

Community and the Luhmann-Habermas Debate: A Neo-Weberian Ideal Type Solution

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² If this were the seventeenth century it might be appropriate to include as a subtitle: **to Epistemological Problems in Conceptualization of Ideal Type Models of "System" and "Agency" in "Community"**. However, the twenty first century favors short and succinct titles!

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A Neo-Weberian Solution**

ABSTRACT

When we examine any social science concept it is very important to be clear about the epistemological and ontological status of that concept. That rule applies to all of the key concepts used in sociology and rural sociology, such as “agent,” “community,” “situation,” and “system”. One key issue is the relationship between epistemology and ontology. What is the epistemological status and the ontological reality of the “objects” of investigation? For example, when we discuss something called “community” as a “reality” and not just as a concept, what is the ontological status of that reality? Connell (2003) proposes a solution based on Luhmann’s social theory. Habermas (1990 [1985]) critiques Luhmann’s (1984) systems theory on that point. But both Habermas and Luhmann assume that their view of ontological reality is correct. It may be that “community” is not epistemologically “real.” Since we do not have true laws of “community” it is not possible to provide a definitive answer to the ontological status of community. At the very least, the ontological status of anything called community can be questioned. Community is seen here as an artifact of our implicit “realist” epistemology. I propose a Neo-Weberian approach that emphasizes the use of an “idealist” epistemology and Ideal Type Models (ITMs). Weber’s use of ITMs is illustrated by his study of the Protestant Ethic. The implications for the study of “community” are drawn out. In rural sociology we study many things, such as rural communities, without being clear about the epistemological and ontological status of our concepts. We ignore the fact that we do not have true laws (Cohen 2001, Dewdney 1999). Hence, it is difficult to construct good theory and there is a gap between theory and methods.

Key words: rural sociology, sociology, Luhmann, Habermas, epistemology, ontology, system, agency, community, ideal types, real types, laws, science, Weber, Protestant Ethic, Spirit of Capitalism, Calvinism, this-worldly asceticism, merchant elite, Rachfahl, Netherlands, Low Countries, Synod of Dordrecht, Gomerus, Voetius, Counter-Remonstrants, Arminius, Cocceius, Remonstrants, Oldenbarneveld, Amsterdam, Antwerp, Belgium, Ideal Type Models, ITMs, definition of the situation, situationalism, W. I. Thomas, Dorothy S. Thomas, Park and Burgess

“The typical community is vanishing and it would be neither possible nor desirable to restore it in its old form.” – W. I. Thomas (1923)³

Part One: Luhmann, Habermas and Weber

I. Introduction:

In a recent issue of the Monitor on Psychology (Benson 2003) there are a number of articles on “the rural psychologist.” It is not surprising that a publication of the American Psychological Association would emphasize the role that psychologists have in helping people in rural areas. Nor is it surprising that rural areas have a shortage of mental health professionals. But when we learn that “rural communities experience roughly the same rates of mental disorder as urban and suburban communities” and that some behavioral problems such as “youth aggression, suicide and substance abuse, may be even more common” it is somewhat surprising.

After all, the image that many rural sociologists have of the rural community is that it is a good place to live. The stereotypical view is that in rural communities people have fewer problems than in suburban and particularly urban communities. The National Advisory Committee (NAC) on Rural Health and Human Services has made mental health one of its two main priorities for 2003. Michael Enright, a psychologist who heads the Mental Health subcommittee for the NAC, will deliver a report to the Secretary of HHS (Tommy Thompson) in February, 2004. It is likely that Enright’s Ph.D. in psychology will not encourage him to pay much attention to the more basic question: What is a rural community?

³ W. I. Thomas formulated the concept of the “definition of the situation” in several books and articles. But he also paid attention to community. In the same book (The Unadjusted Girl) where he discusses the definition of the situation he also discusses the family and “community as a defining agency.” The quotation refers specifically to the Polish *okolica* or neighborhood. Thomas has in mind a traditional community such as he and Znaniecki describe in The Polish Peasant in Europe and America.

When we compare a “rural community” to an “urban community” are we talking about the same thing? When we add to that the notion of a “suburban community” then are we really using the term community to mean nothing much more than a geographical location? The terms are not defined very carefully by psychologists. They focus on the individual. If they go beyond the study of the internal aspects of individual behavior (e.g. motivation, intelligence) then they might study the way in which the individual interacts with others in the same family or peer group. For most so-called “rural psychologists” the term “community” is left unexamined. Rural-urban differences are treated as matters of “cultural differences” among individuals (Kersting 2003). Even the Institute for Rural Health at Idaho State University seems to be oriented to a framework emphasizing “disorders” that are characteristic of individuals.

But if rural psychologists do not focus attention on the study of community, what is the contribution that rural sociologists make? Do rural sociologists have clear-cut answers? The answer must be that rural sociology as a discipline has not settled on a specific “operational definition” of the notion of community. In different research studies the use of the term community may be quite different. It is not uncommon to read about the African-American community or the Native American community. Even the United States as a whole is often described as a community. American society is a community, of course, but only in a very loose sense. Social science theory requires more precise terminological distinctions, such as those made to distinguish a community from a neighborhood or a network. But, the distinctions made in introductory textbooks (e.g. between such terms as an “organization” and an “institution”) are not maintained in consistent ways.

In a recent dissertation Connell (2003) has argued that the term community has been left very vague in the social science literature. It means many different things to many different people. No one definition prevails. He does an excellent job locating the problem. But, Connell also tries to

move beyond that “Tower of Babel” (Phillips 2000) in the literature. Certainly that is a worthwhile goal. But does he achieve that goal?

Connell (2003) argues that the way to clarify the true meaning of community is to approach the topic through the theoretical lenses provided by the German social theorist Niklas Luhmann. In this essay I will argue that Luhmann’s approach, while ingenious and intriguing, does not provide as satisfactory a theoretical perspective as a Neo-Weberian epistemological approach. To reach the objective of a better sociological understanding of rural communities we must take a different approach than the one that Luhmann advocates. But, if a different approach is required, then what should that approach be? Can a Weberian or Neo-Weberian approach really do the trick?

When I first thought about this question I assumed that I would find a more valid approach in the work of Juergen Habermas. I knew that Habermas and Luhmann were social theorists who shared a common “universe of discourse” and had worked together. I also knew that Habermas had critiqued Luhmann. I assumed that Habermas’ critique would provide fairly clear answers. I was wrong. I “discovered” that both Habermas and Luhmann tend to assume that the study of any “object” in social science can be done on the basis of an epistemology that posits the “reality” of that object. That is, both Habermas and Luhmann rely on a “Cartesian” view of the relation between researcher (subject) and community (object).¹

To take a simple example, stated in everyday language, it is commonly assumed that rural sociologists study rural communities. We may not agree on exactly what we mean by “rural community” but we know that we are talking about something “real.” The attitude is: I know one when I see one. For many practical purposes that is, of course, a perfectly adequate way to proceed. Hence, if a rural sociologist and a rural psychologist start to investigate rural communities and their problems they can begin easily enough by agreeing that the place to start is in geographic areas which are

not too close to significant urban centers. Ithaca, New York, is not a rural community. Even Guelph, Ontario, is not a rural community. Ithaca is not New York City and Guelph is not Toronto, but neither “town” is what we usually think of when we think of a rural community.

