

Discussant Paper for Human Ecology Studies

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Wagner Paper

I will begin my discussion with Dr. Wagner's and Dr. Lanham's paper. It is a most interesting paper and has considerable importance to us even though the author's failed to discover significant associations between selected independent variables and attitudes held about zoning. I was somewhat disappointed, however, with several aspects of the paper. The authors failed in my opinion to construct a theoretical model using the literature review as the underpinnings. The authors go to some length to demonstrate that literature exists in the area of land use but fail to show in my opinion why and how the variables relate to each other. No formal theory was articulated and no central theoretical position was taken (conflict theory, stratification theory, etc). The paper gave me the impression that the authors reviewed the literature and selected specific findings from human ecology studies to see if they were applicable to zoning attitudes. The authors did not in my opinion demonstrate how general studies in land use were generalized to zoning, for example, what connection could be made between Firey's classic study on sentiments and symbolism and land use in Boston and attitudes toward zoning. Conceptually it is possible (I think) and may have been done but I saw no evidence of it in the paper. What I am suggesting is that the conceptual gap between the land use studies quoted and attitudes toward zoning was not bridged.

The conceptual scheme presented in Figure 1 gave me initial hope that the authors were going to "put it all together" but this proved not to be the case. The conceptual scheme has considerable merit but could be improved upon by showing the interactive effects of the variables selected for presentation. The figure suggests some sequential ordering of factors

which probably does not reflect reality since most of the factors included are clearly interdependent.

Due to the aforementioned problems of theory, I believe the authors begged the question in regard to the development of testable hypotheses. They appeared to select some variables and say they will be significant in reducing the variance in the dependent variable. I would like to pose the question why and how should the variables be related to attitudes toward zoning? I am suggesting that no rationale was given for positing that the variables should be significant. They lay the case for attitude formation in general (Figure 1) but do not relate the model to zoning.

I also would like to know why the particular variables were selected for testing when the literature review was developed along different lines. The literature citations and conceptual scheme (Table 1) appeared to me to lay a case for such factors as various types of attitudes, sentiments, beliefs, values, differential socialization and so forth but basic demographic and SES variables were used in the research.

I would also like to know how two specific independent variables were operationalized. Those variables were "areal sampling unit household resides" and "cognitive knowledge of zoning regulations and procedures."

Another question I have is relative to the sampling procedure employed. How were the samples drawn? What criteria was employed in the selection process?

The frequency distribution findings are interesting in that what most people would anticipate was discovered (negative feelings). What was quite surprising was the relative lack of variance explained using the large number of "traditional" independent variables. In regard to the analysis I would

very much liked to have seen a correlation matrix included. I believe there is significant multi-collinearity and that given a correlation matrix one could see the relationships more easily. I would also suggest some mention made of the adjusted R^2 . Table IV presents percent of proportion reduced which I assumed is the multiple R^2 when the column is summed. If this interpretation of Table IV is correct then little variance was explained for the dependent variables especially y_1 and y_2 . It should be noted however that the authors spend considerable time discussing those findings and appear willing to suggest policy decisions on the basis of them. I would submit that in essence the study proved for the most part that the independent variables selected for analysis proved to have relatively little predictive power which I believe is an excellent finding. I would, however, be reluctant to tell decision makers to consider basic demographic and SES factors when they were not predictive, relatively speaking. I would have been much more impressed with the paper had the authors discussed why they felt that the variables were not predictive. What did they discover during the course of the study which may have been useful in explaining the apparent collapse of the variables in explaining the attitudes toward zoning? What factors would they now employ if they were to do the study again? I would suggest they consider the factors they started with (sentiments, various attitudes, beliefs, etc) but abandoned for some unexplained reason.

Catton Paper

Dr. Catton's paper begins with a most interesting implicit chastising of American Sociologists. He suggests that we become culturally blind. He suggests that it is important to step outside of what I shall term the "comfortable surroundings." I mean by comfortable those established patterns of observation and perception which become operative for us to the extent that we need new experiences to enhance the relevance of our research activity. In this respect I agree without reservation with the author. I would go even further and say we should occasionally step outside of our "comfortable" theory and methodology and get "turned on" again.

I wish Dr. Catton had explored this idea much further since we agreed so well but he did not and subsequently we did not.

He immediately turned to criticisms of directed research using the Lynds' work as a case in point. He submits that research should be broad scope in nature (broad scope in terms of discussion of interpretations).

While this may be entirely appropriate for specific micro-level studies (some minute problem) I find this position fraught with pit falls.

