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Theory and Method in Human Geography: Historical Materialism Says It All

Mickey Lauria



Working Paper No. 3

1990

COLLEGE OF URBAN AND PUBLIC AFFAIRS
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Mickey Lauria, *Director and Editor*

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ABSTRACT

Rather than develop a conceptual argument on how to conduct research in the "Post Everything Era", I analyze specifically what I have done in my last two major research projects: a comparative analysis of local political changes in response to manufacturing plant closures and the role of the gay community development in gentrifying a neighborhood in New Orleans. This analysis provides the basis for a reconstruction of my views on appropriate research strategies, a foil from which to discuss current epistemological, methodological, and theoretical lessons learned. In particular, I discuss theories of causality (social construction), appropriate unit of analysis (the fallacy of structure versus agency, scale and conceptualization of locality), research design (case study), theory evaluation, and the politics of research.

THEORY AND METHOD IN HUMAN GEOGRAPHY: HISTORICAL MATERIALISM SAYS IT ALL

When asked to participate in the special session on Marxism, PostMarxism and Human Geography at the 1990 Annual Meeting of the Association of American Geographers, I responded by saying that I thought I had little to say. I was informed that my research demonstrated that I had something different, constructive, and interesting to say. If people have responded to my published work in this manner as demonstrated by the inquiry, it makes sense to analyze that work conceptually, rather than develop a conceptual argument on how to conduct research in the 'Post Everything Era'. I chose this self-critical empirical approach because I felt it would help me avoid the common pitfalls of post hoc rationalization and it is consistent with the epistemological, methodological, and theoretical basis of the research under evaluation. Thus, I will analyze specifically what I have done in my last two major research projects: a comparative analysis of local political restructuring in response to manufacturing plant closures and the role of gay community development in gentrifying a neighborhood in New Orleans.¹ I will then explain how this work demonstrates a resolution of, or at least a justifiable position on, some contentious issues in theory and method in human geography.

Local Politics and Plant Closures

The local political restructuring in response to manufacturing plant closures project began while I was at the University of Iowa in the early 1980s. Two Planning Colleagues² were working with the labor center on the climate of agricultural

manufacturing plant closures in Iowa (meat packing and farm implement). Being new to Iowa and the University, they invited me to participate in this project. Since I was interested in local politics and planning this seemed like it could be a fruitful collaboration. This led to a summer project and a legislative report for the State of Iowa on manufacturing plant closures (See Lauria and Fisher, 1983). In this study we analyzed the nature of manufacturing plant closures, openings, contractions, and expansions in Iowa from 1975 to 1982. We later focused on the meat packing industry because of its importance to the Iowa economy. One finding that was relevant to my subsequent study was substantial evidence that plant closings have been effective in extracting wage concessions, and that many of the 'old line' packers under master contract have reduced their labor costs through selective closings putting pressure on workers in the remaining plants.

At the same time, Dave Reynolds in Geography, asked me to give a lecture in his political geography course on locational conflict from a Marxist perspective. Since a chapter in my dissertation focused on locational conflict (See Lauria, 1980, Chapter 3), I figured I would play with these ideas in relation to manufacturing plant closures (which had not been analyzed as locational conflicts). This idea came to me because, at the time, there was a labor dispute in nearby Dubuque, Iowa concerning the Dubuque Packing Company (The Pack). The Pack was threatening to close the plant if labor did not make concessions in the contract negotiations. Labor appeared very unconciliatory and thus there were discussion, plans, and political conflicts over alternative uses of the property and their relative impact on the community. So all the pieces seemed to fit together for a larger research project: I had some background work on plant closures in Iowa with a particular focus on the meat packing industry, I had developed a theoretical/conceptual scheme using the plant closures focus (see

Lauria, 1986), and a major labor conflict/local political restructuring and planning for economic development efforts were underway in a nearby community.

Since my espoused epistemology is realist and my basic theoretical approach is historical materialist and this 'was exploratory research'³ (little work had been done on local political restructuring), the first step was the development of a theoretical history of Dubuque, Iowa focusing on the social relationships between The Dubuque Pack, the community at large, the various segment of organized labor, and local politics. The data were newspaper articles, a few key interviews, and the archival resources of a city council member. From these data a detailed chronology (timeline), a historical narrative, a list of actors and the overt role they played, a list of unanswered questions, and a list of answered questions were developed. This information was then used to delineate further data needs, a list of people to be interviewed, and a set of open ended questions to be used as the interview instrument.

