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On Choosing One's Intellectual Predecessors: The Reductionism of Camic's Treatment Of Parsons and the Institutionalists

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Four years ago, in the *American Sociological Review*, Charles Camic (1992) offered an intriguing and potentially important argument about the intellectual status of *The Structure of Social Action*. Expanding and refining an important series of works on Parsons, Camic (1987, 1989, 1990a, 1990b, 1991) suggested that the theorists with whom Parsons debated in that seminal work—Marshall, Pareto, Durkheim, and Weber—were “chosen” not because of their intrinsic theoretical relevance to Parsons’s project but because of “reputational” factors. He claimed that other social scientists—specifically, the American institutional economists—were intrinsically just as relevant, but that Parsons ignored these figures because their prestige had been eclipsed in the United States, particularly among powerful figures in Parsons’s “local” Harvard environment. Camic asserted that Parsons chose the Europeans as his ancestors—and, we would add, his debating partners—because they were much more powerfully reputed among those who controlled his fate.

If this analysis were correct, much of the current scholarship on Parsons’s theory of action would have to be rewritten. But the implications of Camic’s arguments go well beyond the relatively small group of Parsons scholars. If Camic’s argument about Parsons were correct, central perspectives in the historiography of social thought and in the sociology of knowledge would have to be abandoned as well. Camic’s research program is a *theoretical* one: Parsons’s work is adopted as an empirical exemplar to demonstrate the need for a radical reorientation not only of sociological theory’s beloved genealogies, but of the meaning and identity of the discipline itself. To confront Camic, then, means much more than confronting a particular interpretation of Parsons’s biography and intellectual corpus. It means, on the methodological level, to analyze the claims of a strongly historicist approach to scientific ideas and, on the theoretical level, to confront a subtle new version of an instrumentalist approach to the sociology of knowledge.

We believe that such a complex and sophisticated research program deserves much more attention than it has received thus far. We contend that Camic’s argument is fundamentally flawed, not only in the historical interpretation it proffers about Talcott Parsons but, more important, in the overall theory of intellectual formation upon which it relies and which it evokes.

In the following pages, we offer a critical summary of Camic’s research program, discuss the sociological importance of the topic of predecessor selection, and summarize Camic’s

historical reconstruction of Parsons's early development. We then analyze in depth Camic's own account of how Parsons's selection of his predecessors took place. We question—mainly through the use of Occam's razor—Camic's historical interpretation of Parsons's biography and work, but we also highlight the often hidden theoretical assumptions that underlie this empirical endeavor. By the end of this analysis, we hope to have demonstrated, *in corpore vili*, that the differences between contending sociological readings of the classics generally are not—and in the present case surely are not—caused by differences in historical accuracy but rather by differences in the respective approaches to social theory.

CAMIC'S RESEARCH PROGRAM: A CRITICAL SUMMARY

Since the late 1970s, Camic has developed an ambitious research program around Talcott Parsons's life, work, and theoretical heritage. Focusing mainly on the early phase, which culminates in *The Structure of Social Action* (1937; hereafter, *Structure*), Camic has produced historical contextualizations of Parsons's theoretical and methodological proposals (Camic 1987, 1989), editions of previously unpublished materials (Camic 1991), systematic anthologies of little-known texts (Camic 1991), and, most important, inquiries into the sources of Parsons's maturing theoretical ideas (Camic 1979, 1992).

Taken together, this material surely constitutes one of the most innovative lines of work in the current, very active reappraisal of Parsons's contribution to sociology and social thought. It presents itself neither as the usual hero-worshipping genre practiced by disciples nor as a critical attempt to reconstruct Parsons's project in order to re-propose it as a contemporary theoretical option. Camic is not sympathetic with Parsons's ideas, and makes clear, in fact, that he believes key dimensions of Parsons's work to be fundamentally wrong.¹ At the same time, Camic's approach is very different from earlier attempts to sociologize Parsons's sociology (e.g., Mills 1959; Gouldner 1970). Less inclined to enter openly into the discipline's contemporary politics, Camic is apparently impartial and scholarly. Ideology and political correctness are not immediate concerns if his inquiries; his control of Parsons's corpus and biography is impressively rich and detailed, and his theoretical models present themselves as grounded generalizations fitting the available evidence. In short, Camic declares his intention to treat Parsons's life and work as empirical data, to consider them with all the care that any scrupulous researcher should give his object of study.

What are the basic aims of Camic's research? Camic pursues both a critical assessment of Parsons's intellectual heritage and a sociological analysis of the processes through which this heritage was constituted (Camic 1989:39). He expresses his dissatisfaction with the current scholarship on Parsons in the clearest possible terms: It has produced "surprisingly little in the way of a broader appreciation of where sociology stands with regards to the arguments of *Structure*" (Camic 1989:39). Parsons's early contributions, Camic suggests, have been either neglected or treated superficially. Interpreters have employed a teleological perspective on Parsons's early development derived from the later, more mature writing (Camic 1991); decisive passages in the early works have been radically misconstrued because interpreters have failed to employ contemporary historiographical standards that emphasize contextualization (Camic 1987); the reconstructions of Parsons's early intellectual choices have been distorted by an inadequate account of the full range of alternatives he actually faced (Camic 1979, 1992). In sum, contemporary assessments of the early period of Parsons's development are a consequence of "collective amnesia about sociology's actual past" (Camic 1987:434).

¹ See Camic 1987:434–436; 1989:95. As far as *Structure* is concerned, Camic contends that "it holds its greatest present-day utility only when its original meaning is benignly overlooked" (Camic 1989:40).

However, Camic does not wish merely to correct and enlarge our historical understanding of Parsons's work and life. He views his studies, rather, as empirical efforts to validate the historicist position in the disciplinary debate that has evolved about the meaning and role of classical work (cf. Alexander 1987b). Placing sociological classics in the "vast historical fluidity of knowledge in a universe of groups and their diverse values, interests, relations and circumstances" (Camic 1987:436) is the only legitimate way of putting classics to use. In fact, the goal is not historical reconstruction *per se*, but rather the capacity to understand sociological works "sociologically."²

Camic's big fish is the sociology of knowledge. He defines his goal as producing case studies that focus "on the specific social processes that affect the formulation of a theorist's distinct methodological stance" (Camic 1987:421) and that illuminate how theorists' concerns are "structured by their social roles" (Camic 1979:517). Camic uses historical analysis as a tool to pursue this more general sociology of knowledge, which he identifies in the most instrumental, power-oriented, and conflict-constructed way. He suggests that theoretical works can be interpreted only in their socioinstitutional context, and that the latter is structured by the relentless quest for academic placement, status, and recognition.³

