate on the grounds that logical principles are *not primitive* in Kincaid's sense. Thus, we do not accept MP as a logical principle *sui generis*, but because it is truth preserving, etc. Reflection on this point brings one to the edge of the abyss of the justification of deduction and we will go no further. But, it does point out that even in the most formal of so-called formal reasoning, it is sometimes a struggle to separate out what is empirical assumption from what is 'formal' principle. Second, we can shed some light on the matter perhaps by recalling the claim that explanation involves the application of a model to an empirical system. What the implications of the model assumptions are, under some principle of explanatory inference, are one thing; whether a given empirical system does or does not conform to these assumptions is another.

Kincaid illustrates what he sees as the problems for both IBUE and IBCE through the case of neoclassical economics (Kincaid, 99–100). One might object that 'neo-classical economics' is not so much a specific theory as it is an approach. To make his case, Kincaid proposes to take neo-classical economics to comprise eight claims (Kincaid, 93).

(1) "Economic outcomes must be explained as entirely the result of individual choices."

(2) "Those choices are rational."

(3) "Rational choices are those that maximize self-interest given constraints."

(4) "Choices are coordinated by markets."

(5) "Markets are best understood by focusing on full competition and equilibrium outcomes."

(6) "Full competition entails complete prices, full information about prices and technology, price-taking behavior by firms and consumers, and free flow of resources to new uses."

(7) "Markets produce efficient outcomes—firms equate marginal revenues to marginal products and so on."

(8) "Incomes are returns to factors."

It is often argued that (1) neoclassical theory is an individualist approach, and (2) neoclassical individualism is *incompatible* with non-individualist alternatives. Kincaid argues that both are suspect. As for (1), Kincaid notes that neo-classical theories invoke the concept of 'firms'—a social entity—(often) as an unanalyzable primitive. Households, representative agents, etc. are likewise all social terms that are not reducible to 'individuals'. So, even if neoclassical theory were successful, it would not provide unambiguous support for individualism. As for (2), although neoclassical accounts, on the views of some philosophers, compete with alternative holist accounts, there are other philosophers who see the accounts as complementary. Some aspects of our social life are either presupposed by neoclassical accounts or are beyond (or outside) their scope. Often, the neoclassical accounts are held to complement other accounts. On this pluralistic view, given the richness of our social life there is enough for everyone to do. If the accounts are not competing, then no appeal to the IBE has been invoked—at least, no general appeal. It still may be the case that some aspects of our social life are explained best by neoclassical accounts while other aspects are not.

The problems that neo-classical economics pose for either form of IBE can now be seen. For IBUE, there are three objections: (1) the problem of unrealistic assumptions; (2) the application of IBUE rests on questionable empirical assumptions or assumes holistic variables; (3) unifying power is a problematic notion of explanatory power. For (1), the argument is this: Consider an experiential economic situation or system ES. Suppose it to be an actual market or exchange. An NCE model H is proposed as an account of the workings of that system. In order for us to claim that the NCE hypothesis is the IBUE here, Kincaid argues, we need to know first that it is indeed a unifying hypothesis, that is, that there are a large range of ES's—other markets as well as a variety of other economic processes—where H serves to explain the experiential behavior we are trying to understand. In addition, we need to know second that the hypothesis H is a 'close fit' to the ES, i.e., that H is a 'realistic' model. But that is an empirical question. So, if and when we decide that H is the best explanation of ES, we are grounding this decision on empirical as well as formal considerations. Now how does this problem different from our attempt to apply modus ponens to justify the inference, on the information provided in the cited example, that "Tully is mortal"? Presumably, Tully is mortal but the original argument failed to show it because it presumed false information. But, as we saw, this did not impugn the formality of the principle of inference that we used. Also, when we corrected the premise (an empirical claim) and drew a justified conclusion, no one would say that *this* shows that modus ponens is an empirical principle.

As a matter of fact, Kincaid alleges, when we look to the empirical assumptions that economists make, we find them questionable (Kincaid, 101). In the terms we are using, the economic models are alleged to not 'fit' the phenomena they are proposed to explain closely enough. I leave this dispute to those with more familiarity with the cases than I. The point is that even if this is true, it

does *not show* that IBUE is a suspect, empirically grounded principle. What it shows, at best, is that the proposed neoclassical economic models do not explain what they are alleged to explain.

Kincaid then looks at three neoclassical models of the firm. Are they explanatory? To be so, they have to satisfy certain empirical assumptions. But, even if they did, to be *neoclassical* accounts, Kincaid argues, they have to be sufficiently similar to "spot markets—characterized by many buyers and sellers with fully developed preferences and other traits of perfect competition" (Kincaid, 104). Notice that this is a very different kind of worry than the one examined above. Here the issue is not the empirical fit between hypothesis and experience but rather a question of whether the so called 'neoclassical' theories of the firm are sufficiently 'close' to some core or essential model of neoclassical economics. In essence, a 'spot market', so defined is a central model (or paradigm) of what a neoclassical model should look like. The problem, as Kincaid sees it, is that the so-called 'neoclassical' theories of the firm may not be sufficiently similar to the spot market model for us to assume that whatever success these theories might have will constitute evidence for the unifying power of neoclassical economics.

