Title: Theory Driven, Empirically Grounded, Policy Relevant Research: A Research Dilemma

Abstract: This article presents a case study of the structure and logic of the author’s dissertation, in order to facilitate the preparation of undergraduate and graduate student theses and dissertations. Designed for use in proposal writing seminars or research methods courses, the article stresses the value of identifying the originating, specifying and subsidiary research questions; clarifying the subject and object of the research; situating research within a particular research tradition, and using a competing theories approach. The article stresses the need to identify conceptual problems and empirical problems and their associated conceptual and operational definitions. The article also presents a typology of generalist and specialist forms of social research.

Introduction

One of the central concerns of social work and social science is how to utilize social science theory to inform empirical research, while at the same time producing results which can inform new developments in social policy. However, the difficulties of producing social research which combines theory, data and policy relevance result in a well-known dilemma. Often, one or another of these three elements falls by the wayside along the way of conceiving, designing, funding, carrying out and reporting the results of the project.

There are a number of conceivable hindrances to the production of such research. Funding sources are often more interested in producing empirical findings about policy effectiveness than in building social science theory. Academic disciplines prioritize the testing of theories with data that are not necessarily related to specific social problems or applicable to program and policy analysis and formulation. Furthermore, the need of social science research
to be relevant to a variety of historical periods and geographical settings often means that the
data collected aren’t easily applicable to present-day social policy concerns.

There is a substantial literature on the design of social research (Becker, 1998, p. 5),
including volumes on writing theses and dissertations (Becker, 1986), and on the mysteries of
epistemology (Laudan, 1996; Little, 1991). But there is little published research which explicitly
discusses the dilemmas associated with combining theory, data, and policy relevance in social
research. For instance, a search of the literature in social work and sociology shows that no peer-
reviewed article has explicitly addressed teaching and learning about the preparation of proposals
for research projects, theses or dissertations. This is despite the fact that courses and seminars to
support such work are an important component of the curriculum in social work and in
sociology.

Each research project is the product of an individual’s own sociological imagination, and
conceiving and carrying out research is often as much art as it is science. But there is still value
in seeking to understand and explain how one scholar sought to respond to calls for policy-
relevant empirical research while at the same time identifying emerging theoretical concerns.
This article draws upon the guidance provided by the author’s dissertation co-chairs and the
author’s own experience in order to present the lessons learned from the preparation of the
prospectus and introduction of a single completed dissertation (Authorlastname, 2003). In doing
so, the article seeks to achieve a number of objectives.

First, it conveys the importance of distinguishing between the originating, specifying and
subsidiary questions of a research project (Kimeldorf, 1998; Tilly, 1990). Second, it stresses the
importance of clearly distinguishing between the subject and object of the research (Ragin &
Zaret, 1983). Third, it explains the value of incorporating an explicit section on the thesis’s
theoretical relevance, as well as a section on the topic’s social policy relevance. Fourth, it suggests identifying the specific research tradition within which the thesis is written. Fifth, it explains the competing theses approach to the integration of theory into social research (Fligstein, 1990; Laudan, 1977; Platt, 1966; Romanelli, 1989; Tucker, Hurl, & Ford, 1994). Sixth, it discusses the relationship between the use of existing theories and originally defined concepts derived from those theories as part of the development of the thesis’s conceptual framework. Seventh, it demonstrates how distinguishing between conceptual problems and empirical problems was helpful in clarifying the logic behind the conceptual and operational definitions employed as part of the thesis’s overall research design (Laudan, 1977).

In order to reinforce these objectives, this article presents a retrospective case study of a dissertation which was consistent with the generalist model presented above (Authorlastname, 2003). As such, this article is not applicable to the preparation of theses which are specialist in nature. Also, as a case study, it cannot lead to generalizable conclusions. The article merely serves as one example of an effort to combine theory, data and policy relevance. The paper may be of value to students preparing proposals as well as to teachers of methods courses and thesis proposal seminars. Because a dissertation is a very personal thing, the remainder of this article is written in the first person singular. First, however, the article presents a typology of approaches to social research which seeks to clarify the approaches which are best suited to combining theory, data and policy relevance.

