Stylized Facts and Close Dialogue: Methodology in Economic Geography

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One difference between economists and geographers is the significance attached by the former to stylized facts and the very different significance attached by the latter to the diversity of economic life. The paper begins with this distinction and argues that Krugman's theory-enslaved stylized facts may impoverish theoretical innovation in economic geography just as the efficient-markets hypothesis has had severe consequences for research in finance. An alternative to theory-enslaved stylized facts is suggested, noting the inevitable and antagonistic relationship between theory and empirical observation. My philosophical perspective is neither foundational nor postmodern, but is, rather, a version of philosophical skepticism. Having reviewed recent developments in economic geography, the claimed virtues of objectivity and the supposed dangers of subjectivity are disputed. I suggest that the former is compromised by its reliance upon a ready-made world, while the dangers of the latter are exaggerated by an implied commitment to an uncontested truth. This is the basis for arguing the virtues of close dialogue in economic geography and in the geography of finance in particular. *Key Words: close dialogue, economic geography, finance, skepticism, stylized facts.*

Recent work in economic geography and the geography of finance is based upon in-depth interviews, or close dialogues with industry respondents (see, for example, Clark 1997; Leyshon et al. 1998; McDowell 1997; and Thrift 1996: ch. 6). Unlike other forms of empirical research, close dialogue relies upon the intimacy or closeness of researchers to industry respondents, a level of personal commitment quite at odds with the conventional notions of scientific disassociation and objectivity. As Schoenberger (1991, 1996) has shown in a related context, close dialogue can involve complex relationships between interviewers and interviewees. Thus close dialogue is a mode of case-study research, one that uses structured and unstructured interviews in the context of relationships between nominal equals to reveal the actual logic of decision making. In economics and the "new" economic geography championed by Paul Krugman, stylized facts, such as the supposed persistence of industry-regions, dominate intellectual reasoning. For Krugman (1991), stylized facts are the slaves of theoretical arguments about the proper logic of economic geography.[1] While not all economic geographers use or even accept the use of close dialogue, the current significance attached to close dialogue relative to stylized facts may prove to be a basic difference between geographers' and economists' economic geography.

In this paper, I argue that close dialogue can play an important role in promoting theoretical innovation in economic geography, in general, and in the geography of finance, in particular. As a first step in the argument, I look at the nature and practice of theorizing in the new economic geography, recognizing that Krugman's work has had, and will have in the future, significant implications for how economists understand economic geography, if not for how many geographers practice economic geography. This is important for appreciating what may be distinctive about geographers' economic geography, and what may or may not separate economics and geography as disciplines. The second step in the argument is to suggest that dualisms like theory versus empirical observation (or deduction versus induction) can be practically resolved by reference to a more philosophically skeptical notion of knowledge building. Having established a way of integrating the two sides of the equation, the paper then moves on to look in more detail at the virtues and, especially, the supposed vices of close dialogue. Basically, I contend that many researchers are too idealistic about the possibility of truth in the social

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sciences. While close dialogue is shown to involve subtle and not-so-subtle roles and relationships, I argue that academic objectivity, in a strong sense, is only plausible if we retreat to a theory of knowledge that idealizes facts and strips bare the complexity of life.

Given past reliance on quantitative data for modeling regional economic systems (Clark et al. 1986), and, more recently, the use of legal evidence to analyze corporate strategy (Clark 1993), it may be surprising that I should advocate the use of close dialogue. In researching Anglo-American pension-fund investment strategies, it has become apparent to me that understanding financial decision making has been impoverished by the hegemony of one stylized fact: the claim, attributed to Michael Jensen, that the efficient-markets hypothesis is the best established fact in the social sciences. This "fact" has had far-reaching implications for what are legitimate questions of research (e.g., the role of geography in financial markets), what are legitimate assumptions about individual decision making (e.g., the significance of local context in investment decision making), and what are legitimate arguments about regulation (e.g., the prospects for directing pension funds to urban needs). Likewise, Krugman's stylized facts threaten the hard won work of the past twenty years aimed at integrating spatial heterogeneity into the theoretical core of economic geography. If we are to escape the shadow cast by the efficient-markets hypothesis in the geography of finance, and if we are to sustain a rich, geographically informed economic geography, we need to be clear about how and why close dialogue is a plausible mode of analysis.

Thus the argument I develop in this paper has two interrelated strands. One references the challenge posed by theorists like Paul Krugman to the economic geography that has evolved over the past couple of decades. We are being forced to rethink our status and the bases of our claimed distinctiveness (see Schoenberger 1998). The second strand is about theoretical innovation in geography and economics. To make my argument work, I suppress the full range and diversity of research practices in economics and geography. I resist the temptation to argue for and against different kinds of economics (like neoclassical economics) and geography (including related versions of political economy). Dow (1997) and Sayer (1995), respectively, have useful treatments of these literatures. As well, I tend to emphasize the practice of research rather than outcomes, thereby leaving out geographers' work I find congenial and others' whose work I find antithetical to my own vision. The paper is not intended to be a literature survey.