What about IBCE? Kincaid sees similar problems here as well (Kincaid, 108). The basic idea again is that given two alternative hypotheses, H_1 and H_2, we may opt for H_1 on the grounds that it is the best causal explanation of the data yet H_2 may be preferable on other grounds; it may have greater predictive power, for instance. In such a case, IBCE turns out to be defeasible and hence not a 'foundational' principle (Kincaid, 110).

In the particular case of neoclassical economics, two crucial assumptions about "good explanations" are made (Kincaid, 111). First, it is assumed that "any outcome must be consistent with self-interested behavior". Second, it is assumed that "economic institutions and behaviors, so long as they result from a competitive economic process, exist because they are optimal".

The first assumption comes in 'thick' and 'thin' versions (Kincaid, 112). The 'thick' versions, which detail the range of goals and behaviors of agents are not so plausible and are tied to substantive assumptions about human behavior. The 'thin' versions, which do not spell out the range of goals or behaviors, are more plausible but do not favor neoclassical theories over alternatives.

The second assumption also comes in 'thick' and 'thin' versions (Kincaid, 114). The 'thick' versions again are substantive and less plausible as realistic assumptions about human behavior. The 'thin' versions are more plausible but do not favor neoclassical explanations since the details of real markets are often at odds with the characteristics of ideal neo-classical markets (Kincaid, 116).

Having lodged these criticisms, Kincaid offers two caveats (Kincaid, 116–7). First, these reservations are reservations about judging theories by appeals to explanatory power. They do not show that neoclassical economics is worse than the alternatives. Second, the reservations should not be read as suggesting that neoclassical economics has no virtues whatsoever. The main point, again, is that we cannot make blanket assumptions about *either* the explanatory power of any given theoretical approach for all applicable situations or its commitment to individualism. Each case has to be examined on its own merits.

4. An Analytic Framework

Note that much of Kincaid's analysis, or indeed any of the alternatives he rejects, hinge on a number of conceptual as well as empirical points. In order to address any of the deep methodological questions that he raises, he first needs to spell out exactly what 'model' of reduction, explanation, theory, etc., he is employing. That done, he can proceed to argue (as he does for the most part successfully) that questions of individualism versus holism so framed are indeed empirical questions (or raise empirical issues).

A general pattern of analysis and a meta-philosophical moral emerges from a consideration of Kincaid's strategy. In general, philosophical problems and issues have both empirical and conceptual components. In addressing these problems, one needs to disentangle the two sets of factors. This is complicated by the fact that the line between the conceptual and the empirical is not hard and fast but, in fact, often depends upon the conceptual framework one initially adopts (cf. e.g., Carnap 1950). The resolution of a philosophical problem thus hinges on a decomposition of the problem into its component parts.

The analytic framework of a Decomposition Analysis can be understood as an extension of a fundamental formula from the field of population genetics. The expression of the relationship between expressed organismic phenotypes, the underlying genetics and the ambient environmental factors takes the form of an equation, P = G + E + G x E, where "P" represents the expressed phenotype of an organism, "G" is the underlying genome of the organism, "E" is the relevant ambient environment and "GxE" is an interaction term that reflects the fact that the effects of the environment and the genetics are 'coupled'. The significance of this equation for the present discussion is three-fold. First, expressed phenotypes are the products of both genetic and environmental factors. Similarly, philosophical problems can often be construed as resulting from conceptual decisions and empirical judgments. Second, identical phenotypes can be produced by different combinations of genes and environments. The philosophical moral is that one cannot immediately infer from an observed phenomena what the underlying conceptual or empirical components are. Third, there is an interaction term. I take the analytic analogue of this to be the fact that in a philosophical dispute between proponents that accept alternative conceptual models, for example, no knock down drag out resolution of their dispute by an appeal to empirical considerations will be possible. The significance of the empirical data will be colored by the models one adopts to organize them. A similar point can be made in the other direction. The significance of particular models or conceptual formats will be colored by the 'facts' one chooses as anchors.

It is considerations such as these that complicate the philosophical morals that one draws from case studies of the sort that Kincaid analyses. For example, the disputes between the reductionists and anti-reductionists or between holists and individualists are not merely disputes about what the social facts are. They are also disputes about what are the proper conceptual frameworks to adopt. How are we to understand the concept of reduction, for example? What qualifies as a proper scientific explanation? What qualifies as a proper causal analysis?

What, indeed, is the aim and scope of theories in the social sciences? These questions are often construed as conceptual problems that need to be resolved in advance of deciding the merits of particular applications. A proper appreciation of the meta-philosophical significance of Kincaid's analyses shows that these hopes and expectations are naive indeed. How one resolves these disputes is a question in itself. I think that the answer is some form of 'pragmatic' equilibrium along the lines suggested by Quine and Carnap but I will not stop to pursue this further here (Bradie, forthcoming; Carnap 1950).

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