But beyond that it is not always altogether clear whether or not a geographically isolated small population settlement is a rural community. Do the majority of the people living in that settlement have to be engaged in agriculture? Is occupation the main criterion? We are all familiar with the difficulty that emerges when we try to provide a clear and precise definition. If our goal is theory rather than simply practice then the possible permutations make things more complicated. In order to develop a heuristic theory of community it becomes even more difficult. The level of analysis that social theorists like Habermas and Luhmann engage in is so far removed from the practical workaday world of the rural psychologist or rural extension agent that it may seem more like pure “philosophy” than empirically-based theory. But that level of “discourse” is important. A really fruitful theory of community requires moving beyond common sense categories and everyday platitudes.

In this essay I will try to show that both Habermas and Luhmann are mistaken. It would be possible to argue that Habermas is more correct than Luhmann, or vice versa. But they both seem to miss one very important methodological consideration. To explain that will require some complicated exposition.

II. Laws in the Exact Sciences: Real Types and Ideal Types:

Let me start with the idea of a scientific law. A true **law** can be defined as a statement of empirical regularities found in data collected to test hypotheses which are in principle applicable to all times and all places. Luhmann does not discuss community in terms of laws. Habermas does not discuss community in terms of laws. Neither of them is discussing the notion of community in terms of scientific laws based on exact measurement (Dewdney 1999, Schermer 2001). They do not get beyond a

conceptualization of generalizations that is essentially “philosophical” and speculative rather than “scientific” (Cohen 2000).² But that, of course, is not uncommon in social science (Ritzer 2003). Indeed, few sociological theorists move beyond the kinds of discussions that characterized physical science in the Middle Ages!

But it is possible to move beyond the kinds of speculations that Habermas and Luhmann engage in. I believe that Max Weber (1864-1920) had an insightful approach to the solution to that problem, based in part on his understanding of the work of philosophers (Bakker 1995) and based in part on his grasp of the essentially sociological argument put forward by Wilhelm Dilthey (Bakker 1999, Lessing 2001).

Moreover, I will try to indicate why Weber’s methodological views are more adequate than those of either Habermas or Luhmann. The philosophical issues involve the philosophy of social science (Turner and Roth 2003), particularly issues related to epistemology and ontology. I believe that making a priori assumptions about ontology, in a context where the epistemology is not a matter of exact science, is problematic.

Yet, both Habermas and Luhmann make a priori assumptions about ontology in the context of a social science epistemology. Weber, on the other hand, recognizes that in the social sciences we cannot make such a priori assumptions about which philosophical ontology can be applied to a specific problem. Hence, Weber formulates the notion of an ideal type epistemology. The context in the exact sciences is the statement of empirically-based laws. But in the non-exact sciences there are no laws and therefore there are no real types. Hence, in contradistinction to the implicit notion of real types in the exact sciences, Weber explicitly formulates a notion of ideal types.

But Weber does not fully elucidate the implications of his ideal types and he tends to make ambiguous statements. He also takes ambivalent positions. Instead of formulating his own philosophy of social science he makes statements in the context of debates with other theorists or historians.

There is no systematic statement of his methodology. The first few chapters of Economy and Society are helpful, but they are not as complete as the various scattered remarks in Weber's oeuvre taken as a whole. But Weber never tied those scattered remarks together into one cohesive whole, no doubt due in part to his early death, at age fifty six. Hence, it is necessary to formulate a Neo-Weberian methodology that does more than simply hint at solutions or beg the question. Such a Neo-Weberian methodology is based on the notion of conceptualization of "generalizations" rather than laws. Social science generalizations, I believe, are based on Ideal Type Models (ITMs). But before explaining Weber's methodology and the Neo-Weberian extension of Weber's views it is important to start with the Habermas-Luhmann debate itself.

III. Habermas' Critique of Luhmann

Juergen Habermas (1999 [1985]: 368-385) writes a succinct critique of Niklas Luhmann's (1984) Soziale Systeme. In essence, Habermas argues that Luhmann's use of the notion of "system as an agent" is based on a false premise. Habermas clarifies why, in his opinion, Luhmann was led to that false premise. In this analysis I accept Habermas' main point about the ontology of the system that Luhmann uses. I can accept the idea that there is a sense in which systems do not have agency. However, I do not accept the implication that Habermas draws out of that in terms of a further epistemological point. Habermas tries to argue that systems do not have agency but individuals do. I think that is incorrect. In my view, there are no scientific grounds for believing that either systems or individuals have agency. It is a question that cannot be settled scientifically. It has the metaphysical status of a Kantian "antinomy."

Responding in 1985 to Luhmann's (1984) book on social systems, Habermas writes the following (Habermas 1990: 368):

Niklas Luhmann has presented us with the "basic outline" for a general theory of society. In it, he draws up an interim balance for the expansive and decades-long development of his theory, so that we can now survey the project as a whole. At least, one thinks oneself better able to comprehend

what is going on before one's eyes. It is not so much the disciplinary tradition of social theory from Comte to Parsons that Luhmann tries to connect up with, as the history of problems associated with the philosophy of the subject from Kant to Husserl. His systems theory does not, say, lead sociology onto the secure path of science; rather, it presents itself as the successor to an abandoned philosophy. It seeks to inherit the basic terms and problematics of the philosophy of the subject, while at the same time surpassing it in its capacity for solving problems. [emphasis added]

That statement by Habermas indicates that there is a sense in which Luhmann's solution to certain sociological problems goes back to philosophical assumptions. Those philosophical assumptions are often discussed in terms of the philosophy of "the subject." The philosophy of the subject is the philosophy of the human individual as a social actor and independent agent, with some degree of free will. Most of the Post-Cartesian philosophers, like Kant, assume that the individual human being is a rational actor. Indeed, the important difference between "Man" and the other animals was felt to be the capacity to use "Reason."

Luhmann's innovation is that he challenges the idea that individual human beings are the main social agents on which our attention should be focused. For Luhmann, following a Durkheimian line of reasoning that was congenial to the later Parsons, it is more important for sociological theorists to be concerned with the degree to which social collectivities have agency.

Habermas stresses the fact that social theory has often viewed the individual as the ontologically real entity and the collectivity as an epistemological construct. But, instead of thinking of Luhmann's notion of system as having agency as less correct than the concept of human beings having agency, I would like to suggest that both "systems" and "human actors" are not ontologically real. They are ideal types. That is, the notion of a "system" should not be mistaken for a real type. An ideal type is whatever we choose to make it, given our more limited theoretical and empirical objectives, short of real laws. A real type is real in so far as it is a component of an exact law. The empirical problem cannot be settled a

priori. Without the law the type cannot be ontologically real in the same way as, say, “gravity” or “uranium” are “real”.

The difference between a concept or type being “real” versus “ideal” gets confused when we think in common sense terms about concrete, material reality. For example, there is a sense in which a computer is real. The computer screen that is in front of me as I write these words seems real to me. The letters appear on the screen one by one as I type. Moreover, the chair I am sitting on and the table where my books and papers are arranged are all “real.” But that is not what is at issue.