Notice also that my argument about methodology overlaps and relates to recent work in feminism, sociology, and anthropology, and (perhaps surprisingly for some) economics. Most important, my goal is to show there is a close connection between the goals of research and the context of research, even if there can be no ideal research methodology in the human sciences.[2]

Economic Geography and Stylized Facts

The practice of research in the social sciences is greatly affected by fashions *and* cultural habits (Barnes 1996). As graduate students, we inherit an intellectual world made by our supervisors and their supervisors, just as we are responsible for teaching subsequent generations of students the practice of research in relation to the intellectual problems we help develop. In many respects, we work away from the traditions we inherit to create our own style, just as our supervisors worked away from their own inherited milieu. If we were to go back a couple of intellectual generations, we would find the subdiscipline of economic geography dominated by a few basic issues, including the convergence of interregional economic growth and why there is a spatial hierarchy of settlements. Not everyone worked on these issues, but they held center stage and demanded respect, if only because of their simplicity and coherence. Now in the 1990s, center stage is dominated by industry case studies; the quantitative revolution has been side-stepped and displaced by a more qualitative and speculative mode of analysis in the hope of representing the spatial scope and diversity of economic life. Quantitative economic geography persists in the discipline but is not now the customary mode of analysis, notwithstanding the remarkable achievements of some of its practitioners (see Webber and Rigby 1996).

The challenge for geography is to make sense of economic diversity in relation to broader, higher-tier processes of economic change. Gertler's (1996, 1993) work on the evolution of the German machinery industry is representative of the best of this kind of work, relying upon close dialogue with industry sources to construct an understanding of the dynamics of the industry and its place in the global economy set against common expectations framed in the literature. He builds up a picture from below, from the detail gleaned from dialogue and the knowledge of others. By contrast, Krugman's (1991) economic geography harks back to an earlier era. He is concerned with the kinds of issues that dominated the discipline twenty years or so ago. Why are there industry-regions? Why is there a persistent spatial hierarchy? His methods of analysis, drawn from research in international economics, are focused upon providing answers to these questions, ignoring the diversity of economic life in the interests of general law-like propositions. Krugman uses stylized facts like the existence of increasing returns to scale, joined together with his analytical methods in a self-referencing circle: his chosen stylized facts allow for the application of analytical methods, and those methods, developed in the study of international economics, explain his chosen stylized facts of economic geography.

Whereas economists have focused upon a set of issues that were important to geographers a few decades ago, using methods of research that dominate mainstream economics, geographers have systematically worked away from those issues over the past few decades to develop different issues and different methods of research.[3] Both sides are very much aware of this gap. Krugman (1991) acknowledges that his analysis of industry-regions may not find favor with geographers, but he is not particularly concerned by criticisms. His audience is clearly the economics profession. Not surprisingly, critics of Krugman emphasize his unwarranted abstraction, argue against the significance of the issues he identifies, and in general suggest that his findings have little empirical value, given the geographical diversity of economic systems (see, for example, Martin and Sunley 1996). It seems clear that many geographers do not accept Krugman's idealized problems, stylized facts and modes of representing the problems of economic geography. One implication is that geographers do economic geography better: that a fine-grained, substantive appreciation of diversity, combined with empirical methods of analysis like case studies, are the proper methods of economic geography (*pace* Geertz 1983).

There are other significant differences between geographers' and economists' economic geography. Some geographers dispute the relevance of general or even partial equilibrium models of spatial-economic systems. While recognizing the commonalities of such models with analytical and mathematical techniques, there is a suspicion that analytical elegance and tractability drive the focus of analysis rather than the empirical problems. At base, geographers dispute the plausibility of assumptions like homogeneous information, limited transaction and adjustment costs, and the presumption of spatial-economic convergence (Clark et al. 1986). Krugman is aware of these themes, and has made significant contributions through the application of trade-based models of imperfect competition to understanding the dynamic properties of spatial-economic systems. Even so, many geographers are aghast at the presumption of equilibrium-focused spatial-economic convergence evident in recent economists' studies of interregional growth (compare Martin and Sunley 1997 with Barro and Sala-I-Martin 1994). There is also considerable unease about the implications of spatial-economic convergence models for public policy. The presumption of convergence denies the value of (or need for) local economic-development programs and denies the spatial and functional segmentation of labor markets structured by race and gender discrimination.

Not all economists share a commitment to methodological aestheticism (see Woolley 1993), or the principles of convergence (see Nelson 1995). And not all economists are disinterested in the future of the urban poor. Hahn (1991) and Baumol (1991), both of whom made their reputations using mathematical methods and (in Hahn's case) general equilibrium analysis, have wondered if the preference for elegance and tractability is justifiable. They question the relevance and plausibility of a great deal of economic research, and suggest that social responsibility demands a reassessment of the concentration upon mathematical methods and models that impoverish the scope of economic imagination and commitment. Echoing these sentiments, Romer (1994), criticizes his past research on capital growth models, suggesting that his research was compromised by the accepted practices of macroeconomic theorizing and, as a consequence, failed to acknowledge more important, though less analytically tractable, issues of endogenous growth. Most significantly, Basu and Fernald (1997) show that the evidence for increasing returns to scale in U.S. production is less robust than assumed. They find "substantial heterogeneity across sectors" and "evidence of decreasing returns to scale." And they attack "parables" like increasing returns to scale, pointing out the problems posed by such simplistic notions for understanding the actual performance of the economy.[4]

At the same time, a new generation of economists have attacked the foundations of equilibrium-oriented models of the economy. Whereas financial markets are assumed to be the most efficient markets of western economies, Shleifer (1998) and his colleagues have demonstrated the existence of systemic inefficiencies or irrationalities. Whereas it is widely assumed that financial markets are self-organizing and self-correcting systems of decentralized decision making, Shleifer's work questions the robustness of arbitrage processes. Whereas it is presumed that speculation is a superficial aspect of markets, it seems speculation may be integral to the functioning of finance markets. All these observations are empirical, and rely upon detailed analyses of trading patterns and processes. In these ways, Shleifer's work questions the plausibility of Jensen's stylized facts about financial markets and seeks to overturn comfortable assumptions made about the coherence and order of economic systems. Notwithstanding Krugman's (1996) attempts to incorporate the diversity of the economic landscape into his models of economic geography, the underlying theoretical building blocks of his approach are being questioned within economics. The profession is increasingly doubtful about the stylized facts that have legitimated assumptions about the coherence of economic processes in general.