There is, finally, another goal of Camic's endeavors, one that is significant, if less overt. He wishes to advocate a radically different conception of the nature of sociology from Parsons's own. To be sure, Camic does not want to expose the shortcomings of Parsons's understanding as such; nor does he wish—as the earlier wave of critics did—to substitute for Parsons's ideas theories of his own. Nonetheless, over and above the particular objections to this or that part of Parsons's theorizing, Camic strenuously objects to Parsons's effort to develop a common vocabulary for sociology at large. The problem with Parsons's presentation of his intellectual development, and contemporary presentations as well, is that they have successfully imposed a "pre-emptive suppression of methodological alternatives, intellectual legacies, conceptions of action and social structures and personality, approaches to the causes and solutions of the problem of order, and perspectives on human voluntarism" (Camic 1989:95). By concealing the fundamental "social variety" of the world (Camic 1987:434), Parsons gave to his theories "a peculiarly self-referential quality, [a] tendency to regard the ideas he personally encountered in local surroundings as those ideas most worth engaging and vesting with epochal import" (Camic 1991:xii). By deconstructing Parsons's work, by showing how socially contingent and how historically bounded his intellectual selections actually were, Camic proposes his alternative vision. He believes that we must abandon any vision of sociology as a coherent (even if decidedly nonconsensual) intellectual discipline and see it rather as a kind of catalogue. Instead of attempting to develop a generalized theoretical logic, we must resign ourselves to the reconstruction of historically dependent concepts and learn not merely to accept but to advocate the equal validity of theoretically irreconcilable lines of argument.

These arguments form the main lines of Camic's research on Parsons's early life and work. They also form the warp and woof of his 1992 paper on the mechanism of predecessor selection, which may in certain important respects be taken as the most systematic statement

² Critics have noted the fuzziness in Camic's definition of "sociological" (Tiryakian 1990:452). Unfortunately, this critique has not received from Camic an adequate answer: "He [Tiryakian] asks what I mean for sociological, assuming (as Parsons himself) that the desideratum is a fixed definition that rules some things in and some things out. As my paper should have made clear, this is not my point of view. . . . The history of sociology presents (to quote Levine) 'a gallery of sociologies' and definitions of sociological are context dependent" (Camic 1990b:458). As we will see in the discussion of his most recent paper, however, our problem is not so much in the fuzziness of Camic's definition of "sociological" as with its reductionism.

³ Camic's essays do include occasional sentences pointing in the opposite direction. However, these ambiguities, as we show below, are more attempts to overcome the intrinsic weaknesses of reductionism by the resort to residual categories than real theoretical openings.

of Camic's research program. Our analysis of this particular paper on predecessor selection, then, should be considered in a representational way; it applies, *pari passu*, as much to Camic's larger research program as to the particular work whose details we take up here.

THE IMPORTANCE OF PREDECESSOR SELECTION PROCESSES

The topic of predecessor selection can hardly be considered a small one. All significant intellectual work stands in complex relationship with a set of authors and problems, with what can loosely be called a "tradition." Any significant work is impregnated by past contributions, and any researcher sits "on the shoulders of giants" hoping to see farther and better.⁴ This connective process, however, unfolds in sharply different ways from the cumulative mechanics so praised by positivists. Post-Mertonian sociology of science, and intellectual history more generally, have long since demonstrated that relationships with intellectual predecessors are creative ones: To relate to an intellectual tradition means always to interpret it. Readings of earlier authors and their problems change, often dramatically, in the light of new *Problemstellung*. Similarities are discovered between authors once considered diametrically opposed, just as differences are highlighted between authors once considered to have been intellectual bedfellows. Different strands—or "phases" or "works"—of the same *corpi* are played one against the other. This creative appropriation is highly consequential, for the reception of an influential intellectual work can radically alter the intellectual space provided by extant traditions, making new alternatives available to contemporary scholars and to emerging cohorts alike.

Camic is surely right, therefore, in pointing out the need for a better understanding of the predecessor selection process. He is also correct in insisting that to ignore predecessor selection is to miss an important resource for understanding the construction and evolution of disciplines. Whether he provides an adequate response to the problems to which he draws our attention is, however, another question.

Camic's point of departure is the analytical distinction between what he calls the study of "intellectual influences"—the general milieu of experiences and ideas within which writers are socialized—and what he rather ambiguously terms "predecessor selection," a process, apparently intentional, through which a writer identifies preceding authors as explicit reference points.⁵ Concentrating on the latter element, Camic believes that, although the identification of a theorist's *intellectual* predecessors is indeed a concern in current scholarship on sociological theory, it is usually based on a narrow understanding of the social processes involved. A vast majority of contemporary scholars, Camic believes, ascertain predecessors by looking simply at the relationship between the ideational content of the writer under scrutiny—most particularly "the thinker's own statements" (1992:423)⁶—and the ideas of others whom the writer has quoted and referred to.

This model, where the relationship between scientist and predecessors is established mainly on the basis of intellectual content, Camic calls the "content-fit model." According to Camic, this model, although hegemonic, suffers from serious weakness, both substantive and methodological. It ignores the variability in the "historical and socioinstitutional

⁴ Merton (1965) has demonstrated in loving and copious detail how this metaphor—standing on the shoulders of giants—has been reconstructed continuously over centuries, acquiring new meanings in the process.

⁵ Camic's definition is ambiguous because, although the term "selection" seems to imply a conscious intention, the issues of consciousness and intentionality are, as we will see, some of the principal points Camic glosses over. The complexity of the very concept of "intention" in this regard is well illustrated by the thoughtful and very revealing essay on intellectual historiography recently issued by Jones and Kibbee (1993), which can be read as an autocritique of the kind of narrow reading of intention stressing conscious intent that had earlier informed Jones's work, and which continues to inform Camic's work today.

⁶ Unless otherwise cited, henceforth all page numbers refer to Camic 1992.

circumstances,” severs the “predecessor selection process from its socioinstitutional context,” and neglects “the possibility of predecessor choices not based primarily on content” (p. 423). Methodologically, the content-fit model makes use only of the thinker’s own statements “as the primary or exclusive evidence” and fails to “investigate adequately the dark side of predecessor selection,” by which Camic means that there may be other potential predecessors whom the thinker did not choose (p. 423–424).

Because of these weaknesses in the content-fit approach, Camic proposes an alternative model, concentrated not on ideational content but on “socioinstitutional circumstances,”⁷ circumstances that highlight the role and force of reputational mechanisms. The operation of these mechanisms, according to Camic, is actually hidden by using the content-fit approach, for the latter concerns itself mainly with the explicit and self-established links expressed by the author. By accepting these statements at face value, the scholar does not take adequately into account the “published work prior to publications, unpublished papers, course materials, letters and so on, during the time [the author’s] ideas were working out” (p. 422). Because explicit statements are “a poor indicator of the bases on which [the author] made these selections and decided to exclude other figures from among his predecessors” (p. 422, n. 2), they hide the reputational factors that have been the true causes of predecessor selection in sociology—“during much of its history” (p. 439–440) at least. Camic offers the historical case study of Parsons as proof.