In a science like physics, chemistry, biology, geology or astronomy it is, of course, taken for granted that pulleys, water, trees, rocks and stars are real. But those material objects are not what the sciences are about. In grade school we get a very simplistic view of science that stresses concretely real objects like frogs and water, but that elementary understanding is suitable only for children. The concretely real is not yet a “real type.” A real type is something that exists as a reality in the context of a scientific theory that is based on an empirically-based and widely accepted law. It is a law of chemical reaction, for example, that the atomic structure of the component elements affects the reaction. The concept of “atomic structure” is real, even though it is very, very abstract.

Weber understood the difference, although he did not make it absolutely clear. But he argues in various scattered essays that the concepts he is interested in are “ideal type” concepts and not “real” because he recognizes that they are not stated in the context of laws. Hence, the epistemology and ontology implied by Weber in his use of ideal types is a better solution to the empirical question that lies behind the essential problem at issue between Habermas and Luhmann. It is a better solution because it is based, at least implicitly, on more adequate conceptualization of the epistemological status of scientific concepts and the ontological status of scientific “objects.”

Habermas seems to think that he is closer to the empirical reality of human actors than Luhmann. But, in the absence of any true laws, there is no clear reason why Habermas should be correct and Luhmann should be wrong. The solution that Luhmann proposes is a priori but the solution that Habermas proposes is also a priori. Habermas stresses the reality of agency, but in doing so he simply makes assumptions about human actors versus systems as actors. Habermas makes a priori assumptions about human agency. He does not consider the possibility that the notion of “agency” has the same ontological status as the discredited concept of “phlogiston” (Cohen 2000: 225-236, 515-516). Luhmann and Habermas both make a very important mistake. Yet, a solution was available. That solution was glimpsed at by Weber.

IV. A Neo-Weberian Solution:

We have already alluded to the Weberian solution to the dilemma that is posed when we consider the point at issue between Habermas and Luhmann. Should we think in terms of individual human beings as having agency and utilize a kind of “methodological individualism” in discussing “community” or “structure”? Or should we emphasize the way in which structures have agency and think of collectivities as a “reality sui generis”? I believe it is a false dilemma. Moreover, it is not just a matter of different “aspects” of the same underlying phenomena. That is, the idea that the whole question hinges merely on one’s perspective is not really an adequate solution.³

Even though he is, of course, in a position to do so, Habermas himself does not utilize the epistemology or ontology implied by the Weberian ideal type approach in a systematic manner. Hence, since even Habermas does not seem to appreciate the full implications, it is necessary to provide further clarification of the epistemological implications of the Weberian approach.⁴ In the process of clarifying Weber’s epistemology, however, it becomes apparent that Weber was not explicit enough in formulating his ideas concerning ideal types.

Hence, a Neo-Weberian approach to ideal types is proposed here as the solution to epistemological and ontological issues at stake in the Habermas-Luhmann debate. The essence of my Neo-Weberian approach is to postulate Ideal Type Models (ITMs). An ITM is considered to be a set of ideal types. A set, of course, can have one or more members.⁵

I would like to argue that almost all concepts used in social science are ITMs. That is a Neo-Weberian epistemological argument because Weber did not take quite so radical a position. Weber vacillated somewhat in his statements concerning methodology. Moreover, he is often not very precise in his formulations. Weber, for example, never explicitly discusses the notion of a “model.” While there is much information about his methodological approach in Weber’s writing, it is not altogether surprising that even Habermas has not necessarily drawn the right conclusions about Weber’s epistemology since Weber himself is not necessarily completely clear. I do not want to be in the position of having to defend the arguments about epistemology and ontology that I make here as being purely Weberian. That would be a discussion of interest mainly to a small group of Weber experts.⁶ Instead, I will simply assume that my position is Neo-Weberian in that it may somewhat alter some of Weber’s original intentions. Nevertheless, I find inspiration in Weber’s methodological, theoretical and substantive work, so I will use Weber’s oeuvre to draw examples.

In this analysis of the epistemological problem at issue I argue that neither Habermas nor Luhmann gets it quite right. This is relevant to discuss here since Connell (2003) has recently argued Luhmann’s systems theory is heuristic for investigating the concept of “community” and community itself. But the objections that Habermas raises against Luhmann’s epistemology suggest that Luhmann has not provided a definitive solution. Therefore, Connell’s solution may also be less than conclusive. I would like to propose a third alternative, based on Weber. Instead of Weber’s original formulation, however, I propose a modified

version of Weber's ideal type approach. It is argued that the Neo-Weberian approach supersedes both Luhmann and Habermas and is more useful for social science. The example of "community" in the Netherlands in the seventeenth century will be used to illustrate the three different epistemological approaches.

Parsons takes it for granted that the individual human being has agency. Luhmann was influenced by Parsons but rejects Parsons' use of Lockean assumptions about the subject. It has taken quite some time to "de-Parsonize" Weber. However, that is not surprising. In 1921, when Park and Burgess wrote their famous textbook, Weber was not mentioned once. It was the translation in 1930 of The Protestant Ethic and the Spirit of Capitalism (Weber 1930) that first made Weber well known to a wider audience in North America. The earlier translation by Frank Knight of The General Economic History (Weber 1927) had not quite done the trick. So it is not surprising that Parsons' The Structure of Social Action (1937) would have been viewed by many as a definitive interpretation of Weber. In the 1960s many people started to question the Parsonian view of Weber as a sociologist who "converges" theoretically and methodologically with Durkheim. That is, in sociological theory the transition from a Parsonian interpretation started in the late sixties. But, in sociology and rural sociology it is not as common to explore the philosophical issues. Even theorists ignore the questions of epistemology and ontology posed by seventeenth century philosophers like Descartes. There may be some lip service paid to Kant, Descartes, Locke or others, but sociologists in North America tend to ignore the scholarly literature on such thinkers.⁷ We think we know what Locke says. Yet, it is only now, in the twenty-first century, that we have escaped from the epistemological implications of Parsons' Lockean views.

What is Weber's methodological position when we strip away the layers of Parsonian interpretation? I believe that it is far more radical than has generally been assumed by most sociologists. I believe that Weber, like

Luhmann, goes right to the heart of the problem posed by Kant, Descartes, Locke and other philosophers. Weber does exactly what Habermas says Luhmann does. Weber, of course, did not have to concern himself with the disciplinary tradition of sociological (or social) theory. For Weber the main concern was with finding a solution to problems posed by his colleague Heinrich Rickert, the Heidelberg philosopher. Since I have rehearsed the general philosophical issues at stake elsewhere I will not repeat the contextualization of Weber's methodological position here (Bakker 1995). Suffice it to say that Weber drew on ideas proposed by Dilthey, Windelband, Lask and Rickert, as well as Simmel, and offered an independent view. Weber's conceptualization of epistemological and ontological questions is contained in scattered essays that he published in the period 1903-1920, some of which have been largely ignored.

IV. How Does This Apply to "Community"?:

I believe that Weber's solution to the problem of the "philosophy of the subject" is precisely what makes him a sociologist rather than a philosopher. In essence, Weber did not make an a priori claim about the subject and human agency. Instead, he proposed that sociologists and others should establish ideal types. Since sociology cannot develop true laws for most of the things that are of interest to sociologists⁸, the only option is to develop a *Wissenschaft* that is not an exact science but which is nevertheless well rooted in empirical materials, particularly comparative and historical information about societies and sub-components of societies.