Thus in the second stage, missing data were collected or surrogate information collected and analyzed, interviews conducted (taped and transcribed). At this point the new data and interview information were integrated into the timeline and historical narrative. The empirical and theoretical knowledge was then teased from the narrative and data and additional questions posed (See Lauria, 1986:57-61).

While one could clearly see this case played itself out through my theoretically developed 'political economic scheme of locational conflict resolution' (Lauria, 1986:44), some more interesting questions developed that could not be explained (only described in theoretical language) by the use of that scheme. First, the state in all its complexity intervened in multiple ways and in some cases contradictory fashions. Second, the alternatives that each actor considered were a very limited subset of possible actions, when compared to similar situations elsewhere. Thirdly, informal (according to my

scheme) actors roles were significant and not accounted for. Thus it became very clear that my theoretical conception of the state was inadequate. This forced me to reevaluate this case in light of some more recently developed theories of the local state focusing primarily on a combination of Dear and Clark's (1984) concept of para-apparatus with the notion of corporatism at the local level, Offe's (1975) negative selection mechanisms, and Duncan and Goodman's (1982) conception of the local state as the embodiment of local social relations as they construct and reconstruct local political relations (See Lauria, 1986b). From this I was able to develop a new theoretical focus for a second case in Cedar Fall, Iowa: Rath Packing Company. The research methodology for the Rath case mirrored the Pack case so there is no need to describe it again⁴. This led to a comparative paper presented at conference where a discussant suggested that a different result was occurring in the Monongehela Valley south of Pittsburgh. The discussant provided all the necessary contacts and the study was replicated in the Monongehela Valley during the summer of 1986 (See Lauria, 1987, 1980).

Gay Community Development and Gentrification

My research on community development in the 1980s was confined to reworking research conducted in the late 1970s for my dissertation. Given my epistemological, theoretical, and political position, there was little opportunity for such work in Iowa⁵ (a motivating factor for getting involved in the plant closing work mentioned above). In 1984 a Ph. D. student in geography, Larry Knopp, approached me about being his advisor and potential dissertation topics. He had political interests in working with gay community development. Since I feel that such personal commitment is useful in

motivating a student to successfully finish we maintained this focus. After several discussions, we focused on the absence of empirical and theoretical work on the role of gays, and gay community development, in the renovation and gentrification of many inner city areas. Gay males had been noted in a few gentrification studies, usually as an indicator of heterogeneous lifestyles and culture in these areas. At the same time I was asked if I could contribute an article for a special issue on the 'urban renaissance' for Urban Geography. I asked the editor if a theoretical/methodological piece (not empirical) would be appropriate. Thus, Larry and I developed a theoretical and methodological paper (Lauria and Knopp, 1985) making connections between the creation of the gay identity, changes in the U. S. urban political economy, and gentrification. This article became the basis for a NSF proposal and Larry's dissertation proposal. In doing this we had placed the phenomenon in the context of the political economy of advanced capitalism, developed theoretical and empirical hypotheses, and a basic research design.

We decided that Minneapolis would be a possible case. The theoretical justification for this was that the gay community had locally elected openly gay political candidates, a gay rights ordinance was passed, and that there was a recognizable gay area that had also undergone significant renovation activity. The practical reasons for this choice were my knowledge of the city and my contacts in the University and community based politics. Larry spent the summer of 1985 in Minneapolis conducting the pilot study for the NSF proposal (See Knopp, 1986, 1987). At this time I interviewed and was subsequently offered a job at the University of New Orleans. During my interview I learned that the French Quarter was the center of gay social life in New Orleans and that the neighboring Faubourg Marigny was a gay residential community that had recently gentrified. So New Orleans became a candidate for

another case. Ultimately, because of the lack of NSF support (a story in itself) and the less clear presences of gay participation in the gentrification process in Minneapolis (discovered in the pilot), we scuttled our original comparative case design for a single case design.