PARSONS, THE INSTITUTIONALISTS, AND THE MECHANISMS OF PREDECESSOR SELECTION

Camic starts his case study by summarizing *Structure’s* “substantive-theoretical argument,” referring to one of his earlier papers for a discussion of its methodological claims (Camic 1987). He decomposes *Structure’s* substantive-theoretical argument into two interrelated components: the critical (the attack on utilitarianism) and the constructive (the relevance of social components of the personality, ultimate values, the definition of society as a reality *sui generis*, the relevance of regulatory ethical rules).⁸ Camic then posits that “nearly all interpretations of this famous theoretical argument have been conjoined to historical accounts of the process by which Parsons worked out his position, accounts that accord central significance to Parsons’s relationship with the four turn-of-the-century European predecessors identified in *Structure*” (p. 426). According to Camic, there are two capital sins in such interpretations: the assumptions either that *Structure* was a synthesis of the predecessors’ distinctive theoretical stances or that Parsons had been attracted to these earlier positions because of the similarities between their problems and his own. As a result, these interpretations have assumed that “Parsons did not draw upon” other earlier work—“of which he was aware”—because this “work [was] deficient in content-fit and therefore not pertinent to the project” (p. 426). But this assumption, Camic asserts, is nothing but a self-deception: Accepting at face value *Structure* and later retrospective accounts by Parsons himself, scholars have failed to investigate the actual selections Parsons made while *Structure* was just getting underway. If enough documentary materials are studied carefully enough, Camic argues, a very different story about predecessor selection can be told.

Which story? It is well known that Parsons had been exposed during his undergraduate years at Amherst to the influence of two teachers, Walton H. Hamilton and Clarence E. Ayres, both prominent figures in the second wave of institutional economics. This move-

⁷ Cf., p. 423. This alternative is elsewhere defined as “social-organizational” (p. 439) or, more simply, “social” (p. 433).

⁸ In this paper, we cannot treat adequately the peculiarities of Camic’s reading of *Structure*. A detailed and powerful confrontation with Camic’s thesis may be found in Gould’s essays (1989, 1991).

ment, which originated in Veblen's attack on neoclassical economics, stressed the limits of utilitarianism, criticized approaches that appealed to biopsychological forces, recognized the role of noncommercial incentives in economic behavior, paid cultural factors their due, and stressed interconnection and interdependence as valuable components of any social explanation (p. 429). In short, according to Camic, in "oppos[ing] utilitarian views of action," institutionalist ideas paralleled the general orientation of *Structure* in significant respects (p. 430). Camic stresses, moreover, that during his undergraduate years Parsons was deeply involved in institutionalist ideas and later in his career acknowledged that Hamilton and Ayres were the principal agents of his conversion to the social sciences (e.g., Parsons 1976). But Camic goes much further than simply stating that the institutionalists' ideas could easily fit the *Structure* project. He argues that, in fact, they were even more fit for this project than the ideas of the European foursome Parsons actually chose (p. 431).⁹ Why, then, did Parsons not encompass them in his postmaturing professional work, paying them only marginal attention instead?

Here Camic's alternative model takes center stage: It was not a matter of content but of reputation. Beginning in the 1920s, institutionalists had an increasingly negative reputation in the academic world, for they were widely perceived as on the losing side in the ongoing conflict with neoclassical economics. Moreover, unlike institutionalist ideas, the neoclassical approach was deeply entrenched in the more prestigious academic institutions and leading departments. These considerations applied even more strongly to the Harvard environment that Parsons entered in 1927 as an instructor of economics. Harvard's leading figures were very critical of the institutionalists. At Harvard, Camic argues, "to attach one's ideas to those of the institutionalists was to ally oneself openly with a losing cause" (p. 435). Moreover, within the same Harvard intellectual network, Marshall, Pareto, Durkheim, and Weber—the foursome who were to form the core of *Structure*—were considered positively, or at least not considered in a negative light. Marshall and Pareto, of course, were major neoclassical thinkers. Durkheim was becoming a figure of major interest, and Weber was already held in high regard. In short, and here Camic enters into the more detailed brief for his alternative view, Parsons was "part of a well-signposted intellectual network that warned him of the defectiveness and uselessness of some lines of work while announcing the greatness, brilliantness [sic] and fruitfulness of other lines" (p. 437). Camic goes on to explain in some detail why he believes this signalling to have been theoretically wrong-headed, and he insists that institutionalism still has an important theoretical value (p. 438). Yet Camic's paper is less about vindicating institutionalism than about demonstrating the validity of his broader instrumentalist approach to the critical role that reputational networks play in the construction of science. With certain reservations, discussed below, it is upon this reductionist approach to the sociology of knowledge that Camic ultimately makes his case.

CAMIC'S SOCIOLOGY OF SCIENCE: VARIATIONS ON A FAMILIAR THEME

Although Camic presents his constructions of the two modalities of predecessor selection as something new in the debates that surround the history of social thought, and in debates about the thought of Parsons in particular, the contrast merely supplies a new name for the venerable dispute between internalists and externalists that has been at the center of discussion in the history and philosophy of science for the last half-century at least. Naive

⁹ On this point, Camic seems to point especially toward Pareto (p. 438; see also Camic 1987). Perhaps precisely because he refuses to confront analytical theory seriously, Camic seems unable to understand what Parsons did find of interest in Pareto.

internalists take every thinker at his word; assuming the truth and legitimacy of his or her predecessor-construction, they devote themselves primarily to reconstructing and interpreting the theorist's own rationales. Externalists typically make the opposite mistake. Assuming the ideational contents of a work to be unproblematic, they devote themselves to finding the so-called social—organizational, economic, or political—factors that “caused” the thinker to adopt such ideas.

By focusing on such factors as the contrasting reputations of Parsons's intellectual reference groups, Parsons's job market prospects, and the status structure of Parsons's local organizational environment at Harvard, Camic obviously takes the externalist position on Parsons's predecessor choice. We believe that his explanation of the interpretative shifts in Parsons's work suffers accordingly. External factors can never independently account for theoretical choice. Choices are certainly affected by external factors, and they are to some extent strategic; but, also, they always involve intentions in a broader sense, intentions that rest upon subjectivities.

This broader theoretical subjectivity must be explored. If we consider how theorists choose their predecessors, their colleagues, or even their enemies, we can understand subjectivity as supplying “selection criteria” that filter external factors. How can one study choice without studying criteria? Yet, we argue, this is precisely what Camic tries to do.

THE ARGUMENT AND ITS SELF-CONTRADICTIONS

Any criticism may be sustained through systematic argument (see, e.g., Alexander 1983); through discussion of what we view as alternative and more successful approaches to the study of classical figures (e.g., Wearne 1989 on the early Parsons; Taylor 1975 on the early Hegel); or through a disciplined confrontation with the empirical case study at hand. In what follows, we engage in each of these forms of criticism, but we initially pursue a different, more textual approach. We point out how the limitations of externalism produce debilitating contradictions in Camic's own argument. For the uncomfortable truth is that Camic cannot himself maintain the integrity of his reputational model. He abandons it in his most critical arguments, and takes up the very alternative his paper is intended to refute.

