The Quantification of Society

A Study of a Swedish Research Institute and Survey-based Social Science

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Abstract

This thesis is concerned with the contemporary history of quantitative surveys in Sweden. The core epistemic practice of constructing surveys is examined empirically through a case study of the *SOM Institute* (Samhälle, Opinion, Medier) at *University of Gothenburg*. The *SOM Institute* has performed surveys in Sweden since 1986. However, the methodology of quantitative surveys with representative sampling techniques dates back to the 1940s. A central theme in this theses is to follow how these methods and techniques have been made to work under different historical circumstances.

Theoretically, this thesis relies on concepts that are derived from classical Actor-Network Theory (ANT) and then further developed. This conceptual tool-box is then utilized to select moments in the history of surveys that are of special importance for understanding how Swedish society has been quantified.

Special attention is drawn to how the accuracy of surveys is established through mutual reinforcement with previous data. By closely studying how the *SOM Institute* conducted their first postal surveys in the 1980s, the relation and importance of other, contemporary surveys is emphasized.

Moreover, the creation of a state-science interface is described by going back in time to the 1950s and the creation of the first academic surveys. This was also the moment in history when random samples were established. Here, the impact of the creation of the welfare state and the role of science in this political project is discussed and related to the expansion of the social sciences.

To further understand the border between academic science and pollster research, a controversy that took place during the elections of 1985 is studied. The controversy was ignited because pollster data predicted that the conservative party (Moderaterna) would win the elections. However, this turned out to be false. What followed was a debate concerning both the accuracy of different methodologies and the political bias of different surveys. Academic scientists succeeded in creating a position that guaranteed value-free social science, which later would have an impact on the future of social scientific investigations.

The dissertation concludes that the way social phenomena are quantified today, must be understood in a historical context that includes the epistemic practice of social scientists. The creation of large-scale quantitative surveys not only presupposes certain aspects of modern society, it also transforms these societies.

Keywords: Quantification, survey, SOM Institute, social science, epistemic practice, Actor-Network Theory, welfare state, center of calculation.

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In a more general sense, I have developed many insights during the local seminars in theory of science. My colleagues have been generous in their intellectual discussions, and it has been a true pleasure to be a part of this seminar culture. As I have presented my work to the seminar, I have always felt that the discussions improved and refined many of my thoughts. Amelie Hoshor, Erik Joelsson, Mats Fridlund and Henrik Lundberg have been discussants of my manuscripts and I have benefited widely from their comments.

Moreover, Gothenburg is home to an extended seminar culture that materializes outside the university. Here, I met Karl Palmås and Otto von Busch who taught me how to apply philosophical ideas to other aspects of life. Meanwhile, I have engaged in exciting discussions in the wilderness of the Internet. In the comment fields of blogs, I have debated many subject matters that relate to this dissertation with Rasmus Flesicher, Magnus Eriksson, Marcus Nilsson and many others.

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I am solely responsible for any remaining errors.

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Abbreviations

ANT   Actor-Network Theory

FSI   Forskningsgruppen för Samhälls- och Informationsstudier

IMU   Institutet för Marknadsundersökningar

SCB   Statistiska Centralbyrån (*Statistics Sweden*)

SHOT   Social Construction of Technology

Sifo   Svenska institutet för opinionsstudier

SND   Svensk nationell datatjänst

SNS   Centre for Business and Policy Studies

SOU   Statens offentliga utredningar (Government whitepaper)

SSD   Svensk samhällsvetenskaplig datatjänst

SSK   Sociology of Scientific Knowledge

STS   Science and Technology Studies

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Chapter 1

Introduction

It is an ordinary November day and I am browsing through a library archive of newspaper articles. I read that the neighboring town of Borås is perceived as a boring place by its inhabitants. People who live there go to cafés, restaurants, and the movies to a lesser extent than the average Swedish urban dweller does. A recent study conducted by the *SOM Institute* and the *Swedish Property Federation* (Fastighetsägarna) contends that this difference is due to a low population density and a population that is also older in high age. Conversely, the people of Borås have a strong local identity, and are satisfied with life (*Borås Tidning* 2010-11-02).

Every now and then, a curious reader like myself will wonder what the *SOM Institute* is all about. From the library archive web site, it is only a matter of seconds before I arrive at the official website[1], where I learn that the three-letter abbreviation of SOM stands for *Society*, *Opinion* and *Media*. The *SOM Institute*, founded in 1986, is described as an independent scientific research organization based at the University of Gothenburg. Each year, the *SOM Institute* administers a large statistical survey called the *SOM survey* (SOM-undersökningen), which

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[1] http://www.som.gu.se, accessed 2010-12-10.

measures the behavior, habits, views and values of the Swedish
population at the national, regional and local levels.

I continue my search through local papers and read that
Kungsbacka, only an hour's drive away, is the most attractive
city in western Sweden. The people of Kungsbacka are satisfied
with life, have a positive attitude, stay healthy, and are highly ed-
ucated. Moreover, they are tolerant, enjoy multiculturalism, and
almost eighty per cent are married or live in shared households.
Some people think that Kungsbacka feels somewhat too rural,
but the city has promised to build hundreds of new apartments
to promote a more urban feel (*Kungsbacka-Tidningen* 2010-10-
11).

I extend my searches through the newspaper database to also
include national papers. After the elections, many columnists
sought answers as to why the right-wing populist party *Swe-
den Democrats* was voted into parliament in 2010. According
to one article, this was partly because 45 per cent of the popu-
lation wanted to lower migration quotas, a statistical fact that
is repeated in several other news articles. These are some num-
bers reported by *SOM Institute*, as well. The attitudes toward
immigration have been measured since the 1990s by the insti-
tute. (*Aftonbladet* 2010-10-03) Another article states that the
votes for *Sweden Democrats* were so numerous because there is
a knowledge gap that made people with low education and low
incomes distrust the scientific and authoritative facts regarding
migration. This finding may be another cause for the rise of pop-
ulist parties, and it is suggested that the solution would be for
scientists to explain better to the public how scientific expertise
is created (*Dagens Nyheter* 2010-10-12).

I continue my search, slowly moving back in time. Over the
past decade, the facts from the *SOM Institute* have been widely
reported in the news media. I find 1,694 articles that I down-
load to my computer and start reading, randomly at first. I read
about fluctuations in opinions and values, trust in social insti-

tutions, lifestyles and social identities, abstract concepts from the social sciences. However, I also read about election results, attitudes toward nuclear energy, voting behavior, trust in the royal family and newspaper readership. One particular news report clearly differs from all the others. It concerns the decline in response rates for all types of surveys during recent years:

> [...] and there is no light on the horizon of the sky of statistical drop-off rates. Partly the modern [mobile] telephony have made it harder to get hold of people, and partly we are today a people fatigued by surveys, says Åsa Nilsson, who is project leader of the large SOM survey, a postal survey to nine thousand people.

> - What we see is a type of survey fatigue in society. There are nowadays just so many surveys, there is very many market surveys.[2] [i]

In all of the other articles, only the results of the surveys were reported. We learn from year to year what the facts of Swedish society are, what people think about different matters, how they live their lives, what is average, what seems to be constant and what changes over time. However, hardly anywhere, except in the brief radio interview transcribed above, it is described *how* surveys are made, what *possibilities* they afford, what *problems* they encounter and what *efforts* and *resources* are needed to create a survey of the population. On this superficial level of mediated facts, the surveys of the *SOM Institute* are blackboxed.[3]

What we see, what we read about and what we hear on the radio is merely the output of something that has produced scientific facts on a previous occasion, and all we learn about the

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[2] Sveriges Radio, Ekot (2010-06-01).

[3] The concepts of interfaces, blackboxing and assemblages will be thoroughly elaborated in Chapter 2.

input is that it is taken from a large survey, with a high but hard-to-achieve response rate. We are informed through a mediating interface about society in numbers, sometimes represented in tables and graphs, written and printed as an easy-to-read article. However, usually a number is sufficient. To learn that 45 per cent of the Swedish population wants to lower migration requires no further explanation, this figure is a type of scientific fact that is embedded in the general understanding of what society is. We do not need to know how these quantified facts were actually assembled, unless we are the dissenting type of person who wants to question these numbers.

Before approaching this black box theoretically, I will return to my first experience with this type of knowledge.

My interest in the *SOM Institute* started almost ten years ago, when I was an undergraduate student at the Faculty of Social Sciences in Gothenburg. During my studies in sociology and communication, I was given datasets from recent surveys to practice regression and factor analysis, to make tables and figures, to learn how to distinguish between causes, associations, and to calculate confidence intervals. I was thrilled with the power of large numbers. In a matter of seconds, they gave me the answers to the social scientific questions that had attracted me to university studies in the first place. However, what affected me even more were the theoretical questions that animated my curious mind. How were these surveys really made in the first place? Where did they come from? How do you get almost 9,000 people to respond to surveys every year? Who are using the facts that are generated? Why is this type of knowledge considered important in our society? The questions were general and bold, but as I continued my studies in Theory of Science at the Faculty of Humanities, I learned a whole new way of looking at the production of scientific facts and I slowly began to think more profoundly about this process of how *society can be quantified*.

Perhaps the most obvious way of opening up this black box

of scientific fact production is to make a distinction between the inside and outside of science. After all, it is a basic presupposition in cybernetics[4], from which the concept of the black box is borrowed, that every system has an inside and an outside defined by borders which, in turn, define the identity of the system. Whereas encountering numbers in the media, as reported above, constitutes a circulation of facts in our everyday life, the statistics I learned as an undergraduate student provide a glance into the inner workings of the very production of statistical knowledge. In its theoretical form, such a division has been called the internalist explanation in contrast to externalism, a debate which has been repeated, debated and declared obsolete at various times throughout history (Shapin 1992).

From the internalist perspective, understanding the fact production of the *SOM Institute* would be a task of disseminating and analyzing the inner workings of the scientific method. Such an investigation could be a practical exercise of methodology on how to assess statistical measurements, which scales and criteria to use, or evaluating and refining the survey method. Such a study could also take the form of a philosophical analysis of what the limits of quantification are, how causality functions with respect to statistical association and what claims of certainty the modus operandi may hold. This final task was optimistically pursued by the schools of thought we today call inductivism and logical empiricism, championed by notable names such as Alfred Ayer, Rudolf Carnap and Otto Neurath. Later, Karl Popper developed his critical rationalism, also called falsificationism, which was notably influential in the social sciences. The so-called positivism dispute marks perhaps the most important watershed between the school of thought labeled critical theory and the critical rationalism of Popper (Adorno et al. 1976). Thus, one

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[4] See for example N. Katherine Hayles' (1999) extensive discussion on the implications of cybernetics on science and subjectivity in *How We Became Posthuman*.

way of making sense of the black box would be to go beyond the input and output and to determine in detail how each of the components work, how they are elaborated and developed, and which parts are replaced, patched and refined from an inside perspective. A critic would then naturally respond that the outside is equally important. The inner components are adapting, are made to work, and even defined by the environment, which is the outside of science. This type of reasoning was pursued in the 20th century by none other than sociologists themselves.

The externalist explanation, which was constructed in many ways as the opposition to internalism, was first introduced by the sociologists of science. Indeed, as early as the late 1920s, Karl Mannheim argued that sociology needed to reflect upon its own sociological foundations. At the core of this sociological externalism lies the assumption that the cognitive aspect of science and thought itself is always a social process. Mannheim argues:

> Strictly speaking it is incorrect to say that the single individual thinks. Rather it is more correct to insist that he participates in thinking further what other men have thought before him. (Mannheim 1936: 3)

Scientific thinking, in this context, is a collective process brought about and shaped by scientists thinking together. Groups of scientists always belong to certain strata, which, in turn, are sociological categories themselves. Moreover, Edgar Zilsel argued that scientific thinking in general and sociological thinking in particular were derived from a historical contingency as feudalism turned into capitalism during the rise of modern societies. As universities were established, the ideas of humanism were spreading and labor was introduced as the main economical value; the time was ripe for scientific thought and practice to emerge in Western societies (Zilsel 1942). Continuing along the lines of analyzing the black box, we should hence look at its

external milieu, and the adaptations of science to a society out there.

The sociologists of science did, however, preserve the distinction between internalism and externalism, and such authors as Robert K. Merton (1938; 1942) constructed a normative and internal core of sociological objectivity. He argued that sociology as a discipline depended completely on a liberal order to organize autonomously and could not function as an objective science in a dictatorial or authoritarian society. For the proper norms[5] leading to scientific objectivity to exist, science needed this liberal society to guarantee its autonomy. External norms would determine internal norms and therefore guarantee the purity of science and prevent it from becoming the "handmaiden of theology or economy or state" (Merton 1938: 260, see also Kullenberg 2008). The little black boxes function and dysfunction according to an external environment.

What we see in early externalism is thus a doubling of epistemology; the internal is conditioned by the external, and scientific thought is mirrored by the collective thought of a society. However, such a condition requires even more firm ground to be studied scientifically. To put the condition in the words of Zilsel,

> Yet the genesis of science can be studied also as a sociological phenomenon [...] there is no reason why the most important and interesting intellectual phenomena should not be investigated sociologically and causally. (Zilsel 1942: 560)

The early sociologists of science paved the way for David Bloor's Strong Programme, which opened up this line of think-

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[5] These are widely known as the CUDOS-norms, an abbreviation for Communism, Universalism, Disinterestedness and Organized Skepticism, outlined in Merton's 1942 article "The Normative Structure of Science" and elsewhere. These have been criticized by later scholars for being too simplistic or not reflecting the actual practices of science. See for example Mitroff (1974); Mulkay (1976); Ziman (2000).

ing to also include the natural sciences. In his *Knowledge and Social Imagery* (1976), Bloor stated without compromise that "The search for laws and theories in the sociology and science is absolutely identical in its procedure with that of any other science" (Bloor 1976: 17). Moreover, the Strong Programme (which became a part of the Sociology of Scientific Knowledge, SSK) ought to be able to scrutinize its own claims to knowledge by means of the same methods used in other sciences as a sort of feedback loop[6] that would stabilize the black box:

> It would be reflexive. In principle its patterns of explanation have to be applicable to sociology itself. Like the requirement of symmetry this is a response to the need to seek for general explanations. It is an obvious requirement of principle because otherwise sociology would be a standing refutation of its own theories. (Bloor 1976: 5)

If knowledge within the natural sciences is to be explained by sociological factors, the same maneuver would be applicable for the sociologist and theorist of science. The notion of reflexivity has been extensively analyzed by Fredrik Bragesjö (2004), who describes the recursive turning back towards one's own knowledge claims, results and research as a "dual use". Bragesjö subsequently adds a third level of analysis; observing sociologists of science observing other sciences would in turn also be a social enterprise, which through a reflexive method of thinking can be studied and analyzed.

This problem could, in my case, be resolved if I were to think of it reflexively. Why did I choose my object of study? Why the *SOM Institute*? Why have I chosen a certain theoretical path in favor of other resources, concepts and methods? What are the

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[6] In cybernetics, a feedback loop in its general sense means to feed back (parts of) the output signal of the system to regulate and adjust its input.

social causes for me conducting this type of investigation? If I were to seek this answer myself, I would apply a reflexive idiom. If it, in turn, were made someone else's object of study, there would be a new level of analysis outside of my own according to the recursiveness of the positions available for knowledge.

The object of my study is a research institute composed of social scientific researchers, surveys, people who respond to surveys, mediated facts and localized practices of scientific work. From a sociology of science perspective, these phenomena could be studied sociologically; we could examine the norms and values, the historical conditions and the politics of surveys; in short, we could undertake a sociology of the social sciences. My point of departure would be that of a theorist of science analyzing what is at hand, and taking a reflexive stance, I would analyze my own doing so using the same idiom with which I am analyzing the *SOM Institute*.

However, there is another dimension to such an engagement, which would complicate this approach. What I am out to describe is how my study object co-constructs the social reality in which I, the researchers I study and we all live. The facts that are produced are interacting in the world. Perhaps the new houses in Kungsbacka, as I mentioned in the beginning of this chapter, are going to be built because a survey was conducted. Maybe the future planning of cultural activities in the town of Borås will be influenced by the fact that a report by the *SOM Institute* has analyzed the cultural life of its inhabitants scientifically. More importantly, maybe our everyday experience, and even our theoretical understanding of what living in a society entails is already partially influenced by and emerges from the institutions that produce quantified knowledge about our societies. A consequence of the technoscientific societies we live in is that science is no longer a marginal activity only pursued by modest gentlemen.[7] Rather, the social sciences are omnipresent

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[7] See Donna J. Haraway's (1997: 23-45) critical discussion of the subject

in public life. Not only does science have social causes, but it also causes society. The notion of causes is here chosen as a rhetorical device to explain why the sociology of science is not sufficient, no matter how much reflexivity we add to it.

When studying the practices of quantitative social sciences, we can not take social causes for granted[8] because the very act of creating society is what needs to be explained. Even a doubling of reflexivity is not enough for me: if the internal processes in the social sciences are already social, as claimed by the sociologists of science, what if sociology is created using sociology, e.g., statistics performed with statistics? What if the sedimented distinctions between ontology and epistemology are not that clear anymore? This is related to another topic in philosophy, which needs to be resolved throughout this thesis. Namely, can there be such a thing as a society as a whole? This question has been raised by various philosophers, but I will pursue it, as elaborated in Chapter 2, departing from its parts rather than from the whole. From this perspective, the sciences of society are not outside observers of a society out there. The social sciences are as guilty of assembling and composing the fabric holding the social together, as any other discipline.

1.1   Defining the problem: The quantification of society

I want to understand the processes required for the social sciences to become scientific in relation to other activities that engage in describing society, which components are assembled and re-

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of/in science, which today constitutes a wider range of people and holds both promises and worries for our everyday lives.

[8] See Bruno Latour's (2000: 121) discussion of the domain of the social sciences: "The social is not a domain, but only one voice in the assemblies that make up things in this new (very old) political forum: the progressive composition of the common world."

assembled in generating stable scientific facts that are then used, understood and elaborated throughout modern society. On a fundamental level I wish to understand *how it is done* rather than why or how it *should* be done. It seems that to understand what quantification, as a scientific practice, is about on this level, I need to perform a local and empirical investigation. However, that also requires a historical understanding. During the first steps of writing about the practice of conducting surveys, I began by looking at the social sciences as they were close to me and had caught my attention as a student of the social sciences. The main focal point, the *SOM Institute*, started surveying the Swedish population in 1986, and had since then grown into an institute that produced regular surveys with results that circulated widely. For undergraduate students, the datasets of the *SOM Institute* were even used to practice various statistical analyses. To me, as a student in the social sciences, the *SOM Institute* appeared as the blueprint for how to conduct large surveys.

At the same time, however, as I followed each component closer and closer, it slowly became clear to me that what was practiced locally here in Gothenburg, only a couple of decades back in time, had been founded upon a much older and complex bedrock of theoretical debates, methodological advancements, cumulative survey data, and controversies on how to define the inside and outside of science. The current making of surveys, with its rules and procedures, was reflected in history and I curiously departed on a journey to find events in which these procedures were discussed, advanced or challenged.

To understand the problem of quantification as practice, as a pursuit of the original source or the origin of the ideas that gave birth to certain procedures, would probably not answer the question effectively. To conclude that the genealogy of quantifying society leads back to Talcott Parsons, Emile Durkheim or some unknown proto-sociologist would not consider how quantitative social science *is done*, only how it is expressed.

What I am about to do is more of a mapping than a genealogical tracing. As sociology is introduced in Sweden as a discipline after the Second World War, its components must be opened up and laid out on a plane that emphasizes the connectivity of practices. The philosophers Gilles Deleuze and Félix Guattari called such an approach a "plane of consistency" in contrast to a plane of organization and essences in which "[the plane of] consistency concretely ties together heterogeneous, disparate elements as such: it assures the consolidation of fuzzy aggregates, in on other words, multiplicities of the rhizome type" (Deleuze and Guattari 2004: 558). Thus, rather than looking inside a box, or looking outside of it for that matter, I will attempt to connect seemingly heterogeneous empirical elements that are at work in making quantitative social science. I will study how the social sciences create interfaces with the state, how they define their objects of study, how the border between scholarly science and commercial pollster surveys are defined, how society is made knowledgeable through an epistemic assemblage of questionnaires. Essentially, these sites are heterogeneous: they vary from state policies to theoretical debates and methodological inquiries, to boundary work in the open public debate and struggles to achieve epistemic authority and a privileged position to be able to say what society really is.

The main problem, as well as the main challenge, next becomes how these elements hold together, not by a totality or a certain logic but through historical consolidations — sedimentations that were once fuzzy and then progressively grew harder and became increasingly well defined (the opposite, disintegration, is of course also possible). To say that, for example, the inhabitants of Borås think and feel a particular way, may be done with accuracy and credibility only if there is something that embodies that statement. The social sciences never depart from a clean slate; they do not appear out of nothing. Rather, the social sciences require composite parts to be aligned in certain config-

urations. I want to see how they have been connected, how they
have been assembled in a fashion that today renders the quan-
titative social sciences able to speak in the name of the urban
dwellers of Borås or, for that matter, any other object that falls
within epistemic domains. To describe this process of connec-
tivity, I will introduce the concepts of assemblages, blackboxing
and interfaces in Chapter 2 to provide a working ground from
which to start (and from which to stop).

Now, what then is so special about quantification and count-
ing? As I go into further detail in Chapter 2, statistics in general,
and the statistical social sciences in particular, have a special re-
lationship with the state, the governance of territories, and is
a core aspect of administrating modern societies. Within Sci-
ence and Technology Studies (STS)[9], a thematic approach which
may be labeled the sociology of quantification, has emerged and
has produced several interesting takes on the role of statistical
sciences. Sætnan, Lomell and Hammer outline such studies as
follows:

> Briefly stated, *the act of counting its citizens, territo-
> ries, resources, problems and so on, is one of the acts
> by which the State participates in creating both itself,
> its citizens and the policies, rights expectations, ser-
> vices and so on, that bind them together.* (Sætnan,
> Lomell and Hammer 2011: 2, italics in original)

To count the social world is to hold it together; counting

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[9] STS is the common denominator for a number of scholarly traditions
studying, among other things, the production of scientific knowledge, techno-
science and scientific cultures. Thomas S. Kuhn's (1996 [1962]) *The Struc-
ture of Scientific Revolutions* is often regarded as an inspiring work for STS
analyses of technoscientific practices and as a departure away from rational-
istic philosophy. Sociology of Scientific Knowledge (SSK), the Social Con-
struction of Technology (SHOT) and Actor-Network Theory (ANT) were
influential traditions in the 1970s and 1980s. For overviews and recent de-
velopments of the field, see Sismondo (2004); Yearley (2005).

makes society a whole. Counting creates what Deleuze and Guattari calls "molar aggregates".[10], binary divisions of insides and outsides. The act of counting creates territories and categories, which are subsequently divisible into smaller units: Kungsbacka is a town inside the larger unit of Sweden; the Swedish population is divided into two sexes; there were 11 types of labor in the 1988 SOM survey (Björkqvist 1989: 66). These categories change over time; in 1950 (see Ingulfsson and Hagman 1950: 142) the Gallup Sweden survey defined three types of marital status: unmarried, married and divorced. In the mid-1980s, only two were defined: "living alone" or "cohabitation/married" (Björkqvist (1989: 64).