Again the method was similar to that of the plant closing work. The development of a theoretically directed history of the Faubourg focusing on its renovation, demographic changes, and the role of gay males. The data sources used were newspaper articles, city planning documents, and key informant interviews, with the addition of participant observation (Larry moved into the Marigny for six months). From this narrative, a list of important actors for indepth interviews, new data sources, and a survey instrument were developed. The indepth interviews were conducted, taped, transcribed, and analyzed with the use of 'Ethnograph', new data collected and analyzed (primarily census, city, and housing price data), and the survey distributed, collected and analyzed. A dissertation (Knopp, 1989) and three articles (Knopp, 1990a; 1990b; and Knopp and Lauria, 1990) were produced.

Abstraction, Idealization, Summary

I guess it all begins with a precipitation event: a question a student, colleague, or friend asks; something you read that is stimulating (in a positive or more often negative way); an actual event that is either disturbing or intriguing. We usually file it away until our plate is a little cleaner, or pass in on to a student and/or research assistant, while we subconsciously chew on it a bit.

The first conscious step is to logically place the phenomenon in the context of extant theory; in my case, this context is the political economy of advanced capitalism,

urban political economy. This does three things to focus the empirical research effort:

- 1) determines the empirical evidence necessary to explain the phenomenon,
- 2) uncovers areas of theoretical underdevelopment - theoretical hypotheses, and
- 3) highlights areas where empirical knowledge is limited - empirical hypotheses.

From this position one can develop the first cut at a general research design. This design should include; the specific phenomenon to be evaluated, types of data necessary, methods of analyzing the data, and the justification for the specific cases to be evaluated.

The third step is to develop a theoretically directed historical chronology of events (See Sayer 1989:265-268 for a similar discussion - what he calls a realist concrete study). Historical accounts can not be accepted as objective data, but rather must be interrogated with theoretical concepts. This theoretically interrogated historical chronology is constructed because unreflective history(ies) (those derived from primary sources) and reflective histories (those derived from secondary sources), although not atheoretical, are theoretically chaotic or inconsistent. Thus a theoretically consistent history, data base, needs to be developed in order to direct further research efforts and to be used as part of the chain of evidence for empirical verification. This is done primarily through the available primary sources (newspapers, archival sources, some knowledgeable persons, etc.) and secondary sources (existing articles, books, etc.). This theoretical chronology is used to delineate actors, (to evaluate with other sources), to develop other primary and secondary sources of information and/or data, to identify holes and gaps in empirical information, and to further develop theoretical and empirical insights.

At this point one should be ready to develop the specific research design. This should include: the delineation of cases (some may not be workable), reevaluation of research questions and the development of hypotheses, determination of data collection methods (survey, interview method, etc.), and the development of the appropriate instruments and methods of analysis. After the analysis is complete, the historical narrative is redeveloped with the information (and/or questions gained) integrated. The empirical and theoretical knowledge gained should be culled from the narrative and the additional questions encountered rephrased.

CONTEMPORARY ISSUES CONCERNING THEORY AND METHOD IN HUMAN GEOGRAPHY

In rereading my sketchy description of the two research projects, I noticed that I had not overtly addressed many of the currently controversial issues in theory and method in human geography. I speculate that this can be so for one (or some combination) of the following reasons: 1) researchers in general, and myself in particular, do not deal with these questions while they are actively engaged in a research project (i.e., research practice is more ad hoc than we wish to believe or admit), 2) I internally have resolved these issues sufficiently so they do not overtly crop up in my research practice, 3) they have little practical importance for research design, and/or 4) many researchers, myself included, utilize poor research design. Although there is an element of truth to all of these, the following discussion should make it clear that I find the first and second reasons most compelling.

I think it is clear from the description of the projects above that research design is more ad hoc than we wish to believe or admit (in print). This is because research design questions are confronted and resolved during research projects. It is rare that

they are completely resolved a priori. Thus I have developed an iterative process of research design. This can create a problem of comparability of research findings within the research project itself (replicability is not a problem with realist epistemology); one cannot redo much of the research because things have changed (although secondary data analysis and archival analysis can be redone). On the other hand, it does allow one to adapt and improve the research design as one learns during the research project. It is this iterative learning process combined with the rationalization process required for communication (See Sayer, 1989:262-264) that has allowed me to internally resolve these issues (See Lauria, 1985). Since the resolution of these contentious issues has been internalized, it is apparently necessary to conceptually resurrect and reevaluate them.