Without going on at length about the philosophical problems involved, let me just state that the concept of community is always an **ideal type** concept, nothing more and nothing less. In other words, the conceptualization of "community" cannot involve a **real type**. In order to grasp what the distinction between an ideal type and a real type means for the study of something we call "community" it is necessary to quickly summarize a few points about the philosophical debate on determinism versus agency. In the exact sciences there are true laws which state causal

linkages and provide for prediction. A chemical reaction is subject to physical science laws. Yet, we speak of sociology as a science. That would imply that there must be some deterministic relationships (e.g. where “x” causes “y” in the presence of “z” *ceterus paribus*). Yet, when we think of human beings we tend to try to leave some room for something that is not entirely deterministic. We call that elusive something “agency.”⁹

V. Determinism versus Agency:

If we think of “science” we often think of the exact sciences. The exact sciences involve the study of natural phenomena so they are sometimes called natural sciences. Most natural phenomena are physical so we sometimes call them the physical sciences, or bio-physical sciences. But what is most characteristic of the bio-physical-natural sciences is that a very “exact” method is utilized. That may be one reason they are also called the “hard” sciences.

Take astronomy as an example, since it makes some things very clear. In ancient times in the Middle East, circa 1,000 BCE or earlier, observations of the night sky made it clear that the stars seemed to move in a very regular pattern. In the earliest stages of “Egyptian” and “Babylonian-Chaldean” astronomy there was also a very significant astrological component. But gradually, beginning especially in the eleventh century, the astrology and the astronomy started to separate. By the fifteenth and sixteenth centuries astronomy became an exact science and mathematical astronomers were dubious about some of the more grandiose claims of astrology. Nevertheless, it was really only in the last few decades that solutions to perplexing astronomical problems were formulated that took into account not merely the rotation of the earth around the sun (heliocentric theory) and the tilt of the earth relative to the sun (inclination of the ellipsis) but also the precise position of earth as a planet in this galaxy, the Milky Way, relative to billions of other galaxies in the universe (Carl Sagan’s “billions and billions”).

What astronomy as a science has discovered in terms of very precise predictions of the orbits of planets, asteroids, moons and other bodies, is based on very careful measurements made over hundreds of years, but also on a few fundamental **laws**. A law of exact science is a statement of a pattern that is always going to hold in all times and all places for a whole universe. The law of gravity, for example, is true for all places-spaces in the universe (S-u) and for all times (T-u) throughout eternity. The law of gravity, of course, was formulated by Isaac Newton circa 1688 (Goldstone 2000; Cohen 2001).

In bio-chemistry and physical chemistry a parallel law is the law of the periodicity of the elements. The fundamental periodicity of the elements according to their atomic number is accepted by all physical scientists. Improvements in the precise details of the Periodic Law have occurred since Mendeleev, but the basic idea has been around for more than two hundred years.

Without going into detail on these issues (Cohen 2001) it is sufficient to point out that sociology does not have one recognized exact law that is generally recognized by ninety-five percent of all sociologists. (The notion of one hundred percent agreement is not necessary.) There are no laws in sociology, or, more precisely, no laws have been developed yet. The methodological question is whether or not any such laws can be developed. Can sociology ever be an exactly science which has true laws that hold for all times and spaces (T-u and S-u)?

No one knows the answer. It may be that sociology will become an exact science someday. But now sociology is not an exact science with true laws. The same holds as well, of course, for rural sociology. There is much precise information and “tons of data,” as they say. Speculations of all kinds abound. But laws do not exist. At least, they do not exist yet.

Rather than continue to discuss the problem abstractly, let us take a case study example. The case that comes to mind immediately when we think of Max Weber is his famous study of the Protestant Ethic. How does

Weber's Protestant Ethic thesis help us to understand his methodology of Ideal Type Models? How does Weber's use of Ideal Type Models in the study of the Protestant Ethic in the seventeenth century help us to see how a concept like "community" is an Ideal Type Model and not a real type or real type model? Moreover, how does it help us to see that historically-specific questions are not a matter of "system" versus "agency"?

Part Two: The Case Study Example

I. The Protestant Ethic Study as a Case Study:

Concepts in sociology are viewed by Max Weber as having the epistemological and ontological status of ideal types. They are not "real" in the same way as the atomic number of Oxygen is real. Weber developed his ideal type approach in the early 1900s at about the same time as he was writing and then defending his essays on the Protestant Ethic (Weber 1904, 1905) and the Protestant Sects (Weber 1906) in relation to the "Geist" of modern capitalism.

Weber did not think of "Protestants" in England and Holland in the late seventeenth century as having or not having "agency." Instead, he thought of the problem of the rise of a spirit of modern capitalism as a problem involving a new set of parameters for "social action." He argues that "this-worldly asceticism," which had previously characterized only those who devoted themselves to a thoroughly religious life in monasteries and cloisters, became a way of thinking, feeling and acting for middle-ranking merchants, craftsmen and traders.

The Weber Thesis has been criticized by many people for many reasons. Indeed, some sociologists have even labeled Weber's ideas a completely "misconstruction" (Hamilton 1996). But those criticisms beg the question because they do not take Weber's methodological stance seriously. They try to evaluate Weber from an epistemological perspective that is not

his own. Weber, however, was not someone who ignored historical “facts” or who made simplistic “causal” arguments.

In another essay (Bakker 2003) I re-examine criticisms of Weber’s famous Thesis on the Protestant Ethic that were made by Felix Rachfahl in 1909-1910. The criticisms mainly concern Weber’s alleged ignorance of details concerning the case of the Netherlands in the seventeenth century. Rachfahl maintains that most Dutch merchants were not strict Calvinists. He argues that “Weber was mistaken about Calvinist influence in Holland” (Hamilton 2000: 170). Weber responds, I believe, by indicating that he is not concerned with Calvinism per se. What he does not do very clearly in the Replies to Rachfahl is describe his newly discovered epistemology.

I believe that Weber was writing about the Protestant Ethic and the Spirit of Modern Capitalism with an epistemology that is based on what I am calling Ideal Type Models. It is an Ideal Type Model of “this-worldly Protestant asceticism” that Weber is concerned with, not Protestantism itself. He examines the Protestant Ethic as it emerged in modified Calvinist beliefs and in various sects such as the Quakers and Anabaptists. But he is not doing a historical study of Protestantism. He is also not mainly concerned with the big financiers and very rich merchants (*Heeren*) but focuses on those who were most likely to be motivated by “this-worldly asceticism,” the middle stratum of Dutch society.

Rachfahl, a historian, interprets specific historical events as refuting Weber. For example, he sees the execution of Johan van Oldenbarneveld as evidence that Calvinism did not have the impact Weber implied. But Weber’s argument is not invalidated by the case of Oldenbarneveld. In general, Weber’s Replies to Rachfahl show that the empirical concerns Rachfahl emphasizes do not necessarily detract from the heuristic value of a sociological statement of “elective affinity” between ideal types. It is not a question of essentialist Calvinism causing economic forms of Capitalism, but of Protestant this-worldly asceticism tending to “hang together” (*innere Zusammenhang*) with the ascetic “*Geist*” of modern capitalism. Properly

interpreted, Weber's Thesis matches the evidence. In the Netherlands the Protestant Ethic was one factor in a very complex historical situation and Weber was fully aware of the complexity of that historical situation.