Approaches to Social Research: A Typology

This article is applicable mainly to what are referred in the below typology as generalist research projects, rather than works which are referred to as specialist in nature. This section defines what is meant by generalist and specialist contributions to the literature.
Works which contain most of the following sections are defined as being generalist in nature: (1) a research topic (which specifies the research questions and clarifies the subjects and objects of the research), (2) a literature review supporting the topic, ideally including its theoretical and policy relevance, (3) a discussion of the research tradition within which the work is being done (an optional component), (4) a conceptual framework including theoretical perspective, conceptual definitions and identified hypotheses, (5) identification of the ontological, epistemological, and methodological assumptions (an optional component), (6) identification of the empirical data to be utilized, including discussion of how the data are relevant to the topic chosen, the literature and research tradition identified, and the conceptual framework employed, (7) a research design capable of employing the identified data in order to solve an empirical problem by connecting operational definitions to empirical data and presenting and interpreting the results. The author contends that in order to be theoretically driven, empirically grounded, and policy relevant, a research design at least broadly consistent with such a generalist framework for social research is required.

An alternative to a generalist research project is one which makes a specialist scholarly contribution. Such contributions focus mainly on one of the above seven elements. Examples would include (1) a paper proposing research on an important research question or research problem, (2) a literature review concerning an important social work or social science-related topic, (3) a historical narrative of an important research tradition within social work or social science, (4) a purely theoretical book, article or thesis, (5) a specialized discussion of the philosophy of social science, (6) a purely empirical article which presents important original findings, (7) a purely methodological contribution such as discussion of a new form of empirical research or data analysis. Research which focuses on just one of these seven elements is defined
as specialist in nature. Other contributions to the literature may focus on two or three of these elements. A simple typology such as this may help to clarify the kind of contribution a scholar wishes to make.

**Research Topic as an Exercise of the Sociological Imagination**

I formulated the initial question for my dissertation in a Coney Island restaurant in downtown Toledo, Ohio in August of 1999. Nursing a cup of coffee, I was contemplating the rejection of a funding proposal for dissertation-related research which sought to apply Perrow’s society of organizations perspective to a study of older adult volunteer and voluntary association participation (Perrow, 1991). I picked up a copy of the *Toledo Blade* and read a brief item about the way in which the growth of property tax exempt property was seen as reducing funding for public schools in Ohio's large cities. At around this same time, I was reading Jonathan Kozol's book *Savage Inequalities*, which discussed how unequal property tax revenues reinforce educational inequality (Kozol, 1991). I jotted down on a napkin the following question: “Is it possible that there is something about the growth of public and nonprofit institutions - the very institutions often held responsible for addressing America’s urban social problems - which may have exacerbated these problems?”

Next, I remembered something which Bill Gamson said at an American Sociological Association panel in Toronto in 1997, “Professors like it when you change the question and ask the one you wanted to answer.” Furthermore, perhaps because of the emotional impact of reading Kozol’s book, I had a question consistent with a classroom comment of Howard Kimeldorf: “You have to write a dissertation which grips you in the heart.” Later, he clarified, “A dissertation comes from the heart, but is guided by your head.” (Howard Kimeldorf, classroom comment 1996, and personal communication 2006, cited with permission.)
The first lesson here is that getting away from the ivory tower (in this case going to Toledo) may be associated with exercising the research imagination. In other words, identifying and cultivating a research site may come before identifying a research topic, not afterwards. This experience also demonstrates the importance of not giving up when one’s original topic doesn’t pan out. Finally, it shows that one’s theoretical predilections can be applied to more than one topic as well as at more than one level of analysis. For instance, I moved from the individual and organizational levels in the field of gerontology, to the organizational and institutional sector levels in the fields of economic and fiscal sociology. But I used the same theoretical perspective in both (Perrow, 1991).

Originating, specifying and subsidiary questions

Possessed of an initial question, I consulted Kimeldorf’s memorandum on four key questions which should be answered by a research proposal (Kimeldorf, 1998), all four of which are identified in this article. The first question is: “What do you want to know?” In other words, what is the substantive problem? Kimeldorf suggested distinguishing between the originating and specifying questions of a research topic. An originating question addresses the larger issue or concern which first draws you to a problem. A specifying question concerns the unique way in which a study seeks to address a problem that is related to the originating question.