Camic's central claim, occasional ambiguities notwithstanding, is that Parsons ignored the American institutional economists in *Structure* and turned instead to the Europeans for one reason only: The former's reputation was too negative to help Parsons in his early efforts to establish his career, or at least to produce “a credible theoretical statement” (p. 437). This central claim we call Hypothesis 1 (H1), and we return to it in the closing pages of our discussion. This central claim, however, rests upon a secondary one, which we call Hypothesis 2 (H2): Institutional theory would have provided just as viable, as useful, and as complementary a platform for the theory that Parsons constructed in *Structure* as did the European theories with which Parsons actually engaged. It is upon the validity of this secondary claim (H2) that Camic's reputational argument (H1) rests. If H2 is refuted, H1 is decisively weakened, although it would still remain necessary to assess the reputational factor in and of itself. We must first devote ourselves, then, to Camic's secondary claim.

Our initial argument against H2 is to demonstrate that in his effort to verify it Camic employs the very methodology that he is engaged in trying to refute, namely, that of content fit.¹⁰ Camic devotes much of his case study to an extensive, highly internalist interpretation of the similarities between the ideational contents of *Structure* and those of institutional economics. In fact, in his introduction to this discussion he goes so far as to acknowledge

¹⁰ Here are Camic's own words describing the empirical case study upon which H2 rests: “[T]his example involves a review of the content of *Structure* and an examination of its fit with the content of American institutional economics” (p. 424).

that "content factors cannot be excluded when examining Parsons' selections[,] [for] thinkers with solid reputations for work whose content was obviously irrelevant were outside the range of possible predecessors for him" (p. 436). Only after spending many pages to assure his readers that a content fit does exist does Camic discuss the impact of reputational factors on Parsons's predecessor selection.

Judging from such perlocutionary actions rather than formal illocutionary aims, one can only conclude that Camic actually believes that it is ideational congruity—content fit—that sets the agenda for arguments about the nature of predecessor selection. He would seem to be suggesting, in spite of himself, that analysts should begin by investigating content fit. Reputational factors are, perforce, reduced from primary to secondary considerations. They cannot explain the general field of selection, but only why, within an already highly restricted field, one rather than another viable predecessor was chosen. Once again, it is Camic himself who puts this argument very well, asserting that only "when it came to work not ruled out by this broad criterion [of content fit], [was] reputation . . . a decisive differential" (p. 436) for the early Parsons.

How could the substance of Camic's argument so contradict its formally stated aims? In fact, such self-contradictions are not at all infrequent in theoretical argument. They often reveal strains produced by unresolved, and unresolvable, theoretical claims. That Camic commits such a self-contradiction should be attributed not to a personal failing but to the inadequacies of the externalist position he seeks to uphold. In a certain sense, in fact, it is a testimony to Camic that he allows this contradiction to surface. His commitment to the historical materials he has uncovered, and his highly ambivalent yet still substantial sense for the integrity of Parsons's early project, lead him to insist that such ideational parallels in content can and should be found.

THE RESIDUAL CATEGORIES

The best way to control for an interpreter's claim that a text is contradictory—our claim here—is to find statements indicating that the author is aware of these contradictions himself. Camic's text is helpful in this regard. It is clear that Camic senses the hole that he has dug for himself. He tries to dig himself out in two ways. First, immediately after the introduction to his discussion of content fit, quoted above, he issues a *mea culpa*: "This discussion of the topic of content fit should not be viewed as contradicting the argument just made *against* the current use of the content-fit model" (p. 424, italics in original)! It is difficult to understand what "should not" means, for in the discussion that follows this statement, which sits at the heart of Camic's paper, Camic himself contradicts the argument against the content-fit model he has just made. Certainly, he "should not" have done so, but we are glad he did, for it is our claim that content references simply have to be made.

The manner in which Camic illustrates the logic of this *mea culpa* reveals the difficulties he has created. "Just as studies seeking to challenge class-based explanations of educational attainment require a careful examination of class factors before the effect of these factors can be qualified," he writes (p. 424), "so here I examine content-fit factors to demonstrate empirically the deficiency of the content-fit model and the need to supplement it with an alternative approach." We respond, first, by noting the linguistic anomaly: It is hard to see how an approach diametrically opposed to an alternative can be understood as being supplemented by it. More important than the linguistic slippage in this statement, however, is the logical fallacy the syllogism involves. Camic is not merely "examining" the alternative, content-fit model; he is incorporating it as a central part of his explanation. Challenges to class-based explanations of education do not begin by arguing that class factors actually provide the most important, indeed, the framing factor in explaining it!

This admission *faute de mieux* adumbrates the second move that Camic makes to dig himself out of the hole, an effort that only serves to dig him in deeper still. In the concluding section of his paper, Camic begins to speak of the two models not as theoretical alternatives, one of which is right and the other wrong, but as equally necessary empirical strategies whose employment depends merely on the nature of the facts at hand. Introducing the notion of “mutual dependence,” he puts forward the thesis that the relevance of reputation versus content is empirically contingent on the organizational characteristics of intellectual disciplines. If a discipline is only loosely organized, he suggests, an original work will not need to “content fit,” and reputational factors will count for more. In more tightly organized disciplines, however, fitting with the contents of predecessors is more important, and reputation will count for less.

No doubt this is sometimes the case. Camic does not seem to realize, however, that his own study has demonstrated something quite different. Writing about a period of only loose disciplinary dependence, he has shown that reputation analysis must be subordinated to content-fit models—that it is the latter that must be used as the principal frame.¹¹ At any rate, each of these positions—mutual dependence, empirical variation, subordination—contradicts the strong claim for reputation with which Camic’s paper began and upon which his broad and ambitious claims for a new sociology of knowledge relies.

It is difficult to understand how the central argument of Camic’s essay could be allowed to change its meaning so fundamentally in such a short space. Our explanation is that the theoretical logic of the originating argument was impossible to sustain. As a result of the tensions generated by an untenable position, Camic’s reputational model wavers between the theoretically significant claim that it may account for predecessor selection as such and the much more modest allusion to its utility in explaining a subset of choices among a larger set of predecessors that has already been selected on entirely different grounds.

SELECTION CRITERIA: THE MISSING FACTOR

H2 asserts that institutional theory would have provided a viable theoretical reference for Parsons’s foundational work. Our first argument against H2 has been indirect; we have shown that in trying to sustain it Camic has employed the very method, content fit, that his entire exercise is designed to dispute. Our second argument against H2 is more direct. We argue that Camic has not demonstrated that the contents actually fit at all. Camic’s interpretive method, we suggest, is not up to the hermeneutic task at hand. He is using blacksmith’s tools for a carpenter’s job.

Indeed, if “content fit” is an appropriate goal for comparative cultural analysis, it can hardly be an appropriate means, or method; it is simply too general and vague. Only the most casual intellectual historians have ever suggested that a fit between categories as broad as “the common moral foundation of human action” or the “limits of theories based only on self-interest” (phrases Camic invokes in the section from pp. 424–430, *passim*) can provide the basis for evaluating intellectual influence.¹² It is necessary to point to the

¹¹ It could still be argued, of course, that while Camic’s own analysis of Parsons emphasizes the primacy of content fit, Parsons himself did not use this approach to choose his predecessors. It seems highly implausible, however, that Camic should rely so heavily on content selectivity while arguing that great social theorists themselves either cannot or do not avail themselves of the same technique. Insofar as Camic proposes these techniques as scientific methodologies, he is contending that they correspond to strategies intellectual actors themselves employ. To suggest anything else would be enormously condescending.