To measure, count and quantify always implies a simplification, and while simplification is not in itself a problem (it is instead a requisite for quantification), the return of these categories back into the world are. Sætnan, Lomell, Hammer again relate:

> Counting acts in and upon the social world. Of course, this also means that *not* counting has an effect on the aspects of the world we (do and/or don't) count. What we choose to count, what we choose not to count, who does the counting, and the categories and values we choose to apply when counting are matters that matter [...]. (Sætnan, Lomell and Hammer 2011: 1, italics in original)

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[10] See the chapter "Micropolitics and Segmentarity" in *A Thousand Plateaus - Capitalism and Schizophrenia* (2004: 229-255) In this chapter molar aggregates are contrasted with molecular intensities, where the former consists of binary over-codings, such as sexes, classes, races, socialist-conservative views — divisions that define the average and extensive properties. The molecular intensities, on the other hand, are the interminglings, transitions and movements between over-codings of the molar kind. In the social sciences, the distinction between the molar and the molecular has sometimes been referred to as quantitative/qualitative, macro/micro, or hard/soft data.

Falling inside or outside a quantitative category determines what becomes visible or invisible within the whole referred to as society as it appears in the sciences.[11] To count (in both senses of this expression) is to be accounted for and to matter as an entity depends on this visibility.

One way of understanding what becomes a social problem and an object of study for the social sciences would be to look at the content of what is being counted. For example, why were there numerous questions regarding sexuality, marriage, gender roles and syphilis in the Gallup surveys[12] during the 1940s in Sweden? Why was it important to make these issues visible by way of quantification? Why were other issues not? These questions will be discussed further in Chapter 2 and elaborated in the empirical chapters. However, the point of departure is not in the binary distinctions themselves or in the classifications as ready-made science; in fact, it would already be too late to do so according to the philosophical position that I call *actualism*, which is also described in Chapter 2. Instead I will go directly to the surveys, and the practice of making them.

On an even more profound level, which will be examined in Chapter 3, there is another type of visibility manifested by the survey-practice itself. Not only are categories, definitions and theories important, but I will argue that the very act of intermingling with the world when engineering surveys provides a much closer account. By studying methodological reports and evaluations, I analyze how social science is recursively applied to refine and calibrate the surveys to provide high response rates. The problem of quantification will thus be treated as a problem that can be sought out and analyzed empirically as a case study.

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[11] The sciences are naturally one of several instances referring to the society as a whole. In this study, however, I will limit myself to the scientific concepts, rather than the common understanding experienced in day to day life.

[12] See the tables provided by the Swedish Gallup Institute in Håstad, E. et al. (1950: 340-346)

1.2   Aims and research questions

The aims of this thesis are threefold, and can be divided along
the following lines. There will be one empirical, one positive and
one negative contribution in this study. Another way of making
such a distinction would be to say that there is one empirical
part, in the sense that it would attempt to shed new light on a
story yet untold or told in a different way than has been done be-
fore, one theoretical and conceptual development and one critical
part, in which arguments are made in relation to other theories.
Throughout this thesis, these three aims will naturally intersect;
however, it is still valuable to separate the aims properly in this
introductory section.

First and foremost my aim is to contribute, on an empirical
level, to a contemporary history of surveys in Sweden. I wish to
give the reader a glimpse into the core practice of constructing
surveys. I argue that this knowledge is important not only to
academics, but to anyone who finds it interesting and of a cer-
tain value to understand how the quantitative facts engage in
our everyday lives and the processes of governing our collective
destiny. The quantification of society ought to matter not only
as an intellectual object of study but also as a valuable subject
to anyone who wishes to unlock and engage in how scientific
knowledge bases exist in our lives.

The research questions, can be ordered as follows:

- Social scientific theories, methods and every-day proce-
  dures need to be understood as epistemic assemblages,
  rather than the diffusion of ideas. Conceptual frameworks,
  which are transmitted within a subsection of the academic
  community, and the dispersion and germination of ideas,
  practices and methods have to be followed throughout lit-
  erary works, reports and documents. However, to under-
  stand how the ways of conducting social scientific research
  are communicated, one must empirically study how they

are embedded in a local community. Ideas are not realized by thought alone, but are made as practices. The research question subsequently becomes how do the social sciences assemble their research instruments and practices to produce facts about society?

- The quantification of facts, data, measurement scales, and territories makes knowledge about society *scientific* in a special way. In modern societies, there has been a multiplication of statistics and quantified knowledges. New domains, such as social medicine, immunology, sociology and economics, have been equipped with statistical methods and technologies. Thus, the process of producing measurable units of society, units which may be aggregated and enunciated as scientific knowledge, becomes a research problem in STS. Statistics is not merely applied from the outside but is an integrated part in both the theoretical and the methodological work of social scientific research. Thus, the question in this respect becomes how the quantitative social sciences create a certain visibility of the social with numbers and how these numbers are defended as an epistemic position.

- The very act of counting has a special relationship with the state, with governance and policy-making, which has been sedimented throughout the history of modern societies. From this perspective, counting appears to be very static, almost as if states were built on numerical constructs. However, when studied in detail a certain plasticity is revealed, where the status of quantitative knowledge is negotiated, contested and accepted. The research questions here becomes as follows: how are the forms of knowledge bases used to create society as a whole and how do they act upon the world we live in?

To place these questions in context and to understand the
empirical case better, the role of the social sciences in modernity
needs to be understood more thoroughly; moreover, I will need
conceptual tools to work my way through the *SOM Institute*. As
previously stated, I will explore more thoroughly three concepts,
namely — blackboxing, assemblages, and interfaces — to under-
stand the dynamics of quantitative social science in our modern
societies.

To perform this primary empirical aim I must develop a the-
oretical and methodological tool box that constructs a second
aim, which is positive. Additionally I wish to contribute concep-
tually to the enterprise of conducting empirical studies on the
social sciences. I will use a certain object-oriented interpretation
of what is usually referred to as classical Actor-Network The-
ory (ANT)[13] to secure a philosophical position from which I can
depart. From this perspective, I have found the most powerful
tools for moving throughout the vast networks that constitute
the quantitative social sciences. By elaborating the concepts of
interfaces, black boxes and assemblages, I attempt to evoke ele-
ments of social scientific research practice, which have for a long
time been a topic of internal methodological reflections, and turn
them into objects of study in STS.

As a consequence of my empirical and positive aims, I in-
evitably must engage in a critical encounter with other traditions
in STS and theory of science.[14] Here, I will argue that the social

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[13] By classical Actor-Network Theory I refer to the early works of Bruno
Latour, Steve Woolgar and Michel Callon. As a reaction to the sociological
accounts of scientific knowledge, ANT was developed in the late 1970s and
1980s, in works such as Latour and Woolgar (1979); Callon (1986); Latour
(1987). The philosophical roots of ANT can be found in Michel Serres and
Algirdas Julien Greimas. Moreover, in contrast to early SSK, ANT used an-
thropological methods to describe science in action. Classical ANT has been
challenged so called post-ANT, by authors like John Law and Annemarie
Mol. For an overview, see Sismondo (2004) and Yearley (2005).

[14] Theory of science should not, in this context, be confused with philosophy
of science. As a local tradition in Gothenburg, theory of science includes

sciences are material processes. If the natural sciences were once deconstructed by showing how their hard and material practices were essentially social, I wish to do the opposite for the social sciences — namely, to bring out the quantitative survey practice for inspection just like any other scientific laboratory and show how it becomes stable, durable and integrated with the very fabric of our modern societies.

1.3   The entry point: The *SOM Institute*

The epicenter of this study will, of course, be limited. I have chosen to follow the *SOM Institute* as an entry point for understanding the role of the social sciences, their functioning and connections. The *SOM Institute* is an organizational conglomerate of three social scientific departments at University of Gothenburg, and this institute has conducted a large survey on the topics of "society, opinion and media" in Sweden since 1986. Throughout the past decade in particular, the *SOM Institute* has become an influential producer of social scientific knowledge and is often quoted in public debates and the news media. As an entry point, I could have chosen other sites, both in time and space. This possibility is a general problem in the case-study approach, which could be summarized as follows: would I have arrived at the same conclusions if another point of departure had been selected? Such a question is however hypothetical, and instead, I shall here briefly describe in what directions it is possible to move.

This study started off by stepping into the middle of things. More precisely, I began to look at how the surveys of the *SOM Institute* were made by studying methodological reports and discovered the problem of response rates, as described in Chapter 3.

STS, Science Policy Studies, continental philosophy as well as traditional epistemology. See http://flov.gu.se/english/about/history/, accessed 2012-03-13.

To say something about society, social scientists need society to
respond back to them. It may be argued that this is merely a
practical problem, something that can be delegated to some lo-
gistic enterprise. However, when looking more closely, I found
a whole theory of society that not only mattered in scientific
registers but was also used as a constitutive element to get any
work done at all. A critical issue regarding response rates is the
ability to achieve statistically significant results. In Chapter 4,
I therefore look at the relationship between the state and statis-
tics, how the social sciences were benefited and supported by
funding the expensive surveys and what problems such a config-
uration entails (for example, loss of autonomy). The autonomy
of science, however, not only depends on a degree of economical
self-sufficiency but it is also often manifested by distinguishing
itself from other activities. This distinction is also the case for
the social sciences, and in Chapter 5 I look into a controversy
between academic scientists and commercial pollsters in the 1985
elections in which a struggle for epistemic authority is played out
in an open public debate and in academic journals.

   All of these stories are partial and to some degree also con-
cern transient historical episodes. I could have followed response
rates in many other surveys, traced down details and made com-
parisons, and made genealogies. I could have written the history
of statistics and the state much more thoroughly, going further
back, and looking at other disciplines. I also could have chosen
to strive for methodological purity by, for example, only looking
at how scientific controversies regarding the social sciences are
represented in the mass media, applying a rigorous interpretive
tool box.

   I have used an approach in which I go from one location to
another[15]. In the methodological reports of the *SOM Institute*

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[15] An example of a similar approach is when Catharina Landström encoun-
ters two black-boxed instruments in a Gothenburg laboratory, she finds links
that lead to the instrument manufacturer in California, and by following

I encountered an important fact: "Surveys in Sweden usually have a response rate between 65 and 75 per cent" (described in Chapter 3). This fact urged me to investigate how surveys are made, how they function and what strategies are employed to refine them. This initial disassembling subsequently revealed several components of which some I decided to delve into further. When raising the issue of response rates, I discovered an entire controversy as I stepped into a debate in 1985 between commercial pollsters and academic social scientists, as mentioned above. I followed this debate, not from the beginning to the end, but until it took me further. The debate was liked to historical events that went farther back in time and raised the questions regarding the relationship between the social sciences and the state.

The key issue when conducting a study like this one is determining which links are important and which are dead ends. For example, one day I stumbled upon a letter in the university library from a young Jörgen Westerståhl, a key social scientist in Gothenburg, which was sent to the editor of the social democrat newspaper *Socialdemokraten* Fredrik Ström in 1934. With a curious mind and an expectation to make an important discovery of an unforeseen link to what constituted the local social scientific tradition here in Gothenburg, I carefully read the thin typewritten pages of the letter. It contained a translation of a text by Theodor Plievier called "Deutschland Erwache", an anti-fascist poem commenting on the current circumstances in Germany. Reading the poem excited me, but even though it may have provided biographical information about both Westerståhl and Plievier himself, as well as revealing key circumstances regarding the lives of European intellectuals in the 1930s, the poem had to be left aside, because it did not constitute a link in the

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these connections it is possible to analyze their agency in assembling molecular biology research (Landström 1998: 35). Following one such link will not exhaust the question for all instruments used in molecular biology, but it will express certain qualities to the composition of a larger assemblage of instruments, practices, relations and translations.

scientific assemblages I had set out to study. To qualify as a relevant source, the links I have chosen to follow must meet the criteria of further illuminating how quantitative social science is *done.*

In the field of STS, controversies, uncertainties, challenges to the credibility of science, historical ruptures, and moral dilemmas are sought out and studied. Such processes do not follow pre-defined patterns, nor do they have a clear beginning or end. To navigate among them, I will need to elaborate some significant concepts and also re-think their consequences in the following chapter. To clarify how this study has unfolded a path spanning more than half a decade, it is of value to first look how this thesis is organized.

1.4   Outline of the thesis

Throughout this thesis, the practice and maintenance of scientific work are my objects of study, and for the empirical cases, I have selected four moments in the history of the *SOM Institute* that have revealed aspects of these activities. The outline of the thesis may be summarized as follows:

- In Chapter 2 I introduce my theoretical concepts and philosophical position. I elaborate on how my interpretation of classical Actor-Network Theory can be applied in an empirical investigation. Moreover, I discuss in what way my concepts and theories differ from other traditions in STS and theory of science.

- In Chapter 3 I analyze the introduction of the postal survey, how it is stabilized (blackboxed), made to work, and is slowly becoming a routine interface to collect data. In this chapter, I go back and forth in history following references and methodological reports to find out how they weaken or reinforce each other. The primary focus is to

look at the methodological side of surveys, to see how they are (re-)assembled and displaced and finally to crystallize the first SOM survey of 1986.

- In Chapter 4 I describe how an interface was created between the social sciences and the state. The focus is here to describe how a certain social scientific definition of "society" converged, and was made useful for the development of the Swedish welfare state.

- In Chapter 5 I examine a controversy on epistemic authority that took place close to the 1985 elections, in which the roles of pollsters and social scientific research were negotiated. This chapter will seek to provide a new understanding of the methodological technology transfer between pollsters and academic knowledge, while also describing how the accuracy of election research becomes a controversial political strand of debate.

- In Chapter 6 I describe how the *SOM Institute* became self-referential and managed to produce surveys autonomously that spanned over long periods of time. This expansion configured the institute as a center of calculation through which public opinions can be effectively measured and analyzed from year to year.

- In Chapter 7 I conclude the results of the thesis and discuss the contribution to STS.

Finally, the original Swedish quotations used in this thesis are attached. They are referred to by Roman endnotes when appearing throughout the text.

Chapter 2

Significant concepts and philosophical position

In this chapter, which has two major parts, I will further define the concepts used to navigate the quantitative social sciences, and select further paths that lead to a richer picture of my object of study. Additionally, I will address with the theoretical and methodological issues that are at stake when developing principles for selecting and analyzing relevant parts, events and controversies. I will introduce three concepts that will guide the analysis — interfaces, blackboxing and assemblages — and show how they are interlinked. Moreover, this chapter will discuss the meta-theoretical challenges and philosophical strands of debate that are implicated in my current approach to the empirical study of the quantitative social sciences. Finally, I will discuss the concept of centers of calculation as a preliminary way of approaching my empirical object of study.

2.1   Concepts

In Chapter 1, I approached social scientific knowledge as it was circulating in the news media. The facts were simple and to

a certain extent given, in the sense that methodological problems, diverging interpretations and the complexities of scientific research remained unspoken of. However, these brief flashes of facts hardly revealed anything about how science is done, except for the short radio report about declining response rates. As we will see in Chapter 5, however, social scientific facts may very well be under close scrutiny and highly controversial, even in the news media.

To further develop my theoretical approach and to refine how I shall proceed methodologically, I will use examples from the coming empirical chapters to illustrate my lines of thought. Additionally, this will serve as a progressively closer introduction to the *SOM Institute* because taking apart its pieces also reveals what role the institute has in the broader context of social scientific research. Regarding the first concept, I will make a link between what is reported in the news media and scientific literature. Thus, I will first describe a press conference that was held on the 28th of June in 2011, when the 2010 SOM survey was presented to the media.

Interfaces

> The researchers talk about a Gothenburg effect and
> a slow norm shift. It entails that increased spread
> of corruption seems to lead to a wider acceptance of
> corruption. This is shown by the new results of a
> new report from the *SOM Institute* at University of
> Gothenburg, which was presented on a press conference yesterday. (*Göteborgs-Posten* 2011-06-29)[ii]

The quote above comes from an article in the largest local newspaper, *Göteborgs-Posten*, published one day after the *SOM Institute* held a press conference on their latest findings. Social scientific knowledge has become a public matter, and the topic of corruption seems to matter to the extent that it makes it to the

news agenda. Now, what happens when I try to reverse engineer this newspaper article to find my way into the core of scientific research? Is there a one-to-one relation between a fact in the daily paper and the facts that the scientists are dealing with? How do you go from a five-hundred-page research report to a brief newspaper article?

Each year since 1986, the *SOM Institute* has published their findings and results in a large volume. The results are, however, not only meant to stay inside the academic ivory tower; the *SOM Institute* actively circulates them, and they appear in public debates on several occasions. One could, of course, read the report for a couple of days and then write a summary or a review. However, it is much more convenient to have the results of the report summarized and explained by someone else. Along the same lines, if you want your research to circulate outside the report and reach out to people who do not have the time or means to spend a couple of days in the library, as a researcher you need to translate[1] the numerous words, graphs, tables and conclusions into a compressed yet credible statement.

As highlighted by the quote above, one such interface emerges from the press conferences that the *SOM Institute* holds on a regular basis. A widely debated subject in Gothenburg is corruption, fueled by scandals that were revealed in 2010. There are, of course, many approaches to address this subject, one of which is the social scientific one in which norms related to a specific behavior of corruption are studied. Because the subject was of high value on the news agenda, the local paper *Göteborgs-Posten* sent a reporter to attend the press conference, which then made it possible to quote the SOM researcher Henrik Oscarsson when writing the article.[2] Consequently, the press conference

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[1] Translation is used here in a wider sense than that implied by its linguistic meaning; see section 2.1.

[2] E-mail correspondence to Åsa Brevinge, the author of the article, 2011-08-29.

functions as an interface between a research practice and a jour-
nalistic practice, two very different activities, and the result is
that facts are progressively made to circulate.

In 2011 the *SOM Institute* held one press conference in Stock-
holm and one in Gothenburg on the 28th of June. I recorded and
analyzed the latter.

When you enter the press conference you get a copy of the
five-hundred-page report handed out to an audience of approxi-
mately 25 people, most of them academic researchers and some
of them reporters from press, radio and television. The public
service TV broadcaster *Sveriges Television* is filming the event,
and on the white-board, the Twitter hashtag #somgu[3] has been
written. As the second largest city in Sweden, Gothenburg is not
considered to be the epicenter of media impact, so the conference
takes place in an ordinary lecture hall at *Annedalsseminariet*,
where some of the social science departments are based.

The press conference is opened by the three editors of the
report, Lennart Weibull, Sören Holmberg and Henrik Oscarsson,
the first two introduced as the co-founders of the institute. At
first, Weibull discusses how the survey was made, while referring
to the report that was handed out as people entered the room:

> Here we have everything. The *SOM Institute* is a
> scientific institute where we work extensively with
> methodological developments [...] Thus, the sample
> is 9,000 and we make three emissions and [of] ques-
> tionnaires. One is more [focused on] political, one
> more on media and culture, one a bit more on life-
> styles and health. Our base questions in the SOM
> survey are in all three of these questionnaires. This

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[3] So-called Twitter hashtags have become increasingly popular over the past
couple of years. They enable a search function on the market-leading micro-
blog Twitter and are often used at academic conferences. The *SOM Insti-
tute's* Twitter username is @SOMinstitutet.

> is not something you need to know, since it is all here
> [in the report]. But if one is interested to look it up
> [more closely] [...] from *page 595* and onwards you
> have the three questionnaires in extenso documented
> in the book. (my italics)[iii]

Weibull summarizes how the survey was made, but the details are too extensive to fully communicate in a two-hour seminar, so the audience is referred, or *linked*, to a particular page in the report. To get to the questionnaire, you need to go one more step; open to page 595 of the report.

The press conference continues with short presentations of each of the chapters in the report. The audience learns that levels of trust in political institutions are as high as they were during the 1970s, that political interest increases when elections are near, that Swedes have more positive attitudes toward immigration, that women are more active in social media, and that people living in rural areas far away from the center of decision making are more skeptical towards wolves in the forests than people living in urban environments. Every now and then, especially when a number or graph is quoted, the report is once again linked to statements such as "As you can see on page X in figure Y". Some of the descriptions are general, and some take a more technical turn. Sören Holmberg, for example, describes new techniques for measuring "job performance" when evaluating how public institutions are perceived:

> We were inspired by American research on consultants and politics when we made our measurements, our financial ratios, as [presented] on page 109. [...] Secondly, evaluation; not of trust this time which we measure nowadays in other instances, not personal satisfaction, but the evaluation of job performance, how one perceives, that the job you are expected to do, how well it is done. That is called job perfor-

mance in American [English]. (The words "name
recognition" and "job performance" appear in En-
glish in original) [iv]

The results, which appear on page 109 and are narrated by
Holmberg during the press conference, are quite devastating for
two of the institutions that were measured. The *Swedish So-
cial Insurance Agency* (Försäkringskassan) and *Swedish Public
Employment Service* (Arbetsförmedlingen) both indicate low job
performance.

Another interesting finding is presented by John Magnus
Roos, a researcher at the *Centre for Consumer Science* at Uni-
versity of Gothenburg. He argues against the widespread be-
lief that so called shopaholics consist mostly of women buying
purses, makeup and clothes. His data show that gender is not
an important factor. In fact, shopaholics are young people who
are dissatisfied with life in general and whose degree of empa-
thy is lower than average. The following day, this is reported by
the local public service radio station *P4 Göteborg*, where Roos
is interviewed by the reporter Anna Olofsson, who attended the
seminar. The radio station publishes an interview on their web-
site the day after the press conference:

- If we know more about the personality type [of the
shopoholic] then we can both prevent these problems
and help the person in need of support, says the re-
searcher John Magnus Roos. (Olofsson 2011)[v]

Thus, from this article, it is possible to reverse engineer a
widely circulated fact back to a press conference, which in turn
refers to a scientific report. The press conference, situated in
time and space, works as an interface between journalists and
researchers, and provides explanations, quotes and references in
the style of "says researcher...".
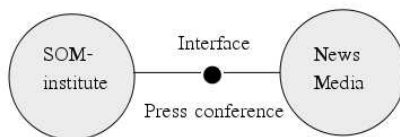
Figure 2.1: The press-conference interface between complex scientific research and news media.

Such an interface[4] like this (Figure 2.1) reveals as much about journalistic practice as it does about scientific practice. In the same manner that a graphical user interface of a personal computer translates the commands and instructions understood by the different components of the computer into visible and interactive folders, files and mouse-clickable icons, the press conference translates the work of the social sciences into quotable conclusions, easy-to-remember numbers, and provides links to a five-hundred-page repository of information.