This is an especially useful distinction that Kimeldorf borrowed and adapted from his mentor Maurice Zeitlin, who in turn borrowed and adapted it from his mentors (private communication, Howard Kimeldorf, 2002, used with permission). However, Charles Tilly suggested beginning research with a wide-ranging question (here termed the originating question), exploring theory relevant to such a question (here most closely related to the adoption
of a specifying question), and finally posing an even more manageable subsidiary question which can be answered with available data and is relevant to the larger question (Tilly, 1990).

What I had so far was an originating question, the “Is it possible...?” question identified above. Next, I asked myself: “Had anyone else raised an issue related to the originating question?” Yes, it turned out (McEachern, 1981; Mullen, 1990; Quigley & Schmenner, 1975; Swords, 1981). But there had been no recent sociological study of the social system of real property or the question of property tax exemptions.

Still, the question as initially posed was not specific enough. I would need a second, or specifying question. That question was: “Is it possible that the growth of the property tax exempt real property of public, nonprofit and religious organizations has had significant unanticipated negative consequences for urban schools, governments and communities?” Just as I earlier wondered whether an unintended consequence of the growth of demand by large organization’s for volunteer labor was a decline in grassroots voluntary association membership, I was now wondering about the unanticipated consequences of a another form of purposive social action (Merton, 1936), namely the granting of property tax exemptions to public, nonprofit and religious property.

In some cases, a specifying question can serve as the research question. In such cases, the research process can begin on the basis of that question. In other cases the specifying question needs to be still further refined and narrowed along the way. Often a third, subsidiary question should be identified (Tilly, 1990). My subsidiary question was the following: “Has the growth of property tax exempt public, nonprofit and religious sector property been characterized by the displacement of developed property from the tax rolls, or has it primarily involved the development of undeveloped land or underdeveloped or deteriorating property? In short, did this
growth involve displacement or development?” In other words, starting with the identification of originating and specifying questions, I added a third, subsidiary question.

However, this subsidiary question, as well as the competing displacement versus development theses which were embedded within it, originated in yet another kind of question. That question was a counterfactual question, a “what if” question about what might have happened differently if something had not already happened. The development thesis suggested the necessity of asking a counterfactual question concerning what would have happened to the property becoming exempt if it had not in fact become exempt. After all, if the property which had become exempt was underdeveloped land or property which was no longer viable for residential, commercial or industrial use, and if it was now to be put to good use by government, nonprofit or religious organizations, how would that produce a negative social consequence?

Answering a counterfactual question about what would have happened to such parcels of property if they had not become exempt would have been difficult or impossible. The data requirements and design dilemmas would have likely been insurmountable. However, it was still important to entertain and recognize the possibility that there was a counterfactual question associated with the originating and specifying research questions. This is because counterfactual questions often help shed light upon the logic of the research design necessary for answering the research questions at hand. For instance, identifying that counterfactual question helped me to realize that the mere removal of the land value of undeveloped land from the tax rolls was likely a positive externality-generating form of development, rather than a negative externality-generating form of displacement. Recognizing the counterfactual question was valuable in helping me to constrain and discipline my research as it proceeded. And asking it was responsible for the formulation of my subsidiary question and for the overall research design.
Charles Tilly has argued (Tilly, 1996, p. 596): “Counterfactual explanation makes social science a powerful complement to ethics and politics.” Clearly, counterfactual questions can contribute to research which is theoretically guided and policy relevant. Accordingly, one lesson learned from this experience is to move from originating to specifying questions, explore whether there are any relevant counterfactual questions, and finally to consider whether a more specific subsidiary question should be asked.

Finally, this experience shows that while there is value in applying the research design framework suggested by your advisors, it is also important to apply other approaches you find in the literature. For instance, I applied Kimeldorf’s guide to drafting a research proposal, but also adopted Tilly’s approach. Also, on the suggestion of David Tucker, I identified competing theses (rather than seeking to test one thesis against a null hypothesis), identified the research tradition in which I was writing, and clarified which conceptual and empirical problems I was addressing (Fligstein, 1990; Laudan, 1977; Platt, 1966; Romanelli, 1989; Tucker et al., 1994). However, I applied these perspectives on the basis of my own reading and interpretation of these approaches, and related them to earlier work which had relied upon a realist epistemology (Bhaskar, 1975, 1986; Steinmetz, 1993; Wright, 1986). Similarly, no scholar would wish to follow the precise steps of this article in their own research. A final lesson here is that even after data collection has begun, it’s not too late to ask new counterfactual, specifying or subsidiary questions or to further clarify one’s personal philosophy of social science.