The currently contentious issues that I wish to speak to are analytically distinct, but highly interactive, and ultimately need be resolved on a case by case basis (during particular research projects). The first set of issues concern epistemology and thus causality and empirical confirmation. The second set concerns scale and subsequently conceptualizing locality. The third set pertains to reasons, intentionality, or more appropriately politics, research practice, and scholarly production. I have chosen these three issues for I think they are the most useful in understanding my research practice.

The first set of concerns, although the most important, is somewhat easy to deal with and once dealt with only needs slight modifications at the margin. Not being a philosopher, establishing an epistemological referent is, on the one hand, very difficult but on the other quite easy. Ultimately it seems to involve a leap of faith: "This is how I believe I know what I know." From this, much will follow. First one's early training is from an unquestioned epistemological position, most often a variant of

positivism. Later, when you are trying to develop a research project or theoretical argument, you are forced to confront epistemological choices again. This often leads to a long excursion through philosophical arguments, each one as convincing as the other. After much confusion and sifting things out, things start to settle and one accepts a position partially based on its congruity with your theoretical and methodological leanings. In my case, with my graduate training in the anti-positivist 70s and with my political theoretical leanings vacillating between marxist and anarchist views of society, Marx's philosophy of science and historical materialism was most convincing. This was later more clearly codified for me as a particular branch of Realism. I don't think it is necessary to sketch what I mean by Realist philosophy of science, that has been done clearly elsewhere (See Baskar, 1979, 1986; Keat and Urry, 1975; and Sayer, 1984). This realist epistemological basis accommodates a social constructionist conception of causality. In using the term social constructionist, I mean to depict a general approach first coherently put forward by Berger and Luckmann (1966), in which people, through the dialectical process of externalization, objectivation, and internalization, both create and are constrained by society. The implication of this is, as I have argued elsewhere (Lauria, 1985 and Lauria and Knopp, 1985) that social relations are the causal relations in society. Dialectical logic is then used to create explanations.

Dialectical reasoning precludes pure inductive or deductive reasoning, singular causality, or linear one-way causality. An important element of dialectical reasoning is the concept that society, or human relationships, are constantly evolving. Thus, dialectics demands a focus on processes not patterns. Patterns are the crystallization of social processes that are at the same time evolving. Thus dialectical reasoning assumes a social constructionist approach (Berger and Luckmann, 1966). According to

Benson:

Social arrangements are created from the basically concrete, mundane tasks confronting people in their everyday life. Relationships are formed, roles are constructed, institutions are built from the encounters and confrontations of people in their daily round of life. Their production of social structure is itself guided and constrained by the context.

An important constraint is, of course, the existing social structure itself. People produce a social world which stands over them, constraining their actions (1977:3).

This focus on creation and process suggest a second major element of dialectical reasoning. That is, that social phenomenon's interconnection with a larger whole, for example the social formation, rather than isolated practices. Thus the 'totality' contains newly emerging social relations as well as existing patterns.

A third component of dialectical reasoning is the expectation that the process of social construction often contains contradictions. Contradiction, as used here, is not synonymous with conflict. Rather, a contradiction exists when social relations that are an integral part of society (necessary), undermine the reproduction of the existing patterns of those relations. Thus the dynamic of social change is mirrored in dialectical reasoning.

One implication of this epistemological approach is that empirical research is not to be used as an absolute backdrop from which to confirm or refute theoretical hypotheses. First, this is impossible, for all empirical research is theoretically derived. Second, theoretical hypotheses are constructed of necessary relationships while empirical explanations need combine necessary and contingent relationships. What this means is that it is difficult, if not impossible, to 'operationalize' theoretical hypotheses a priori. Thus we cannot 'test' them. What we do is explain how necessary and contingent relations work themselves out in real social systems. It is important to note that contingent relations are causal; it's just that, since they are not necessary, they

are less useful in refining theory. On the one hand, extant theory allows us to unravel, from our observational blend, the necessary from the contingent causal relations. On the other hand, the theoretical value of empirical research is its assistance in refining and discovering (new) conceptual knowledge of society's ever evolving necessary relations (See Sayer, 1989: 258-259).