The situation in the seventeenth century is complex, but the evidence does not contradict his ideal type sociological argument about Protestantism. Weber was fully aware of the "partial subduing of Puritanism in Holland" but did not over-emphasize the working of Protestantism in the Netherlands. In the process of discussing the Protestant Ethic in the Netherlands Weber also clarified his ideal type approach. Part of the reason Weber did not clarify his methodology in 1904-1905 was that he had not explicitly formulated that methodology at that time. He was still in a process of discovery. Hence, Rachfahl cannot be faulted for not fully grasping what it was that Weber was trying to do. Weber himself was probably not entirely clear on the epistemological problems involved. Rachfahl saw Weber as making a historical descriptive argument. But Weber was not making a strictly historical or "idiographic" argument and he was not using words in quite the same way as a descriptive historian used words in the early 1900s. He was using ideal types.

Here his methodology is extended to encompass the notion of an Ideal Type Model (ITM). It is further argued that notions of "community" are always ITMs and never Luhmann's "systems". When Weber writes about the situation in the Netherlands in the seventeenth century he is concerned with such "systems" as the political system and the military system. But, he was not a Structuralist or a Structural-Functionalist. He was not thinking of "systems" are ontologically real in that way that the early Durkheim seems to have thought of collectivities as a reality sui generis. The idea that something can be sui generis assumes that it is universal, but Weber never argues that the "Protestant Ethic" was universal. Instead, he saw it as clearly limited in both time and space.

Max Weber is correct concerning historical details of the Dutch case. He makes it clear that the "liberal" ideas of the rich merchants of

Amsterdam do not imply he was wrong about the Protestant Ethic. Weber makes it clear that the history of the Netherlands in the seventeenth century does not provide a clear-cut refutation of his arguments. But what he is not perfectly clear about is the way in which his arguments are not really intended as idiographic historical descriptions in the first place. He also does not make it completely clear that he is not postulating nomothetic scientific classifications. Instead, he is operating with something that can be conceptualized as mid-way along the continuum between the purely idiographic and the purely nomothetic.

Let us take as an example the discussion of strict Calvinism in the Netherlands. Weber comments on the Dordrecht Synod, which was a reassertion of strict Calvinism against the “Arminian Remonstrants” by the Counter-Remonstrants. The strict Calvinist view on observance of the Sabbath was taken by men like Gijsbert Voet (1589-1676), known as Ghijsbertus **Voetius**. Voetius was opposed to Johannes Cocceius (1630-1669), who maintained that since the observance of the Sabbath is a Hebrew custom it is not, strictly speaking, a necessary part of commitment to Christianity. In general, Cocceius based his theology on study of the original Hebrew text rather than a “systematic theology” where components had to fit together. Voetius was opposed to such ideas espoused by the Remonstrants, who were asking for more liberties. (They were “remonstrating” or, we might say, “silently” demonstrating, against strict observances, even though they continued to attend religious services very regularly.) Voetius is referred to as a “Counter-Remonstrant” (*Contraremonstrant*) because he was opposed to a somewhat less stringent application of “systematic theology” to everyday life behavior. (An analogy can be made between “Counter-Remonstrants” on the “left” within Protestantism and “Counter-Reformation” Catholics on the “right” outside of Protestantism.) These kinds of debates came to a head at the Synod of Dordrecht in 1617-1619.¹⁰

The importance of the Synod of Dordrecht (Dortmund) for seventeenth century Dutch Calvinism cannot be exaggerated. Both Weber and Rachfahl must have appreciated that point. While many groups in the Netherlands may have deviated from the strict wording of the Dordrecht (or, Dort) decrees, they nevertheless set an important standard. (The Synod set an absolute standard in religious dogma, in a world in flux and military upheaval, in somewhat the same way as Greenwich sets the absolute standard of time.) After 1619 the ritual re-visiting of the documents every so many years (usually every three years) was one way in which strict Calvinism was upheld in the absence of a state church (Rooden 1998). As Weber says, "... the set of beliefs around which the great political and cultural conflicts in the most highly developed capitalist nations in the sixteenth and seventeenth centuries – the Netherlands, England, and France – were fought was Calvinism" (Weber 2002a: 55). The political conflicts in the Netherlands in the seventeenth century were often carried out under ideological banners which tested the limits of the idea of predestination and the "Calvinist complex" of ideas. Voetius was one of the theologians who managed to get the Dordrecht Decrees passed. He argued that any form of "rationalism" or "latitudinarianism" was wrong and dangerous.

The conflicts among elites and social classes did not start in the seventeenth century, of course. The roots of conflict among members of elites and elite groups in various cities existed long before the United Provinces became independent de jure in 1609, or even de facto after 1574. Moreover, religious ideas were part of class, status and power conflicts even before the Hapsburgs took over in the Low Countries.

Almost one hundred years later it is possible to be somewhat less passionate about many of the arguments. We are not in danger of being burned at the stake if we get it "wrong." Moreover, Weber's ideas have had one hundred years to be absorbed. They are not quite as fresh as they were in 1904. Even Weber himself might not feel quite so compelled to defend them in every way if he were still alive today.

II. Some Questions:

Weber tends to deny the historical possibility of a Protestant Ethic before the sixteenth or seventeenth centuries. However, with respect to the history of the Low Countries, broadly conceived, from 1300-1800, there clearly was a kind of “ascetic vocational” ethic that existed before Luther’s translation of the Christian Bible into German. Yet, at the same time, the removal of hindrances to the full blossoming of an ascetic vocational ethics was no small matter. Weber may have been correct to point to the importance of the intensification of asceticism among members of Protestant sects (including Lutheranism as a sect in the early stages of the Reformation). It is one thing for a merchant to work individually, without institutional and organizational support for his this-worldly asceticism. It is quite another to belong to a sect where such activity is encouraged, indeed required.

Despite the complexity of the situation in the Low Countries over a five hundred year period, it is clear that a slow undercurrent of asceticism in merchant and manufacturing activity was building up over a long period in certain restricted areas, particularly urban Flanders, and that the slow buildup (Oberman 1963) eventually became a major structural change during the Protestant Reformation. The intellectual and theological shift did not happen all at once (Ozmet 1980). Ethical commitment to one’s vocation may have existed among laypersons before 1500 in certain restricted areas, such as Antwerp and Brussels or Bruges. But after 1500 the revolutionary implications of the schism in religious ideology made for a completely different *Geist* or Ethos. In parts of the Holy Roman Empire which are today considered Germany the struggles of the 1510s and 1520s are a significant indicator of the extent to which the change, when it finally boiled over, had revolutionary social implications (e.g. Russell 1986, Oberman 1994) To the extent to which Weber did not clearly see the gradual build-up before 1500, albeit in a very small and limited geographical area, he should be faulted. Rachfahl was quite correct to call

him on it. R. H. Tawney and many others later took up the same argument.¹¹

The American Revolution took place in 1776-1783, more than one hundred and sixty years later. The early seventeenth century situation was quite different with respect to the separation of church and state than the much later eighteenth century context. Things were taken for granted in the eighteenth century that were hardly dreamed of in the seventeenth. The parameters for acceptable behavior were very tightly defined. The conflict with Hapsburg Spain lasted for a long time before it was fully resolved in 1648. The Dutch Protestants were far from lax, even when they were not “Counter-Remonstrant” strict Calvinists. Yet, at the same time, statements about the relative laxity of Dutch Protestantism abound in the literature that is critical of Weber. Rachfahl was arguing that the Dutch case could not be supportive of Weber’s arguments since the rich merchants of Holland were not, generally speaking, strict Calvinists. But the rich merchants did not oppose the trial of one of the great founders of the Dutch Republic, a Benjamin Franklin of Dutch independence, John of Barneveld. Weber’s remark on the execution of the “Attorney-General” Jan van Oldenbarneveld is found in a long footnote to his First Reply to Rachfahl in 1910.