The Object and the Subject of the Research

Ragin and Zaret defined the object of social research as the observational unit (Ragin & Zaret, 1983), as represented by the data you are employing. The object of research is not to be confused with the objectives of research. The subject of research concerns such things as
relationships among variables, the nature of a social mechanism (Elster, 1989; Hedström & Swedberg, 1998; Stinchcombe, 1991), or any number of other subjects associated with qualitative or quantitative social research.

The object of my primarily quantitative historical comparative sociological research was the property valuation of real property in 17 urban cities and counties in Ohio since 1955. The subject of my research was the nature of change over time in the relationship of real property which is taxable (residential, agricultural, commercial, or industrial), real property which is tax exempt (property used for governmental, nonprofit charitable, nonprofit educational or religious purposes), and real property which is subject to tax abatements. In other words, I was concerned with how these three property forms, which I considered to be three sub-systems of a social system of real property ownership, varied with respect to their taxable status. More specifically, the subject of my research was whether this change involved the operation of a particular social mechanism of theoretical interest. That mechanism was the process of externalization of costs (Perrow, 1991, 2002), in this case the externalization of costs from the exempt and abated sub-systems to the taxpaying sub-system. The externalized costs were seen as being generated when property owners within a taxing jurisdiction (a school district or a city) experience significant exemption-related reductions in revenue, via the displacement of valuable developed property from the tax rolls. When this happens, it is necessary either to increased property tax millage rates or to reduce the level of the millage-funded amenities. That in turn generates what is known as a negative property tax capitalization effect on the rate of growth of property values in that taxing jurisdiction, compared to adjacent taxing jurisdictions not facing increased millages or declines in the amenities financed by the property tax (McEachern, 1981; Oates, 1969; Tiebout, 1956). I defined that process as displacement when it involved the transfer of viable
developed property from the tax rolls to exempt status, although I argued that the transfer of undeveloped land was more consistent with a development thesis. The lesson here is that distinguishing between the subject and the object of research is an important step in clarifying the logic of how to engage in a theoretically informed analysis of empirical data.

**Literature Review: Social Policy and Social Science Relevance**

The second question which Kimeldorf suggested should be answered is, “Why do you want to know it?” Answering that question provides the intellectual rationale for the choice of topic and the research questions which have been identified. In order to do so, I next included a section on the relevance of my topic to social policy and to sociology. With respect to policy relevance, Zald has called for social research which is relevant to the key policy choices and social problems being faced by contemporary institutions, as well as how they are related to the study of elites and of power (Zald, 1987, 1999). Flyvbjerg has argued that social science can take account of social power by rejecting narrow scientism and by returning social science to its earlier status as an activity which is both practical and intellectual (Flyvbjerg, 2001).

Across the country, there have been longstanding and still unresolved policy debates about the propriety of property tax exemptions (Brody, 2002; Grimm, 1999), as well as about state policies designed to compensate for the effects of exemptions (Bowman, 2002; Carbone & Brody, 2002). However, my dissertation was not an example of normative economics, which makes judgments about policies, but of positive economic sociology, which examines the consequences of a public policy (Stigler, 1966).

Next I discussed the dissertation’s sociological relevance. I pointed out that while very little previous sociological research has studied property tax exemptions or the social system of real property, Harvey had discussed the concentration of public buildings in the inner city
(Harvey, 1973), and Molotch had discussed real property in his theory of the urban growth machine (Molotch, 1976). I argued that research on property is an important aspect of the larger discourses on space and place and the natural and built environment (Gans, 2002; Harvey, 1982; Swanstrom, Dreier, & Mollenkopf, 2002; Tickamyer, 2000). Summing up, I pointed out that from a policy relevance standpoint my dissertation responded to Zald’s call for policy-relevant social research (Zald, 1999). From a sociological relevance standpoint it responded to broader theoretical questions about the extent to which we live in a society dominated by large organizations and large societal sectors which externalize their costs onto the rest of society (Perrow, 2002). The lesson is that by including an explicit section on policy relevance and/or sociological relevance within a proposal or within the introduction of a thesis, the relationship between sociological theory, social research and social policy can be further strengthened.