The primary function of the translating interface of the press conference is displacement (Callon 1986), to have a few reporters and journalists pass by the *SOM Institute*, to pick up a few ref-

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[4] The concept of (social) interfaces has previously been used in conventional sociology as defined, for example, Norman Long: "[...] interface analysis grapples with "multiple realities" made up of potentially conflicting social and normative interests, and diverse and contested bodies of knowledge. It becomes imperative, then, to look closely at the question of whose interpretations or models (e.g., those of politicians, scientists, practitioners or citizens) prevail in given scenarios and how and why they do so" (Long 2001: 88). This sort of anthropocentric definition is not sufficient for my intentions. Just like the concept of blackboxing, I use interfaces more in the sense of how this terminology is used in engineering, where there is no distinction regarding whether an interface links hardware or software components together or connects and translates a piece of equipment to a human user (for example the graphical user interface of a desktop computer) or humans to humans in a situated physical location, such as the press conference discussed herein.

erences and quotes in their everyday work of writing articles about corruption, shopaholics or job performance in public institutions. An interface works smoothly when there is a mutual agreement or a mutual interest in the notion that there is something there for everyone; for facts to be able to circulate, one way (among many) is to create an interface in which the complexities of scientific research are re-packaged into pieces desired by the circulators, which, in this case, is the news media.

When arriving at an interface, someone has already made an effort to modify scientific facts to make them mobile enough to be transferred to some other location. The modification may be perceived in many different ways, depending on the destination. A methodological report, with its minute details, is supposed to circulate between peers in the scientific community. Results and simple numbers make facts ready to be communicated in the mass media. Moreover, an interactive exhibition at a science fair may even spark children's interest in genes, atoms and planets in outer space.

To go further, however, to translate from the easy reads of news media to esoteric science, we need to go to the reports that are only linked to at the press conference. As an interface translates, it simplifies, and linked to every interface there are black boxes. There are black boxes of journalism, methods, technologies, standards and writing styles. More importantly for this study, there are black boxes of the social sciences, which to move forward, must be opened.

Blackboxing

The notion of black boxes, or blackboxing, is introduced in Bruno Latour's (1987) book *Science in Action* and has since then played a central role in his works in describing not only how scientific knowledge is made durable but also how the very fabric of societies is held together (see Latour 1999a: 183-185). As mentioned

in Chapter 1, Latour borrows the concept from cybernetics.[5] However, it has a richer meaning and much wider consequences when used to study scientific activities as they unfold. It is time to leave the cybernetic meanings of static systems, feedback loops and self-regulation behind and approach the concept anew in pursuit of a powerful tool for studying scientific work empirically.

Science in Action begins with the example of the role of the *Eagle microcomputer* in discovering the double-helix structure of DNA. Usually, the story of this discovery follows the involvement of Nobel Price winning scientists and their struggles in competing to become the first to prove the existence of natural phenomena. Latour, however, goes back in time, to the moment when the scientific community still remained uncertain about what the structure of DNA really looked like. Thus, not only are facts, models and theories uncertain – but – the equipment, methods and previous statements are also uncertain. As science is made, scientists need to get rid of fuzzy complexities, defunct hardware and contradicting theories.

When scientific instruments work, they are treated as unproblematic black boxes that generate outputs from inputs. However, they first must be *made* to work:

> But it was not a good machine before it worked. Thus while it is being made it cannot convince anyone *because* of its good working order. It is only after endless little bugs have been taken out, each bug being revealed by a new trial imposed by a new interested

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[5] In *Science in Action* (1987: 2-3), black boxes are initially defined according to their cybernetic meaning: "The word black box is used by cyberneticians whenever a piece of machinery or a set of commands is too complex. In its place they draw a little box about which they need to know nothing but its input and output." However, as we unfold the epistemic practices on a deeper level, we shall encounter several other black boxes, which are invisible from this introductory point of view.

> group, that the machine will *eventually* and *progressively* be made to work. (Latour 1987: 11, italics in original)

This debugging of black boxes occurs either before the machines work or when they break down. When they work, however, they remain almost invisible. According to Latour, this is not exclusive to complex scientific instruments such as telescopes, bubble chambers or lasers, but it also holds for everyday equipment that we use. Mobile phones, microwave ovens or credit cards are devices that we take for granted, even though most of us do not have much of a clue as to how they function in every minute detail.[6] To move along in our lives, and get things done, we are unable to hesitate and examine every component that makes up our tools. Hence, a key element in practice is to *ignore* complexities and surrender to a kind of technoscientific ignorance. Of course, we may at any time dissent and ask why there is no cell phone reception in a certain neighborhood, why the fees are high on credit-card transactions or whether microwave ovens emit dangerous radiation or not. The cost of such dissent will interrupt our lives in regard to our everyday tools. We would have to seek evidence, gather information and form alliances with other concerned people, or even run the risk of being singled out for questioning such mundane things that most people consider unproblematic. Questioning scientific instruments, in particular, would be an even more daring enterprise. To challenge, let us say, the accuracy of the Hubble Space Telescope or the Large Hadron Collider, we must learn and acquire the terminology, skills, and perhaps even the social status of a scientist to be taken seriously. We may even have to build a better telescope or particle accelerator to prove that the existing equipment contains errors. Simply walking down the street, claiming that the

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[6] Even more mundane technologies, such as clothes, cups and tables, depend on extensive networks to function, see Bragesjö and Hallberg (2011); Michael (2006).

Large Hadron Collider will be the end of the world because it will
create a black hole that implodes the universe will most likely
do nothing more than label us conspiracy theorists or lunatics.

Philosopher Graham Harman (2009) thoroughly analyzes the
status of black boxes in Latour's works in his *Prince of Networks*,
an entire book dedicated to understanding Latour's philosophy
anew and also using it as one (of several) building blocks in
Harman's "object-oriented ontology".[7]

> For Latour, the black box replaces traditional sub-
> stance. The world is not made of natural units or
> integers that endure through all surface fluctuation.
> Instead, each actant is the result of numerous prior
> forces that were lovingly or violently assembled. While
> traditional substances are one, black boxes are many
> we simply treat them as one, as long as they remain
> solid in our midst. Like Heidegger's tools, a black
> box allows us to forget the massive network of al-
> liances of which it is composed, as long as it func-
> tions smoothly. Actants are born amidst strife and
> controversy, yet they eventually congeal into a stable
> configuration. But simply reawaken the controversy,
> reopen the black box, and you will see once more that
> the actant has no sleek unified essence. Call it legion,
> for it is many. (Harman 2009: 34)

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[7]   Harman's object-oriented ontology is a fresh synthesis of two thoughts that
are often regarded as incommensurable. By re-interpreting an early Mar-
tin Heidegger in his *Tool-being - Heidegger and the Metaphysics of Objects*
(2002), Harman argues that our interaction ("ready-at-hand", "Zuhanden-
heit") with objects, tools, and equipment, and their subsequent withdrawal
away from us ("presence-at-hand", "Vorhandenheit") takes precedence over
the common interpretation of Heidegger — that existence ("being-there",
"Dasein") is the essence of humanity. Consequently, according to Harman's
(2009) reading of Latour, the concept of black boxes is very similar to Hei-
degger's position, as well as his notion of equipment ("Zeug").

As Harman notes, black boxes never come alone. Moreover, even though Harman is not primarily interested in methodology, he suggests (just as Latour does) that reawakening controversies and opening black boxes is a way of rendering visible the vast networks of alliances that make a black box black. You may hit speed dial on your mobile phone and in a matter of seconds talk to someone on the other side of the globe. However, it is not the device in your hand that is making the phone call. It is a vast network of chipset manufacturers, telecommunications carriers, antennas, satellites and crews of administrators. The device in your hand will and must to be usable, reduce this complexity into a few keystrokes and an invoice appearing in your letter box every month. When it fails, however, the alliances may start to unfold. Maybe your carrier has failed to make a deal with a carrier in the country you try calling to; perhaps an antenna in the base station fell to the ground in a recent storm or there was an unlikely failure in an underwater communications cable connecting the continents. As a consumer, you hardly ever have the opportunity to inspect the business deals that are struck between carriers, telecommunications base stations or underwater cables. Instead, another black box enters the scene: you call customer support, and they might tell you that there is a "technical error".

As a consequence, following the processes of blackboxing can be a strenuous task as one encounters even more black boxes along the way. A core activity of (techno)scientific work is to make sure that black boxes work, which means that when we encounter facts that are visible, as discussed in Chapter 1, when machines of quantification seem to work almost flawlessly, the other components appear to be almost invisible. To proceed, Latour suggests that we ought to "arrive before the facts and machines are blackboxed or we follow the controversies that re-open them". (Latour 1987: 258).

In the introductory chapter the following was stated based on reading the tabloid *Aftonbladet*:

> After the elections many columnists seek answers to
> why the right-wing populist party *Sweden Democrats*
> was voted into parliament in 2010. According to one
> article, this is partly because 45 per cent of the pop-
> ulation want to lower migration quotas, a statistical
> fact which is repeated in several other news articles.
> These are numbers from the *SOM Institute* as well.
> The attitudes towards immigration have been mea-
> sured since the 1990s by the institute (*Aftonbladet*
> 2010-10-03).

The statement "45 per cent of the population want to lower
migration quotas" is a typical example of ready-made science. It
is backed by a scientific author, the "*SOM Institute*", which in
turn has been measuring attitudes towards immigration "since
the 1990s". We learn nothing more about how the black box
"*SOM Institute*" works than we learn about antennas, cables
and carriers when we dial a number on our mobile phones. We
only receive the output, and most readers of *Aftonbladet* are
most likely satisfied with that. A black box remains black when
it stands uncontested or when it does not break down. To learn
more, the *SOM Institute* must be followed to the point where we
are able to see some uncertainty, controversy or failure.

Blackboxing and time

The historicity of a black box does not necessarily suggest that
going back in time means that black boxes are more open. This
is sometimes true for machines as they are invented, in which
case you would usually travel back in time to find the origin
of a technology in a research laboratory. Blackboxing is, how-
ever, anti-genealogy because when black boxes break down, when
they fail, they are as easily reversed, perhaps to an even more
primitive stage than that when they were invented. Moreover,
blackboxing is a process that never ends. Machines, concepts,

methods and procedures must be constantly used, maintained
and upgraded to keep working, even though this is usually in-
visible to the ordinary user. Moreover, even though we usually
never think about it, there are technicians employed around the
clock to keep our mobile telephony working, scientists in labs do-
ing everyday research on standard procedures to keep our facts
straight (Landström 1998), and teachers in schools repeating the
instructions of grammar to pupils five days a week to ensure that
we will be able to write correct sentences.

On a more profound ontological level, the historicity of black
boxes is also applicable for actants, entelechies and hybrids (these
concepts will be explained below):

> 1.2.8 Every entelechy makes a whole world for itself.
> It locates itself and all the others; it decides which
> forces it is composed of; it generates its own time; it
> designates those who will be its principle of reality.
> It translates all the other forces on its own behalf,
> and it seeks to make them accept the version of itself
> that it would like them to translate. (Latour 1993
> [1984]: 166)

It is thus imperative not to study blackboxing in conventional
linear time frames, as if something was first invented and then
everything became routine work thereafter. The *SOM Institute*
is actually a good example of this. From the 1999 survey to the
2010 survey, looking at the methodological chapter, the box is
more open than it was roughly a decade earlier. The method
documentation chapter from the 1999 survey is six pages long
(see Lithner 2000); the one from the 2010 survey is 33 pages
long (see Nilsson and Wernersdotter 2011). At least in writing,
it seems to take more words to account for the same type of
survey.[8] Word counts can, however, be deceiving; instead, the

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[8] The notion of "same" here is meant to indicate "by design". The 2010

openness of a black box is defined by the amount of work needed
to make it work a decade later. Lithner writes in the year 2000:

> During recent years it has become more difficult to
> attain a high response rate, and moreover the SOM
> surveys have successively increased in size. This year's
> number [response rate] must hence be regarded as
> high. In the 1999 survey the response rate was 67
> per cent, which corresponds to the level of the av-
> erage fourteen nationwide SOM surveys undertaken
> thus far [...] (Litner 2000: 398)[vi]

and for the 2010 survey:

> From the 2000s the level [of response rates] has how-
> ever declined. If the average up until 1999 was 68 per
> cent, the results for the investigations during the first
> decade of the 2000s was 63 per cent. The 2008 survey
> became the first one with a result below 60 per cent,
> which was also the case in 2009. This year's survey
> did however reach 60 per cent again. (Nilsson and
> Wernersdotter 2011: 557-558)[vii]

Then, another seven pages are spent analyzing which people
are not responding to the questionnaires and why. What was
unproblematic a decade earlier is debugged, analyzed and pro-
gressively made to work again. Black boxes are deceptive in this
way; they only withdraw when they function as they were sup-
posed to. Layer by layer (or rather, variable by variable), the
missing respondents are located. One such example is a group
of young men:

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survey covers a larger population (9,000 instead of 2x2,800) and contains
more questions.

> The poorer response rate among the young groups
> is especially clear among young men. For men in
> the ages 20-29 years the response rate is 36 per cent,
> compared to 48 per cent among women of the cor-
> responding group. (Nilsson and Wernersdotter 2011:
> 562)[viii]

The traditional way about thinking scientific discoveries is
that phenomena *were* discovered; then, once they *are* discovered,
it is as if they were there all along. Similarly, the standard way
of thinking about innovations is that once someone invents a
technology, a method or a formula, it is there for us simply to
use (if we can afford it or know how to use it). This, however,
only works when studying ready-made science. When analyzing
science in action, time is relative to the speed of black boxes.
Intercontinental telephony functions at the speed of fiber-optic
cables, as long as they work. However, when they break down,
their speed is reduced to the time it takes for technicians to
locate and mend the failed components. The quantitative social
sciences, I shall argue, are no different in this respect. As we saw
in the prelude of this chapter, they are no less cutting edge than
the latest cellular networks because they precisely make ready
made science for us to use, for news media to write about, for
decisions to be based upon, to a greater extent than was possible
a decade before.

Assemblage

In theory of science, research questions are usually of the process
type or questions of becomings. This is evident in the classical as
well as in the larger contemporary field of STS. When Thomas
Kuhn investigated the structure of scientific revolutions, he set
out to describe the incommensurable changes and ruptures in
the sciences that had occurred throughout history (Kuhn 1996
[1962]). When David Bloor explains scientific knowledge, he is

determined to find out "What are the causes of this variation, and how and why does it change?" (Bloor 1976: 3). Moreover, Bruno Latour's well-quoted "first principle of science studies" is to "study science *in action* and not ready made science" (Latour 1987: 258). What is called normal science (Kuhn), taken-for-granted knowledge (Bloor), or ready-made science (Latour), seems to be of lesser interest to the theorist of science.[9] Rather, it is change, destabilizations and programs of action that draw attention to the politics of science, the re-shaping of societies through technoscience, and the uncertainties, risks and hopes scientific knowledge entails.

Following this tradition, I will ask the same type of questions regarding the quantification of society. The following is a general way of phrasing this question; what are the components that need to be assembled to quantify society? Such a question calls for a stricter definition of the concept of assemblage, which has a special meaning and determines the core terminology of this study.

The notion of assemblages has been used within and outside STS contexts. A contemporary example is Michelle Murphy's study of the *Sick Building Syndrome*, where it "describe[s] the material and yet relational way things came to matter" (Murphy 2006: 13). More concretely, Murphy describes how an actual building is assembled and re-assembled in different configurations, producing scientific knowledge, social relations, the chemical composition of air and as labor-group resistance. At a certain point, through certain connections between practices and material conditions, the notion of a "sick building syndrome" was possible to define, investigate and act upon (Ibid.: 144-145).

Similarly the ethnologist Jakob Wenzer studies the "indie music" scene of Gothenburg in terms of assemblages that are "coherent yet disparate parts, through which some type of whole or

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[9] The phenomenon of stability has although been of interest for sociologists of science, see Collins (1992 [1985])

unit is shaped, expressing some type of identity and appropriates a territory" (Wenzer 2007: 48). A rock band is an assemblage according to Wenzer: a composition of people, instruments, places that connects to and appropriates territories in the urban landscape.

In thinking about assemblages, there are striking similarities between Murphy and Wenzer. First, they both begin by stepping into the middle of things, be it a building or a local musical scene. From this middle position (which should not be confused with an outside position), they explore the territorialities of the different assemblages they encounter. The building connects to monetary flows of late capitalism, to technological devices of measuring air flows, and to principles of dividing labor in an office. The rock band, on the other hand, is connected to an economy of independent record labels, to acoustic rooms in night clubs, to the streets and rehearsal rooms of city-sprawl areas. Assemblages are in this way always territorial[10], even though the territories may be less clearly defined (for example, a public sphere). Moreover, the assemblages of Murphy and Wenzer create and connect to expressive regimes. Murphy's building creates scientific knowledges about health with respect to air quality and gender roles at work, and it connects to an entire systems approach to industrial hygiene. Similarly, Wenzer's rock band connects to populations in bars and clubs to express the genres, band compositions and refrains falling under the wider label of "indie music".

Following these approaches, I am setting out to find the "historical regularities" (Murphy 2006: 15), or sedimentations, that are encountered as I disentangle the different assemblages that produce quantitative social science. As described in Chapter 3, I have found a type of *epistemic assemblage* producing certain visibilities, a mediating *state-science assemblage* in Chapter 4 link-

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[10] See also Deleuze and Guattari: "The first concrete rule for assemblages is to discover what territoriality they envelop, for there is always one [...]" (2004: 555)

ing quantified knowledge to governmentality, and an *assemblage of difference* performing boundary work in Chapter 5, which in turn establishes the distinction between science and non-sciences. These interlocking assemblages, I will argue, work together under certain circumstances — to produce society as a quantified whole. The analysis of these assemblages is an analysis of becomings: how is society becoming a quantifiable object? How are people becoming objects of study? How are the social sciences becoming scientific?

However, assemblages do not have inner essences or qualities that determine a whole. Following Manuel DeLanda, assemblages are "wholes characterized by relations of exteriority", where the "properties of component parts can never explain the relations which constitute a whole". Instead, "relations of exteriority guarantee that assemblages may be taken apart while at the same time allowing that the interactions between parts may result in a true synthesis." (DeLanda 2006: 10-11).

Consequently, when analyzing the parts in detail, it is not statistics or quantification as such, survey methods or questionnaires per se, or intrinsic qualities to different involved parties that determine how each component is created at a certain time. In the quantitative social sciences, there are scales, measurement techniques and methods that have looked very similar throughout the past century. They have been blackboxed. This may even be desirable when comparing the results of different surveys in different countries and to preserve longitudinal data for comparison over time. However, this does not mean that they are there because of their intrinsic qualities. Rather, they are there because they function with relations of exteriority. They may be absorbed within academic communities, by a state, or by circulation in the news media; they need resources to be repeatedly executed as scholarly research; they need translations to be reported to a wider public; and they need to be adapted, patched and fixed when they are inscribed in a questionnaire sent out to

a statistical sample of the population.

Relations of exteriority can even reinforce or weaken other surveys as they are compared and disseminated by the academic community. The social sciences function because they can create these relations of exteriority. This is not the same thing as externalism, as outlined in chapter1. Externalism demands an outside position with respect to the epistemic practice of science. Analyzing relations of exteriority, on the contrary, demands that you step into the middle of assemblages and start opening black boxes, only to find your way to the next assemblage, the next event by crossing interfaces that translate more relations of exteriority. However, stepping inside science in this manner does not resemble internalism because the scientific method as such can have no essential properties (at least not in practice; there can only be an average if you count something first, for example, a population).

2.2   Actualism as a philosophical position

If blackboxing urged analysis to open up anything that only superficially appeared as inputs and outputs, taken-for-granted equipment used in everyday research that upon closer inspection revealed more components, more black boxes seeming to point towards infinity, then I needed a kind of a counter-concept to understand how things hold together as they do. Thus, I introduced assemblages as a way of describing how elements, sometimes heterogeneous ones, could be brought together to form composite formations such as the *SOM Institute* as relations of exteriority. An important reason for enrolling more actants in an assemblage is the creation of interfaces, for example, a press conference, where academic research and news media can meet, exchange, and translate scientific knowledge. Before proceeding, this manner of thinking must be explained in philosophical terms, for the sake of clarification and for the sake of pushing

my lines of arguments a bit further. Along the lines of the works of Graham Harman, I will label this position as philosophical actualism, a position of which Harman himself is critical. Then again, sometimes the best concepts are found in criticisms.

First and foremost is an excursion to ontology. The second part of Latour's *The Pasteurization of France* (Latour 1993 [1984]), "Irreductions", contains the philosophical points of departures for (at least the Latourian version of) Actor-Network Theory. It is written in the style of a philosophical treatise, similar to Wittgenstein's *Tractatus Logico-Philosophicus* or Spinoza's *Ethics*, which perhaps makes it difficult to transpose directly to an empirical procedure. However, Latour's dedication to STS is present throughout the entire text, and I shall argue that it makes important contributions as an empirical "Vorhanden"[11] tool. The two first axioms of "Irreductions" are as follows:

> 1.1.1 Nothing is, by itself, either reducible or irreducible to anything else.
>
> 1.1.2 There are only trials of strength, of weakness. Or more simply, there are only trials. This is my point of departure: a verb, "to try." (Latour 1993 [1984]: 158)

This first passage travels along the lines of the processes of blackboxing that I described earlier. As for black boxes, they do not have essences or inherent properties; they simply lead to other black boxes and they are not *reducible* to their components, only associated, linked and intertwined with them. To make black boxes work, they must be "tried out" or put to trial. Only then can they function, and only when they do what they are supposed to, is it possible to forget that they are even there.

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[11] See footnote 2.1

This, however, only leads to one direction: a recursive loop, where the complexity increases the more you zoom into it. How does it hold together?

> 1.2.3.1 There are neither wholes nor parts. Neither is there harmony, composition, integration, or system. How something holds together is determined on the field of battle, for no one agrees who should obey and who command, who should be a part and who the whole. (Latour 1993 [1984]: 164)

The Latourian ontology is completely flat, underdetermined and even, as Harman argues, actualist (Harman 2009 :16). The closure of a black box or the holding together of an assemblage of black boxes follows no underlying logic, no virtual or abstract principle; only what is at stake in a given empirical, concrete situation determines the contingent composition of the boxes making science possible.

Latour's actualism is on a collision course with several other approaches, both in philosophy in general, and in STS in particular. If we begin with the latter, the sociology of science as presented in Chapter 1 is disqualified with the "principle of irreduction" on the grounds that the holding together of scientific knowledge can not have social causes to begin with. That would be ascribing the closure of blackboxing to causes that are of a particular kind[12], leading to an incomplete description of closure. Instead, any notion of "social", according to Latour's

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[12] This debate is often simplified for rhetorical reasons. As far as I can tell, no sociologist of science has ever claimed that social causes are the only causes that bring about scientific knowledge. Rather, the opposite is true: key figures in the social studies of science have repeatedly stressed that social factors are composite factors, not determinate ones. As David Bloor programmatically claims, "Naturally there will be other types of causes apart from social ones which will co-operate in bringing about belief." (Bloor 1976:4-5).

essay "One More Turn after the Social Turn" is to be studied, not presupposed:

> The same could be said of us, the so-called actor-network theorists. We extend the principle of symmetry to social sciences and we claim that they, too, are part of our problem, not of our solution. (Latour 1992b: 275)

Furthermore,

> Only when science in action and society in the making were studied simultaneously did this essential phenomenon [common production] become observable. (Latour 1992b: 282)

When studying the natural sciences, which has been the main objective of STS, the notion of "society" or "social causes" constitutes one pole in the continuum between nature and society, the two poles that make up what Latour calls "the modern constitution" separated as distinct transcendent domains of reality. However, according to Latour in *We Have Never Been Modern* (Latour 2001 [1991]), this separation is merely the result of what he calls a "purification", a constant struggle to put natural phenomena and natural laws on one side and society and its social forces on the other.