Two Sides of Knowledge

Quite simply, it might be supposed that the crucial issue dividing many economists and geographers is the status of theoretically enslaved stylized facts as opposed to detailed case studies (and all that implies). But, as I have tried to suggest, while this is an important divide with respect to the current practice of economic geography, it is also a divide that has begun to appear in economics.[5] It might be supposed, then, that victory is at hand, that the use of stylized facts will lose favor in the face of greater importance attributed to methods of analysis that place great value on the details of economic life. This is possible, but improbable. Anyone familiar with the "culture of economics" will readily appreciate the apparent reluctance of the profession to change its methods of analysis, notwithstanding challenges by postmodernists, feminists, and others (see generally Dow 1997). In any event, such a victory (if that is what it should be called) would be a hollow one given the actual relationship between stylized facts and empirical diversity.

To explain, let us look more closely at the relationship between economists' stylized facts and geographers' empirical diversity before looking specifically at close dialogue. On their own merits, stylized facts have three specific virtues. When integrated with their underlying theory, stylized facts are a reference point for assessing the significance of empirical observations drawn from a wide range of circumstances. Stylized facts are also a test of the relevance of empirical observations, structuring the scope of empirical analysis given unbounded possibilities. And stylized facts are a form of commonly accepted coherence, encouraging systematic research over-and-against the threat of anarchy. In other words, stylized facts are thought to be essential for social science, forcing individual researchers, if they wish to be accepted, to test their empirical observations against third-party standards of *significance, relevance,* and *coherence*. Thus stylized facts perform two interrelated functions: one, they narrow the range of possibilities, and two, they encourage the incremental development of disciplinary research programs. In this context, we can see how and why the efficient-markets hypothesis became the ultimate test of the significance, relevance, and coherence of research in finance, spilling over and influencing the research agenda in economics and the social sciences over the past two decades (compare Fama 1970 with Fama 1991).

What about geographers' empirical diversity? What are its virtues? For argument's sake, three can be readily identified. For a start, empirical observation is an opportunity to assess the substantive content of stylized facts and their parent theories. At one level, utilizing case studies and individual observations to reflect upon the depth or quality of stylized facts, as opposed to their claimed generality, this assessment may be entirely unsystematic. At another level, empirical observation may be quite systematic, seeking to validate or even deny established stylized facts in the search for other, more robust stylized facts. Empirical observation also offers an opportunity for the free association of one's imagination and intuition. Here lie the roots of intellectual innovation and invention. In this respect, empirical observation is also a measure of one's own intellectual development, reflecting the importance commonly attributed to our own agendas, as opposed to the inherited intellectual agendas of others. For many analysts and philosophers of knowledge, empirical observation is about *content, innovation,* and *autonomy* (see McDowell's 1994 discussion with reference to Davidson and Kant). Shleifer's (1998) attack upon the efficient-markets hypothesis combines all three virtues in an attempt to break out of a closed, self-referential social science institution constructed by others.

We might continue this discussion of the virtues of stylized facts and empirical observation, introducing examples and identifying circumstances to demonstrate the value of their respective claims. It is clear from the literature in economics and geography that there is a lively debate about what can be achieved using these methods (see McDowell 1992a, 1992b; Schoenberger 1992). But it should also be obvious that, for all the debate about their respective virtues, these two methods are joined together—they seem to be at once separate *and* immanent critiques of one another. Following John McDowell, it could be suggested that they are joined as two weights located at either end of a seesaw.[6] If we were to begin with empirical observation, we would be inevitably drawn to the other end of the seesaw, to theory-enslaved stylized facts, because we need the discipline of theory to make sense of the relevance of our empirical observations. At the other end of the seesaw, to begin with theory-enslaved stylized facts would lead us inevitably to empirical observation because stylized facts without content are sterile and ultimately irrelevant exercises in analytical formality. Here on the seesaw, with empirical observation and stylized facts separated by the imperatives of their respective customary practices, the underlying logic maintaining the connection between the two methodological options can only be a genuine commitment to intellectual curiosity. We might also imagine that the two ends are connected by a tense, dialectical relationship, suggesting that each is the mirror image of the other while being antithetical to the other.[7]

It is tempting to look for a resolution of this methodological stand-off, to resolve the balancing act in favor of one end rather than the other, and to resolve the apparently unending conflict between options by reference to a superior, integrating claim. For instance, advocates of stylized facts contend that empirical observation inevitably reflects theory, there being no real difference between the two except that one is on one side of theory while the other is close by but on the other side of theory. Simply put, the argument is that empirical observation is saturated by an implicit order, inevitably structured by hidden theory, and is never theory-free, as sometimes implied by its more naive supporters. In this case, given a choice between beginning on the stylized side, as opposed to the empirical observation side of theory, it would seem best if we begin with the former and then test our arguments with the latter. While seemingly entirely sensible, this formula makes an untenable assumption. It supposes that the economic and social world is given, and that the process of theorizing is a process of closer and closer approximation to that given world. In this respect, I would argue that the real challenge facing social science is to make sense of the claimed world of others, a world in which inherited theories do not seem to be able to sustain their claims of uniqueness. Paradoxically, we seem to have too many theories for the empirical observations available and too little theory that makes sense of the scope and diversity of the world.