**Research Tradition: What is a Research Tradition Anyway?**

Also related to Kimeldorf’s “why do you want to know it” question is another question: “Within which sociological research tradition do you want to situate your work?” According to Laudan, progress in science takes place within the framework of distinct research traditions (Laudan, 1977). First, I reviewed literature on the relationship of sociological theory to economic theory (Baron & Hannan, 1994; Zald, 1987). I cited calls for a sociology of vital institutions that would address property rights and the relationship of state structure and industries (Zald, 1999), calls for research on the structure of institutional spheres (Hinings & Greenwood, 2002), and the need for research on the link between organizations and social systems (Stern & Barley, 1996). After all, the institutionalist tradition often examined the unanticipated consequences of purposive social action (Merton, 1936), looked at the dark side of organizations (Hinings & Greenwood, 2002), and recognized that institutions didn’t always work
as assumed (Stinchcombe, 1997). Property-tax exempt public, nonprofit and religious real property represent what Tilly called a “systematic, durable social structure” (Tilly, 1996, p. 592). Property tax exemptions are an example of the institutional mechanisms discussed by Polanyi, of the highly institutionalized processes discussed by Selznick, and of the institutionalization of economic action discussed by Stinchcombe (Polanyi, 1944; Selznick, 1996; Stinchcombe, 1983).

I concluded that the institutionalist tradition, broadly considered, was the research tradition most closely related to my topic, question, theory and method (North, 1993; Rutherford, 1994; Scott, 1995; Stinchcombe, 1997). The lesson of this section is that by identifying the research tradition within which one is writing, and by systematically comparing your specific topic to that tradition, it becomes possible to identify literature which is either directly or indirectly related to your topic. The fact that the topic of the other research in the same tradition may differ from yours doesn’t mean that its theory and method won’t be applicable to your topic.

**Conceptual Framework: Conceptual Problem Addressed**

Having identified a research tradition, it next made sense to identify the theories that stemmed from or were compatible with that tradition, and to explain how they fit into the conceptual framework of my dissertation. First I related Perrow’s conceptualization of externalization of costs to the broader set of economic theories about external economies and diseconomies (Marshall, 1920; Mishan, 1971), more recently known as positive and negative externalities (Cornes & Sandler, 1996). Perrow’s concept, externalization of cost, is the latest in a long tradition of explicitly institutionalist interpretations of externalities (Kapp, 1950; Papandreou, 1994). Institutionalists contend that externalities are pervasive within capitalist economies but also exist in any society which has experienced the ascendance of large organizations (Perrow, 2002). Finally, I discussed the emerging sociology of externalities

Having identified my general theoretical framework (the theory of externalities), and the more specific theoretical perspective (Perrow’s concept of externalization of costs), I next identified the specific conceptual framework I would use. I linked a conceptual definition of negative externalities to an original conceptual definition of displacement. I explained that displacement involved the removal of viable developed taxable property from the tax rolls, thereby externalizing costs onto the taxable sectors. The operational definitions of displacement and development and the logic of a displacement percentage statistic were discussed later in the research design section. I then explained the relationship of positive externalities to my original conceptual definition of development, namely that development involved the removal of undeveloped land or non-viable property from the tax rolls for use for exempt public, nonprofit or religious purposes.

One lesson here is that there is value in defining a series of conceptual definitions. One set is fairly abstract. In my case the more abstract set included the conceptual definitions of externalities. That set was derived from the theoretical literature but was re-worded in a way which made sense to me. The next set was less abstract and was more amenable to serving as the basis for later operational definitions of those same concepts. (In my case this second set included the definitions of development and displacement.) In other words, just as the research question moved from an originating to a specifying to a subsidiary question, my conceptual framework moved from a general conceptual definition to a more specific conceptual definition.