Purification has led modernity to a dead-lock, where politics is supposed to take place only in the domain of "society" and is removed from "nature". The modern constitution short-circuits the way we think about science because the purification process only leaves us with two distinct alternatives: either we explain society with nature or, the other way around, we explain nature with society (Latour 1992b: 277). The third position, which I already mentioned in Chapter 1, to explain society with society, follows lines similar to those used to explain nature with society.

It is, however, neglected by Latour because his main interest is the natural sciences throughout his works.[13] My goal here is to show that black boxes of a hybrid character are also present in the social sciences and that they contribute to the purification of a "society" pole in the modern constitution as much as the natural sciences do. There is, however, one big difference. If the natural sciences create and purify objects that contain "no society" in the modern constitution, a microbe, an atom or a planet observed by a telescope, which is "purified" of politics, values and society, the social sciences on the contrary stand before a positive task: to make the society pole full of social objects, to populate it with human existence.

To study the social sciences, the *amodern* philosophy of Latour's must then be taken into serious account. The black boxes that I continue to open — response rates, questionnaires, public opinions, measurement scales — need to be treated as hybrid objects (or hybrid networks, Latour 2001 [1991]: 11), that function both in the work of translation (convincing, negotiating, mobilizing, transporting) and in the service of purification (constructing "society" as a transcendent pole from where "social phenomena" can be explained).

The recognition of hybrids suggests a very different methodological approach to scientific statements. To clarify this difference, I will introduce a straw-man[14] analyst. The statement "45 per cent of the population want to lower migration quotas", as exemplified earlier, is a fact that circulates in the news media, and is produced by the *SOM Institute*. Now, a critical analyst could take this statement and several just like it and argue that this presents a "shallow picture of society and human desires",

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[13] This is true for his major works written in the 1980s and 1990s. Recently, however, Latour has shown a great interest in the sociologist Gabriel de Tarde. See, for example, Latour and Lépinay (2009).

[14] Straw men never exist in reality, and because I provide them no names, the term should be interpreted without associations to anything but the argument at hand.

"fails to recognize the deeper meanings of xenophobia", or "introduces a mechanistic quantitative description of a complex society". Even if such criticism would be justified, it does nothing more than question the style of reasoning, which is already there. To challenge ready-made science in such a way fails to recognize the workings of hybrid networks.

Instead, following the trajectories of assembled black boxes places my argument on an entirely different vector. Departing from a press conference and then moving to a media report, I proceed to a SOM-report. In this report, I find a much more complex assembly of new boxes, each of them containing even more boxes, some of them stable enough only to be mentioned briefly (such as the "simple random sample", see Chapter 3), and some of them described in detail with the "hood left open" for the reader to scrutinize and evaluate the content (as with a series of post cards, telephone reminders and questionnaires). Following black boxes makes the account of what the *SOM Institute* is doing much richer than measuring ready-made results against a pre-defined framework of reference. Because these components are *taken out* of the box, they are no longer confined to the specific event of the survey. However, they are still related in the sense that they can progressively be made to share the same goal. See Figure 2.2.

Translation and actualism

However, how do they hold together? In *Pandora's Hope*, Latour introduces one level above the black box, "punctualization", which designates "routine use", in which the inner components of a black box are not even visible (Latour 1999a: 184); the more we open up the boxes, however, the larger the crowd becomes, and the more difficult it becomes to confine each component to one particular use. After all, we can use the same questionnaire for another survey at another time; we may use the same private contractor to order a different type of field work. The simple

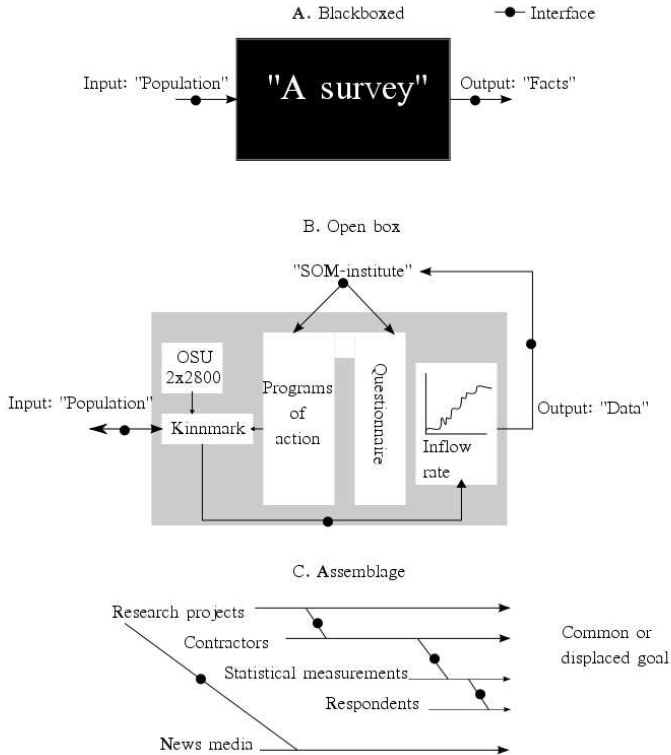Figure 2.2: Three phases in the blackboxing of the *SOM Insti-tute*. First a black box (A) is encountered in for example news media. The components only become visible (B) when turning to the scientific reports. When the components are taken out of the box, they still glue together by way of goal displacement (C) which has to be assembled, and put back into the box in order to keep producing more black boxes (A).

random sample is so mobile that it can be used even outside the social sciences, for example in immunology and social medicine. The same researchers that are included in the SOM survey for a few years may pursue another career move and leave. What needs to be done is to study how these components are translated, negotiated, combined, or, as I will call it, how they are assembled and interfaced.

The process of assembling and interfacing the components that makes up social scientific research is referred to as "translation" by Michel Callon. In his now famous article, "Some Elements of a Sociology of Translation", the process is defined:

> Translation is the mechanism by which the social and natural worlds progressively take form. The result is a situation in which certain entities control others. Understanding what sociologists generally call power relationships means describing the way in which actors are defined, associated, and simultaneously obliged to remain faithful to their alliances. (Callon 1986: 81-82)

Following this argument, it is possible to create a terminology for describing the common production of science, society and objects of study (whether these are referred to as nature or society matters less). The social sciences are in a progressive process of being shaped and re-shaped. To maintain a degree of stability they need to, on the one hand, translate their results and facts through a creation of interfaces to promote circulation while simultaneously adjusting, patching and tweaking the black epistemic boxes that make up their surveys. This is practice, for what is at hand is not a scientific discovery or novel innovation but rather continuous everyday work, where year after year questionnaires, respondents, research projects and various alliances and expressions must be re-assembled. In the intersection of two

objects, there is translation, and translation requires an inter-
face. A questionnaire is an interface between a respondent and a
survey, a press conference is an interface between researchers and
the press, and sometimes, as shown in Chapter 5, the press may
itself become an interface between researchers in a controversy
(an infrastructure of interfaces sometimes referred to as the pub-
lic sphere). I stress the importance of interfaces for a particular
reason. Translation, as described by Callon above, requires a
concrete time and place, it needs a certain format that is crucial
for understanding the social sciences on the same level as the
STS field previously has approached the natural sciences.

The Latourian actualism allows for no underlying structure,
no totality or logic of a whole to determine, condition or influence
a particular event; strictly read, it does not allow for qualitative
historical ruptures, epochs or conditions to ground the unfolding
of our collective societies. There are only irreducible actants
engaging in trials of strength, and modernity is nothing more
than a vast network of hybrids and black boxes stacked on top
of each other (see Latour 2001 [1991]: 46-87, Harman 2009 :127).
There can be no capacities *in potentia*. Nothing can be stored
waiting to be released. There can be only concrete *in actu* events,
sometimes referred to as moments of "translation" (Callon 1986).

In trying to study the social sciences, this is not a marginal
issue to me, but it must be resolved to know how to move from
one site of analysis to another, to be able to understand what
the black boxes entail and how they hold together. The issue
of actualism is at the heart of the question regarding where a
description begins, and where it ends.

Let me give an example. When the SOM survey of 1999 was
opened (see Chapter 6, I found the component "Kinnmark DM
AB". It qualifies as a black box because by reading the method-
ological appendix we learn nothing more than the input (a task
was delegated) and the output (data were returned). Kinnmark
is a private contractor that has been enrolled to pursue the ac-

tivity of "fieldwork" in exchange for monetary payment. To further understand the 1999 SOM survey, this box may be opened, and it would be possible to reconstruct in more detail how the fieldwork unfolded at this very moment, what obstacles it encountered and solved, what had to be tinkered with and what was unproblematic (further black boxes, already assembled). In Chapter 5, I will allow a controversy about election polls in 1985 guide me towards a pollster called *Sifo*, which played a similar role as that of Kinnmark in performing fieldwork for SOM.[15] These considerations hold well in regard to an actualist understanding. However, what happens when we consider the notion that this component, Kinnmark, may on the one hand have many other clients, may very well be part of other networks in which they perform other types of work (market surveys, other research etc.), and on the other hand, this component may be, and may have been, replaced by other contractors (for example *Sifo*, IMU, Gallup), even other types of agencies (such as SCB[16]).

As Harman notes, the analysis must be utterly concrete to not deviate from the actualist momentum:

> Since every actant is entirely concrete, we do not find its reality in some lonely essence or chaste substrate, but always in an absolutely specific place in the world, with completely specific alliances at any given moment. Everything is immanent in the world; nothing transcends actuality. In other words, Latour is proudly guilty of what Roy Bhaskar and Manuel DeLanda both call "actualism". For Latour the world is a field of objects or actants locked in trials of strength — some growing stronger through increased

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[15] See (Lithner 2000: 398) for a list of contractors for each year from 1986-1999.

[16] *Statistics Sweden*, a government agency responsible for central statistics production.

associations, others becoming weaker and lonelier as
they are cut off from others. (Harman 2009: 16)

When three of Latour's major works are combined, the "Ir-
reductions", *Science in Action* and *We Have Never Been Mod-
ern*, there is a continuum spanning across ontology, methodology
and historical understanding that resists combination with sev-
eral other approaches to treating how (scientific) knowledge is
produced. The main issue here is the questions of the invisible,
unknown, or hidden domains of knowledge to which Latour sel-
dom pays any attention (compared to, for example Kuhn, who
spends a great deal of effort in describing how scientific revolu-
tions actively forget the knowledge foundation of past paradigms
by rewriting history.[17])

Thus far, I have only dealt with the positive domains of sci-
entific knowledge: interfaces, black boxes and assemblages as
productive elements. However, a core problem in the theory of
science and STS is what is unknown, what hides in the uncon-
scious or hidden domains of imperceptibility, what is included
and what is left behind. This means that to address this com-
plex issue, I also need to adapt a terminology to discuss what
evades a concrete epistemic assemblage.

To these questions, at first glance, pure actualism provides
little room to navigate. I will propose that the black boxes I en-
counter have withdrawn hardened functions, which makes them
combinable and plastic. This feature has been called "immutable
mobiles" by Latour (1999: 306-307), and it resembles what Star
and Griesemer (1989) call a "boundary object". Because, as
mentioned before, blackboxing is a process of forgetting, embed-
ding and hard coding tasks and processes that are needed to
produce something that is superficially positive knowledge. For
example, a pre-compiled dataset of statistical information gath-

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[17] See especially the chapter "The Invisibility of Revolutions" in Kuhn (1996
[1962])

ered from a survey makes computerized statistical calculation possible, and quite user friendly compared to doing it manually, precisely because at any given moment, it provides us with the opportunity to forget thousands of questionnaires and how they were collected and assembled and to instead paying attention to creating bars and diagrams for a scientific report. Nevertheless, this is very important, for all of the details and components are there, at *full work*.

Actualism and Kuhn

Actualism creates an entirely different approach from that followed by Kuhnian thought. Whereas the process of forgetting is historically very dramatic in Kuhn's writing, I shall argue that it is shallow compared to the sort of object-oriented aspect of blackboxing. Taking a look at a look at a central passage in *The Structure of Scientific Revolutions*:

> In short, they [textbooks] have to be rewritten in the aftermath of each scientific revolution, and, once rewritten, they inevitably disguise not only the role but the very existence of the revolutions that produced them. Unless he has personally experienced a revolution in his own lifetime, the historical sense either of the working scientist or of the lay reader of textbook literature extends only to the outcome of the most recent revolutions in the field. (Kuhn 1996: 137)

This leads Kuhn to think about different historical paradigms as incommensurable, and the disguising of past revolutions leads to a linear and cumulative scientific progress. However, while these two points are refreshing for the history of science, they are indeed very clumsy for the more close-up studies of scientific activities. A paradigm would then appear as a monstrously large

black box in which a whole generation of scientists is only able to think within the box, while the actual workings of the machinery is veiled. According to Kuhn, only when a sufficient number of anomalies appear do scientists start to doubt the validity of the whole paradigm.

Two problems arise here. The monstrous aspect of Kuhnian historicity leads to a sort of over-determination. In the reports of the *SOM Institute* we find, for example, a terminology resembling the sociology of Durkheim, Parsons, Merton, etc. The methods of surveying and quantification are also borrowed from the intensified use of these methods in sociology towards the end of the 19th century. Although this is true on one level, I argue that it adds very little to our understanding of what is done and what that practice means. The abstractness of paradigms, rather ironically, makes the common production of scientific objects and other objects invisible. To provide a crude example (which is unfair to attribute to Kuhn himself), if I read in the local newspaper, "The researchers talk about a Gothenburg effect and a slow norm shift" (as already quoted in the prelude section of this chapter) and then conclude that this is knowledge within a Durkheimian paradigm because it talks about norms and norm shifts, I would instantly remove myself from a process that has significant value in translating the research practice of the *SOM Institute* into a circulation of facts. The concept of norms is indeed built into theoretical tools used (which in turn may be blackboxed); however, if we ignore the fact that another actor, the *Göteborgs-Posten* local newspaper, made use of and highly valued the much-debated question of corruption scandals, the role of science and its interfacing with other assemblages is abruptly veiled in darkness, and analysis would end on what I consider to be a shallow level.

Another more serious flaw in Kuhnian-inspired theories is their human-centered character. For science to change, either the scientists need to change their beliefs, theories and everyday

practices or they must be replaced by a new generation of scientists.[18] This is not true for technology or technoscience. The following are two examples, one simple and one advanced.

Example 1 — The hammer

A carpenter uses a hammer[19] as a routine piece of equipment when building houses. It is connected to other objects such as nails, human users, and wooden planks. Hammers are constructed objects, and in one respect, they reconfigure the human user too, who has to learn how to use it. One could even say that hammers are paradigmatic technologies of house building because they imply methods, can be calculated with by architects, among others. However, the hammer may *also* be used to commit a brutal murder. Thus, it becomes a piece of evidence in a murder investigation, is placed in a plastic bag, checked for fingerprints and may even become technical evidence used to incarcerate the murderer for several years. A skilled carpenter knows the difference between a good and a bad hammer, but when driving nails into wood, his or her attention lies somewhere else than with the technological advances, means of production, and the costs associated with the hammer. It is precisely because it is blackboxed, that the hammer may withdraw from full inspection and reflection, and this is why it is a powerful tool. As the house is completed and populated with new people, these inhabitants in turn do not need to know anything about hammers, even though hammers may be implicated in the house and need to be used again as the house is repaired. The hammer is thus

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[18] Of course, paradigms may extend over centuries, but it can still be said that Kuhn also classifies the durability of scientific beliefs around scientists and communities of researchers.

[19] Selecting this example is a tribute to Heidegger's tool analysis in §15 in *Sein und Zeit* (1972 [1927]), where the hammer is used as an example on how a piece of equipment is always related to other objects and that equipment must withdraw from consideration to be used for something.

more than its intended use with nails and planks, more than the carpenter's skills, and more than evidence in a courtroom. The hammer survives the house.

Example 2 — Experiments in relativity

Even though I consider SSK to be unsuitable for my theoretical needs, Harry Collins and Trevor Pinch (1993) have produced a textbook example of how scientific experiments may reinforce each other throughout historical paradigms. In their chapter "Two Experiments that 'Proved' the Theory of Relativity", Collins and Pinch set out to understand how the 1919 solar eclipse experiment led by physicist Arthur Eddington was accepted very swiftly by the scientific community, even though the results of the actual experiments were quite poor and inconclusive due to the harsh conditions associated with photographing light, which was supposed be displaced by the large gravity field of the sun (and thus proving the theory of relativity). The experiment was very difficult to perform at the time: cameras had to be mounted on remote islands synchronized with the solar eclipse and were sensitive to temperature and vibrations due to the long exposure times needed to create the photographs.

A contributing factor to the quick acceptance of the inconclusive results of the Eddington experiments was, according to Collins and Pinch, that beginning in 1881, Albert Michelson (later in collaboration with Edward Morley) performed a series of experiments with a wholly different purpose. They wanted to measure the "aether drift" that was thought to occur as the earth moved across space. It was believed that light traveled through the medium aether, and thus the movement of the earth would produce slightly different speeds of light along different directions. However, these experiments, which were conducted over half a century, failed to account for any significant variations; thus, many considered the speed of light to be constant.

Now, it may seem that the Eddington experiment and the

Michelson-Morley experiments are disconnected. Collins and Pinch link them, although the experiments are concerned with two different subjects:

> The way the 1919 observations fit with the Michelson-Morley experiment should be clear. They were mutually reinforcing. Relativity gained ground by explaining the Michelson-Morley anomaly. Because relativity was strong, it seemed the natural template through which to interpret the 1919 observations. (Collins and Pinch 1993: 52)

As the Michelson-Morley experiments continued to fail, they unintentionally reinforced Einstein's theory of relativity because the latter presupposes the constant speed of light. The results of the Michelson-Morley experiments, even though they were considered a failure, could become a component part in strengthening the Eddington experiments, even though Eddington had a wholly different theoretical purpose.

The focus here is a somewhat dramatic comparison. Just like the hammer can be used for both carpentry and murder, scientific results, methods and machinery can be used for very different purposes in different configurations and epistemic practices. Although carpentry and relativity are radically different activities, the parts and components can be taken out of their contexts because they are rendered mobile by way of blackboxing. Assemblages, architectural or scientific, mobilize and assemble their equipment, most of of which are already there. This is what I mean by withdrawn hardened functions in objects; a component may very well transgress its use in a specific time and place and be laid out as further composite parts in new assemblages. However, assembling and selecting what components to choose is not only about actively knowing where to go. It is equally important to forget. Be it about the theoretical func-

tioning about the hammer or the aether wind, exclusion is as
important as inclusion.

This is, I will argue, also the case for the social sciences,
especially with respect to their uses in quantification, which will
be the topic for the next section. The durability of surveys and
opinion research is not exclusive to certain scientific views or
the theoretical style of a generation of scientists. The social
sciences constantly interact with other assemblages, and they
continuously must adjust their interfaces to make sure they are
connected. This is, in its simplest form, the meaning of common
production.

Social scientific assemblages have looked very differently through-
out history, with specific problems and possibilities. To obtain a
more thorough picture of how these assemblages became impor-
tant in modern societies and why this tedious practice of making
surveys is worthwhile, I must refer to previous research on the
social sciences.

2.3   The emergence of social scientific knowledge

A concrete social scientific assemblage such as the *SOM Insti-
tute* did not emerge from a historical vacuum, even though, as I
mentioned above, blackboxing invents its own time with respect
to its reversibility, functionality and durability and is in need
of constant patching to function smoothly. Nevertheless, in my
empirical case, the *SOM Institute* was not invented from scratch.
Thus, there is a larger history of the quantitative social sciences,
some of them resembling the *SOM Institute* very closely, oth-
ers being different. Here, I will address previous research that
may not be methodologically coherent with what has been pre-
sented so far, which in turn makes it even more important to
reflect upon meta-theoretical implications. It is time to take a
look at how the functions, the institutional developments and

the relations to state powers have been described and analyzed. To clarify my position, I will first discuss briefly what paths I have chosen not to follow, then move back to the concepts of blackboxing, interfaces and assemblages.

A Social Scientific Episteme?

In his work *The Order of Things* (1994 [1970]), Michel Foucault outlined what he called an "archeology" of the sciences, with the purpose of building a framework for understanding how the modern sciences had emerged towards the end of the 18th century. Foucault's thesis was that there was a deeper epistemic re-arrangement from what he called "Classical thought" and into the "Modern episteme". This re-arrangement marked a general shift in what "thought" was able to take as an object of knowledge in the first place. The dawn of modern thought, Foucault argued, was marked by three main empiricities, or "quasi-transcendentals", "life", "labor" and "language", which became the foundation for the new sciences of biology, political economy and linguistics but also provided a fertile ground for the human sciences to reflect upon and to analyze "man". The argument, which from time to time was harshly criticized[20], was an argument of a radical contingency:

> Before the end of the eighteenth century *man* did not exist — any more than the potency of life, the fecundity of labour, or the historical density of language. He is a quite recent creature, which the demiurge of knowledge fabricated with its own hands less than two hundred years ago: but he has grown old so quickly that it has been only too easy to imagine that he had been waiting for thousands of years in the darkness for that moment of illumination in which he

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[20] For an overview of the immediate reception in the late 1960s, see Miller (1993: 148-161).

> would finally be known. (Foucault 1970: 308, italics
> in original)

Consequently, the study object of human sciences, which here includes psychology, sociology, ethnology as well as linguistics, did not exist before the arrival of the modern episteme and presupposed that biology defined what life was, that political economy regarded labor as the driving motor of economies, and that proto-linguists started to analyze language as something that was spoken by men. The introduction of these processual features allowed life, labor and language to become ever-changing activities that in turn could be studied scientifically in a whole new fashion compared to the rather static structure of Classical thought. Life, labor and language had acquired historicity, and because of these processual quasi-transcendentals, it appeared as if man had been there all along as a living, laboring and speaking entity.

This daring project of Foucault, to connect the human sciences with all other contemporary sciences, to find the common "table" on which they are all arranged and interrelated, is a seductive figure of thought. Quantitative sociology, which features a laboring population that lives and speaks and gives rise to norms and values, attitudes and behavioral patterns that can be measured and counted with statistics, finds its own place in the modern episteme, and it could not have existed before.