I am also skeptical of claims of virtue attributed to empirical observation. In some quarters, strong arguments are made regarding the plurality of empirical observation, implying a commitment to representing the world in all its diversity and variety. These arguments are often counterposed with those of theory, suggesting theory's advocates are elitist and reductionist in denying the diversity of the world. This is a theme to be found in geography and anthropology over the past century and has been resuscitated in recent years by postmodernism. With Richard Rorty, the claims on behalf of pluralism are profound, even if not entirely consistent given his agnostic stance with respect to truth. Even so, at a minimum, this movement is hostile to so-called foundationalist and essentialist notions of theory-building, preferring instead to articulate the dimensions of diversity outside of the parameters set and policed by theory's advocates. Not surprisingly, voices of dissent are identified by including reference to gender, race, and culture—issues suppressed by theory's advocates in the search for a stripped-down, parsimonious view of how the world is. The best recent example of this argument, intersecting with economics and geography, is Gibson-Graham's (1996) critique of political economy and its advocates, including Harvey (1989).

Neither move, the move to theory nor the move to pluralism, actually denies the connection between the two ends of the seesaw or the intimate conflict inherent in dialectical reasoning. Any resolution at one end or the other only sets off a reaction at the other end, by the partner in the dialectical relationship. Perhaps the search for resolution is necessary for contemporary social science, given the myopia of theory's advocates and the unbounded naiveté of pluralism. The rhetoric of debate seems to reflect a polarized or dichotomous choice of social-science method. But the search for resolution may also be a political strategy within the contested institution of social-science practice, rather than a genuine commitment to knowing. It would be more honest to acknowledge the existence of the seesaw, or dialectical relationship, instead of pretending that only one is warranted or that one must (in the end) dominate the other.

In any event, it is apparent that at any time, each has its own role to play in the development of knowledge. In this regard, I greatly appreciate Siebers's (1992) argument to the effect that there is no unique match between the subjects of research and the tools we have at hand to study those subjects. Once we leave behind the ready-made world, its ideal order, and the specially designed tools of research that sustain that world, we are left with a complex task of fashioning knowledge in specific settings. It does not make sense to stick with an ideal methodology when we need both sides of the seesaw to (re)construct a version of the world (for an earlier statement along these lines, see Clark 1985). Putnam (1994:63), in responding to a question concerning Rorty's truth and Pierce's pragmatism, observed that we must "recognize that while truth is uncertain, any truth worthy of the name has to be subject to tests and subject to public discussion." And in drawing an analogy between scientific truth and social truth, Putnam says (of the latter), "we must test it and retest it and allow others to test it. And we must constantly discuss the methods of verification." Stylized facts and empirical observation are part and parcel of an inevitable and never-ending test of claims in the construction of social knowledge.[8]

Close Dialogue as World-Making

So far, I have argued that exclusive claims made for and against empirical observation in relation to stylized facts are misplaced. Implied is an impossible *absolute* choice. But there remains a question about close dialogue as a legitimate method of empirical observation. One does not have to believe in stylized facts to doubt the value of close dialogue; many advocates of empirical observation prefer the "objectivity" of third-party data to the apparent "subjectivity" of close dialogue. Indeed, some empirically minded critics appear to believe that the proper scope of empirical observation is defined by the independent structural parameters of the world. The implication is plain. At best, close dialogue may be irrelevant and, at worst, misleading and lacking substance in the face of the structure of the world. It may be better, it is argued, to use methods of empirical analysis focused upon structural parameters that match the logic or implicit order of the previously identified world. Yet again, the given world reappears, supposing a degree of coherence and structure that is unbelievable while implying the existence of a world separate from our interpretations and representations of that world. Even some advocates of empirical observation slip unknowingly to the theory end of the seesaw without realizing the consequences of that slippage for our imagination.

At the same time, there are reasons to be concerned about the intimacy of close dialogue. It can be indulgent, even isolating, given the special knowledge suggested by shared confidences. Worse, close dialogue may promise unique insights into the closed world of industry organization and relationships but, actually, only ever deliver information tainted by suspect motives. How can we avoid indulgent isolation? We must be willing to inch back along the seesaw towards the theory implied by stylized facts. In a sense, we must be simultaneously committed to the relationships essential to close dialogue and yet willing to "betray" our informants by reference to the skepticism of cold-hearted theory. This does not mean (yet again) that theory-enslaved stylized facts are the ultimate test of close dialogue. Rather, we need an external check on our enthusiasm, a sense of skepticism that works both ways: from theory to close dialogue and from close dialogue back to theory. In this sense, my philosophical stance has much more in common with Hume's reflective and evaluative skepticism (see Bauer 1995) than perhaps with recent postmodern developments that tend to emphasize the impossibility of knowing (compare with Gibson-Graham 1996; Righter 1994).[9]

With respect to recent research on the geography of finance, Hume's reflective and evaluative skepticism is essential in developing a better understanding of the local context of decision making. We have inherited a body of theory that is at once extraordinarily idealistic about the efficiency of markets and quite removed from the actual practice of investment decision making. Both elements are open to criticism and dispute as has been suggested in the work of Shleifer and his colleagues (Schleifer 1998; compare with Houthakker and Williamson 1996).