Both Cox and Mair (1989) and Sayer (1989) do an excellent job in discussing the confusion concerning scale and thus defining locality. Cox and Mair (1989:121-122) argue that the confusion about locality, the attempts to define it concretely and abstractly, arise out of the 'conceptual leakage' from the abstract-concrete distinction to other dualisms, residual of poor prior conceptualizations, e.g., social/spatial, global/local, structure/agency. Likewise, Sayer (1989:258-260) argues the problem is due to a similar confusion concerning two meanings of a concept being general: a) meaning regular or represented in a series of repeated events and b) meaning necessary, the nature of an object and its relationships. Certainly Smith (1987:65-66) is correct in saying that the scale of analysis must correspond to the scale of the real process. Although he recognizes the plasticity of the scale of real processes, its not clear that he recognizes the implications of this for research design. In other words, what is the scale of a real process, say economic development? Cooke (1983) has suggested it be defined by labor markets. This makes sense since he is concerned with spatial development. But does this mean actual labor markets, or potential labor markets. In other words what is the labor market for Zenith let alone the micro electronics industry. I am suggesting that I question whether locality studies researchers actually study phenomenon/relationships solely at the local scale. I think not, unless we are defining local non-geographically, for example sociologically.

The formalistic question remains, how does one determine the proper scale of analysis. First and foremost, not abstractly (See Savage and Duncan 1990:68). Your particular research question will determine your first approximation. Secondly, the intensity of the research effort at different scales will be related to the theoretical importance of the relationships under study. Thirdly, the scale of the research effort will always be plastic. For example, in examining plant closures, local economic development strategies, and local political restructuring, I started by looking at plant closures in the U. S. in general (via secondary sources) and the meatpacking industry in more detail, and finally at the specifics of The Dubuque Pack and Dubuque Iowa. So in a sense, initially it was a telescoping of scale with increasing intensity of research. But the intense local research led to a flaring of scale in very specific theoretically selective realms - extra local politicians and agencies, finance capital, and other meatpacking corporations.

The last set of issues concern reason, intentionality, or the politics of particular research agendas. Smith (1987) is politically concerned about the 'empirical turn' for he sees this (and I might add postmodern epistemology) as a retreat from Marxist Political Economy but more importantly, socialist politics. Beauregard (1988), on the other hand, argues that there is nothing inherently conservative about locality studies, in fact, "locality research provides a potential for political action outside the realm of ideological debate and not readily available to an isolated theorizing" (p.54). At the same time, he argues locality research combined with political action (practice) establishes a methodology for the verification of radical understanding. I find this to be the case with my own work, many of my colleagues' work at Iowa and elsewhere in the U.S., and in fact with Neil Smith's political activism. Subsequently Beauregard argues that it is the postmodern cloak to some locality research that leads to a retreat

from Marxism (p. 56-57). Although Graham (1988) argues that postmodernism is not inherently right wing, I find Beauregard's arguments more convincing. Postmodern philosophy definitely negates truth, collective action, and prospects for progress (See Walker, 1989 for an elegant and emotional defense of these). These concepts are undeniably components of Marx's philosophy, method, and politics.

Although Graham (1990) and Graham and St. Martin (1990) make an interesting argument that a non-essentialist Marxian epistemology has been espoused that is not totally relative (researchers can choose a theory based on how they want to change reality), I find it difficult to let go of the so called false dualism of knowledge and reality. I find all her arguments concerning the real dialectical relationship between knowledge and reality convincing but also consistent with a social constructionist epistemology that maintains an essentialist view of theory. Just because by reconstructing knowledge we change behavior and thus alter reality does not mean that reality is totally relative. It is hard for me to accept that any theoretical choice and thus perspective taken on a particular research project will yield equally relevant or accurate findings. Researchers can and have constructed inaccurate knowledge and, if acted upon, this knowledge can and has changed reality albeit not necessarily in the fashion the actors had hoped (witness supply side economics). Thus it seems to me that if we want our reconstructed knowledge to change reality in a predetermined fashion (for political reasons I presume) we need something in addition to a predetermined political perspective to fashion our theoretical interrogation of existing reality and thus to inform our practice. For this reason I cling to my 'correspondence notion of truth' with all its essentialist baggage. It necessarily follows that I find no absolute fault with the currently unfashionable 'r-word' (reductionism), but rather find fault with inaccurate reductions, abstractions, and thus theories. I rely on the three

pronged Marxian measuring rod of correspondence, logic, and politics.