While the social sciences[21] have debated their philosophical foundations in bursts during certain historical episodes, highlighting topics such as the nature of social life, the relationship between individuals, groups, society, etc., they have also necessarily been forced to render empirical facts and measurements knowledgeable in an epistemological field. This field may have

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[21] In this section I will consider only the social sciences that use quantification as a core epistemological component. Thus, interpretative, qualitative and anthropological accounts will be left aside for the moment.

certain boundaries, limitations and possible ways of describing society, but it must hold together in certain shapes and configurations; otherwise, the social sciences would appear as nothing but mere speculation. This *holding together*; the assembling and re-assembling of activities of quantification, classification, and measurement, is what I am looking for when choosing entry and exit points of study. However, this is not to be found in *The Order of Things*. Empirical events in the form of concrete assemblages such as the *SOM Institute* are only meaningful if they can be linked to other entities, such as populations, territories and institutions. To stay on track with my commitment to actualism, an analysis of epistemes would be far too broad, and in some ways, it already contains the answer to the research questions I am posing. However, there is another line in Foucault (and later works inspired by him), which serves my purpose much better.

The Modern State and the Sciences of Man

A wide body of research has departed from Foucault's concept of "bio-power", which appears in the first volume of his work *The History of Sexuality - The Will to Knowledge*(Foucault 1977 [1976]). Another publication of importance here is the lecture "The Birth of Social Medicine" (Foucault 2000) held in 1974, which elaborates the concept further.

The core argument of the *The Will to Knowledge* is that a new scientific outlook appeared towards the end of the eighteenth century and onwards, not only regarding sexuality, which is the core topic of Foucault's later works, but also regarding the relationship of social bodies and society in general. This is expressed in the curious interest in scientific and administrative institutions concerning the "population":

> One of the great innovations in the techniques of power in the eighteenth century was the emergence of "population" as an economic and political prob-

> lem: population as wealth, population as manpower
> or labor capacity, population balanced between its
> own growth and the resources it commanded. Gov-
> ernments perceived that they were not dealing sim-
> ply with subjects, or even with a "people," but with
> a "population," with its specific phenomena and its
> peculiar variables: birth and death rates, life ex-
> pectancy, fertility, state of health, frequency of ill-
> nesses, patterns of diet and habitation. All these
> variables were situated at the point where the char-
> acteristic movements of life and the specific effects
> of institutions intersected [...] (Foucault 1977 [1976]:
> 25)

The interest in the population and its status as "problem-
atic", resulted in a breeding ground for emerging sciences of pop-
ulations all over Europe, which is the main territory of Foucault's
historical accounts. Biology, immunology, social medicine, soci-
ology and psychology flourished during the 19th century and
were incorporated into applied administrative work, such as ur-
ban planning, vaccination programs, food and water safety, prison
facilities and crime prevention, as well as in conscription armies.
In "The Birth of Social Medicine", Foucault states the following:

> With this view, France, England, and Austria be-
> gan to evaluate the active strength of their popula-
> tions. Thus, birth and death rate statistics appeared
> in France and, in England, the great census surveys
> that began in the seventeenth century. But at the
> time, in both France and England, the only health
> interest shown by the state had to do with draw-
> ing up of tables of birthrate and mortality, which
> were true indications of the population's health and
> growth, without any organized intervention to raise
> the level of health. (Foucault 2000: 139)

With the inception of social medicine, a science closely related to the social sciences and never completely distinct from them, a tripartite arrangement was needed: the epistemic grounding in "biohistory", the knowledge of organs, diseases and the role of hygiene. What Foucault calls a "medicalization" of core institutions acting on the human body was also needed. Hospitals, roads and infrastructure were adapted to intervene in case of sudden outbreaks of plague, tuberculosis or viral epidemics, institutions that needed collective funding and organization. Third, social medicine required an "economy of health", in which health was integrated into a system of laboring populations, which in turn were able feed back to the wealth of society by working and reproducing (Foucault 2000: 134-135).

In the lecture series *Society Must be Defended*, Foucault describes bio-politics (Foucault 2003) as measures directed towards the life of the population, its fertility, reproduction, its ratios between births and deaths, and how to prevent this general body from becoming unhealthy, which "became bio-politics' first objects of knowledge and the targets it seeks to control [...] [the] moment that the first demographers begin to measure these phenomena in statistical terms" (Foucault 2003: 243).

Central to bio-politics are the regulatory mechanisms that, during the 18th and 19th centuries, became intense objects for the medical, social and hygienic sciences by means of quantification.

> The mechanisms introduced by bio-politics include forecasts, statistical estimates, and overall measures. And their purpose is not to modify any given phenomenon as such, or to modify a given individual insofar as he is an individual, but, essentially, to intervene at the level at which these general phenomena are determined, to intervene at the level of their generality. (Foucault 2003: 246)

This dissertation, however, is not a study of bio-power or bio-politics in general terms, but in a narrower sense, it should be understood as an investigation of the social and epistemological consequences of building surveys, quantitative social science, and research institutes and their relationshipss with other social institutions. To narrow the theoretical ambition, we must return to quantification. Joshua Cole partly extends the Foucaultian analysis in *The Power of Large Numbers - Population, Politics, and Gender in Nineteenth-century France* (Cole 2000). However, the attention of the details of French statistics shift slightly from bio-politics in general to quantification itself. Cole concludes:

> The discovery of "population" between the 1770s and the 1830s transformed the relationship between the French state and the French people in several ways. First, the study of population allowed researchers and government offices simultaneously to combine and subdivide groups of subjects or citizens into new social aggregates whose collective bodies shared some essential characteristic. These aggregates were perpetually in motion, but their size and vitality at any given moment could be quantitatively fixed through enumeration. (Cole 2000: 212).

What Foucault only glosses over quickly is at the center of Cole's analysis: that quantification, enumeration and measurements lead to social aggregates, which render the population visible in a manner entirely different from that achieved by qualitative argument. Cole also stresses the epistemic authority sought by statisticians in 19th century France, in "[the] ability to restrict the possible field of debate and to prevent any potentially distracting proliferation of opinions, speculations, counterexamples, or alternative views" (Cole 2000: 9). The aggregation of the social, as it is enumerated and calculated, is an epistemic

activity distinct from, yet interconnected with, theoretical reasoning and empirical observations.

Thus we must look at a broader picture rather than think of statistical sciences merely as utilities for the rise of the Swedish welfare state. The combination of census and statistics provides concrete strategies for solving social problems; but even more importantly, however, it promotes the construction of "social bodies" (Horn 2004), which constitute the contents of the geography[22] of the social. Silvana Patriarca (1996) argues that the rise of statistical thinking in 19th century Italy was a process that occurred in parallel with the rise of the modern Italian nation. She shows that through a centralized survey the numbers produced by statisticians in the newly founded national surveys produced a "power of association". By measuring the whole Italian peninsula, the barriers between the northern and southern provinces could be torn down, thus creating associative links through numbers. As holistic investigations replaced the old regional ones, a unified map of the new nation emerged, thus securing territory through statistical "spatialities".

David G. Horn (2004) addresses the subject of Italy, arguing that the foundation of Italy's Central Institute of Statistics in 1926 was intimately intertwined with the practice of government during the Mussolini regime. Using the concept "social bodies", Horn conceptualizes how the "social" was created, not as an ontological demarcation from the natural sciences but rather as a landscape where certain problems would emerge, such as consumption, infertile marriage, or crime. Thus, I am treating statistical knowledge as constitutive both in the sense of it being a practical knowledge for state-intervention and in providing and producing a spatiotemporal framework for the experience of pop-

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[22] The geographical terminology is not metaphorical. The social sciences provide in the historical examples I draw from a very concrete mapping of the "social", allowing for the localization of social problems as well as social associations constituting entities such as the nation, the region, the poor, the middle class, etc. to emerge from these landscapes.

ulations, nations, classes, and social problems. Statistical knowledge regarding society makes it whole, or as Desrosières (1991) puts it, statistics do the work of "holding together" knowledge, practice, and the state.

Biopolitics and assemblages

In Foucault, there is a double articulation in the emergence of the social sciences; on the one hand, there is the qualitative function in biopolitics as an administrative, surveying and organizing science in what he called the emergence of disciplinary societies. On the other hand, as mentioned above, the positive domain of knowledge became possible through the void that had to be filled because there was a tectonic rupture between the Classical and the Modern episteme, a reconfiguration that was external to the social sciences themselves and occurred in conjunction with how the other sciences rapidly discovered new grounds of knowledge.

This could have been another way of describing what Latour calls the modern constitution, if it were not for the drastic philosophical differences between Latour and Foucault. Latour argues that the purification of the modern constitution is an ongoing, tedious process. If it is not maintained, it breaks down, and we realize that all we have are "savage hybrids". The modern episteme, as described in *The Order of Things*, on the contrary, would postulate that the qualitative reconfiguration that took place towards the end of the 18th century, made thought possible in only one particular way[23]. This has sometimes been called a "structuralist" explanation, even though this is a bad word[24],

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[23] For example, the presence of fossils was an unthinkable figure, a monstrous anomaly, before modern biology introduced "historicity" into the study of life, see the chapter "Monsters and Fossils" in *The Order of Things*, especially 1970: 156-157.

[24] Foucault himself rejected this label as nothing but a fancy term for "commentators"; see "Preface to the English edition" in *The Order of Things* (1994 [1970]): xiv.

both because it is quite empty of meaning, and because of its association with linguistics.

The figure that is key in understanding these differences is what I previously referred to as actualism. First, however, there is a similarity in at least one respect that must be addressed. In a very interesting passage in *Pandora's Hope* on how human and non-human agency are related, Latour writes the following:

> Purposeful action and intentionality may not be properties of objects, but they are not properties of humans either. The are the properties of institutions, of apparatuses, of what Foucault called *dispositifs*. Only corporate bodies are able to absorb the proliferation of mediators, to regulate their expression, to redistribute skills, to force boxes to blacken and close. Objects that exist simply as objects, detached from a collective life, are unknown, buried in the ground. (Latour 1999a: 192-193, italics in original)

The concepts of dispositif and assemblages are closely related in their collectivity, positivity, and also in the sense that they are actualist concepts. They configure and enable a collectivity of human and non-human agency to express knowledge in specific ways. The Hubble telescope can be considered such a dispositif, or assemblage, composed and held together by hundreds of scientists, thousands of technical components, billions of dollars and even the gravity of planet Earth. All of these links need to be interfaced and the black boxes must be patched and fixed. The result is nothing less than images of distant galaxies. Remove the humans, and the telescope slowly runs out of power or burns up in flames as it falls through the atmosphere. Remove one lens, and we see nothing more than we did before Galileo. Thus, it is not the biopolitical side of Foucault that is a problem for Latour.

Instead, the problem is the model of epistemes. Indeed, along
the lines of Foucault, Latour also acknowledges "Kantianism" as
one of the leitmotifs of modern thought (pre-dated by Hobbes
and Boyle, see Latour 1991: 57ff). However, the introduction of
hybrids and a-modern networks, which when multiplied, fold to-
gether heterogeneous elements through moments of utterly con-
crete translations, there can be no prior historical rupture, as
Foucault argues in *The Order of Things*: no void emerges si-
multaneously in all of the sciences. To put it in another way,
Linnaeus and Darwin, even though the former belonged to the
Classical episteme and the latter in the Modern, would, accord-
ing to Latour, have done the *same primary things*: collapsed the
inside/outside division by bringing samples of minerals, birds
and flowers back into their labs, inscribing them into systems
and classifications; and forced them to crack open, while strug-
gling with kings, churches and perhaps even public opinions to
support their assemblages. Neither Linnaeus nor Darwin were
ever modern, even though the latter lived during a time when
the proliferation of hybrids had become much more swift, more
efficient, and even more desired by institutions of immunology,
public health, anatomy and medicine.

Foucault's dispositifs are diagrams, shapes of visibility that
are repeated throughout several institutions — schools, prisons,
factories — yet they are historically contingent in the sense that
they emerged and proliferated towards the end of the 18th cen-
tury and onwards. According to Foucault, the sciences, espe-
cially those of man[25]/society, became enmeshed and functioned
as composite parts in these diagrams. In modern societies, there
emerged a science of crime, of behavior, of normality and de-
viance, of bodies and diseases. Problems needed to be solved

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[25] The notion of "man" in Foucault's work sounds at times a bit dated,
implying a certain kind of universality. This is, however, not the intent. For
a criticism of the universality of the subject and its patriarchal shape, see
the second volume of his *History of Sexuality - The Use of Pleasure* (1985).

because the fear of plagues, mass behavior and sudden instabilities had grown stronger with the swift changes in modern industrial societies, and these had to be calculated and accounted for. Knowledge became a function of control but not necessarily in an authoritarian form; instead, it was primarily productive because it multiplied fields of visibility never seen before. It inquired in minute details into the conditions of the individual in a society, its mental health, its physical health, its social position, it sexual pleasures. If "man" emerged in the modern episteme as an object of positivity, through a violent rearrangement and rupture, he also emerged as a very concrete object of knowledge through medical, psychiatric and sociological inquiries and practices. It is this latter interpretation, of concrete practices and their functioning in core institutions of modern societies that I find the most interesting in the study of the social sciences. Moreover, it is this part of Foucault's analysis that is also compatible with the actualist presuppositions that I make.

2.4 The black boxes of quantification and statistics

Thus far we have looked very directly at how blackboxing takes place, how it communicates via interfaces, and how black boxes and interfaces are combined in epistemic assemblages. There is, however, another riddle that must be solved, namely that of quantification and statistics. Once more I shall give an example from the sociology of scientific knowledge or, to be more precise, of one work by Donald MacKenzie. I will also consider Latour's notion of centers of calculation, the history of statistics and its epistemic status, and further advance into some more recent research in the sociology of quantification.

Pearson and Yule — A statistical controversy

In his article "Statistical Theory and Social Interests" (1978), Donald MacKenzie analyzes a controversy and an emerging breakthrough in statistical methods that took place during the first one and a half decades of the 20th century between Karl Pearson and Udny Yule, both regarded as two pioneering statisticians today.

The controversy between Pearson and Yule concerned how to measure association on a nominal level. In 1905, Pearson had suggested the tetrachoric coefficient as a solution of how to quantify nominal scales, something that Yule criticized openly for several years.[26] MacKenzie's elaboration of this controversy is interpreted by analyzing of their respective differences in social interests:

> [...] Pearson's commitment to eugenics played a vital part in motivating his work in statistical theory. Pearson's eugenically-oriented research programme was one in which the theories of regression, correlation and association played an important part [...] Regression was originally a means of summing up how the expected characteristics of an offspring depended on those of its parents; the bivariate normal distribution was first constructed by [Francis] Galton in an investigation of the joint distribution of parental and offspring characteristics. (MacKenzie 1978: 53)

MacKenzie's point holds that advances in statistics, even though regarded to be esoteric and mathematically disembodied, are guided and influenced by a set of social and cognitive interests that orient the goals and directions of what to develop and

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[26] The controversy is much more elaborate than this. To save space, I refer to MacKenzie's article, to be read in its entirety.

what to disregard. The early 20th century statistics in Britain were thus, at least partially, influenced by a need for eugenics and population control. In Britain, at the time, eugenics and "national efficiency" were regarded as legitimate political options and were even discussed in governmental departments. Yule, on the contrary, had no affection for eugenics and instead argued that heredity was a largely unimportant factor compared with environmental factors (MacKenzie 1978: 58-59).

What we have is thus a classical social explanation of how statistics develops in line with the needs defined by group interests (such as the eugenics movement) and larger social interests (for example, state governance). What MacKenzie pays less attention to is what happens next:

> Contemporary statistical opinion takes a pluralistic view of the measurement of association, denying that any one coefficient has unique validity [...] Yule's Q remains a popular coefficient, especially amongst sociologists. Pearson's tetrachoric coefficient, on the other hand, has almost disappeared from use except in psychometric work. (MacKenzie 1978: 65)

I am not in the position to evaluate whether this is valid or not for statistics in general. What I, on the other hand, find necessary is to consider the dispersion, usage and effects of statistical methods within the terminology of blackboxing. During the early 20th century, many of the core statistical measurements that are today used in the social sciences were developed[27], for

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[27] See, in particular, Alain Desrosières' overview of three concurrent types of policy in late 19th century Britain, each of them pushing statistical research and development in different directions to solve poverty problems. For example "[a] 'eugenic' policy would therefore mean limiting the fertility of the poor. In order to defend these ideas, Pearson formulated the concepts of correlation, of regression and the chi-square test. Thus, though discredited, this configuration nevertheless laid the foundations for mathematical statistics subsequently taken up into inferential statistics [...]" (2011: 49).

example the chi-square test, Pearson's r, advances in correlation and regression by Galton, etc. (see also Hacking 1990: 180-188; Desrosières 2011: 47-50).

Just like the Michelson-Morley experiments, which also applied now deprecated statistical methods, may very well come to reinforce or weaken the decisions regarding what black boxes to open and which ones to leave closed. Statistical methods may be blackboxed, taken out of their context of discovery and applied widely, or, they may be broken, considered obsolete, or simply veiled in historical darkness for other reasons, perhaps only to emerge in the detailed archives of the history of science.

An example of a successful blackboxing is the Pearson r coefficient. In a textbook on social scientific methods, written by Gothenburg researchers close to the *SOM Institute* and taught in many social science classes locally, an interesting passage appears:

> The calculation of Pearson's r is complicated to say the least [...] Even though it can be useful to on some occasion make the calculations yourself [...] not long ago the researchers had to employ assistants to be able to do these calculations at all [...] it is of course [today] the computers that calculate Pearson's r for us. (Esaiasson et al. 2002: 392)[ix]

The Pearson product-moment correlation coefficient (r) demands time consuming effort and plenty of mathematical skills to calculate manually. Intitially, such calculation was delegated to research assistants (involving more human actors). Today, we finally have computers that can perform the same calculations in milliseconds. A statistician, or social scientist for that matter, must of course be able to master the use and interpretation of computer output, but in routine work, he or she is able to forget about the assistants and the hard work that it once took to use this statistical tool.

Thus, it is possible to conclude that the statistical measurements developed in the context of the British eugenics movement can be dislodged from their context of discovery through blackboxing and find their way into the software packages that are used today in statistical calculation, which are used as standardized measurements and tests to evaluate the quality of survey data. This de-contextualization not only means that it is possible to forget the tedious work that had to be performed before computers. It also means that it would be absurd to accuse someone calculating the Pearson's r of being a follower of eugenics, just as it is equally absurd to accuse someone of militarism for using the internet just because the internet was originally conceived as a military computer network. For statistics, Latour's actualist principle still applies: the fate of facts and machines are in later users hands, and their qualities are a consequence of collective action (Latour 1987: 29, see also the above sections on Latour).

However, not only are statistics blackboxed as they are assembled as research practices. They also function as interfaces that are able to translate research results into comprehensible facts. The time is ripe to go further along this line and to investigate in particular how the modern state has requested such scientific information.

Quantification interfaces

Thus far, I have provided an example from a press conference held by the *SOM Institute*, where research and social scientific facts were translated into the type of source material used by reporters of news media. This moment of translation is an interface between the research practices of the *SOM Institute* and another practice of journalism.

There is, however, another relation, which is much more profound historically, between the social sciences of quantification and the modern state. On a superficial level, it could be ar-

gued that this relationship is self-evident, as witnessed by the etymology of *statistics*, which from the Latin words *statisticum collegium* came to designate the state sciences in the late 18th century or the English *political arithmetic*. However, etymological statements do not describe the processes through which the social sciences and the state are co-produced. The study of common production must go through the study of concrete interfaces. Moreover, an interface never features a one-to-one translation. Everyday research practice, which is sometimes veiled by esoteric knowledges that are incomprehensible to the outside observer, always change in appearance as it moves from one location to another.

Ever since the first surveys were performed in Sweden, social scientists as well as pollsters have reflected upon the role of their knowledge production and their impact on political life. Sten Hultgren, one of the key persons in importing the Gallup methodology to Sweden and further developing it during the 1940s, for example, describes a minor controversy in 1947, the year before industry-sponsored advocates had ordered a survey from the Swedish Gallup Institute to measure attitudes towards state-owned enterprises. With the numbers from Gallup, the results were interpreted in a very biased way, promoting only the results that showed negative attitudes towards state ownership, in line with the industry's agenda. This led to parliamentary discussions on the danger of surveys conducted by private interests, and it was suggested that a state-controlled institute of public-opinion research should be established (Hultgren 1990: 28-32).

In more recent times, a multitude of different pollsters have emerged, with varying degrees of private ownership, as well as large university-based surveys. SOM researchers have themselves measured how often their surveys are quoted both in news media and in official parliament documents to estimate the impact of their activities. In a 2008 article, Sören Holmberg presented a

Surveyor	1990-1995	1996-2001	2002-2007
Sifo	94	149	104
Temo	25	58	93
SOM Institutet	1	15	47

Figure 2.3: Reference frequencies to opinion polls in the Swedish parliament. Reproduced and abbreviated from Holmberg (2008: 149).

survey[28] on how often opinion polls had been quoted or used as a reference in the Swedish parliament (Figure 2.4).

This provides a general picture of how quantitative opinion research is used in one particular legislative institution, namely the parliament. Qualitatively, the use of statistical arguments may vary widely, ranging from being featured large sections in official whitepapers and reports to brief rhetorical devices to strenghten lines of argument. In this context, the *SOM Institute* can be viewed as one of several actors that together define the social landscape, how it is arranged and what its general opinions are, to serve as a composite part in a social ontology. A line of argument may be strengthened or weakened by means of scientific reference.

During the turn of the millennium, a typical reference to the *SOM Institute* among parliament documents was made in official government whitepapers. One such example is SOU 1999:68, a whitepaper from the Department of Culture concerning a tragic fire in 1998 that took place in Gothenburg, and killed 63 young individuals. The fire disaster triggered a debate on trust in public institutions, such as the state, the news media and authorities

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[28] The quantitative content analysis of the parliament documents was performed by Jenny Wiik and Per Hedberg. It was never published, but in an e-mail correspondence with Wiik, I managed to get hold of the draft (Wiik 2008), in which the details about the search variables in the documents are presented.

such as the police, especially among immigrant groups, who had
been the major victims of the fire. The *SOM Institute* provides
detailed statistical results from their surveys that measure trust
in institutions; and the whitepaper concludes the following:

> Individuals with an immigrant background clearly
> have less trust in the "state" and Swedish authori-
> ties compared to Swedes without an immigrant back-
> ground. The difference is very large concerning the
> trust in the police, medical care and courts. The
> royal family and the Swedish [state] church also en-
> joy less trust. Immigrant groups, on the other hand,
> have clearly greater trust in institutions that are "au-
> tonomous" from the state, such as large corporations,
> banks, universities and colleges, and radio and tele-
> vision. (SOU 1999:68: 142-143)[29] x

To use the terminology at hand, as blackboxing is completed,
as complexities are able to withdraw and as the SOM survey is
assembled and completed, the results may be translated into one
of several interfaces in the administration and problem-solving
activities of the state. This is something that can be followed
around from one reference to another. Methodologically, there is
no need for interpretation. The common production of the "so-
cial" does not require a detour via the "signifier" or "discourse".
It is possible, as long as empirical material can be assembled
properly, to follow references to texts, instruments, scales, prac-
tices, people, etc. in a very concrete sense.