The point here is not so much to dispute the market-efficiency hypothesis as to indicate that its widespread acceptance has led many researchers to ignore the spatial and temporal diversity of agents and institutions. Embedded in the theory is an expectation that the practice of decision making is irrelevant in the face of structural, competitive imperatives. The arbitrage process should systematically strip out of markets suboptimal behavior, leaving only market-consistent behavior (for an early statement, see Alchian 1950). Missing in the literature are explanations of apparent trends in local decision making, the process of product innovation in "thin" (incomplete and missing) markets, and an understanding of the interaction between the prejudices of investment institutions with respect to the urban economy in all its variety. Not surprisingly, the stylized facts claimed to be relevant to the geography of finance are so lacking in content that cutting against their abstraction is one object of geographers' research (see Thrift 1996). Close dialogue is useful in this context because of the potential richness of substantive observation, the opportunity it promises for intellectual innovation, and its relative independence from the doctrine of market efficiency. Close dialogue can be used, as it is used in the industry, to document and assess the actual practice of investment decision making, given the extraordinary variety of practice and the decentralized nature of market behavior (see Greenwich Associates 1996).[10]

While we cannot afford to ignore the doctrine of market efficiency, close dialogue can be used to prompt a reconstruction of our understanding of investment institutions and decision making. This means, then, ordering the collected information into a coherent argument, thereby making it accessible to others who do not share in either the process of close dialogue or the presumption against market efficiency that informs analysis. In this sense, close dialogue is used as the raw material for a reconceptualization of economic behavior. It relies upon the process of codification which, according to Bourdieu (1990), provides a means of building up a general picture of the observed world, bridging local observation with broader interests and concerns, and thereby *making* a world rather than simply accepting as given a *ready-made* world composed by theorists. From codification, it is a short step indeed to stylized facts. But notice that these stylized facts emerge from codification as the product of empirical observation of market behavior, rather than being imposed by virtue of the imperatives of certain theoretical expectations or analytical needs. In this sense, Kaldor's "special inquiries" reemerge from history confronting and overtaking the theory-enslaved stylized facts that underpin the "new" economic geography.

Choreography of Close Dialogue

There remains, however, a serious charge often leveled at close dialogue: that it is vulnerable to systematic and random "errors," relying, as it does, on respondents telling the truth about themselves and their industry. For those skilled in questionnaire design and survey techniques, this kind of vulnerability may be countered in a number of ways. Interviewees' responses can be cross-checked against disguised control questions designed to test the veracity of respondents. Informants may be told in advance that they are part of a larger survey implying a form of cross-checking involving their peers. And informants may even be rewarded, or promised a reward, if the interview "goes well." These strategies and others are part of the tool kit of any survey researcher who relies on the opinions of respondents. They are not, however, fool-proof, as Lewontin's (1996) critique of Laumann et al.'s (1994) study of sexuality in America has shown. There may be questions that respondents are uncomfortable in answering and there may be reasons, hidden from researchers, for systematic misrepresentation by individuals and whole groups. In this sense, close dialogue is an art as much as it is a science.[11]

While relevant to my argument, there is another dimension to the problem of truth-telling. Much of social-science survey research presumes that expertise resides with the researcher, and that the proper design of questions in the light of anticipated right and wrong responses can cope with the possible "errors" noted above. Implied by this logic is an asymmetrical distribution of power: metaphorically speaking, social scientists hold the cards and deal them out to respondents in a preestablished pattern. Respondents can only respond. They cannot reshuffle the deck and deal them back to social scientists. This may be the case in opinion polling and perhaps focus group interviews about prearranged topics. But it does not accurately capture the intimacy and intrigue involved in close dialogue in the finance industry. Why? For two reasons. First, recent research on finance has made it clear that few academics appreciate the scope of intellectual innovation in the higher reaches of financial institutions. Not knowing this makes academics involved in close dialogue with industry specialists vulnerable to analysts' concealment and obfuscation. Knowing this, on the other hand, makes academics vulnerable to seduction and cooption. Second, it has become just as clear that knowledge of the industry is valuable to both sides in close dialogue. Academics moving between respondents are part of a complex web of information flow. Information is the object of research and the medium of exchange for the industry as a whole.

For those researchers who rely upon methods like close dialogue to understand social phenomena, many feel they have an obligation to just record and report their respondents' views (see Emerson et al. 1995 on ethnographic field-work methods). The asymmetry of power suggests an ethical obligation to voice the opinions of those who are ordinarily not important members of institutions of authority. But in the finance industry, where the social status, education, and salary of respondents are at least equal to (and sometimes significantly higher) than academic researchers, power is more equal and contested between the parties to close dialogue. Indeed, the possibility that respondents may deliberately represent issues in a manner beneficial to their or their institution's interests, but in a manner not easily detected by academic researchers, suggests that we should be wary of invoking any ethical obligation to simply record and report. Once we recognize this possibility, it is apparent why critics of idealized versions of social science, like Lewontin, are so effective; he simply asked whether we can trust what we are told. And if we cannot, we should be careful of claiming the truth of our knowledge.

If information is both the object of research and the medium of exchange, why are finance industry informants so willing to engage in close dialogue? There are a variety of likely motivations. For some respondents, being interviewed is an affirmation of status, an external validation of importance which may, or may not, be indicative of their actual standing in the firm or industry. Not surprisingly, some respondents exaggerate the importance of their positions and functions, at one level depending upon the interviewer to confirm their importance and, at another level, demanding respect if they are to help research. It is also true, of course, that the apparent knowledge and experience of the interviewer (advertised prior to the interview as part of the strategy of gaining access) can also be a significant inducement to cooperate. Implied here is a reciprocal relationship: access is made possible by an informal agreement to exchange information, sometimes involving an elaborate and highly choreographed process of sequential revelation that joins both sides of the dialogue. This is particularly true of industry informants who are experienced interview subjects. It is also possible that access is offered in the hope that the interviewer will pass on interviewees' versions of events and circumstances, the significance of his/her firm, and related "information" to other respondents (see Abolafia, 1996).