This was my way of answering the third question posed by Kimeldorf: “What do you think the answer is?” (Kimeldorf, 1998). I hypothesized that there were two possible answers.
In other words, there were two competing theses which I would consider in my dissertation: displacement and development. These were not causal or explanatory hypotheses, in which independent variables are used to explain variation in a dependent variable. They were theoretically informed alternative characterizations of the nature of the growth and development over time of an important set of social institutions, the sectors associated with the ownership of property tax exempt real property. One lesson here is that theory is relevant not only to explanatory studies but to descriptive studies as well. Although exploratory research is often unable to test hypotheses drawn from general theory (Tucker et al., 1994), it can still generate logical hypotheses that are empirical generalizations about the data at hand (Romanelli, 1989). However, implicit in such an approach was some combination of ontological, epistemological, and methodological assumptions.

**Ontological, Epistemological, and Methodological Assumptions**

Michael Mann has pointed out that in some respects it matters little whether the sociologist advocates positivism, interpretivism, realism or some other epistemological point of view, since in reality the sociologist operates “as if they could apprehend and describe reality through the process of operationalization, and as if they could rely on absolute standards of scientific proof for their results to be evaluated” (Mann, 1981, p. 548, emphasis in the original). This is an important observation, but it is also important not to surrender to epistemological defeatism, whereby a scholar fails to pay attention to key ontological, epistemological and methodological assumptions. The adoption of methodology as “window dressing” (Laudan, 1977, p. 58), as well as the “let us keep the untidy problems of philosophy out of social science” approach (Layder, 1990, p. 5), can combine to inhibit a clear theoretical and empirical approach.
However, the burden of identifying such assumptions should not rest entirely on the individual scholar. This is one of the benefits of identifying the research tradition within which one is working. A research tradition should provide the ontological, epistemological and methodological tools for solving the empirical and conceptual problems arising from within that tradition (Laudan, 1977, p. 86). Furthermore, it isn’t always necessary to explicitly discuss such questions in the thesis itself. My dissertation did not do so, although I defended inclusion of a qualitative historical chapter on property tax exemptions in Ohio history by pointing out that both literacy and numeracy are important to research (Emigh, 2002).

My prospectus, however, did discuss my ontological and epistemological assumptions. I adopted the intermediate version of the microfoundations thesis, which requires an approximate account of the underlying mechanisms operating at the individual level (Little, 1991, p. 196). I also included a methodological appendix that consisted largely of a discussion of the post-positivist, realist-compatible approach of Laudan to my research (Laudan, 1977). In my prospectus, I also defended my simple mathematical model (a ratio of two ratios) by pointing out that sociologists should not let statistical methods govern their research design or choice of sample size. After all, an overly mathematicized approach can often result in the asking of less important questions (Sørensen, 1998; Stern & Barley, 1996), and to narrowness in the conceptualization of institutions (Stinchcombe, 1997).

While these methodological, epistemological and ontological assumptions were not included in my dissertation, they were explicit at the stage of the prospectus, when, arguably, they were most relevant. The lesson of the inclusion of at least some discussion of such matters in the thesis proposal or dissertation prospectus is that it ensures a level of comfort on the part of
the writer and the reader that the approach being used is appropriate given the questions being asked and the data being used.

**Data and Research Design: Empirical Problem Addressed**

In my research design section, I addressed the fourth question asked by Kimeldorf (Kimeldorf, 1998): “How do you intend to find out?” Kimelforf suggested first reviewing how others have sought to answer similar questions, explaining the logic of how you are doing so and further explaining how you will know if your answer is correct. In this section, I discussed how I would engage in an empirical test of my competing theses, with the full details deferred to a chapter presenting the results. But at the suggestion of Tucker, I embedded this discussion of competing theses in the context of Platt’s approach to problem solving and Laudan’s distinction between the solving of conceptual and empirical problems (Laudan, 1977; Platt, 1964; Tucker, 1994).

Conceptual problems involve complex theoretical issues which can often be solved by a strictly theoretical discussion. But in their simplest form, conceptual problems can be solved by providing clear conceptual definitions of abstract concepts and by devising somewhat less abstract conceptual definitions that are amenable to being operationalized using empirical data. After all, concepts are the building blocks of theory, and each attempt to define theoretical concepts helps solve conceptual problems. In addition, in the process of testing competing hypotheses stemming from such conceptual and operational definitions, it is inevitable that there will be further progress in understanding and refining such definitions. In this way, a theoretical contribution can be made by such research.