However, there is another dimension to this observation. *Who*
gets to define the social ontology that is included in a govern-
ment whitepaper? As I will elaborate in chapter 5, this is a

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[29] SOU is the abbreviation for *Statens offentliga utredningar*. These govern-
ment whitepapers are published annually and sequentially numbered. I cite
these sources in the format they have in Sweden ("year:number: page").

case of *epistemic authority* in which the boundary between science and non-science constitutes a very important distinction. In Figure 2.4 the two pollsters *Sifo* and *Temo* are, in quantitative terms, quoted more often than the *SOM Institute* in the official papers. To ascribe to this setting a certain logic beforehand may turn out to be deceptive. Is there competition, or is there collaboration, symbiosis and sharing of data? The answer can only be determined in specific settings and may vary throughout history. The matter of concern is nevertheless to understand the usage of quantitative facts regarding a society, to solve problems within that same society.

2.5 Centers of calculation

Throughout this chapter I have elaborated on the three concepts of black boxes, interfaces and assemblages, with inspiration from lines of thought that are present throughout the currently wide body of literature usually referred to as STS.

The primary challenge has been to avoid falling for the temptation of studying ready-made science. Thus far, I have said very little about the content of the SOM survey, its expressive or discursive sides, how groups of society are represented, how scientific authority is signified, etc. This is indeed intentional because my conceptual tools primarily ask for the *format* of the quantitative social sciences: their logistics, their instrumentation, their communicative technologies, their inscription devices — *not* the "final outcome" of ready-made science. Latour sums this up very well:

> The very definition of a "society" is the final outcome, in Sociology Departments, in Statistical Institutions, in journals, of other scientists busy at work gathering surveys, questionnaires, archives, records of all sorts,

arguing together, publishing papers, organising other
meetings (Latour 1987: 257).

The concept Latour is launching in the final section of *Science
in Action* is "center of calculation". The primary function of
the social scientific center of calculation is to express the social,
to describe society as a whole (as opposed to, for example, a
cartographic, zoological, environmental, or economical centers of
calculation, whose tasks are to express other domains of facts and
knowledge). How many centers there are, how far they reach, and
how strongly their expressed knowledge influence other actors
are empirical questions. Centers can be counted, followed and
analyzed, and I will argue that by opening black boxes, studying
interfaces, and observing how surveys are assembled from year
to year is a productive way of proceeding to understand how
these centers emerge, are made to work, become harder and more
powerful, and sometimes disappear and run into obstacles.

Centers of calculation only function if things can be black-
boxed, standardized, organized, relied upon and made durable
over time. Moreover, the centers can not be isolated; they need
to bring questionnaires in and out, they need to circulate their
facts and make them appear in various other places (in the news,
in the parliaments, in reports and books). This is why they must
debug the black boxes when they break and configure their in-
terfaces to be able to constantly reassemble the center.

There is no absolute criterion for when a couple surveys or
investigations turn into a center of calculation, nor is there a for-
mal award or institutional instance (perhaps with the exception
of totalitarian societies) called *the* center of calculation. Rather,
it is an object that, according to my actualist position, emerges
from and acts upon the world. Its strengths, weaknesses, move-
ments and performances can only be demonstrated through em-
pirical investigations, which is the purpose of the following four
chapters.

Chapter 3

1986 — Blackboxing the postal survey

In this chapter, I will look into the practice of creating surveys, questionnaires and methodological programs of action. As mentioned before, I will consider the process of blackboxing as a core element in not only making surveys work as productive epistemic assemblages, but also as tools that negotiate the credibility of scientific facts. The aim is to challenge the accustomed distinction between theory and methodology and to open up a space where these two domains are intermingled into a constitutive practice, which is crucial to the empirical social sciences. As this field is unlocked, I will analyze how aspects of survey methodologies have specific features built into them, rendering some aspects of social life visible and some invisible.

Prelude — The contradiction of surveys

In surveys, according to the "Gallup methodology" one frequently runs into a problem of circularity. Before the survey is initiated you must presuppose the existence of a public opinion to the matter at hand,

and to formulate a reasonable question, you must fur-
thermore in advance presuppose certain characteris-
tics concerning that public opinion. However, only
as the investigation is completed, you will be able to
obtain knowledge about the existence and properties
of the public opinion! (Westerståhl 1950: 60-61)[xi]

How is it possible to know what people in a society think?
Moreover, how can such a society be defined in the first place?
As the quote above by Westerståhl highlights, social scientists
need to presuppose that there is a public opinion already out
there, and know how to interrogate it, to send out a survey that
actually measures it. In a Popperian-style falsification model,
this contradiction is basically unproblematic. You generate a
hypothesis, then you test it. If you are wrong, you simply gen-
erate a new one. However, when resources are finite and when
surveys must be constructed, dispatched and returned, these pre-
suppositions are more than just hypotheses. They are, as I will
show, also deeply intertwined with the very act of assembling
a research instrument, how to make it work, and consequently,
when it works, how to blackbox it.

This chapter will circle around the year 1986, when the first
SOM survey was made — a postal survey that successfully ren-
dered high-quality social scientific results. However, to under-
stand the problems of conducting a postal survey, I will first
make a brief excursion into the history of survey-making in Swe-
den, beginning where Westerståhl makes his observation of the
contradiction of measuring the public opinion while simultane-
ously pre-supposing it.

When social scientists in Sweden first encounted a large dataset
collected over a period of time during the war, it had not been
generated by academic researchers themselves. The data had
been compiled by the Gallup Sweden pollster, who had estab-
lished an institute that attempted to copy the American Gallup

model of making election predictions and then sold the results of the polls to different organizations.

During the early 1940s, in a tiny Stockholm penthouse, Sven O Blomquist had assembled a small crew to make the first Gallup-style surveys, which were to be sold to newspapers. According to Sten Hultgren, who had been appointed to edit the Gallup survey, the interest in public opinion studies of this kind were sparked very swiftly, and both politicians, government officials and academics followed the work of Gallup closely (Hultgren 1990: 15-19). During these years, Elis Håstad led the higher seminars at Stockholms Högskola, and he invited Gallup to participate. In 1950 Håstad edited the book *"Gallup och den svenska väljarkåren (Gallup and the Swedish voters)*, in which social scientists attempted to evaluate the Gallup methodology as well as draw some conclusions about the public opinion in Sweden during the 1940s. Håstad describes the data collection during these years as indispensable and unique (Håstad 1950: 9).

The encounter between academic researchers and the pioneering pollsters led to fundamental questions concerning both how to theorize the public opinion and how to measure it. In a sense these two questions, traditionally separated as ontology and epistemology, were intertwined and problematized. Political scientist Karl-Ingmar Edstrand, for example, pondered how to interpret that sometimes a large section of the respondents, which answered that they "did not know" (the "do-not-know group") when asked about matters, were supposed to belong to a public opinion:

> When can one assume that a public opinion is at hand? What are the boundaries of the do-not-know group, the group that does not have an opinion? [...] Neither will progress be made by defining the concept [opinion] — which is possible, and has been done — in such a fashion that it will include practically

all thoughts and expressions of meanings. (Edstrand
1950: 187)[xii]

This struggle to define what the public opinion is made up of,
is a recurring theme in political philosophy and early sociology.
My line of inquiry is, however, of another kind; I shall dwell on
the question of how to *measure* public opinion and how to render
the public opinion visible using surveys and questionnaires. I will
circle around what in the 1950s was given the name "do-not-
know"-group and, furthermore, on the problem of drop-off rates
— the imminent threats of response rates being insufficiently
high. In this manner, I will be able to guide analysis towards
the practice of assembling the survey, blackboxing and opening
it back up again, to give a kind of object-oriented record of how
surveys became scientific instruments in post-war Sweden. To
keep track of my main object of study, the *SOM Institute*, this
story will begin some 35 years later in 1986, when the first SOM
postal survey was created.

3.1   1986 — Point of departure

As scientific experiments take place for the first time, it is often
the case that blackboxing plays a particularly interesting role
in rendering problems and solutions visible. It is a recurring
theme in the STS literature, especially in historically oriented
authorships. To name two classics in the field, both of which
were discussed in the previous chapter, Collins and Pinch's study
of the Eddington experiments (1993) and Latour's introduction
to *Science in Action* (1987) on the discovery of DNA's double-
helix structure both point to the productive aspects of departing
moments of uncertainty, before science is turned into everyday
work.

The 1986 SOM survey is documented in the first publication
of the SOM report series. This is the first survey of the newly

founded *SOM Institute*, but I will treat it neither as the beginning of a story that progresses forward nor as an epiphenomenon of a pre-existing structure. Rather, I will use it to describe what has to be done to make the survey happen in the first place and to find more blackboxes that open up social scientific practice.

The 1986 survey is edited by Sören Holmberg and Lennart Weibull, and my first site of interest is their four-page introduction to the *SOM Institute* and the three-page methodological report right written by research assistant Bo Reimer. According to Holmberg and Weibull, the reasons for choosing a postal survey are mainly that a large sample can be made at a reasonable cost and that this in turn allows for the surveys to be made on a regular basis. This choice is supported with a reference to previous experiences:

> The [our] experience of survey methodology has been present at the Department for Political Science for a long time. The above mentioned research program *Dagspresskollegiet* uses postal surveys in their local [newspaper] readership reports, and research into public administration has worked with surveys in several local municipality studies. The department has also been in contact with organisations such as *Testologen*, *Forskningsgruppen för Samhälls- och Informationsstudier*[1] (FSI, Torsten Österman) and Stiftelsen för opinionsstudier[2] (Sten Hultgren), which all perceive themselves as being well experienced in survey methodology. (Holmberg and Weibull 1986: 2, my italics)[xiii]

The SOM survey of 1986 did not stand alone, but made al-

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[1] The FSI connection only appears here in the 1986 survey report. Since my main focus in this chapter is the calibration of data, I will not elaborate further on this connection.

[2] The correct name is Stiftelsen för opinionsanalyser.

liances with their own in-house research project *Dagspresskollegiet* that ran surveys through *Testologen*, *FSI*, and *Stiftelsen för opinionsanalyser*. According to my actualist methodology described in the previous chapter, these references must be followed back in history to see how they are mobilized as alliances that support the 1986 scientific statement.

Alliance I: The Public Opinion Analysis Foundation

Stiftelsen för opinionsanalyser, or, in English, *The Public Opinion Analysis Foundation*[3] was co-administrated by Sten Hultgren, who had a history of making surveys for Gallup Sweden, as mentioned above. Hultgren is particularly interesting because there is a record of his experience with the postal survey in his partly autobiographic book on the history of Gallup Sweden (Hultgren 1990).

In 1977, Hultgren and the sociologist Lars Gråby had taken the first steps to form the foundation, to start measuring political opinions, sometimes with a reference to George Gallup's visionary idea of providing "the voice of the people" through surveys (Hultgren 1990: 76). Their first postal survey was made in 1979, and Hultgren described in retrospect the success of the methodology: "Unexpectedly the "postal survey" produced more reliable results than the "visiting interviews", instead of jeopardizing the representativity of the surveys (Hultgren 1990:77)[xiv]". On the one hand, the postal surveys that Hultgren and his team had created had a higher drop-off rate (approximately 35 per cent compared to 20-25 per cent of the visiting interviews). However, compared to the election results (a comparison made by asking what the individual respondent had voted in the previous election, then comparing it with the actual election result) showed that the deviations from this external control measure were very

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[3] This name is used by Hultgren himself in the introductory chapter of Boalt and Bergryd (1981: 7).

small. The success or failure of the postal survey in this case, is manifested by a comparison with an external referent.

One interesting method chapter is found in one of the first publications of the Public Opinion Analysis Foundation. In *Political Value Patterns and Parties in Sweden*, sociologists Gunnar Boalt and Ulla Bergryd use the data produced by the foundation to analyze different factors associated with political-party preferences. The introductory chapter is written by Hultgren himself, and with a reference to "Appendix A", it is possible to read a brief two-page method report. To get people to respond to the mail questionnaires

> [w]e tried to stimulate our respondents, telling them our intention to help social planning, the support we had got from political parties and trade organizations and the risk that the survey collapsed if they did not respond. We guaranteed their anonymity and promised some compensation for the trouble (Boalt and Bergryd 1981: 126, English in original.)[4].

This led to a response rate of approximately 65 per cent. This is comparable to the 68 per cent rate of the SOM survey of 1986. However, in the late 1970s, there were no other postal surveys to compare with, or at least, no other surveys were mobilized to support the Foundation's research. To understand whether the results are valid or not, they must be compared to an external referent, and one such instance is the distribution of districts, sexes and age cohorts:

> The weak points, with too few respondents were localized to middle aged men in district 3, Western Central Sweden and to older men as well as young women in district 4, Eastern Central Sweden. Three

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[4] Appendix A has no expressed author, but as it is announced in Hultgren's introductory chapter, it was most likely written by himself.

of the seven districts were distributed ideally already before the weighting. (Boalt and Bergryd 1981: 127, English in original.)

Moreover, *The Public Opinion Analysis Foundation* had made pilot studies comparing their survey results with readership analyses performed by newspapers to find yet another type of external check point, concluding that "Our survey technique had a reasonable validity if we use reading habits 1974-1979 as a criterion" (Boalt and Bergryd 1981: 126, English in original).

In 1984, the Foundation handed over their datasets to the repository for social scientific data (Svensk Samhällsvetenskaplig Dataservice (SSD)), to make it available to the general research community. As this was done, Lennart Brantgärde wrote a methodological evaluation in the SSD report series to assess the reliability of the Foundation's postal survey. In his evaluation, Brantgärde discusses the postal survey as "Normal is that 30-35 per cent of the respondents do not respond to postal surveys, while the corresponding share is 15-25 per cent in [visiting] interviews"[xv] (Svensk Samhällsvetenskaplig Datatjänst 1984: 3)[5] Although the response rate is lower than the visiting interviews, he is impressed with the very good correspondence with distributions in age and gender as well as how the data correlated with previous election results. He concludes metaphorically that the Foundation's data give "[...] almost the impression that some kind of plane [tool] has sliced smooth and evenly thick slices from north to south, from east to west[xvi]" (Ibid.: 5). Moreover, Brantgärde argues that the Foundation's results are important contributions because as far as he knows, the postal survey had never before been used for opinion research (Ibid.: 14).

The Foundation, however, discontinued its postal surveys in the mid-1980s. In his 1990 book, Hultgren writes that "*Statistics*

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[5] The year of publication is indexed as 1984 but must in reality be a few years later because the report also considers data from 1985.

Sweden [SCB] has signed an agreement with the Foundation to continue the surveys with the same design and procedure as in the past, and to hand over the results to SSD" (Hultgren 1990: 86)[xvii].

Alliance II: *Testologen* / Sverige-NU and Dagspresskollegiet

If the association with *The Public Opinion Analysis Foundation* is quite abstract, an even more interesting link to the pollster *Testologen* and its surveys called *Sweden Now* is revealed. *Testologen* was founded in 1970 as a private market research institute, and their main activity was not political subject matters, although during the 1970s, they had started to measure issues such as attitudes towards nuclear energy, taxation levels and computerization (see Holmberg 1980: 17ff). However, and more importantly, they measured newspaper readership, something that interested the Gothenburg researchers at the Section for Mass Communication studies in the Department of Political Science. Above all, they created *postal* surveys called "omnibussar". The earliest dataset created by *Testologen*, which is today available at the SND[6] repository, is from 1972, and the newspaper readership section is described as follows: "The 1972 survey contains a number of questions about reading-habits. The respondents had to indicate how often they read a number of papers within the daily (big city), weekly and monthly press." (SND 2011, English in original.).

By the time SOM was founded, the research project called *Dagspresskollegiet*, based at the Section for Mass Communication, had been in collaboration with *Testologen*'s Sweden Now survey since 1979 to study the role of newspaper readership and its relationship with other media types. In the report *Läsvanor*

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[6] As mentioned before, the SSD's contemporary name is SND. It has an online catalog of all their datasets, which are available freely for research purposes.

och läsintressen 1979-1983, Jan Strid and Lennart Weibull describe it as utilizing "a couple of pages, of a total 20 pages"[xviii] (Strid and Weibull 1984: 7) in the Sweden Now questionnaire. The rest of the questions were defined by *Testologen* or other entities collaborating with them. Strid and Weibull wrote the following:

> The latter [parts of the questionnaire] contain questions on several other phenomenon, from the uses of consumer products to political views. It is difficult to estimate whether this mixture of questions present in the "omnibus"-questionnaires have direct effects on the questions about news reading. (Strid and Weibull 1984: 7)[xix]

The *Dagspresskollegiet* research project was thus piggybacking on the *Testologen* postal surveys, and while this generated productive data, it was, however, considered insufficient both with respect to how many questions they were able to ask and, more importantly, with respect to the fact that the researchers were not in full control of how the survey ought to have been assembled. For example, even though the response rates were high, the researchers were left with another black box regarding the minute details of the analysis of drop-off rates:

> Another problem concerns the response rate in a postal survey of this kind. Experience says the response rate in a postal survey is approximately 70 per cent. This may affect the representativity in the study. It [representativity] may however be tested by means of the type of drop-off evaluations — made by for example *Testologen*. (Strid and Weibull 1984:8)[xx]

In Strid and Weibull's 1984 report, the methodological appendices refer to the material produced by *Testologen* instead

of, as is the case in the SOM reports, method chapters written exclusively by the researchers themselves.

Returning to the SOM report of the first 1986 survey, there is a direct overlap and continuation from the collaboration between *Dagspresskollegiet* and *Testologen* to the very heart of SOM 1986. Lennart Weibull writes a very interesting, and perhaps even rare piece of social science as he presents the findings on newspaper readership in the report (Weibull 1987: 8-19). The first results from the *Dagspresskollegiet* were from 1979, as they started to collaborate with *Testologen*. The questions in SOM 1986, about reading habits, trust in different types of media, and measurements on which newspaper was read, were constructed in a way that made them comparable with the earlier measurements. Not only does this add a certain historicity to the SOM survey, with its present results going back to times before the survey had even been created. More importantly, it makes it possible to perform a very fine calibration and comparison in methodology, which on the one hand guarantees that the actual SOM survey is working all right but, in a more general sense, also reinforces the of the postal survey as such.

Because the SOM 1986 and Sweden Now 1986 surveys ran in parallel, the results can be compared side by side. They are almost identical. To give a few examples, the SOM survey shows that 89 per cent of the population read at least one daily paper once a week, and the Sweden Now survey shows 90 per cent. The per centage of households subscribing to a daily newspaper is 78 per cent in both surveys, and the per centage of people reading the daily newspaper before 8'o-clock in the morning is 65 per cent according to the SOM survey and 63 per cent according to the Sweden Now survey. The results are very similar, and even though there are minor differences between the methodological setups, they are similar enough in design to compare.

If the collaboration between *Dagspresskollegiet* and *Testologen* had left certain parts of the practice of survey-making black-

boxed, the *SOM Institute* now has complete control over the data
and the questionnaires, while simultaneously being able to check
whether they did everything right in comparing with previous
surveys. This is a case where in which blackboxing tends toward
more openness. To debug a complicated device such as a survey,
you must open it first. However, to check if it works, you need to
find external referents. The best way is of course to have another
survey being made independently of your own survey. However,
for example, election results may also be used, which is also the
case in SOM 86.

The value of the *Testologen* data were still strong, even after
SOM 86. In 1988, Ingela Strid made a secondary analysis of the
datasets of the Sweden Now surveys to measure interest patterns
and life styles and how they correlated with consumption, leisure
activities and media usage. The time series of Sweden Now made
possible advanced factor- and regression analyses, aided by com-
puterized calculations. The problem with the *Testologen* data,
however, was according to Strid that "*Testologen's* sample of
interests is unfortunately not based on any kind of theoretical
assumption about how new interests spread in society. Their
surveys are commercial in nature, and the choice of which in-
terests [to measure] is in the first place decided by the buyer."
(Strid 1988: 18)[xxi]

Thus, while the *Testologen* results clearly reinforce the SOM
86 results, the problem of autonomously determining which ques-
tions to ask and in what fashion to do so, is a recurring theme.
Thus, on the one hand, the need for a dedicated survey for the
researchers was a firm motivation to construct SOM 86; on the
other hand, the reinforcement of the commercial data was needed
to verify that the same survey worked well enough.

There is however another important line of surveys, made
with another type of methodology but stretching much further
back in history, that the results of SOM 86 may be verified
against; a line that takes references back all the way to the 1950s.

Alliance III: The Election Surveys

Moving further along with the 1986 SOM report, there is a chapter written by Sören Holmberg. It begins with a brief history of Swedish surveys in election research, a history that according to Holmberg, begins with the Gallup Sweden surveys in 1944, 1946 and 1948. This type of research, led by Jörgen Westerståhl, moved to Gothenburg in the 1950s, where it established its "stronghold". There are also references to the years 1954 and 1956, when Westerståhl and Bo Särlvik began conducting regional- and nation-wide surveys (Holmberg 1986: 26), which I will further investigate below. Because the alliances with these surveys go far back in time, more than three decades, I will have to dwell on them for a while to find an entry point into a black box.

To begin with, these references are of a second order compared to the *Testologen* direct result comparisons mentioned above. They are there to give a historical background, to fold the future history of the *SOM Institute* into a much longer tradition of research, beginning already in the 1950s. In frequent publications, Holmberg and a many political scientists in Gothenburg give the same historical record of election and opinion research. The publications begin with the Gallup Sweden surveys in the 1940s and continue with Westerståhl and Särlvik's work in the 1950s (see Holmberg and Gilljam 1987: 13-16; Holmberg and Petersson 1980: 12; Holmberg, Gilljam and Oskarsson 1988: 3, 7-9). From these pioneering works stem the *the Election Surveys* (Valundersökningarna), which are recurring measurements of election behavior performed by the Gothenburg researchers.

Before tracing down this line of surveys, I shall briefly return to SOM 86 to describe the function of the Election Survey of 1985, a function which is much more direct. In the same fashion in which the *Testologen*/Sweden Now surveys were used as calibration devices concerning newspaper readership, the numbers of the Election Surveys are placed side by side with the SOM

results to ensure that they do not deviate. The results are compared in 11 tables and sometimes they "look remarkably alike in SOM 86 and Vu [Valundersökningen] 85[xxii]" and sometimes "[The perceptions of] economy were much more weakly associated with the party affiliations of the voters in SOM 86 than in Vu 85" (Holmberg 1987: 29, Figure 3.1)[xxiii]. This way, the results can be properly calibrated and evaluated by means of statistical regression analysis with previous experiments or, rather, with previous surveys. However, the Election Surveys differ in methodology. They are based on visiting interviews rather than postal surveys.

The next reference to turn to would be the methodological report of the Vu 85 survey, which predates SOM 86 by only one year. Holmberg's chapter above does not provide any details, but the surveys are documented extensively and published in technical reports by SCB (*Statistics Sweden*) and the Department of Political Science at the University of Gothenburg. The Election Survey was financed by the Swedish Parliament, and the field work was performed by SCB, who hired 200 interviewers for the 1985 survey (Holmberg, Gilljam, Oskarsson 1988: 3). The response rate was 77.8 per cent, a bit lower than the rates in 1982 and 1979 but higher than the rate in 1976 (Ibid.: 11-12).