In this context, it is important to recognize that interviewees may adopt one or a variety of roles over the course of an interview. To illustrate, five common types of roles can be identified in my own research on financial markets. To begin, there is *the conversationalist (and tester)*. Here the interviewee opens dialogue in an expansive manner, talking about what he/she knows, the current situation in the industry, and the importance of research for a better appreciation of the nature of finance and investment. There may be other topics. Favorite restaurants may be identified, hotels queried, and personalities discussed. In many instances, the interviewee's expansive manner conceals an interest in the interviewer's own knowledge of the industry: who they know and the level of their appreciation of apparent symbols of status and income. In this respect, the conversationalist is also testing the interviewer. The trick in this situation is to simultaneously indicate an appropriate knowledge and appreciation of the interviewee's circumstances without capitulating to his/her charms and thereby undercutting the one distinctive and special claim of academic research—our relative independence from the imperatives of the industry.

Having survived the conversationalist, in the next interview, we may be face to face with *the seller (and buyer)*. Here dialogue opens with a quick burst of information which you (the interviewer) are seeking. This may be prompted by the interviewer having been given an early opportunity to set out the nature of the project and the kind of information sought. Or the interviewee may seize upon the material provided by the interviewer before the interview to demonstrate that you really need his/her advice. From there on, information becomes harder and harder to draw out of the interviewee as he/she reverts to the alternative game of selling information to the interviewer in exchange for more information from the interviewee. Once understood, this exchange relationship may be very fruitful, obviating the need for the exchange of personal information and confidences. But it may also prove to be quite frustrating. The type of information she/he is seeking may not overlap with the information you have. And the more apparent this becomes, the more likely the interview is to be interrupted and terminated prematurely, ultimately by the interviewee invoking his/her other commitments.

In large part, we tend to assume that the interviewee is representative or illustrative of her/his firm. And yet it is not uncommon to talk with respondents who take the opportunity to criticize the firm and dispute other senior managers' versions of the current circumstances of the firm and the industry. More often than not, the respondent will demand assurances that he/she will not be directly quoted. Such assurances have to be credible. Thus there is an incentive for the interviewer to exaggerate the confidentiality of interviews while casting the respondent in an important role of truth-teller: the ultimate check on reality. In these ways, the respondent introduces us to a world of conspiracy, silences, and denials. Both sides of close dialogue then collaborate in the fiction that the respondent is the person who really knows what is going on and going wrong in the firm. This is the world of *the insider (and critic)*. In many respects, the insider welcomes the chance to tell his/her side of the story. And she/he hopes that her/his side of the story will hold sway in the writing up of the case. On the other hand, it is also rare for such a respondent to demand to see the final version. Oddly, they trust us with their truth.

Then there is *the player (and enemy):* a person who is the ideal respondent, given his/her place in the firm, a person who understands very well what you are looking for and is willing to engage in close dialogue. He/she can provide new information and can be an important check on the information collected. These respondents' virtue is their place in the industry, the fact that they know key elements of the project and have access to firms and respondents not available to the interviewer. The player is the ultimate wheeler-dealer. This may be demonstrated by the hectic nature of the interview, being coincidentally included in on-going commentaries and conversations with the respondent's employees, friends, and acquaintances. It also may be demonstrated by the urgency of the interview, the need to get the issues out into the open before a real, approaching deadline (the New York market's opening, the Singapore opening, etc.). But appearances are deceptive. He/she may also be the enemy of truth. Just as he/she wheels and deals on the phone, calculating advantage and disadvantage with every move, so too may our conversation be integrated into the player's chess board. The player does not discriminate between us and the rest of the financial world. While it is flattering to imagine that we may be that important to the player, the fact that he/she treats everyone this way, whatever their relationship, means that we cannot trust anything said to us.

And not least of all there is *the interviewer (and confidant):* the person for whom dialogue has a rather different goal than that commonly acknowledged in the initial stages of discussion with the targets of research. For some, the interviewer is naively presumed to be neutral, almost invisible.[12] But experience shows that one's gender, age, ethnicity, and status may all (together or separately) matter a great deal in establishing contact and encouraging the exchange of confidences (see Pierce 1995). Interviewers may be very skilled at using their identity (identities) to justify common interests or beliefs. Naturally, the skilled interviewer may "change" as the interviewee changes, at the limit, becoming a chameleon in the interests of her/his empirical agenda. It is also possible that genuine rapport develops, and that one interview becomes the opening for further interviews and, at the limit, a long-term relationship. Here there can be real dilemmas: information provided may, if publicly identified, compromise respondents' professional careers. In this context, Kaldor's stylized facts may become a necessary strategy for concealing sources while interrogating theories.

There are other kinds of respondents, many of whom are friendly and cooperative, concerned to help rather than hinder research. As well, there are others (perhaps fewer) who are hostile or extraordinarily devious.[13] The point in identifying respondent types in close dialogue is simple but profound. Like Lewontin, I do not believe that respondents tell us the truth if truth is defined as neutral, uncommitted observations about the given world. Just as I am interested in close dialogue as a means of world-making, so too are respondents committed to constructing worlds through their dialogue with researchers. This is not only because all people have their own life-projects which require articulation (and concealment) from others. In the finance industry, where information is both the object of analysis and the medium of exchange, respondents are skilled and calculating informants. It does not make sense to deny this fact of life. Nor does it make sense to retreat from close dialogue, given the "subjectivity" of respondents. Rather, Putnam's checking process and Hume's skepticism should be recognized as the most appropriate responses to a world that is made and remade by the interpretations of ourselves and our respondents.