I have attempted to explore the issues that seem to divide Rachfahl and Weber in another essay (Bakker under consideration 2003). Weber seems to score several points against Rachfahl. For example, the case of Oldenbarnevelt tends to support the Weber Thesis in so far as it indicates the vital importance of strict Calvinism in the rise of the Dutch Republic. However, we benefit from the fact that Weber was forced to be much clearer than he had been in 1905. The 1920 version shows a number of refinements that can be credited to the exchange of opinions with Rachfahl (Weber 2000: 157-203). The enormous wealth of information found in the case of the Low Countries has only been hinted at in the exchange. Weber himself makes many useful remarks and cites well-known Dutch and Flemish scholars of his own day. But it will be valuable for sociologists and

historians to continue to link the Low Countries to the Weber thesis. The five hundred years of Low Country history from 1400 to 1900 need a fuller exploration than Weber has given to them in his work on the Protestant Ethic.¹²

But no empirical work can ever provide a definitive empirical substantiation for the thesis that the relationship between the *protestantische Ethik* and the “*Geist*” of modern capitalism. Hunt and Murray (1999: 242-243) raise arguments concerning the history of business and conclude that Weber was wrong, but even they acknowledge that there was a “new age” of business after 1550. They do not examine Weber’s ideas as based on Ideal Type Models but simply as historically idiographic-descriptive statements. The detailed study of business enterprises is important as an empirical test of historical aspects of Weber’s sketch. But, those are different issues, not primary for Weber’s own sociological concerns.

In this essay I have touched on the ways in which Weber constructs Ideal Type Models of the “*Geist*” of modern capitalism and the “this-worldly Protestant asceticism” of those times. Any historical criticisms that do not consider Weber’s “middle of the road” epistemological intentions and tries to see his work as a contribution to descriptive history or exact science tends to miss the point. They do not really refute Weber’s important heuristic ideas. The empirical case of the Netherlands in the seventeenth century tends to support Weber’s Thesis concerning the elective affinity between Ideal Type Models of this-worldly Protestant asceticism and the spirit of modern capitalism.

If Weber were an idiographic historian or an exact scientist interested in law it might be possible to show that he does not prove a historical or a physical science explanation of modern capitalism. However, he never makes the argument that the evidence he cites supports the argument that Calvinism caused Capitalism. For Rachfahl to assume that that was Weber’s Thesis was based on Rachfahl’s limited knowledge of Weber’s Ideal Type approach. He should certainly be excused for not fully

understanding what Weber was trying to do since Weber himself was just beginning to articulate his sociological methodology.

III. Part Three: Conclusion:

A. The study of community

The study of “community” involves studying something that has not been adequately conceptualized in the literature. Dave Connell’s dissertation serves to accentuate the need for clearer definition of this key term in sociology and rural sociology. But Connell chooses to try to solve the problem by utilizing an approach articulated by Luhmann. I believe that Luhmann’s systems theory cannot solve the problem of community.

Instead, I believe that a Weberian or Neo-Weberian approach is necessary. It consists, in essence, of conceptualizing “community” in terms of a specific historical and geographical context. In other words, the concept of “community” has the same epistemological status as the concept of a “Protestant Ethic.” For a comparative and historical sociology the concept of community is an Ideal Type Model. It can only be conceptualized as an ideal type and not as a real type. However, the goal of that conceptualization is not “history” in the sense of idiographic description. Instead, it is “sociology” in Weber’s sense, as exemplified in his study of the specific historical context of northwestern Europe (Netherlands, England, Scotland, parts of Germany, Switzerland) in the seventeenth century. Weber does not argue that there is a “system” called capitalism which is always the same. He argued instead that there is a “spirit” of modern capitalism that is unique during one period of history and he makes it very clear that by the nineteenth century that unique “*Geist*” was already largely gone.

That implies that there is no general and complete answer to the question of “community.” Although Ferdinand Toennies believed that there is something real called *Gemeinschaft*, he was wrong. He was

wrong in so far as he did not recognize that the concept of *Gemeinschaft* is merely an ideal type label. At best it is a carefully articulated Ideal Type Model that can be used as a heuristic device. It is possible, of course, that there was a real shift between the Middle Ages and the Modern Era. But to describe that shift as a process of social change from Community to Society does very little to elucidate matters and does a great deal to confuse matters further. The concept of “community” as *Gemeinschaft* in Toennies (and in much of rural sociology) is a reification. The word “community” cannot refer to something that is ontologically “real” unless that word is used within the context of a law in exact science. It would have been much better if Toennies had conceptualized *Gemeinschaft* as an Ideal Type Model, useful for empirical investigation.

The quest for a complete and general definition of community is a quest for a “real type” or Real Type Model. That cannot be accomplished. We cannot make “community” into a Real Type Model unless we situate our conceptualization of community into a context of the statement of exact laws.

The notion that we can determine what community really is by using the approach advocated by Luhmann is naive because Luhmann starts with false premises concerning the goal of sociology as a social science. Luhmann leans too far in the direction of positivistic epistemology, in part due to the Durkheimian ideas that were taught by Parsons. The use Luhmann makes of analogies with systems theory in physical science are misleading, to say the least. For example, when Luhmann discusses “cybernetics” and tries to extend ideas concerning cybernetics to sociological theory, he ignores the way in which cybernetics in engineering and physics is based on a very precise set of physical measurements. Engineering is an application of physical science laws to specific circumstances. That is possible because those laws are valid for all times and all places in the universe; they are true laws. But

cybernetics cannot be used in sociological theory to cover situations where there are no laws and where there are no relevant precise measurements.¹³

Rural sociology has often been naïve in terms of the philosophical assumptions built into theory. Very frequently rural sociologists have done empirical studies of a situation in one time and place and have then over-generalized the findings from that time and place to a universalized generalization for all times and places. The “diffusion of innovations” literature in rural sociology sometimes suffers from this desire to formulate what appear to be empirical “law-like” generalizations. Similarly, the sociology of community and the sociology of religion literatures suffer from the same “abstracted empiricism” and over-extension of findings.

B. Conclusion:

Both Luhmann and Habermas are influenced by the Cartesian view of the relationship between the “subject” (ego) and the “object” (community). For Habermas the stress is on the way in which the active subject is able to define the situation. One way to think of Habermas is to emphasize the degree to which he is a Neo-Kantian. Kant, of course, emphasized the ways in which we see what we learn to see rather than some phenomenologically real “object” or “other.” The man or woman who has lived in a small town in Iowa all his or her life is likely to have a different “apperception” than the visitor from another state who merely spends a few days in that town. So, in a sense, Habermas stresses that “beauty is in the eye of the beholder.”