Since 1956, large surveys financed by the Swedish Parliament had been conducted to measure voting behavior every fourth year; most of them were delegated to the Gothenburg researchers (Holmberg and Gilljam 1985). These surveys were, and still are, quite impressive in international comparison. In 1982, approximately 3,600 people were surveyed, most of them in quantitative person-to-person interviews performed by SCB (*Statistics Sweden*) by visiting people's homes. The entire breadth of fieldwork in 1982, for example, involved more than 200 people from *Statistics Sweden* (Holmberg and Gilljam 1985), and in 1976, there were 256 people, with each performing seven interviews (Petersson 1978).
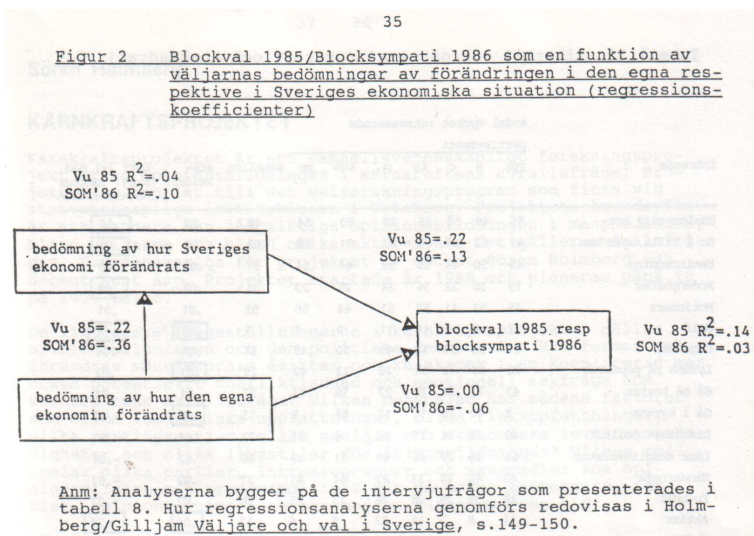
35

Figur 2   Blockval 1985/Blocksympati 1986 som en funktion av
            väljarnas bedömningar av förändringen i den egna res-
            pektive i Sveriges ekonomiska situation (regressions-
            koefficienter)

Vu 85 R^2=.04
SOM'86 R^2=.10

bedömning av hur Sveriges
ekonomi förändrats

Vu 85=.22
SOM'86=.13

blockval 1985 resp
blocksympati 1986

Vu 85 R^2=.14
SOM 86 R^2=.03

Vu 85=.22
SOM'86=.36

bedömning av hur den egna
ekonomin förändrats

Vu 85=.00
SOM'86=-.06

Anm: Analyserna bygger på de intervjufrågor som presenterades i
tabell 8. Hur regressionsanalyserna genomförs redovisas i Holm-
berg/Gilljam *Väljare och val i Sverige*, s.149–150.

Figure 3.1: Regression coefficient comparison between the SOM
86 and Vu 85 surveys. (Holmberg and Weibull 1987: 35).

Before the local surveys had been established in Gothen-
burg in the 1950s, as mentioned above, social scientists had
turned to the commercial pollster *Svenska Gallupinstitutet* for
data. The 1950 collection of essays called *Gallup och den sven-
ska väljarkåren*, which is based on pollster data, is sometimes
referred to as the first empirical study of election behavior (see
Holmberg and Gilljam 1987: 14). The social scientists were,
however, quite skeptical regarding the quality of the results of
Svenska Gallupinstitutet because, for example, they used quota
samples instead of randomized samples, which in turn made it
impossible to exactly calculate the statistical margin of error
(Westerståhl 1950: 44, footnote 2, see also Chapter 5). To fully
gain control over the technicalities of the actual decisions on how
to make samples and how to document the work, it seems like
the researchers needed to gain autonomy. However, to make your

own survey, you need resources.

The main methodological imperative was to perform *longitudinal* surveys; thus, the questionnaire had to be preserved in a somewhat original condition. Already in 1954, Jörgen Westerståhl and Bo Särlvik started constructing prototype surveys locally in Göteborg, and in 1956, they performed the first large study and received funds from the parliament in 1960. In 1982, the Election Surveys performed every three years[7] were claimed to "along with the US surveys, [to be] the most qualified and thorough material for studying voting behaviour in the world." (Holmberg and Nordlöf 1982: 5).

Turning to Holmberg and Särlviks's publications *Svensk valrörelse 1956* and *Svensk valrörelse 1954*, two reports which were not intended as publications in their own right but rather as working papers with their primary results presented in tables, it is possible to reconstruct how these first surveys were made. Historically, these studies were referred to by Holmberg and Gilljam in the mid-1980s as the pioneering attempts at proper election research, pre-dated only by the scientific analyses of the data from the Gallup Sweden pollster (Holmberg and Gilljam 1987: 1415). There are, however, important differences between Gallup Sweden and the works of Westerståhl and Särlvik, which deserve a closer look if we are to understand the differences in both methodology and organization.

A problem with the Gallup surveys, according to Westerståhl himself, was the quota samples that they were using rather than the preferred simple random samples that academic researchers needed to calculate the margin of error properly. Random samples, he argued, were particularly well suited for Sweden because of the centralized census, which enabled a very convenient

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[7] Also, during the national referendums, special surveys were constructed; for example in 1980, in connection to the referendum on nuclear energy, a survey with 4,000 respondents was performed (see Holmberg, Nordlöf and Gilljam 1985).

method of making simple random samples (Westerståhl 1950: 44-
45). This was also the method used in the 1956 and 1954 surveys,
in which random samples were processed by means of punched
cards by *Statistics Sweden* (Westerståhl and Särlvik 1957: 93;
1954: 6). The simple random sample, encountered as a black
box needing no explanation in Chapter 6 in which the SOM 99
report is analyzed, is at this moment still unstable. The quota
samples, as used by Gallup, or the area samples used widely
in the United States were capable alternatives. However, with
"true" random samples, it is possible to easily calculate the mar-
gin of error. In a footnote in Westerståhl (Westerståhl 1950: 94,
footnote 2), I find a reference to a newspaper article in one of the
leading dailies *Dagens Nyheter* from 1948. The article analyzes
the Gallup Sweden results with special attention to non-voters
and the number of individuals that had changed their votes from
party to party. Moreover, Sven O. Blomquist at Gallup is also
interviewed regarding the accuracy of the polls:

> Even though the used sampling methods do not al-
> low an exact calculation of the margin of error, it
> seems that the practical experience, especially from
> the Swedish Gallup polls, indicates that when com-
> parisons with known statistics have been possible, the
> margin of error has only been a few per cent and in
> most cases stay well within the allowed limits accord-
> ing to the sigma formula (2.5-3.5 per cent). (*Dagens
> Nyheter* 1948-12-21)[xxiv]

To find out what the "sigma formula" is, we need to turn to
Sten Hultgren's chapter *Om opinionsundersökningarnas metoder*
(Hultgren 1950: 40) where he defines how to calculate the "sta-
tistical random error"[xxv]:

$$3\sigma = \sqrt{\frac{p\,q}{n}}$$

Furthermore, Hultgren goes on to discuss, in length, the pros
and cons of area vs. quota samples. Even though he does not use
the term "margin of error" himself, Westerståhl is, as mentioned
above, definitive in remarking that this can only be calculated
with "true" random samples. This may perhaps be interpreted as
a minor detail. However, it is one of those remarks that keeps sur-
facing in the academic method chapters throughout the decades.
For example, in 1980, Holmberg and Petersson write "all leading
Swedish opinion [research] institutes nowadays use random sam-
ples" (Holmberg and Petersson 1980: 46)[xxvi], and as we see in
Chapter 6, the "simple random sample" is by then blackboxed
but nevertheless mentioned.

Thus, the earliest references found in SOM 86 are the local
Election Survey of 1954 and the national Election Survey of 1956,
which were performed by the Gothenburg scientists and led by
Jörgen Westerståhl and Bo Särlvik. At this time, the random
samples and the "right" way of calculating the margin of error is
manifested. Because the Gallup Sweden had utilized the quota
samples, in which the calculation of the margin of error is "less
accurate" than in the random samples, I have finally arrived
at an event in which a very basic component, the procedure of
sampling and its accuracy testing, is contested. The black box
is open to the extent that it is technically described, even in the
daily newspaper of *Dagens Nyheter*, in 1948. The choice between
quota samples and random samples is, on a pragmatic level, a
matter of cost. The quota samples are less expensive, and easier
to work with when directing a team of surveyors to interview a
given population. When the defined quotas are filled, the results
can be brought back and calculated and turned into statistics.
The random samples, on the other hand, define respondents with
true randomness, and then the problem of non-respondents ap-
pears. Thus, doing it right according to the standards defined
by the academic community of pioneering Swedish opinion re-
searchers comes with a price. Moreover, this distinction, as we

will see when following the researchers forward in history, becomes one such point where the boundary between science and non-science can be investigated as a leverage point of boundary work.

The local survey of 1954[8] was inspired by the works of Paul Lazarsfeld, and a reference is given to his work (with Berelson and Gaudet) *The People's Choice*, which according to Westerståhl and Särlvik prescribed that the panel surveys should "consist of approximately 1,200 interviews, and the respondents ought to live within one city, possibly a large city, and in one rural municipality" (Westerståhl and Särlvik 1954: 2)[xxvii]. The 1954 report is both a pilot study for the 1956 nationwide survey and a means of producing results on its own. Westerståhl and Särlvik had convinced four of the political parties as well as *Social Scientific Research Council* (Statens Samhällsvetenskapliga Forskningråd) and *Radiotjänst*[9] to contribute with funding.

The 1954 and 1956 surveys that Westerståhl and Särlvik had assembled constitute the oldest reference in the SOM 86 survey; these references are brought back to the present because not only are the references repeatedly described as the beginning of a research tradition, but the results of the line of Election Surveys are put side by side with new surveys for calibration. I must go all the way back to the 1940s to find a moment in time when the random sample is contested, and this I will argue is how one component used in the 1986 SOM survey has been stabilized to

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[8] The publication year of this report is not explicitly stated because it was meant only to be an internal report, but it is most likely 1955. In the reference list, the year 1954 is used. The reasons for not publishing it were that the results were preliminary, that the results could be used "irresponsibly" by the news media and that the results were to be used for a "scientific dissertation" (Westerståhl and Särlvik 1954: 10). Because this report is not in public libraries, I am grateful to Per Hedberg at the Department for Political Science in Gothenburg for lending it to me.

[9] Radiotjänst was the public service radio broadcaster in the 1950s. Today Radiotjänst AB is a private agency for collecting public service radio (and TV) fees.

the extent that it is seemingly uncontroversial. However, there are other aspects of the survey that are less stable.

Elections as external reference

There is one more detail in Holmberg's chapter in the SOM 86 report, which is of significant value as a direct reference: the 1985 election. One of the background questions in the questionnaire asks the respondent which party he or she voted for in the previous election (1985). This is then compared with the actual election result and functions as a measurement of the accuracy of the survey, as an external referent (even though one might discuss theoretically whether surveys and elections are isomorphic to each other on a macro-assemblage level). The constitutionally democratic election of 1985 functions as an accuracy test on a methodological level, just as we saw above in 1948 for Gallup Sweden. If pollsters often try to predict the outcome of an election prior to the actual voting, the academic survey uses the election results the other way around to determine whether the survey worked or not.

As I shall argue in Chapter 5, this is only one aspect of how elections can function both as to support epistemic claims and become a topic of controversy.

3.2   Debugging the postal survey — Towards self-referentiality

As the SOM 86 survey is made, it falls back on a multitude of backing resources, such as other postal surveys, sampling techniques, and election results, but also on surveys made by visiting interviews. The more I followed these surveys down their historical lines, the more details appeared, and the direct references took me all the way back to the mid-1950s. In a sense, 30 years of surveys were already there, present as calibration devices to

determine whether the survey worked or not. This is, however, only one aspect of blackboxing, whereby alliances are brought in to reinforce the present results with experiments (surveys) from the past. There is one component that I still have not opened up yet, a recurring theme not only in SOM 86 but also in several of the SOM reports to come: the works of Anders Ohlsson, who in the early 1980s pursued his licentiate by writing a thesis on response rates in postal surveys and who had previously published a few reports on survey methodology.

The random sample survey, the path chosen already in 1954 by Westerståhl and Särlvik, will by definition encounter drop-off rates, which are rates that are not allowed to become too high. Thus, the external referents of election results are not enough. What is needed is to understand in detail why some people respond and others do not.

Anders Ohlsson (1986: 11) argued that the main reason for choosing postal surveys is cost. The methodology of the previous Election Surveys, which utilized person-to-person interviews, was certainly far too expensive for the Gothenburg social scientific departments to bear by themselves. The visiting interviews required many personnel and had to be funded by external money, for example by the Swedish parliament (as in the case of the Election Surveys).

However, the postal survey was not uncontroversial at the time. Person-to-person interviews had been used for many years and had consequently become entrenched as a reliable method. With postal surveys, however, the experiences were not as extensive and had to be examined closely. Thus, a question that arose in the early 1980s came to be *how does the epistemic practice of the postal survey really work?*[10]

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[10] Similar events in the practice of scientific work have been noted by several STS-scholars. Maybe the clearest example is when Latour studies the computers designed to calculate the double helix structure of DNA. What is at stake before the fact is stabilized is (during a certain event of discovery)

The Ohlsson report, published in 1986, is called *Att svara eller inte svara - det är frågan* (To Respond or not to Respond — That is the Question), and it is pre-dated by two preparatory works published as reports at the Department of Political Science in Gothenburg (Ohlsson 1983a; 1983b). Ohlsson's works are meta-studies concerning the construction of surveys and people's motives for responding to them. As I will return to in Chapter 5, the postal survey was considered by some sociologists and pollsters (for example Hans Zetterberg) to be unsatisfactory for scientific research. Ohlsson's text is thus very important because it presents a theoretical model for how postal surveys can be made to work and what forces are at work when people choose to respond or not to respond to the questionnaire.

Ohlsson's work will have a great future; it will be quoted in the methodological reports in the SOM surveys for twenty years ahead as a gold standard for what counts as a sufficient response rate. Ohlsson writes: "As concluded before, approximately 70 per cent of the selected population will return the questionnaires of surveys that concern societal issues" (Ohlsson 1986: 40)[xxviii]. This standard is mentioned in various shapes in every methodological appendix[11] of the SOM surveys, from the first survey in 1986. The six first years contain the formulation "Surveys in Sweden usually have a response rate between 65 and 75 per cent". In the mid-1990s this standard was lowered, and from

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whether the machines really work or not. Even though my case is not as dramatic as the story of Watson and Crick, there is a similar problem as scientists waver between "once the machine works people will be convinced" and "the machine will work when all the relevant people are convinced" (see Latour 1987: 1-17). This is certainly also the case for the Gothenburg researchers because their survey needs to be convincingly scientific to be said to work and people must be convinced that it will work in order to be motivated enough to respond in the first place.

[11] For practical reasons, a full reference to all reports is not given. However, the report series is published under ISSN 0284-4788, and all books are listed at http://www.som.gu.se/bocker/publikationslista.htm, accessed 2012-02-02.

then on the formulation goes as follows: "Social scientific surveys have, in the best of cases, a response rate between 60 and 70 per cent, when performed in Sweden".

Sociologizing the decline in response rates

What is a reasonable response rate of a survey? In other words, what is the lowest threshold level for a survey to remain blackboxed? With the Election Surveys, the response rates had been very high, and the historical pattern is quite interesting. The following reflection was made in a 1979 technical report on the Election Survey:

> In the 1956 survey, the rate for respondents dropping off was merely 5.1 per cent. During the 1960s the refusal to respond became more common thus increasing the drop-off rate. The commercial research institutes [pollsters] have experienced a similar pattern – an increase in dropping off rates during the 1960s and the early 1970s. Since the mid-1970s, however, the situation seems to have stabilized. The number of people refusing to be interviewed by *Statistics Sweden* [who were employed to perform the survey] is no longer increasing. (Holmberg and Nordlöf 1982: 9)

Why are the response rates declining? Do people out there not believe in the authority of social research anymore? The answer to this question is neither yes or no, but needs to be resolved as a real problem for the social sciences, which must invest a large amount of energy in taking measures against declining response rates. Once again, the reference to the early surveys by Westerståhl and Särlvik are made; however, this time references are made neither to show the long history of a line of research nor to compare actual empirical results but to understand a problem of a second order: why people respond to a lesser extent.

When respondents refuse the questionnaire, it is unproductive to the social scientists because drop-offs may merely be failures in terms of response rates, distorted statistics, and skewed results if left unanalyzed. The entire survey needs to be patched and changed to keep producing facts. One way of achieving this goal is to add a new layer, a sort of meta-survey, which investigates the function of the survey under ever-changing survey conditions. This can be compared to Callon's concept of *negotiations* (Callon 1986), in which the practical problem of enrolling study objects are solved, with success or failure, by trying out new solutions and hypotheses. The problem is further illustrated by Holmberg and Petersson:

> A third important circumstance is that [the] Swedish population still accepts interviews as *Sifo* and *Statistics Sweden* are knocking on their doors. The rate of people refusing to be interviewed has to be sure increased during the 1970s, but still approximately 80 per cent of the population agree to being interviewed in their homes. The corresponding number in for example the United Kingdom or the United states are today 65-70 per cent. (Holmberg and Petersson 1980: 19)[xxix]

As Ohlsson writes his book on surveys to understand these problems, he simultaneously performs sociological research as he re-assembles the survey, which makes possible to create social scientific facts in the first place. On the one hand, it is a concrete and pragmatic study; Ohlsson discusses different actions that lead to higher response rates. For example, sending out a letter in advance, before the survey, will increase response rates. Drawing from previous experiences of survey research, mostly from an Anglo-Saxon context, other measures that will increase response rates are: using real stamps instead of printed prepaid systems on the letters, having questionnaires signed by an official sender,

sending many letters of reminder following the survey and giving
a reward to respondents once the survey has been completed
(Ohlsson 1986: 12-17).

Ohlsson describes the survey as "initiating a process of mass-
communication", where the "purpose is not primarily to send a
message, but instead to achieve a direct and formalized feedback
from the receiver [respondent]" (Ohlsson 1986: 19). What is
at stake is not the transmission of information but rather the
production of *programs of action*. Thus, the process of survey
studies, the fieldwork, is primarily a performative act. The added
instructions, as surveys are being *made*, involve the construction
of the questionnaire as a communicative act, and the components
of the assemblage can be broken down in programs of action
(Latour 1992c), which may in turn be added or subtracted.

For example, breaking down the 1986 survey, the survey con-
sists of one questionnaire (Figure 3.2), one post card, two letters
of reminder and one telephone interview. The result is a 68 per
cent response rate, where the respondents have been transformed
into objects of study. By adding and substituting letters of re-
minder, postcards and telephone calls, the programs of action
increase in strength (see Figure 3.2), and the objects of study
are more strongly established and defined. As with Pasteur's
laboratory described by Latour (1983), the urgent task of social
scientists is to concentrate the social on a micro-scale[12] (popu-
lation 2,500) to raise the lever high enough to literally speak of
the statistical macro-scale of Swedish society.

What Ohlsson and the SOM researchers do is develop a the-
ory of communication to understand how to assemble the re-
search instrument more efficiently; a theory based on a Parso-
nian conception of human agency. Because it is necessary to
understand the *effects* of the totality of components, Ohlsson

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[12] The micro-scale here relates to the notion that the population of 2,500
must be able to speak for the entire Swedish population (about 9 million).
Thus, it should not be confused with micro-sociology.

Year	Freq						
1988	68%	S	P	LS	LS	TR	
1987	70%	S	P	LS	LS	LS	TR
1986	68%	S	P	LS	LS	TI	

Figure 3.2: Programs of action. *Explanation*: S - Survey, P - Postcard, AP - Announcement postcard, LS - Letter and survey, TI - Telephonic interview, TR - Telephonic reminder, PC - Postcard, MS - Mini Survey.

explains these in terms of "motivations" (following Parson's distinction between expressive and instrumental actions) related to the concrete situation:

> If the sender is an official authority, or a similar organization, this should probably increase the instrumental motivation for responding [to the survey]. This is partly because the respondent might think that authorities should be collecting the type of data sought for in the survey, but also because the respondent might fear that inconveniences may follow from showing disobedience to the authorities by not responding [...] Repeated reminders from the sender should supposedly work similarly. Some respondents who, indeed decided not to respond when the first letter containing the survey arrived, will probably perceive the consistently repeated reminders as rather discomforting. (Ohlsson 1986: 23)[xxx]

Abstract sociology, in this context, is not at work to speak about general society; instead, it is there to understand how surveys work. Ohlsson is reconstructing the fieldwork situation by making assumptions about the logic of society, folding this type of sociological theorizing back on the survey itself. In this manner, he is facing the object of study with the gaze of a sociologist;
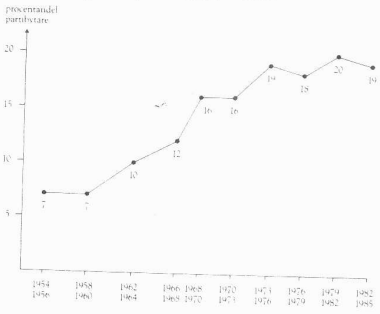
Figure 3.3: The 1988 SOM survey, Published in Björkqvist, Karin (1989: 42, without permission.)

we have also arrived at the level where theory and practice converge because the survey and the questionnaire are recognized as a social event in which respondents are forced (in a positive sense) to respond to become objects of study.

The machinery, according to Ohlsson, operates by both corporeal and incorporeal means. The discomfort of repeated reminders is an example of the former, and trusting an "official authority" to a degree at which consent is achieved is an example of the latter. If discomfort is one side of the coin, the other is far more abstract, containing components of duty, motivation, and trust in authorities. Thus the SOM assemblage invents repeated series of actions at the same time it transforms a statistical sample of the population into its objects of study. The questionnaire is intervening in the everyday lives of the sample population by commanding them to respond, but some people resist by not responding.

Thus, the process of creating and conducting surveys is a practical problem, which is explained using sociological theories of motivation. The possible causes of what makes people respond or not to respond must be properly disentangled; only then can the social scientists provide reliable data to later provide scientific facts. The process is, however, recursive, in the sense that the reliability of the social sciences itself is a key element in the willingness to respond. If surveys fail, or produce insecure data, the credibility of the social sciences would be on the line.

The 1986 SOM survey marks the beginning of what today is a twenty-five-year-long story of social scientific research in Gothenburg connected to a much longer history of the social sciences[13] in Sweden.

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[13] For a summary of Swedish sociology from 1830-1955, see Wisselgren (2000) and Larsson (2001).