Implications and Conclusions

Economic geography is a growth industry on both sides of the disciplinary divide; economists and geographers are increasingly talking about related issues, if not in similar ways or for the same purposes. It may be that, in fact, there are many overlaps and commonalities. It may be that any search for real difference may founder on the remarkable variety of research strategies within both disciplines. In that case, while we may criticize Krugman and his colleagues at the National Bureau of Economic Research and at the Center for Economic Policy Research for their stylized abstraction and presumption in favor of particular analytical methods, I have tried to suggest that such criticisms are more general than we might suppose. Close dialogue is not like conventional social-scientific models of research. And if we are to take it seriously, and thereby continue working away from Krugman's economic geography, there are some important implications that ought to be acknowledged.

For a start, close dialogue denies the most obvious tenet of positivism: the claimed difference between objectivity and subjectivity. It also goes beyond claimed profound distinctions between quantitative and qualitative research methods, nor can we assume that qualitative research is complementary with quantitative research (see Morrow and Brown 1994). Being focused upon the derivation of knowledge through and out of social relationships, close dialogue is very much related to contemporary feminist research. Close dialogue, like Stanley and Wise's (1993) feminism, begins with the personal, relies upon contested social relationships, and demands a level of reflexivity that is antithetical to conventional social science.[14] But it is not antithetical to the actual practice of financial decision making inside and outside of financial institutions. As I have tried to suggest, close dialogue is an essential ingredient in the industry; the exchange of information (including individual interpretations of common data) and the management of networks of information (especially those that rely upon reciprocity) are vital social processes that are ignored only at the peril of the institutions concerned. These observations go to the very heart of the financial industry and the patterns identified by Shleifer and his colleagues.

If there are significant and important connections to be made between close dialogue and contemporary feminist methodology, there are also commonalities to be recognized between close dialogue and the so-called behavioral finance literature. According to Thaler (1994), the stylized facts that have dominated economic theorizing and financial economics in particular are entirely unjustified—the empirical evidence does not support even rudimentary assumptions made by efficient-market theorists about agents' rationality and attitudes to risk and uncertainty. Close dialogue is a means of understanding better the actual practice of decision making. It is a means of reintroducing geography (and history, sociology, etc.) into a world that seems to have been made up for the benefit of theorists. Through close dialogue, our goal must be to reintroduce the deep texture of local circumstances, including crime and corruption (see Clark 1998), that have been deliberately evaded by theorists more concerned with simplicity than the diversity of economic life. At the same time, it is important to acknowledge a lesson from contemporary feminist research: that subjectivity is always situated, even if the very practice of decision making reconceptualizes and therefore remakes the map of financial services (see Gagnier 1991 for an interesting study on nineteenth-century identity that makes this point).

There are many economists who are similarly doubtful about the value of stylized facts and related methods of theorizing, preferring methods of research that emphasize empirical observation of actual behavior in specific times and places. In this respect, the dispute about stylized facts as opposed to empirical observation is less about disciplinary practice and more about a tense, contested relationship between two opposed but inherently connected approaches to the construction of knowledge in the social sciences and elsewhere. The metaphor used here to illustrate their connection is owed to John McDowell (1994): I have suggested that theory-enslaved stylized facts occupy one end of the seesaw and empirical observation the other. We slip and slide between each end, our choice of beginning point determined in part by our disciplinary tradition, as well as our interest in innovation as opposed to research coherence. Other metaphors might be useful (compare Barnes 1996). I have used an imaginary spatial order which gives both approaches their due while suggesting that our initial location between each end is a matter of inherited tradition.

By my assessment, close dialogue is a very useful means of promoting conceptual and theoretical innovation. In recent years, geographers and some economists have used this strategy very effectively, becoming more sensitive to both the spatial and temporal components in the turbulence of the global economy. Given the success of this strategy, and the rewards for conceptual innovation, stylized facts and related methods of research may give way to our fascination with spatial differentiation. It is no wonder, then, that acceptance in geography of Paul Krugman's economic geography has been relatively limited. While not doubting his ability and his apparent success in wringing out fresh insights from old problems (the rank-size rule, agglomeration, and the like), we doubt the significance of his stylized facts. For many, convinced of the distinctiveness and separateness of close dialogue, any move from rudimentary codification to engagement with the other end of the seesaw would be anathema to current practice. But geographers' loyalty to one end of the seesaw carries with it considerable problems, not least of which is the consequent inability to address contemporary policy problems in a comprehensive manner. In this regard, the economists seem to have easier problems and clearer solutions, whereas many geographers seem only to have more detail.

From the previous analysis of the bonds of intimacy and intrigue that typically accompany close dialogue, it should be clear that I am doubtful about the status of absolute truth claims or profound factual claims that flow from such encounters. But I do not doubt the value of close dialogue. Just because there is always an intimate element to such encounters does not mean that what we obtain is profoundly tainted. We must be always cautious of the integrity of information, whether that information is formal or informal. The problem with close dialogue is its lack of cross-referencing with other cases. While it is a powerful strategy for interrogating the claims of stylized facts, it is hardly adequate as a strategy for coalition building. Whereas stylized facts may be shared by analysts sitting quite literally at their PCs, any attempt to cross-reference and integrate sets of separate cases from individual researchers becomes a problem of collective action. The pluralism inherent in close dialogue is a threat to building intellectual coalitions which will ultimately affect political action and policy choices. In this respect, a better appreciation of Kaldorian stylized facts may be a necessary ingredient in any strategy aimed at integrating the various results of geographers' close dialogues.

Thus I do not advocate a reversal in empirical strategy, nor do I advocate another round of theoretical abstraction. But we must find ways of reaping progressive benefits from geographers' current obsession with close dialogue. There is a real danger that the intrigue and intimacy of close dialogue displaces our sense of collective commitment. Not only may we be seduced by the choreography of intimate dialogue, we may also be seduced by the special circumstances of separate case studies. The challenge, politically speaking, is to carry forward into the world of politics and policy a sense of difference (as suggested by close dialogue) and a sense of commitment to collective welfare that may only be possible through codified, even stylized, facts about the world. Resuscitating commitment to collective intellectual enquiry is an essential task for economic geography. It is a task we share with others similarly concerned with overcoming the debilitating effects of fragmented identities and separate loyalties.