While it is true that important ideas, implications, potential decision making information may be partially or entirely ignored in focused research (careful analysis of some specific aspect of a totality of phenomena) the end result is more complete in terms of the phenomena under investigation. What I am suggesting is that extremely valuable information of a more specific nature is provided about some phenomena when the research is more narrowly focused and analyzed in terms of specific implications.

It is readily conceded there are benefits and costs associated with either position. The benefits associated with specificity are that much

about a particular phenomena may be understood (causal relationships for example). The costs are that potential substantive information may be lost or relegated to a lesser role. The primary pit fall of broad scoped research (looking at the totality of a phenomena) is that much may be observed but few causal relationships noted (this is especially true in my opinion of community studies to which the author refers). The researcher may be able to observe and quantify much behavior but not be able to demonstrate relationships among variables. The research, in essence, may observe and measure considerable phenomena and be able to explain little. In this regard, I believe the criticisms of the Lynds' work was somewhat misplaced. They established specific goals to be achieved and selected specific phenomena to be analyzed. They carried these goals through to a logical conclusion. I am not defending the Lynds' work but pointing out that specific researchers may always say that someone else missed the boat in terms of phenomena studied or implications discussed. It would have taken volumns to "adequately" study middletown and discuss all the ramifications of changes taking place.

I get a feeling that the author is saying implicitly that the areas of investigation by the Lynds could have been better directed toward land use than toward stratification. Someday people may be saying that what some of us are doing now was misdirected.

The author's introduction to the New Zealand situation and his subsequent discussion of extra-local control hit another responsive cord with me. He contends that autonomy is often removed from local groups when they become interdependent. I could not agree more if you operationally define autonomy in a very narrow perspective of local decision making and do not consider alternatives opened to people as a result of outside influence.

If local decision making results in a relative uneducated group with reduced awareness of alternative life styles, stifles potential creativity, that maintains the status quo, reduces potential productivity and so forth, is local autonomy desirable? If you respond yes then extra-local interdependence is defined as bad. If on the other had you taken the position, as I do, that extra-local influences and interdependency could and often does lead to increased alternatives made available to people then autonomy could be increased. I am suggesting that one type of autonomy is replaced by another. Group autonomy will be replaced by increased individual autonomy. I believe this is particularly true if the choices of individuals are narrowly defined in local groups that are autonomous from other groups. I am left with a feeling that the author is arguing for group autonomy but autonomy for what?

Out-of-handness is never really defined, as I see it, in the paper. Is it a lack of control of one's destiny? Is it frustration of not having control of decision making? However it is defined is not as important perhaps as the fact that it probably is not a new phenomena as the author implies and perhaps did not emerge with high scale societies which have extensive interdependency. It is not too difficult to imagine individuals in rural isolated communities centuries ago who perceived their life as being out-of-hand (little individual controls since power was vested in some family or individual). What I am suggesting is the causal linkage between interdependence and out-of-handness was not completed to my satisfaction.

The author's apparent association of violence with loss of autonomy is thought provoking but again I fail to see the linkage with increased interdependence. I believe that it is possible to conceptualize the reverse situation in which interdependence provided people with more individual

autonomy (choices) but the social structure blocked them from achieving these new goals. In social systems which were autonomous from other groups a person's life could have been determined for the most part from birth to death but new opportunities were made available to them as a result of extra local influence. More autonomy could result in alternative choices which could be blocked by some means thus leading to frustration and rebellion. It may not be the loss of autonomy but increased autonomy (choices) that elicits violence.

It was most interesting to see the author change his direction somewhat in his discussion of Middletown's and other groups use of land resources. He argues for community autonomy and then criticizes local people for making autonomous decisions in land use. There are potential externalities, especially pollution which he makes a case against, in land use which perhaps could be handled better by community interdependency. The question that I have for the author is how do you reconcile maintenance of local autonomy in land use which have consequences (pollution potential) for other groups?

I was somewhat concerned that the author began his discussion of the problems of local autonomy and interdependence and finished his paper by reference to extractive resource exhaustion and cohesion. I believe he has the potential for two excellent papers but I would suggest a more focused approach so that each is more comprehensible for the reader.

Lassey Paper

The last paper by Dr. Lassey is an extremely thought provoking work. His initial discussion of the difficulties associated with communication of knowledge from generator of knowledge and the user reflects my own bias. If knowledge is to have utility for people it must be communicated to the user in a form he can understand. I would also submit that the mechanism for the dissemination of information in usable form that is proposed is also worthy of consideration.