This aside, I may also partially agree with Graham, for individual intentionality (and thus politics) plays a major role in one's research agenda. As implied in my description of the two research projects, reasons for pursuing research projects are multifaceted; the precipitating event is usually a question asked, a stimulating article, or an actual event. But to determine the intentionality, one must examine the politics of the question, reaction to the article, or reaction to the event. We can all imagine explicitly conservative or right wing motivated research projects. More insidious are the conservative/careerist oriented intentions: your chairperson thinks this is an important topic, it has a high probability of receiving funding, the leading lights in the field find it interesting, its an obvious hole in the literature, its the new fad, and even, to some extent, it's inherently interesting. But much of the locality research (I am not familiar with this for the CURS researchers) comes out of prior progressive political involvement, prior progressive concern, and/or requests from progressive groups involved. This is certainly the case with the two projects described above. So I must conclude that locality research is not inherently a retreat from Marxism, socialism, or progressive politics. This is dependent on how and why the research is done.

CONCLUSION

In reflecting on my own research practice as exemplified by the two recent research projects, I must concede my research design/method has not always been consistent let alone terribly rigorous. To defend myself, I believe this is due to the iterative nature of research design and to the learning process involved. I can certainly say that the design and process have become more rigorous but I insist on maintaining

flexibility. It should be clear that I find the realist epistemology most satisfying, although I agree with Sayer (1979) that it does not solve all our research problems. I find that conceptualizing locality or determining the proper scale of analysis in the abstract to be a red herring. And finally, I believe that the 'empirical turn' and/or locality studies are not inherently conservative or a retreat from theory, Marxism, socialism, and/or progressive politics. In fact, for me and many of my colleagues, it has been a mechanism to promote political practice and practically informed theory. Although not in contemporary language (postmodern lingo), Historical Materialism says it all.

ENDNOTES

1. This latter project was developed with Larry Knopp, who at the time was a Ph.D. student of mine in Geography at the University of Iowa. All of the data collection described below was conducted by Larry and/or Linda Calvert, my research assistant and a Masters student in Planning at the University of New Orleans.
2. Dr. Michael Sheehan and Dr. Peter Fisher, both economists by training teaching in the Graduate Program in Urban and Regional Planning at the University of Iowa.
3. This will be explained in the issue section of the paper. Endnote #5 also partially addresses the unspoken logic behind this statement.
4. A question may arise that since my theoretical focus changed, why did my research design and methods remain the same. I realize that this is a contentious arena, but I argue that research design and methodology is primarily influenced by a researcher's epistemological perspective and empirical focus and secondarily by the substantive theoretical framework involved. In this case, the epistemological perspective and empirical focus remain the same, while the substantive theoretical focus shifts, but only slightly. I am still concerned with how the local social relations construct and reconstruct local politics, I merely dropped the locational conflict framework.
5. Since social relationships are created by interacting with people during the course of their daily lives (see section on social constructionism in text) I find it difficult to develop the relationships necessary for quality empirical research without living in or near the community under study: distance does hinder the quality and quantity of social interactions. These relationships are important for quality imperial research because as it provides the researcher with an insider understanding and /or a basis from which the researcher can evaluate the various insider interpretations available. Sayer (1989:267) makes a similar argument concerning the "double hermeneutic of social science" which requires that researchers understand and negotiate with "actors' accounts". Unlike Sayer, I further argue that an insider's understanding of any social process is an important component of an abstract theoretically derived understanding. This insider understanding (practice) is also important for the development of a progressive political strategy. It is very difficult to develop an insider's understanding without either being involved with the particular project or becoming intimate with some of those persons involved. Thus, in Iowa, there was little opportunity for intensive research/political practice in large inner-city low income communities facing redevelopment activities. Again I realize this is a contentious arena, but feel that the literature in Anthropology and Sociology on participant observation and intense field procedures address the issues well and that a conscientious researcher/observer/participant can account for the bias introduced. I also find that this alternative is the better of two evils, the latter being the bias introduced by the outside objective description.

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