¹² Such an assertion reminds one, in both form and content, of the spurious claims that Nisbet made, time and time again (e.g., Nisbet 1966), for the conservative origins of Durkheim’s work. Nisbet justified these assertions by pointing to the fact that such terms as “morality” and “integration” appeared not only in the work of conservatives like De Maistre and but in Durkheim’s work as well. With such broad brush strokes, he ignored the

intervening factor of cognitive selection. But this intervening factor is precisely what is lacking from Camic's account.

Social scientists and theorists are agents. They possess, protect, and nurture subjectivities that allow them to select, consciously and unconsciously, among the internal and external pressures of their environments, whether these be the reputations of other scientists or the idioms of broad cultural traditions. Some simple thought experiments allow us to see just how important these selection criteria turn out to be. For example, one has many colleagues, both locally and elsewhere, whose reputations are attractive and high. Just so, there is a wide range of broadly congenial "ideational contents" from which to choose. The critical issue, then, is not selection *per se* but the criteria of relevance one applies. It is the latter that must be a primary topic of inquiry if actual predecessor selection is to be understood.

Who would deny that, painted in broad brush, there are many striking similarities between some of Parsons's ideas and some of the institutionalists? There are also striking similarities between Parsons's ideas and those of many other important thinkers such as Comte, Hegel, Tocqueville, or Troeltsch. Camic himself points to Hobhouse and Laski as equally likely candidates for the identity of excluded predecessor (p. 439), going so far as to suggest that, because they too were thinkers with whose ideas Parsons was familiar, they should not have been ignored!

This is a game everyone can play. As Zeitlin (1968) and Gouldner (1970:150, 185–189) once asked in outraged tones about Parsons's canon, why not Marx? As Levine still asks, why not Simmel?¹³ The game is too easy. Not only can everyone play but, actually, everybody has.¹⁴ Thus, our first response to Camic's argument for content fit on the basis of broad similarities is to stress the importance of differences. Simmel, after all, departed in fundamental ways from Parsons's agenda (Alexander 1993; Sciortino 1993). Why should the American have built his theoretical base, or legitimated it, vis-à-vis the German thinker's work?

Our second response cuts deeper, and moves more directly to the issue of the selection criteria themselves. One cannot argue for the plausibility of including or excluding this or that thinker in the interpretative (re)construction of predecessors merely on the basis of this or that piece of content fit. Every work has similarities and differences with every other. Beef soup and vegetable soup have many similarities, meat apart. Yet, for those who have any developed sense of taste, it is precisely the absence of meat that matters. In the end, it is not the broad category of the work but the selection criteria that matters. We must be prepared to analyze the intellectual development of the thinker in a close and detailed way in order to understand the theoretical interests that result.

Structure was never meant to be, and was not presented as, an exhaustive survey of the contemporary literature in European or American social theory. It was presented as a precise statement of theoretical axioms developed in relation to the work of a small number of authors whom Parsons considered the most significant for his theoretical aims. It is active

fact that such concepts also appeared in the works of Enlightenment figures. This was precisely the point of Seidman's (1983) important early work, which focused much more precisely on the actual theoretical and ideological intentions of the social theorists themselves.

¹³ See Levine ([1957] 1980). A coordinated claim is actually common to Levine and Camic. Both impute to Parsons's *Structure* a responsibility for the later reputational misfortunes of their preferred earlier theorists (Sciortino 1993). By removing them from his creative 1937 work, so this argument goes, Parsons concealed them from the attention of later sociologists. However, both Camic and Levine admit that the influence of the pre-Parsons theorists they prefer was decreasing long before *Structure* was written, and both acknowledge—indeed, insist—that neither *Structure* nor Parsons himself was influential among sociologists for quite a long time after 1937.

¹⁴ Indeed, this is not the first time that Camic himself has played this game. His first published essay on Parsons's *Structure* was devoted to asking, why not the true Utilitarians (Camic 1979)? Arguing that Adam Smith surely qualified for the criteria Parsons had laid out for inclusion, Camic thus committed the same mistake—a disregard for Parsons's analytical intentions—that he was to repeat in the later, more historicist work we are considering here.

consideration that is precisely the point. Camic suggests that a work may be considered likely predecessor material simply if “the thinker was aware of relevant previous lines of work and [if] the quality of this *work met contemporary standards for publishability*” (p. 423, italics added). But this standard is absurdly broad. To ascertain the relevance to an intellectual of preceding work, we must ask a question more like the following: When Parsons was preparing *Structure*, what did he think he was doing, and why?

One way to answer this question might be to find out what Parsons actually said about the institutionalists, in his pre-*Structure* papers, in *Structure* itself, and in his later phases—to examine, on other words, the statements that Camic either studiously ignores or under-rates. The first, and most crucial, thing Parsons said was that the institutionalist position was untenable because it confused analytic and concrete approaches to social facts (1937:122, 125).¹⁵ Why was this distinction important to him? Because, by this time in his development, Parsons was a neo-Kantian, concerned with producing an analytic, structured, quasi-a priori framework for theoretical analysis. This was indeed one of the bigger tasks of *Structure*, as Camic (1987) himself has clearly recognized.

Whether or not this was a good choice is not our concern here. Maybe Parsons would have benefited from a more open attitude to Pragmatism and historicism or, indeed, to empiricism. The fact is that he did not make this choice. His selection criteria were different in decisive ways.

Looking at what Parsons actually said about the institutionalists also demonstrates that Camic is wrong to suggest that Parsons “never recognised any substantive similarity between his approach and that of his two teachers” at Amherst. In fact, Parsons stressed several times that some of the institutionalists’ critiques of neoclassical economics were sound,¹⁶ and he always recognized the relevance of institutionalism in his intellectual formation (Parsons 1959, 1970, 1976). At the same time, on these occasions and others, he also remarked that, in his opinion, institutionalism had taken the wrong path. Institutionalists had argued against utilitarianism in an empirical way, whereas in *Structure* Parsons was determined to make an analytical critique. Although institutionalism advocated sociology as an encyclopedic science of holistic phenomena, virtually every section of *Structure* is intended to demonstrate the limits of this conception, and to suggest, in contrast, the need to differentiate sociology analytically, not empirically, from the other social sciences. The institutionalists had, in a sense, done the right deed for the wrong reasons, and reasons are what *Structure* is all about.