Luhmann, on the other hand, while starting with the same Cartesian notion of a subject viewing an object stresses the extent to which both subject and object are “cybernetic systems.” The subject can exhibit a certain degree of “autopoiesis” but the community can do the same. It is a very attractive modification of the simpler idea that it is only the subject

that is active. For Luhmann all “systems” have a certain degree of “agency.” When we speak of “America going to war” that is not merely a metaphor or other kind of trope. America, a complex system and macro-level “community” acts as a unified cybernetic system in a manner analogous to the actions of individual human beings or much smaller systems (like universities or corporations). It is an interesting way of extending the notion of agency. The idea of system is turned back on itself and the human being is viewed as really just a system within systems. The insight involved is analogous to the idea that the earth is not the center of the solar system (geo-centric theory) but rotates around the sun (Nicholas Copernicus’ helio-centric theory).

Yet, Luhmann’s social theory does not fully acknowledge one very important epistemological point. When we discuss a society as a social system we are not discussing something ontologically as real as a human being. That is the case not because the human being is more ontologically real than the system (or community) or because the system (or community) is more real than the individual. It is also not just because both system and person are real. Instead, it is a fourth alternative, not seriously explored by either Luhmann or Habermas.

The fourth alternative is the Weberian or, at least, Neo-Weberian epistemological view that neither the system (community) nor the person (individual human being) are real in any epistemologically useful sense when we are not constructing theory in a nomothetic exact science. Both the system and the person are conceptualized within social theory (and sociological theory) in terms of Ideal Type Models. Such ITMs are always situated in terms of time and space.

When we discuss “community” in the abstract in sociology or rural sociology we discuss a conceptualization of an Ideal Type Model of community, nothing more and nothing less. We cannot simply use the word and be done with it. We have to go further than that. We have to be very specific about what historical time period and what geographical

region our generalizations are meant to apply to. In many instances the time period will be “now” (meaning this decade) and the region will be “here” (meaning our part of the country where we happen to be living).

None of this is meant to deny the fact that today, here and now, I, Hans Bakker, am a real person and that each one of you individually is a real person. Nor am I trying to say that on July 4, 1776 or September 11, 2001, there was not a recognizable entity called the United States of America. But I am trying to say that on September 11, 2001, the United States was a quite different “system” than on July 4, 1776. And I am quite a different person at age fifty-six than I was at age six. Fifty years makes a difference in the life of an individual and two hundred years makes a difference in the life of a nation-state.

If we are going to generalize about a nation-state or an individual, or any other sociological “object,” then we have to specify what we are talking about. In the jargon of positivism we have to provide “operational definitions.” But the reason we specify precisely what we mean is not so that we can define it in real terms. Instead, somewhat paradoxically, we try to be as precise as possible so that we can define it in ideal terms!

Even more paradoxical is the fact that when we can make our definitions so “ideal” that we are able to begin to articulate true laws we come up with concepts that have an epistemological status as predictors and that can be considered ontologically “real” for all practical intents and purposes. The very abstract and elusive concept of “gravity” is “real.” But the concept of “community,” although also very abstract, is not.

There is no “law” in sociology that allows us to predict the “force” of community in the same way that the law of gravity allows us to predict the force of gravity, *ceterus paribus*. There is no scientific generalization in sociology that allows us to conceptualize social change in communities as due to “natural selection” in the same way as genetic traits in animal species are due to natural selection. The main reason is that “community”

is not “natural” in the same way as the force of gravity and natural selection are natural.

In so far as community is an aspect of “human nature” then it may be possible to develop laws of community. I think that the field work and other research work that Frans de Waal and his team have done on bonobos is a hint of what could be done in the natural scientific and ethnographic study of human beings living together. Some of those working in human ecology in anthropology have approached the non-cultural side of behavior in similar ways. So it is not entirely impossible to think of some “laws” that are true for “all” relevant times and places on earth and that apply in principle to all homo sapien sapiens. (The “relevant” times and places can be only the last 100,000 years in places where we know there was human habitation.) In psychology, particularly what used to be called “physiological psychology,” there are a number of such laws. Some kind of neurological psychology and evolutionary social psychology is promising scientifically.

But it is not those aspects of community that most sociologists and rural sociologists are principally interested in. Instead, we are mainly interested in human communities similar to the ones we ourselves feel that we have experienced.

For a number of years I lived in a small town in rural Alabama. It is less rural now than it was in the 1960s, but Fairhope, Alabama, like nearby towns such as Foley, are not metropolitan. No one would mistake Fairhope for Chicago. But other than the gross differences it is very hard to make sociological generalizations. One confounding factor in Fairhope is the large number of retirees, many of whom have moved from up North. They live in a small, Southern, semi-rural town but their mind set may still be very urban and they may still be “Yankees”. I tend to believe that nothing I have ever learned in rural sociology or in sociology makes it possible for me to adequately predict whether Elora, Ontario, Hood River, Oregon, or Cortez, Colorado, are precisely the same as Fairhope. Yet they

are all “communities” and they are all “rural.” But what I can do is construct an Ideal Type Model of the small rural town in North America in the 1960s and then try to see to what extent Elora, Hood River, Cortez and Fairhope do or do not conform to that model. I can report my observations and they can be investigated further with whatever evidence is available.

By not recognizing that most, if not all, of our generalizations are based on the logic of ideal types we have often made mistakes. We have often assumed that our generalizations were valid for other countries in changed times. By failing to see our concepts are implicitly ideal type concepts we have often not formulated explicit Ideal Type Models that have allowed for careful investigation of similarities and differences.

In this brief discussion I have not attempted to cover all the bases. I have not detailed the way in which Weber gradually discovered the methodology that I am here trying to defend. That requires a different paper. Also, I have not tried to articulate the ways my Neo-Weberian epistemology is both similar to and different from the Weberian epistemology that I think one can read between the lines in Weber’s scattered writing on the subject.

All I have tried to do is to indicate that I think that both Luhmann and Habermas have not take full advantage of the notion of Ideal Type Models, as utilized by Weber in his famous Protestant Ethic essays. I have tried to indicate that the study of “community” in sociology or rural sociology is more likely to involve the articulation of an Ideal Type Model of community (in one set of times and places) than the precise delineation of what community really is. Finally, I have indicated that what is said here about the study of community applies to all sociological concepts that are not explicitly linked to exact laws. To the extent to which we can construct theoretical laws we can have real concepts. But to the extent to which we merely theorize in terms of comparative and historical generalizations we can only have ideal concepts and Ideal Type Models.

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End Notes

¹ Before Descartes the Scholastic philosophers, especially Thomas Aquinas, argued that the reality of the phenomenal world is due to the existence of God. Bishop Berkeley maintained an extreme view of that “idealist” position. Descartes, who was living in the Netherlands, was concerned to articulate a new epistemology that would be suitable to the new Post-Copernican astronomy and physics of his day. A thought-provoking Neo-Thomist critique of Descartes’ epistemology can be found in Deely (2001: 511-547, 763-830). However, Deely does not mention Weber or even Rickert. His few words on Windelband are inadequate to get a sense of German philosophy in the 1890s.

² This essay is indebted to the work in the history of science done by I. Bernard Cohen (1914 - June 20, 2003). In 1999 Professor Cohen completed an English translation of Isaac Newton’s Principia Mathematica (1687), the final stage of a project he initiated in 1957. See Cohen (2001) on the history of science. Unfortunately, Cohen has little to say about social science and the philosophical ideas relevant to social science.