The model presented in figure 1 is quite interesting and establishes the underpinnings for later elaboration. While the model looks good on paper I have some reservations that it would function as well as the author believes in actual operation. I personally believe that the potential exists for "research loops" to form. This would occur when planning systems discover new areas that need to be analyzed (secondary and tertiary effects which must be studied) which would result in new knowledge sources being generated which would be returned to the planning systems which in turn could be returned to the generating sources and the process would go on and on. I am suggesting that the time lag from information generation to dissemination may be even greater and more complicated than it is now. I believe this is particularly true in developmental activities that have the potential for tremendous unanticipated change. I am suggesting that analysis of secondary and tertiary effects of change could go on and on as new effects are discovered. I personally believe the time lag in academic research from generation of information to recommendation is particularly subject to this problem since researchers are constantly cognizant of the need for increasing the probability that the research findings are correct and that potential

consequences are anticipated.

The model as it is presented assumes that the secondary and tertiary effects of development can be anticipated but I did not see how this was to be achieved. Does the fact that interdisciplinary research provide the key to study of unanticipated consequences? Does the planning group have a unique ability to put it all together for the users or will they overwhelm the decision makers with additional interpretation for them to consider? Or will the planning group synthesize and make specific recommendations for action in which case they become the decision makers?

Another factor that I believe needs clarification is the assumed ability of the planning group to anticipate change. Many developmental projects are conceived long before they are implemented (water shed projects about 20 years after planning). What is there about the model presented that enhances the ability of the group to do this? Will they depend upon the generators of knowledge to interpret what the future will hold or will the model presented have some mystic characteristics that will enhance the ability of the group to achieve this goal? What I am suggesting is that no where in the model do I see any means of enhancing existing techniques for anticipating change.

I am somewhat uncomfortable with the conceptual scheme presented in figure 3. I am not optimistic that the planning function can be carried out primarily by some designated group even though the select planning group would undoubtedly be extremely capable. My reservation is based upon the belief that synthesis of many varied research efforts is beyond the capabilities of any problem oriented group. An interdisciplinary group which attempted to synthesize and collate all of the research done in one specific

research area such as water resource evaluations would be overwhelmed I am afraid. I personally believe that every individual researcher is in a much better position to make recommendations relative to his research area and to delegate the synthesis role to some group may prove to have dysfunctional aspects since the mass of information to be synthesized would probably preclude indepth analysis of numerous implications as I perceive the commission to be for such a planning group.

The author is much more optimistic than I relative to the role the planning group will have in determining what the future would be. I hasten to note that individual biases and selectivity of information analyzed by the planning group must be assumed away if the author's belief is to be realized. I have certain doubts about that assumption.

Table 3 is a beautiful conceptual framework which has tremendous utility but like the social action process or the systems approach in planning, it is very time consuming. If the developmental activity must be instituted in a short period of time as many developmental activities must be, then it has the same problems as the social action process or the systems approach. I am suggesting it will probably by necessity have to be shortened in many operational situations. I do, however, applaud public participation which I believe to be essential. I also recognize the seriousness of misdirected developmental activity which could have tremendous negative consequences for the subject group (good planning is essential).

I believe that social planning must be responsive to reality. A group may research the problem and research the problem and research the problem to ensure that the implications are identifiable before acting. In the meantime the group may have resolved the problem or the problem may have

resolved the group. Either way the planning was of little consequence. It was an excellent paper but I am left with the feeling that the centipede had after consultation with the owl. It appears as though the centipede was having foot problems from walking over the hot asphalt and asked the owl for advice. The owl said walk twelve inches off the ground. The centipede replied that he could not do that. The owl replied that I have solved your conceptual problem and the problem of implementation is yours.

General Summary

I would conclude by saying that the Wagner-Lanham paper focused attention upon the relative inadequacy of demographic and SES variables in explaining attitudes toward zoning. I believe this to be a tremendous finding. I believe they were headed in the right direction with attitudes, sentiments, etc. and would hope they pursue their research in this vital area.

Both Catton's and Lassey's papers indicated that we can benefit tremendously from the experiences of other societies in the land use area. Catton suggests that we as researchers develop "tunnel vision" and subsequently lose sight of much important information. I soundly applaud that position with the reservations mentioned.

Dr. Lassey's paper focuses attention upon the problems of communicating knowledge in usable form to decision makers and users in general. His model has considerable merit and I would certainly like to see it used on at least a pilot basis. I would hasten to add another alternative which is centered about the professional himself. Perhaps professionals should quit writing for each other and write for the people we were hired to serve.