Camic, in fact, may be aware of such possible consequences; if so, the ad hoc statements that appear at various points in his essays are attempts to ward them off. As we have seen, in the very beginning of his discussion of Parsons’s intentions in *Structure*, Camic introduces a distinction between “substantive-theoretical” and “methodological views,” including in the latter Parsons’s discussion of the “analytical” problem. Because he has dealt with the

¹⁵ E.g.: (1) “I do not consider institutionalism a genuine solution of Marshall’s predicament, while I do so regard Pareto and Weber [. . .] Insofar as they do not repudiate theory altogether, which is fatal, they tend to fall back into ‘psychologism’ and ‘survivalism,’ which Marshall successfully avoided” (Parsons [1932] 1991:87); (2) “The institutionalists’ repudiation of the conceptual tools of orthodox economic theory is an excellent example of this [confusion between analytic and concrete]. Though often empirically right in their criticism of conclusions arrived at by the use of these concepts, they are none the less disastrously wrong on a theoretical level in failing to see the possibility of avoiding these consequences by using the same tools in the context of a different conceptual framework” (Parsons 1937:125). (3) Thirty-nine years later, in a contribution to a Festschrift for Clarence Ayres, a leading Institutionalists (see below), Parsons made exactly the same point: “I think, in retrospect, that I had two major theoretical objections to the institutional point of view. The first was that, in the name of a generalized radical empiricism, it denied the legitimacy of analytical abstraction—a conception I found running through all of the principal authors whose work was treated in my [1937] study” (Parsons 1976:178).

¹⁶ Again, e.g.: (1) “These [critiques] do not mean of course that many of the Institutionalists’ criticisms of the orthodox, especially the more dogmatic [neoclassical economists], are not well taken” (Parsons [1932] 1991:87); (2) “There can be no doubt that in large measure these unorthodox theories are empirically right” (Parsons [1935] 1991:212); (3) “Thus, in a *positivistic* context, there is a good deal of truth in the ‘institutionalist’ charge that orthodox economic theory is logically bound up with hedonism” (Parsons 1937:122, original italics).

former issue in another publication, Camic writes, he will not deal with it here, except when "these methodological issues . . . bear directly on the analysis" (p. 424). This move, understandable as it may seem for theoretical economy and simple consideration of space, is, however, highly problematical. First, it makes invisible the fundamental and crucial difference between Parsons and the institutionalists, a difference, after all, that is the very *experimentum crucis* of Camic's paper. Second, this invisibility of what Camic calls the methodological issue in *Structure* obscures the relationship between the institutionalist position and what for Parsons were the book's real theoretical stakes. For Parsons, "analytic" was not only an epistemological position, but also a kind of flag that stood for just about everything important he believed at the time. Deeply affected by the neo-Kantian movements in Germany and France, which were fundamentally removed in both spirit and empirical thrust from American pragmatism, Parsons had become opposed to the very philosophy that inspired the most important American social theory. In these terms, the apparently "mere" epistemological issue was strongly connected with a substantive thrust. It suggested for Parsons not only the importance of morality *sui generis* and religiosity, but also the whole range of methodological and theoretical tools that would allow one to produce empirical explanations of these phenomena in a comparative and historical frame, without having to give up the rigorous and cumulative character of scientific constructs.¹⁷ Whatever the theoretical problems such an overemphasis produced, this very conceptual inflation suggests that ignoring Parsons's invocation of "analytic" can have disastrous consequences for interpreting the rationale of his early work and the predecessor selection involved.

These were some of Parsons's theoretical interests, his criteria of theoretical relevance for choosing the predecessors with whom he would engage. Given these presuppositions, in our view, it cannot seriously be suggested that institutionalism could have provided the same kind of intellectual resources to answer Parsons's questions as did Durkheim's religious sociology or his study of suicide, much less Weber's comparative studies of the world religions.¹⁸ Yet, even if one disagrees with this judgment—and Camic, characteristically, both does and does not—any serious appreciation of the more general orientation from which Parsons viewed the world leads one to understand why Parsons would have rejected the contributions of the institutionalists in a very emphatic way, on intellectual grounds alone. It was not their reputations he objected to but, as we will see more clearly in the next section, their ideas.

SPECULATION FROM FRAGMENTARY EVIDENCE: DISTORTING PARSONS'S INTELLECTUAL CAREER

It should be clear by this point how evidence about Parsons's dissatisfaction with institutionalism—the lack of content fit between his emerging ideas and theirs (H2)—seriously undermines the claim (H1) that reputation rather than ideas inspired Parsons's attachment

¹⁷ In terms of theoretical logic, indeed, Parsons often employed the term "analytic" in an overly broad way (Alexander 1983). At various times, especially in his earlier work, he conflated the Kantian metamethodological commitment with other levels of the scientific continuum in an almost talismanic manner. Parsons attributed Weber's overrationalized understanding of late modernity, for example, to the fact that he had adopted an ideal-typical rather than analytical approach to bureaucracy.

¹⁸ Durkheim and Weber, of course, were leading representatives of the neo-Kantian revival in their respective milieux. It is interesting that in his brief 1976 retrospective on Ayres, Parsons insists at two different points that the institutionalist had not prepared him to understand Durkheim's religious writings, which, he notes, had in fact been assigned to him as an undergraduate by Ayres himself: "I shudder to think how little I understood about what Durkheim was trying to do in those first readings from his work [. . .] In my earlier days at Harvard I was thus exposed . . . on my own to Emile Durkheim, to whose work I had originally been introduced by Ayres, but understanding of whom required far more study than I devoted to it in my undergraduate days" (Parsons 1976:175, 178).

to the European predecessors he eventually chose. Before considering H1 more directly, in fact, we examine one final piece of evidence that Camic invokes to demonstrate H2. This concerns the impact of Parsons's attachments to institutionalism in his undergraduate years. By this path, we will be led, finally, to the central reputational claim itself.

Camic emphasizes that Parsons was deeply affected by his undergraduate courses with Walton H. Hamilton and Clarence E. Ayres, and that Parsons decided to move from the biological to the social sciences as a result of these experiences. On the basis of these early connections and influences—to which Parsons himself freely attested (e.g., 1959, 1970, 1976)—Camic argues that institutionalism must have had a continuing effect on Parsons's thinking in the years between Amherst and *Structure*.¹⁹

This does not seem to us highly compelling evidence. Although Parsons no doubt was a highly precocious 21-year-old, must we insist the selection criteria he employed at that tender age remained intact for another decade hence? Many social scientists “discover” social science in their initial college courses. How many feel themselves compelled to forever evaluate relevance according to the approaches they learned on those occasions, despite the fact that they may continue to agree with their general claims? Such a thesis posits an imprinting factor stronger than any of those identified in Konrad Lorenz's geese!

In fact, the evidence Camic quotes, the detailed biographical information now available on Parsons (to which Camic himself partly contributed), and the essays Parsons published before *Structure* (which Camic himself has rendered easily available) tell us something quite different, and they amount to a not uncommon story. A brilliant student initially exposed to a compelling stream of social theory discovers quickly that there are other compelling theories available, and decides, over the course of a few years, to opt for what he now thinks to be a more sensible one. This maturing student, in such a situation, may not reject some of the general ideas associated with the theory of his early youth; he may retain and refine an interest toward some topic associated with the former approach. Yet, in his later work, after acknowledging that “the controversy between the orthodox and the institutionalists has played a decisive part in the development of the discussion” in which he was interested, he may decide that “the present discussion must be concerned with pointing to satisfactory ways out rather than warning against false paths” (Parsons 1932:87). In light of available evidence, we would suggest that this reconstruction seems more likely to survive Occam's razor than Camic's reputational account.