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Notes

1. By contrast, Kaldor (1985:8) thought stylized facts could be used to subordinate "deduction to induction." He argued that stylized facts may well be created through "a study of statistics or through special inquiries that include 'informal conversations with the owners or executives of small business' (and I presume, the executives of large businesses as well)." But his notion of stylized facts has been thoroughly displaced and lost. I was reminded of Kaldor's contribution by John Agnew and Trevor Barnes.
2. See Martha Nussbaum (1990) on the connection between the form and substance of intellectual reasoning. I have also been encouraged by her recent argument to the effect that we can use all kinds of resources (including literature) to liberate our economic imagination. She makes this point with reference to the sterile models of famine and deprivation that dominate the economics literature (Nussbaum 1996). As will become apparent, I also agree with her that "the literary imagination" is no more antiscientific or antieconomic than the closed imagination of many theorists obsessed with the reigning conventions that rely upon stylized facts in economics and geography.
3. In fact, only a small number of geographers have been able to systematically bridge the gap and publish in the core journals of both disciplines (like the *Annals* and the *American Economic Review*). Few journals appreciate the insights of the other discipline, and few seem willing to entertain the exceptional contribution as opposed to the standard contribution. Maryann Feldman's work is an important exception (Feldman and Florida 1994; Audretsch and Feldman 1996), as is Michael Storper's work (Storper 1989; Storper and Salais 1997).
4. Geographers have tended to accept as given the existence of increasing returns and imperfect competition (and hence path dependence; see Arthur 1994). This crops up in a variety of discussions about regional growth and industrial districts, referenced as useful arguments against orthodoxy (see Clark 1994; Storper 1991). And yet we may have been unwittingly seduced by Arthur and Krugman in the interests of sustaining the plausibility of alternatives to neoclassical economics. Compare with Harrison et al. (1996).
5. A good example of this tension in economics is to be found in the recent debate over Michael Porter's thesis that high domestic environmental standards may actually contribute to long-term corporate success and even national economic competitiveness. Porter and van der Linde (1995) argue from cases. Critics like Palmer et al. (1995) argue from theory, suggesting that theoretical imperatives come first and that cases can only be partial and inconclusive. Whatever the evidence, the presumption is in favor of theoretical presuppositions.
6. My use of McDowell's argument should not be construed as a complete agreement with his whole argument or, for that matter, an exact version of the structure of his metaphor. I have taken liberties with his metaphor and the terms and structure of the opposing sides of the seesaw.
7. In large part, I ignore the theoretical status of dialectic argument in this paper. Nevertheless, my seesaw metaphor could be related to dialectical reasoning. See Harvey (1996) for a remarkable and quite original exposition of dialectical reasoning. My own tastes in philosophy tend to the analytical tradition as opposed to the continental tradition; see Putnam (1992).
8. Here it should be apparent that my argument parallels those who believe that knowledge construction is a social process, maintained in institutions and framed by shared habits and practices (see Kuhn 1970; Latour 1986). In this respect, I am less enthusiastic about critical realism than some of its advocates in geography (see McDowell 1992a). Like Baert (1996), I suspect that critical realism is as exclusionary and idealist as positivism, the philosophy of science it seeks to replace. Of course, we should recognize that there are many versions of realism. In this regard, I am sympathetic to Putnam's realism, especially as it is joined together with a skeptical notion of theory building. See below.
9. An excellent summary of philosophical pragmatism, including the classics and recent commentaries, is to be found in Goodman (1995). Sunley (1996) has a useful discussion of the topic related to economic geography.

10. Thrift refers to this kind of knowledge building as "practical knowledge," an informal type of knowledge "that is learnt from the experience of watching and doing in highly particular context in direct mutual interaction." It is a necessary step towards creating "empirical knowledge" that "is not only cumulative but systematic and coordinated over vast tracts of space and (time)" (1996:101).
11. A point made nicely by Schoenberger (1991) in her assessment of the opportunities and dangers of corporate interviews for research in economic geography.
12. Here I basically disagree with Lamont (1992). In her study of upper-class American and French men, she made considerable efforts to appear anonymous, to be someone with a "blurred professional identity" (p. 20). She believed that such blurred identity "was essential to decontextualize the 'impression management' [strategies] conducted by the men . . . interviewed" (p. 21). She sought neutrality in inconspicuous dress, a low-key approach to conversation, and an average car. But, equally, she was a Princeton (assistant) professor, a woman professional in her early thirties, someone at home with French conversation and culture, and a person of considerable intellectual ability. Was she really anonymous? Did she really control the conversations as she claimed? I doubt that any senior respondent in the finance industry would be misled in such a manner.
13. I have not set out to interview anyone I either suspected of treachery or believed responsible for unethical or morally wrong actions. Even so, this does happen. On such occasions one is struck by the nature of the game: the topics discussed and the topics not acknowledged lurking just below the surface. Often it seems that the respondent is intent on demonstrating his/her reasonableness, charity, and consideration as if he/she can convince us of their ordinariness. But equally, one is suspicious, looking for subtle signs of cunning and evil. In this regard, Alice Kaplan's (1993:189–90) account of her interview with a French Nazi sympathizer is germane to the issue.
14. Of course, there are many versions of feminism inside and outside geography (compare McDowell 1992b with Bondi 1997).

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