³ Lloyd G. Ward of Brock University, who has been very active in promoting the web page on George Herbert Mead, clarifies the idea that an “aspect” is not the same as a “component.” If we use the term in the sense in which Mead would have used it in 1900 then “aspect” refers to the perspective in space, as in “the southern aspect of the White House” as being the White House as seen from the south lawn. The term “phase” has a comparable meaning for “time.” A phase is an “aspect” in time, not “of” time. Luhmann and Habermas are not just talking about different “components” of our understanding of concepts like community. It is not just a “Roshomon” effect, where each observer’s perspective is equally valid.

⁴ If Habermas did not get it right, then who did? I believe that Fritz Ringer (1997) makes some important statements that move us in the right direction. However, as Stephen Turner (2000: 7-10) points out, it is sometimes necessary to use Weber’s insights to study what Weber means! The problems associated with the interpretation of Weber are recursive and illustrate Dilthey’s hermeneutic circle. The interpretation of Weber that I come up with is based in part on the assumptions I make at the outset about Weber’s true intentions. Other initial assumptions lead to different outcomes.

⁵ It is also possible to think of a set as being an “empty set.” While such a set is possible, it is not always productive in social science. On the other hand, raising the possibility that our terminology refers to concepts which, in the final analysis, are essentially nothing but an empty set may not be such a bad idea. Then one has to differentiate between the model as an empty set and the reality to which the model is intended to refer as an empty set. These logical problems are tangential to the issues discussed here.

⁶ That does not imply that the opinions of Weber experts do not matter. But there is not as much consensus among Weber experts as one might wish to see. Hence, it would require a lengthy argument to sort out the differences of opinion among Weber experts on the issues involved. For example, Ringer’s (1997) analysis is not necessarily fully acceptable to all Weber scholars. Yet, it would require a book length monograph to discuss the subtle ways in which Ringer may have mis-interpreted Weber, if indeed he did do so. See Turner (2000). It might be the case that in the final analysis it could be shown that everything discussed here can be found explicitly in Weber’s own writings. But, I assume that even Weber himself was not entirely explicit about the epistemological and ontological significance of Ideal Type Models.

⁷ While it is not consistently made, there is an important distinction between “sociological” theory and “social” theory. In social theory there is more attention paid to philosophical issues. Habermas and Luhmann are really “social theorists” in the European mold rather than “sociological theorists” according to the North American model that has dominated up until very recently. One advantage of French Postmodernist critiques is that various epistemological and ontological issues have been rediscovered. In

North America they were largely hidden by the implicit “modernism” of thinkers like Parsons and even Parsons’ staunch critics. Ironically, Gouldner is as modernist as Parsons.

⁸ That does not mean that Weber feels that there can never be any true laws in sociology. It is possible that some true laws could be developed. I myself believe that neuro-science and socio-biology can help to produce some set of laws which could be called “sociological” or “anthropological.” But such laws would not explain everything. They would “merely” explain in a scientific manner that which can be explained scientifically in terms of laws! The argument is tautological, of course, but it is not a vicious circle. It is a heuristic tautology to realize that science can only “explain” that aspect of reality that is subject to laws of science. The famous quotation about “leaves blowing in the wind” indicates that science can explain the fall of a freely falling object, *ceterus paribus*, but it cannot explain in a completely deterministic fashion why in Boston in the 1830s Charles Follen fights for emancipation of the slaves while his friend and courageous supporter William Ellery Channing does not openly advocate freedom for black slaves.

⁹ In sociological theory the question of agency versus determinism is often discussed in terms of agency versus structure or agency versus system. The idea is reflected in everyday language when we talk about “beating the system” or “fighting city hall.” But the determinism of exact science cannot be evaded. That is one reason why DNA evidence has weighed so heavily in recent court challenges to murder convictions. It does not necessarily matter that the jury was convinced that the defendant was guilty of the crime if DNA analysis reveals the suspect’s DNA was not present but the DNA of another person was. We do not have “free will” or “agency” with respect to our DNA. The error ratio is so small (p less than .00001) that finding DNA tends to imply a unique occurrence. Similarly, nothing really “defies gravity” (it only seems to) and no one can make gold out of mercury using alchemy (but we can construct an atomic bomb).

¹⁰ The “agency” of a Cocceius or a Voetius can only be understood within the historical context. Very clear parameters were in place. Those actions which individuals could undertake were so narrowly defined that even very minor deviations could result in charges of heresy, exclusion from the community, imprisonment and even execution. If someone as prestigious as Jan van Oldenbarneveld could be executed then anyone could be. But that was precisely the point. Oldenbarneveld had to be executed so that thousands of others would be obedient in a time of impending war. Yet, at the same time, the “system” did not operate as a “cybernetic control system,” except in a very metaphorical sense. Even William the Silent had to bend to the dialectic of the situation, but at the same time, the Dutch Republic was both more and less than just the system established by any group of individuals. Everything was in process, including the lives of individuals attempting to meet complex internal and external shifts.

¹¹ Further empirical support for the notion of a kind of “proto-protestantism” or Protestantism *avant la letter* is the remarkable study of Paul Russell (1986). He examines popular pamphlets written between 1521-1525 in which laypeople expressed, perhaps for the first time in history, theological opinions. After 1525 there was severe repression of such lay pamphlets by public authorities in “Lutheran” cities like Augsburg. The eight authors Russell examines include an Imperial army paymaster, a weaver, a journeyman furrier, a shoemaker, a journeyman painter, two housewives and a Pastor’s wife. Hans Sachs, the Nuremberg shoemaker, was financially successful. See Russell (1986: 165-184). In another era it is likely that Hans Sachs would have been regarded as more than just a shoemaker. His arguments against hypocritical Lutherans are closely aligned with Weber’s notion of “this worldly asceticism.” Unfortunately, no comparable detailed study of lay theology in the Low Countries seems to exist.

¹² Weber also does not emphasize the Low Countries in his other work. The reasons for Weber’s relative neglect of the Low Countries are no doubt complex. Schama (1997: 340-341) makes many tantalizing suggestions but tends to mis-read Weber as specifically maintaining that it was the strict Calvinists themselves who were most likely to be the modern capitalist entrepreneurs. He argues that the Leiden textile manufacturers were close to the Weberian Ideal Type Model. He does not recognize that it was not wealth that was a reassuring symptom of predestination but the ability to use wealth to reinvest in an enterprise or to do good works. Notwithstanding strict Calvinist repudiation (Schama 1997: 124) of the possibility that philanthropy would help the Elect to be seen favorably by God, the concept of God’s grace

was not accepted in all its logical rigor. But he also tends to see Durkheim as somehow more relevant to the analysis of Dutch culture (Schama 1997: 569).

¹³ There is much “misplaced precision” in the social sciences. But there is very little relevant precision that lends itself to the formulation of true laws. Until we have a set of measurements that allow for prediction it will be useless to collect “data” about statistical samples drawn from hypothetical populations. The reason that activity is not very productive is that the theory which is used to formulate the “populations” is very primitive. Think, for example, about the largely implicit limitations in space and time in the formulation of the generalizations made in the average introductory textbook. A textbook that introduces sociology to students in the U.S. is perceived by students in Canada as far from universally applicable. If textbooks written specifically for Canadians were used in Texas or California the freshmen students would have to wonder why there was little or no discussion of “Affirmative Action,” the Supreme Court of the U.S., “African-Americans” or the “President.”