Empirical problems are often related to difficulties in developing the operational definitions of theoretically-imbued concepts. An empirical problem is not merely a problem of
measurement. Rather, an empirical problem is a problem related to how one can empirically test theories, in this case competing theories. A test of my two competing theses involved an important empirical problem, one that had never been posed or solved. That problem was how to quantify and interpret the extent to which the growth of property tax exemptions and abatements was consistent with the displacement thesis, the development thesis, or a null hypothesis (lack of a preponderance of evidence for either thesis).

In my case, solving the empirical problem involved the use of a displacement-development index and a displacement percentage statistic (the details of which are omitted here). By solving this empirical problem, I sought to make a methodological contribution about how to analyze trends in the evolution of the social system of real property and its exempt sub-systems. I also sought to make a theoretical contribution by testing competing hypotheses derived from Perrow’s society of organizations perspective and the larger sociology of positive and negative externalities. Finally, I sought to make a policy contribution related to ascertaining the extent to which property tax exemptions carry with them unintended consequences.

The lesson drawn from the research design section is that there is value in seeking to explicitly distinguish between a conceptual problem and an empirical problem, as part of a broader problem-solving approach to social scientific progress (Laudan, 1996). By translating the original set of research questions into a set of progressively less abstract but linked conceptual definitions, a conceptual problem is addressed and a theoretical contribution is made. By further restating the specifying or subsidiary research question as an empirical problem to be solved, it becomes possible to make a contribution to research methods and empirical analysis; to make a further theoretical contribution by testing the competing theses, and to make a policy contribution via the policy implications of the results.
Conclusion

This article has engaged in the retrospective examination of the logic and structure of the introductory chapter of a dissertation in social work and sociology. The article illustrates several steps found useful in producing empirical research that is both theoretically relevant and useful for informing social policy:

1. Present the originating question, specifying question and (where relevant) subsidiary question.
2. Show how the topic is relevant to social policy or social problems and/or the theoretical concerns of social work and/or social science.
3. Portray how the research is related to an explicitly identified research tradition.
4. In a conceptual framework section, discuss the overall theories utilized, contribute to the solving of conceptual problems in social science by presenting in your own words the conceptual definitions of the key concepts to be used, and re-define them in less abstract terms which your research design section can use to generate operational definitions linked to the available data.
5. Base your research design and the character of the data to be employed on a ontological, epistemological and methodological foundation with which you are comfortable and which is compatible with the research tradition you have identified.
6. Describe the overall nature of the empirical data to be employed, reserving details for the results section of the thesis or dissertation.
7. In the research design section, explain the logic of how you solved or plan to solve an empirical problem. Using either a traditional hypothesis/null hypothesis approach or a competing theses perspective, explain the operational definitions of the concepts defined in your conceptual frameworks section, and introduce your basic analytical method.
Ultimately, a thesis or dissertation is the product of the sociological imagination (Mills 1959). Our sociological imaginations lead to myriad questions and therefore it is often helpful to boil them down into originating, specifying and subsidiary questions. Our topics do not always have to be both social policy relevant and sociologically relevant, although presumably they must be one or the other if we write a dissertation in social work or sociology. Furthermore, we work from a wide variety of research traditions, although I have argued that it is helpful to identify the tradition which is closest to your topic.

Within those traditions, we use a wide variety of conceptual frameworks. C. Wright Mills noted that sociologists should not be constrained in the choice of theory by disciplinary boundaries (Mills, 1959), and so we often reach outside of social work and sociology when necessary in order to use other theoretical perspectives which can enrich our research. We grapple with a bewildering variety of approaches to ontology, epistemology and methodology, in order to find an approach which brings order to our often initially inchoate efforts to make sense of that part of social life which we are seeking to understand.

Most importantly, we each seek to research social life in a way which makes sense first and foremost to ourselves. We hope that what we do will make sense to our advisor and committee. Through trial and error, we use some combination of tried and true methods and the invention of a new wrinkle on qualitative or quantitative data collection and analysis. In doing so, we develop a generalist research design which can solve a perplexing but hopefully important empirical problem. Or we make an equally important contribution by engaging in a specialist contribution about particular historical or policy or theoretical or methodological or descriptive topic. In this article, I have reflected upon my own dissertation in order to make a contribution to learning and teaching in social work and sociology about the preparation of empirical research
which is defined as generalist in nature but which is also relevant to both social theory and social policy.

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