Indeed, Camic produces in this paper no evidence even for Parsons's enthusiastic endorsement of institutionalist ideas after he left Amherst. On the contrary, he acknowledges that Parsons appears to have had fundamental misgivings about institutionalist ideas long before the idea of *Structure* ever had crossed his mind: Parsons “went to Harvard in 1927 . . . to study” (p. 434) with the neoclassical economists who were the theoretical enemies of his former Amherst teachers. Before going to Harvard, in other words, Parsons was already persuaded that his prior economics training was inadequate, indeed “way off of the main track” (Parsons 1959:5).

We have finally come to Camic's central contention (H2). Do not these last pieces of biographical evidence, introduced by Camic himself, run directly counter to his critical claim that it was the impact of Harvard and its neoclassical economists' reputation that induced Parsons's turn away from institutionalism? Even Camic's own selective reconstruction of Parsons's progress makes clear that the young theorist's negative judgment of institutionalism had been formed before, not after, Parsons entered into the reputational

¹⁹ On the one hand, Camic acknowledges that “it would be absurd to question the selection of Weber and Durkheim, which the subsequent history of the discipline has strongly vindicated” (p. 438). On the other hand, he argues that “European figures like Durheim and Weber were by no means his [Parsons's] only options from the standpoint of a fit in intellectual content” (p. 438). Both statements cannot be true.

networks of his Harvard years.²⁰ Indeed, there is very good reason to suspect that Parsons selected that network according to his intellectual interests, rather than having his relevance criteria shaped by it. Camic writes as if we should be astonished that “Hamilton, Ayres, and the other younger institutionalists were not even encompassed by Parsons in his work in the late 1920’s and early 1930’s” (p. 430, italics added). To the contrary, we would be astonished if they were. Our suspicion that Parsons may have possessed, even at that early age, a capacity for independent intellectual choice (see Wearne 1989) is reinforced when we consider the impact of the character-forming experience that Camic inexplicably leaves entirely out of his biographical reconstruction. We refer here, of course, to the two years that Parsons spent as a student in European universities, first in England, then in Germany, after leaving Amherst on a travel and study fellowship in October 1924. This period was not exactly unimportant, after all: It amounted to the only graduate training program Parsons ever had. Moreover, while Parsons never hesitated in asserting that his year at the London School of Economics (with Hobhouse, Laski, and Malinowski partly excepted) was an intellectual disappointment, his experience of a year of study in Heidelberg, in 1925–26, where “the ghost of Max Weber” was still very much in evidence, made on him a profound and lasting impression (Parsons 1970, 1980).

Camic not only neglects to consider the consequences of this intellectual *Bildung*, but he fails entirely to mention that it was this same young Parsons who during the summer of 1927 began revising the translation of Weber’s *Protestant Ethic*, which was published three years later. Weber, we can be sure, was not one of the institutionalists, despite the fact that his views in certain ways converged—content fit—with their own. To the contrary, Weber’s acceptance of the importance of marginal utility theory was fundamental to his sociological ideas (Holton and Turner 1985), and he considered himself, in the latter part of his career at least, to be contributing to the development of an analytically differentiated social science. Reading Parsons’s lucid and admiring introduction to that first English translation of a major Weber work, one can hardly doubt the direction in which the young American’s intellectual interests lay. He did not need to be led astray by the fame of Harvard neoclassical thought, nor by the fortune that association with them supposedly was to provide. He was already becoming “Weberian,” and it was certainly this interest that lay at the heart of *Structure* itself.²¹

Why not respect the young Parsons as capable of making his own decisions, as drawing selectively from his intellectual environments, and as directing his own intellectual path? Precisely because he was deeply exposed to institutionalism as an undergraduate, one can assume that Parsons understood its position very well. If his Harvard colleagues had been sending him the wrong signals about institutionalism (p. 437), as Camic assumes, would Parsons not have been able to realize their error? It is Camic, in fact, who believes these signals to have been wrong, claiming that Parsons’s colleagues were “misinformed” (p. 439). Obviously, Parsons did not feel the same way, and he certainly was in a position to judge for himself.²²

²⁰ Since part of Camic’s reasoning for this early influence, and its significance, evidently is based on some of Parsons’s college papers and earliest essays, it is important to note that he does not confront the fact that another scholar (Wearne 1989:26–38), using the same published and unpublished materials, has been able to identify a breaking away of Parsons from the institutionalists’ legacy already in his last year as an undergraduate student. E.g.: “From these two [undergraduate] essays we detect the influence of Veblen and Sumner. But did he [Parsons] acquiesce in their views? There is sufficient reason to suggest that even in these early years Parsons was attempting to diverge from their patterns of thought” (Wearne 1989:36).

²¹ It is possible that Camic does not mention Weber’s relevance because of his belief that Parsons’s Weber is not the true one (Camic 1987:434–36). Whether or not he is correct on this score, as far as Parsons’s project construction is concerned, it is Parsons’s Weber and not Camic’s that should be taken into account.

²² It also seems relevant to make the obvious observation that the differences between Hamilton and Ayres, on the one hand, and Schumpeter, Knight, and Henderson (and Whitehead, we would add), on the other, certainly involve more than reputation and intellectual disagreement. The latter represented intellectual teachers of an entirely different stature and quality.

The author who was accused, not so long ago, of portraying actors as cultural dopes, stands accused by Camic of being a reputational dope, unable to evaluate information according to his own relevance criteria, accepting automatically and unthinkingly the ideas of whoever was most prestigious in his intellectual environment at the time.

REPUTATIONAL REDUCTION AS BEHAVIORISM

By discounting Parsons's capacity as an agent, his capacity for judging, interpreting, and processing his experience, Camic has arrived at his central claim that reputation, not intellectual content, was responsible for the predecessor choices Parsons made. This implicitly behaviorist understanding of the manner in which institutional factors affect intellectual creation betrays the kind of quasi-rationalistic, exchange theory perspective that saturates, and undermines, Camic's understanding of how reputation works. Claiming that Parsons would have been irrational "in choosing as predecessors locally disreputable thinkers like the institutionalists" (p. 435), Camic cites Goode's proposition that "to admire openly the 'wrong' person or achievement can be costly if one's boss is likely to feel less respect for anyone who holds such an opinion" (p. 435 n. 15).

Finally, Camic insists on the point that has been at the heart of his reductionist interpretations of Parsons all along (e.g., Camic 1987, 1989). Noting that "several institutional factors made it especially important to avoid these associations" (p. 435) with the institutionalists, Camic emphasizes the singular and overriding importance of Parsons's position in the stratification system. In a number of key dimensions—job security, professional prestige, departmental power—Parsons's status was low; by contrast, in each of these same dimensions, the position of analytical, as compared with institutional, economics was high. *Ipsa facto*: The insecure disciplinary status of sociology is supposed to have made it necessary for Parsons to gain credible allies from the discipline of economics, dominated by the anti-institutional approach; the low status of Harvard's own sociology department allegedly pushed Parsons in the same direction locally; "the uncertainty about his prospects for promotion in the Sociology Department" (p. 435) induced Parsons to make allies outside of it.

If we assume behaviorist motives internally and the stratification of every environment externally, then such "evidence" seems obviously to prove H1. It could only have been because Parsons so desperately "needed the backing of the economists and other local influentials" (p. 435) that he turned away from institutionalism! And what about the Europeans? Why did Parsons turn to them instead? Camic provides, as always, an instrumentalist explanation for this as well. It was "because they were not 'liabilities'" (p. 435). *Homo economicus theoreticus*.²³

All of this is not to say that reputation does not matter, or that intellectuals are not influenced by the environments in which they live. It is to suggest, however, that the flow of reputation, its origins, and its impacts cannot be understood simply from the perspective of exchange. It is intellectual actors who do the exchanging, actors who have subjectivities that inform their intentions, intentions that establish criteria for choice. Intellectuals make efforts to select from their environments in ways that are consistent with these criteria,

²³ In a move that is all too familiar, Camic betrays the misgivings he has about the position he has taken by denying that he has taken it. Parsons's turn toward the Europeans, he explains, "does not mean that his selection of the four European thinkers was an instrumentalist maneuver that set aside content factors in an effort to cater to the opinions of the local crowd" (p. 436). No, what he actually means, Camic informs us, is that "Parsons' serious intellectual commitment" to the Europeans' position was merely "heightened by institutional factors" (p. 436). If this were an accurate self-description, of course, Camic would have to explain where the prior, intellectual commitment came from; unable to attribute it to reputational factors, he would have to acknowledge the primary role of intellectual ones! This is the opposite of what he actually does.

although certainly they do not always succeed. The point is not to deny that intellectual projects are often also political and disciplinary actions, nor to forget that essay writing is often also an occasion for building alliances and acknowledging debts. The point is to argue that the relevance of intellectual works to one another should not be judged primarily upon these grounds.

In his early work on the scientific community, Warren Hagstrom (1965) wrote that scientists exchange information for recognition. The exchange theory he employed, however, was that of Marcel Mauss, not Homans or Goode. In the Durkheimian tradition, the search for recognition, no matter what is exchanged for it in return, is regulated by some prior understanding of moral force. Camic claims to have made use of Hagstrom's work. It is a shame he did not understand it.

CONCLUSION: ON THE DECONSTRUCTION AND DESTRUCTION OF CLASSICS

Camic claims great originality for focusing attention on Parsons's choice of intellectual predecessors. "To date," he writes, "research in the theory area has treated the inclusion of certain intellectual predecessors without regard to the exclusion of other writers" (p. 424). We will leave aside the intriguing question of just what "research in the theory area" can mean. Does the century-long inquiry into Marx's predecessors not count as research or not count as theory work? However, we insist that this statement is fallacious even if it is taken as applying only to the narrow area of theoretical discussion of Parsons himself.

From the very beginning of *Structure's* public life, one of the principal strategies of those who have disagreed with its central tenets—and Camic has made no secret of his own disagreement—has been to emphasize the self-serving and particularistic manner in which Parsons chose his predecessors. From Coser's (1956) outrage over Parsons's exclusion of early American sociology, an outrage later echoed by Hinkle (1963), to countless indignations that Parsons had excluded Marx (Rex 1961), to Levine's ([1957] 1980) insistence that Simmel deserved to be in the pantheon too, Parsons's critics have consistently disputed his predecessor choice. In ways very similar to Camic's, they attacked Parsons for excluding those whose work they judged to be equally relevant, or they accused him of ignoring figures whose theories, in their view, would have been damaging to his argument. It requires only a modest extension of such arguments to include those interpreters who have argued, not that Parsons actually excluded a theorist from *Structure*, but that he neglected one or the other—invariably the most important—part of that theorist's work. Bendix and Roth launched serious arguments of this sort about Parsons's relation to Weber. The spate of subsequent "de-Parsonsizing" articles (Pope 1973; Cohen, Hazelrigg, and Pope 1975) continued this approach in a fundamentally less serious way.

All of which is to say that no one except Parsons himself and his early and most ardent followers have described him as an "impartial bricoleur, searching out and drawing on any relevant intellectual resources" (p. 432). This straw man does more, however, than create a misleading impression of Camic's originality. Camic also employs it to suggest that the kind of behaviorist and reductionist emphasis on reputation he proffers is the only way that the exclusion of predecessors—the "dark side" (p. 424)—can be explained. Yet many of the most severe critics of Parsons's exclusion of this or that predecessor never found the need to evoke a reputational claim; they launched explanations of exclusion on intellectual grounds alone or upon broader understandings of the impact on Parsons of society at large. Perhaps even more tellingly, some of the interpreters whom Camic criticizes most consistently for their concentration on "content fit," and who have made an effort to promote a more complex and appreciative understanding of Parsons in recent years (e.g., Gould 1981; Alexander 1983, 1988), have themselves written at length about the dark, exclusionary holes

in his selection of predecessors. Gould has extensively discussed, and criticized, Parsons's exclusion of Marx. Alexander (cf. 1987a) makes Parsons's exclusions—of Marx, of the materialist Weber, of pragmatism and phenomenology, and of the later Durkheim's religious turn—central to his reading of the entire postclassical tradition in sociology.

Because he has so persistently tried to interpret the theoretical arguments of Parsons's first and most important book as reflections of the author's personal and institutional ambitions, it is fair to say that Camic's inquiries into the construction of *Structure* have had a destructive intent. We have argued that Camic's search for Parsons's predecessor choice is not original and that his substantive arguments are not correct. It is well to remember, however, that every discussion of a classic has much more than an empirical aim. It is theorizing by another name (Alexander 1987b).

It might be that Camic has tried to destroy the integrity of *Structure* because he fundamentally disagrees with its arguments for the centrality of moral regulation, for the independent integrity of moral choice, for the necessity of theoretical synthesis and an analytical approach to theory as such. Certainly in his own explanations he has proposed to substitute for such emphases a behavioristic understanding of motives and an instrumental approach to the external pressures of institutional life, and a relativistic conception according to which, once they meet "contemporary standards of publishability" (p. 423), a pushpin is as good as poetry. Because our own approach to social theory is very different, and because we understand action and order in a very different way, our empirical evaluation of Camic's arguments about the classics have had a different theoretical aim.

The deconstruction of our discipline's classics need not necessarily have a destructive aim. Sociology is mature enough today to disagree with its own fathers without having to fantasize on their evil and covert intentions. Sounder arguments and better conceptual frameworks may advance and tighten theoretical debates. "Prior publication, unpublished papers, course materials, letters, and so on" are surely helpful in gaining a better understanding of the legacies of great theorists and the history of our discipline. They cannot, however, be used to substitute for genuine theoretical arguments, and they should never be used to conceal them.

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