3.3 Conclusion – An assemblage of alliances

The social sciences are one of several actors rendering society visible. In the case of the *SOM Institute* the institute provides a statistical, average, and quantitative image of the social world while simultaneously creating certain domains of imperceptibility, which are particularly manifested by response-rate drop-outs. Some people simply refuse or forget to respond, and these respondents are rendered imperceptible by the epistemic practice itself, especially when the methodology of simple random samples had already been chosen by academic researchers in the 1950s. This problem is of course not exclusive to the random samples; neither is it solely present in academic research. It is, however, intensified, problematized and progressively solved throughout the surveys that lead up to the first SOM survey in 1986.

In this chapter I have attempted to answer the question regarding how the SOM 86 survey worked by examining its components and their historicity. Thus far, I have mostly looked back in time to see how previous surveys (and experiments in a wide interpretation) have been mobilized to reinforce the SOM survey of 1986. In Chapter 6, I will also discuss how the fate of SOM 86 is in "later users' hands" (Latour 1987 :29).

SOM 86 worked because it already embodied several other surveys that functioned as calibration devices for the current results. Not only other postal surveys but also visiting interviews, provided a litmus test to determine whether the questionnaires, the field work, and the overall setup were in tune with contemporary and historical surveys. Moreover, the works of Ohlsson drew on previous experiences of postal survey methodology to provide explanations for respondents' motivations in sending back the questionnaire to the researchers, explanations that may be labeled a sociology of surveys.

In 1986, the SOM survey was already an assemblage that

mobilized a multitude of alliances. It worked in a state of what Kuhn would call normal science, where theories and methods were quite uncontroversial, with the exception of the postal survey, which I will return to in Chapter 5. At a reasonable cost, the Gothenburg social scientists were able to construct their own survey, in which each step of the survey practice could be controlled by the researchers themselves, rather than having to piggyback on commercial pollster data.

In the next chapter, I will open up another level of negotiations, where I look at how the state played a role in making this tedious practice of response rates and survey-making worthwhile.

Chapter 4

1954 — Assembling Quantification

In this chapter, I will draw attention to the first large-scale academic surveys in Sweden to determine how they were assembled and what negotiations were involved first to facilitate them. A specific type of American sociology served as a model for the kind of studies on election behavior that were successfully performed in the mid-1950s. However, there had been no prior studies of this kind in Sweden, at least not in university-based research. Consequently, the introduction of costly surveys was promoted in an era of an expanding welfare state, and the new social sciences expressed as a cornerstone not only in social planning but also as a presupposition for a democratic society.

4.1   Introduction

To quantify society, surveys have to be assembled and made to work. In Chapter 3 I discussed how the important phenomenon of response rates could be reinforced by previous surveys, how alliances could be assembled successfully and how this assemblage was explained by sociological theories. This blackboxing

of surveys, however, is only one level of the assemblage process that turns the social sciences into producers of scientific facts.

To create a social scientific laboratory that dispatches questionnaires, records facts, and returns findings for analysis and calculation, more than high response rates are needed. Additionally, you need to bring forth other resources that are not only of a financial nature but are also able to produce legitimacy and social connections.

In the previous chapter the conclusion was that the mutual reinforcement between the SOM surveys and previous experiments promoted the credibility and accuracy of the postal survey. To go beyond this idea, in this chapter, I will examine the relationship between quantification and the state in Sweden from approximately the mid-1950s when the first academic surveys on election research were made in Gothenburg. The "state" should not be understood as either a monolithic entity or an abstraction. Rather, my commitment to actualism will be much more concrete; I will draw connective lines between the actual surveys and government white papers that dealt with the expansion of the social sciences in the post war era.

To find an entry point in sync with my previous theoretical assumptions, I will depart in the 1954 local survey on Swedish voters that was led by Jörgen Westerståhl and Bo Särlvik. As this survey was a pioneer in what today is a self-defined tradition of research, certain procedures were unstable and easy to follow. Additionally, this was a moment in history when there had not been any previous academic surveys of the kind. Only the pollster surveys by Gallup Sweden had previously quantified Swedish society in a similar fashion. The report opens up as such:

> In March 1954 a request for funding was made by the Department of Political Science at the University of Gothenburg, for a survey of the elections season during the fall of that same year. The request was sent

to the Liberal party, the Conservatives, the Agrarian Party and the *Social Democrats*, as well as to the Social Scientific Research Council and Radiotjänst. (Westerståhl and Särlvik 1954: 1)[xxxi]

The political parties, the Social Scientific Research Council and Radiotjänst all agreed to contribute, and in total, Westerståhl and Särlvik collected 20,000 crowns[1], 14,950 of which were paid for the survey field work that subsequently was delegated to *Statistics Sweden*. Moreover, much of the work analyzing the results was described as "unpaid" labor taking place at the local seminars. Because it is quite uncommon for a methodological report to include such detailed financial accounts, we have arrived at a black box, which allows us to examine other components further.

The 1954 report was not intended for publication, but Bo Särlvik wrote an article in the journal *Tiden*, published by the *Social Democratic* Party, in which he explained how this type of election research was to be considered important. He argued that this was the first time that the "Lazarzfeld technique", a survey making model originally presented in the 1944 book *The People's Choice* (Lazarsfeld et al. 1968), had successfully been used at the Department of Political Science in Gothenburg (Särlvik 1955).

Westerståhl and Särlvik's two studies of 1954 and 1956 had no epistemic alliances, and until this point, no experiments other than the Gallup surveys, had previously been performed in Sweden. Compared to the multitude of surveys available in the 1980s, these works could be seen as pioneering efforts. In terms of response rates, the study of 1956 produced impressively achieved 94.2 and 93.9 per-cent responses for the two rounds of interviews

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[1] This figure corresponds to approximately 271,000 crowns in the year of 2011 according to the inflation price calculator of *Statistics Sweden* (http://www.scb.se/Pages/PricesCrib.aspx?id=258649, accessed 2011-04-15).

made in a sample of 1,146 respondents, numbers that are considerably high for any type of social scientific surveys. In comparison, the model study *The People's Choice* had concluded that the amount of "missing cases" (14 per-cent) consisted of "[...] a figure which proved to be remarkably low in the experience of subsequent investigators" (Lazarsfeld et al. 1968: 159, footnote 1).

More than half a century later, there are striking similarities between the 1950s studies and the contemporary election studies performed by the Gothenburg researchers. For example, while reading the 2011 report *Åttapartivalet 2010* (The Eight Party Election 2010), the reference to *The People's Choice* is still there, as the time series go all the way back to 1956, and the main methodology is still visiting interviews performed by *Statistics Sweden*. The response rate has, however, declined to 69 per cent. The accuracy, measured in comparison with the official election results, is nevertheless higher than usual (Oscarsson and Holmberg 2011: 99-102).

In Chapter 3, I concluded that the Election Surveys played an important role in calibrating the SOM survey of 1986 and in reinforcing the results of the postal survey. In 1986, however, the Election Surveys were already blackboxed. Moreover, the Lazarsfeld model of the Election Surveys, as mentioned above, seems to have survived throughout half a century. I will argue that this black box of quantification has deeper ramifications for how we consider the history of the social sciences. In this chapter, I will therefore open this black box to study how the *survey work was made to work* in an era when the social sciences expanded drastically in Sweden. Blackboxing and time, as I discussed in Chapter 2, will have implications for how the history of the social sciences is approached. To clarify this further, I will first inspect what I will call the "Kuhnian history" of Swedish sociology.

The Kuhnian history of Swedish sociology

A common understanding of how Swedish sociology has unfolded during the 20th century is that there is at least one large shift or, one historical rupture between a post-war phase of "social engineering," positivism and American structural functionalism that prevailed in the 1950s and 1960s. Subsequently, toward the end of the 1960s, new European theoretical paradigms, often with a Marxist flavor and a class conflict outlook, started to establish themselves, marking the end of the hegemony of quantitative social science. Alternatively, a third phase can be added within this line of thinking. For example, the case-study approaches of Gunnar Myrdal, who already gained recognition in the United States in the 1930s for his work on racial discrimination in *An American Dilemma* (see below), could be added as another phase.

Irrespective of whether two or three phases are put forth as the paradigm shifts of the 20th century, the fundamental pattern is the same; sociology, often thought as one of the root social sciences, is a scientific discipline that works within a society and is affected by social problems identified by other institutions. Clearly, as societies change, sociology changes. These changes, when drastic enough, lead to anomalies and crises, which, in turn, overthrow the dominant normal science. This line of argument crystallized here to the point of simplification, is consistent with the works of the sociologists of science and Kuhnian thinking, as mentioned already in Chapter 1.

Before proceeding, I must confess that during the course of my work, I have myself been caught up with this type of historical thinking, which in many ways is intuitive from a Kuhnian perspective (see, for example, Kullenberg 2011). However, as I discussed in the section *Blackboxing and Time* in Chapter 2, this approach departs considerably from ready-made science, which may be deceptive if we want to understand how science is made durable over time.

Katrín Fridjónsdóttir divides Swedish sociology into three

overlapping phases that she calls "establishment," "consolidation" and "re-orientation." There are similarities between this account and that of Eyerman and Jamison in their report *On the Transatlantic Migration of Knowledge* (1992b).

According to this perspective, sociology dates back to 1903, when the first chair in economics and sociology was established in Gothenburg. However, it took until 1947 for the subject to become an independent academic discipline, even though sociological subject matters had earlier been discussed in moral philosophy (Fridjónsdóttir 1987). From the beginning, Swedish sociology was heavily influenced by American sociology and had a strong empirical focus (Fridjónsdóttir 1991), steering the subject toward quantitative methods and a logical positivist philosophy of science. During the 1960s (Eyerman and Jamison 1992a) the central components of contemporary quantitative sociology were imported: the survey method, scales and tests for measuring attitudes and the overall theory of structural functionalism.

In the early 1940s, there were no systematic, detailed statistics of the Swedish society aside from census data and sporadic studies such as the Swedish Gallup Institute polls, as described in Chapter 3.[2] As the social sciences expanded, the sociologists imported a specific type of American quantitiative sociology "at a particular historical conjuncture, when the case study had all but disappeared from the leading departments of sociology!" (Ibid.: 16).

Moreover, Fridjónsdóttir argues that between 1947 and the early 1970s, there was a strong tendency toward social engineer-

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[2] However, during this time Gunnar Myrdal performed what probably would count as the most famous Swedish work in social science; a study called *An American Dilemma: The Negro Problem and Modern Democracy* (1944). But this study would not influence the rise of the social sciences in Sweden to any significant extent. However, the importance of Myrdal's work in the US should not be underestimated. Rather, his position as an "outside European" was seen as fresh new way of handling the politically sensitive topic of racial discrimination (see Eyerman and Jamison 1992b: 6-11).

ing even in the self-image of sociologists, with the results of the surveys being directly addressed to the authorities (Fridjónsdóttir 1987). Since it was statistical, quantitative and empirical, American sociology seemed to fit adequately into what the government agencies desired. It was *scientific*. Becoming a social scientist in the 1940s meant becoming a proper scientist, and therefore, the "speculative" and "proto-scientific" social theories of France and Germany were excluded.

Later on, in the re-orientation phase, new approaches entered the disciplinary field of sociology. The old school, with its theoretical content, its close relations to the state and its heavy reliance on quantification, withdrew and never seemed to regain its status as a programmatic and unified sociological approach.

Eyerman and Jamison follow this line of thinking, they conclude:

> [...] and the "special relationship" between Sweden and America that was so characteristic of the immediate postwar period seems increasingly to take on the character of a momentous, but temporary, historical parenthesis (Eyerman and Jamison 1992b: 35).

In other words, the special American knowledge transfer that took place in the 1950s was initially strong but was later abandoned. Fridjónsdóttir elaborates with a distinctly Kuhnian approach on how this process happened:

> [...] [I have previously mentioned that] facts "kicked back" and these "kicks" could have given rise to certain "anomalies" in Swedish sociological research [...] Of course did the "change of spectacles" aid the transformations in the subject [of sociology] at the time, by allowing dimensions to appear that more or less had been invisible before. (Fridjónsdóttir 1987: 278-279)[xxxii]

That facts were "kicking back" in the shape of anomalies in a coming paradigm shift, which was a line of thought also held by sociologist Ulf Himmelstrand (who will appear again in Chapter 5). According to Himmelstrand, there were real social changes in the mid-1960s that the current sociological tradition was unable to explain or provide terminology for. Consequently, this "crisis in sociology led to the rise in popularity for Marxist theories and a re-orientation in many of the social sciences" (Himmelstrand 1987: 150). Edmund Dahlström even speaks of a dialectic between scientific paradigms: first positivism, secondly Marxism and thirdly a contemporary (1990) synthesis of redemption and epistemic heterogeneity (Dahlström 1990: 51-52). These Kuhnian approaches (see also Friedrichs 1970; Burrel and Morgan 1979; Brante 1985) have, however, focused more on the content of sociological theory than the social and historical framework that has made possible the emergence of such a knowledge of society in the first place.

My project is not meant to intervene in this disciplinary history of sociology, at least not directly. Rather, I will use it to make three points that are important. First, when looking at the *content* of sociological theories as they unfold, change and disappear from a scientific discipline, the temptation of looking at ready-made science is pervasive. Second, when you describe paradigm shifts in a way that includes naming what has been forgotten, as Eyerman and Jamison do above, you also risk contributing to the deceptive re-writing of history that the paradigm shift itself entails, which is something which Kuhn himself warned for. Third, to follow scientific disciplines as such (in this case, sociology) may prevent you once again from seeing science in the making, which would be surveys in my case.

However, my commitment to actualism (see Chapter 2), has urged me to re-think the extent of a sudden rupture. I cannot study ready-made science this way, especially not from a "disciplinary" perspective. All there is, to me, are assemblages that

need to be followed.

4.2   Creating quantifying interfaces

To understand how a fertile ground for making surveys can be established, I will draw attention to certain aspects of quantification and statistics that are necessary to provide an infrastructure for facilitating them. Gallup Sweden had already built these productive structures. However, the conditions for pollster surveys are in certain respects different from those of academic surveys.

To understand better the problematic situation in which the first Gothenburg surveys were made, the following quote by Westerståhl (made two years before the 1954 study) concludes:

> The question concerning the role of election propaganda has up until now only been studied to a minor extent. However, a few studies have been conducted in the United States and some data has been retrieved from Gallup Sweden's surveys. (Westerståhl 1952: 13)[xxxiii]

As Westerståhl wrote those lines in the women's social movement journal *Hertha*, he had already made efforts to remedy the lack of election propaganda surveys.

In Olof Petersson's recent biographical work *Statsvetaren - Jörgen Westerståhl och demokratins århundrade* (2011), an entire chapter is dedicated to the early election studies of Westerståhl and Särlvik. According to Petersson, Westerståhl attended and contributed widely to the seminars of Gallup Sweden in the 1940s. In Gothenburg, he retained good connections with the ruling *Social Democratic* party. On December 12, 1954, prime minister Tage Erlander wrote in his diary: "Westerståhl [is] now here, asking for my support for [conducting] an investigation on what methods of agitation that give results in

the electoral campaign" (Petersson 2011: 178).[xxxiv] Moreover, Särlvik was recruited by Erlander in 1953 together with Olof Palme (prime minister 1969–1976 and 1982–1986) and the two of them worked as advisors in the electoral campaigns for the *Social Democrats* (Ibid.: 185).

A few years earlier, Westerståhl was the secretary of a government white paper[3] from 1946 that discusses the future role of the social sciences in modern society. In this paper, the trajectory and role of the social sciences was defined:

> It should eventually be stressed, that there is a significant correlation between a democratic form of government and the social sciences. It is not a coincidence that this science has flourished in two of the oldest democratic and constitutional nations, England and the United states. (SOU 1946:74: 20)[4] [xxxv]

In the 1946 white paper, statistics was also described as an important social science in its own right, one in need of development and expected to play a crucial role in such diverse areas as economics, political science, sociology, and social policy research. Moreover, this new science promised to promote the shaping of the democratic social order. However, one problem was that the new empirical social sciences were more costly than what was called "theoretical and limited historical studies" of

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[3] In Sweden, white papers are called "Statens offentliga utredningar" (SOU), which, translated into English, means "Government public investigation."

[4] SOU 1946 No. 74 is entitled "Concerning the state of the Social Sciences at Universities and Colleges." This white paper was delegated to several professors in the social sciences, among them Torgny T. Segerstedt, who became the first professor of sociology in Uppsala. Strictly speaking, the first professorship of sociology had already been established in 1901. However, it took until the 1950s for department-based research to be conducted properly in Gothenburg.

previous times, and there was a need for increased "material and manpower resources" (SOU 1946:74: 20).[xxxvi]

The specific importance of statistics is highlighted even further in a white paper written two years earlier:

> The methodology of statistical science is fundamental to all social scientific work, and naturally indispensable to the question of our population patterns. [...] The development of these methods [of statistics] is of direct value to the activity of the state. (SOU 1944:19: 5)

When investigating the possible uses of the social sciences, the field of statistics binds them together under a unified methodology. Moreover, statistics are indispensable when empirical questions regarding issues such as population patterns are to be identified and solved. By refining these methods (and what is at stake here is, of course, the future funding of statistical disciplines at the Swedish universities), a direct value for the state is anticipated in return, and this direct value is to be centralized because of its importance. Returning to the 1946 report:

> Finally, the distribution of funds, which are to be made available for social scientific research by the state, will be centralized because of the significance of this type of research. (SOU 1944:19: 6)

The problems that the social sciences purported to solve are put in a historical context. The state is described as having "expanded into new territories," where it was not found in the 19th century. This expansion leads to a need for social scientific research, on which the state is dependent" (SOU 1946:19: 8). Moreover, the gradual urbanization of society is described as opening up new fields of research. Further, anti-social behavior and social groups have been under-researched. Finally, the social

sciences also aided the citizen edification process and thus served as necessary building blocks of a democratic society (Ibid.: 5-9).

This era is a moment in history when the social sciences in Sweden are defined as more than just fact-generating disciplines. Instead, the 1944 and 1946 white papers define the social sciences as the scientific knowledge about society as a whole in a paradoxical yet interesting way. The intertwined formula of the social sciences both facilitating and being facilitated by a democratic society juxtaposed with the need for social science in social planning. This former formula is comparable to the contemporary statements of Robert Merton in the 1940s (see Chapter 2) in which he described the interdependency between a liberal society and an autonomous and free science. However, this idea stands in contrast to the concept of science as a "handmaiden" of the state.

During the 1950s, the statistical approach to "engineering society" reached its peak, and a government white paper from 1959 argued that more funds and resources should be allocated to the field of statistics:

> In a modern western society, [...] rational interventions must be based on knowledge of human behavior, individual as well as in groups. The tendency in modern society — both within individual companies and public institutions is increasingly toward planning, which must be adapted to the behavior of the individual as smoothly as possible. The modern forms of statistics are more and more becoming the means to solve this double task. (SOU 1959:33: 13)[xxxvii]

The 1959 white paper suggested that the production of statistics should be centralized to *Statistics Sweden*, and this recommendation was implemented during 1963/64. *Statistics Sweden* had been contracted for Westerståhl and Särlvik's field work in the early Election Surveys, and even though *Statistics Sweden*

charged "business-like" costs, the establishment of a central unit for field work (called Statistiska centralbyråns utredningsinstitut at the time) ensured that there was a permanent infrastructure for survey interviews, which did not depend on private pollsters (SOU 1959:33: 56-57).

To understand how this interface worked in post-war Sweden we need to look beyond the early Gothenburg surveys and analyze what other translations they made affordable.

The Uppsala School of Sociology

The first professor of sociology, Torgny T. Segerstedt, was a key figure in shaping the Uppsala School of Sociology. In a programmatic text that appeared in the first volume of *Acta Sociologica* in 1956, Segerstedt describes the sociological program as being founded within a descriptive and relativist theory of values in combination with a deductive positivist philosophy inspired by C. G. Hempel. Further, Segerstedt describes the program as a theory of society stressing the importance of a norm system, which produces homogeneity, as well as deviant or "defective" behavior causing "social problems" in a society (Segerstedt 1956). Compared to the Gothenburg surveys of the same year, the Uppsala sociologists were much more theoretical and had an international outlook with their writings.

A central problem for the Uppsala School in general, and Segerstedt in particular, was the relationship between *norm - norm speaker - sanction - habit*. This terminology described society on a level of norm systems, and it identified subjectivity as one or several people, or even institutions, speaking these norms. Moreover, sanctions functioned to induce rewards or punishments when a norm was observed or violated. Finally, habits described three forms of repeated behaviors: action, emotions, and verbal elements.

The Uppsala School only elaborated models on the level of the *group*: "By group we mean two or more people in interaction

observing social norms which can be traced to one and the same norm speaker" (Ibid.: 89). Empirically, groups could be study circles, regiments, corporations, or institutions. These groups were further divided in strata, where the status of a person varied with distance to the norm speaker. A perceived social problem in the early days of Swedish sociology was *deviance*.

In the days of urbanization and heavy industrial expansion, it became important to find the causes of deviant behavior to integrate groups that did not conform to the social project. For example, the Romani minority was perceived as being a deviant group, and the sociologists of the Uppsala School tried to find the causes for their behavior regarding if it was a "defect in the norm system".

The *content* of sociological theory in the Uppsala School was close to structural functionalism, which met with the desired scientific characteristics, being both statistical and empiricist. This choice turned Sweden into a landscape of numbers, thus providing a map for planning. Additionally, American social theory seemed to fit well with the rational moral philosophers who were to become sociologists. Influenced by Swedish philosopher Axel Hägerström, the Uppsala School was founded upon the "true nature of the state and society" with a descriptive and strongly anti-normative theory of values. In spite of statistics still being a young science in Sweden (in the sense that there were notably few professors teaching and researching it), it seemed to be the linking device between social theory, philosophy of science, and the possibility of providing the state-apparatus with hard and reliable data.

Moreover, the Uppsala School were occupied with the problem of how to define a common conceptual framework, not only as an ideal but also for pragmatic reasons. If surveys as collective productions were to be meaningful, the sociologists argued that a specific and uniform vocabulary had to be deployed. This was also suggested by the contemporary philosophers of science,

for example Carl Gustav Hempel, who was heavily quoted in the Uppsala writings:

> We shall refer to these two presuppositions as the conditions of determinacy and of (personal and interpersonal) uniformity of usage. Clearly, neither of them is fully satisfied by any natural language. (Hempel 1952: 10)

In logical positivism there is an interesting paradox; the uniformity of usage in concept formation needs to be agreed upon – an activity which by necessity is a social one. It requires that a group of researchers use concepts in a repetitive fashion. This paradox is turned into an irresolvable opposition by Ludwik Fleck, who contrasts the neo-kantian positivism of Schlick, Carnap and the Vienna Circle to the early founders of the sociology of science, Durkheim, Lévy-Bruhl, and Jerusalem. According to Fleck, the study of thought collectives must push the social construction of scientific facts further to include not only the ordinary sociological study object, but also the collective representations that scientists themselves use in concept formation (Fleck 1997: 48-58). Thoughts and concepts travel from individual to individual and are progressively displaced because succeeding individuals constantly add associations. Thus, according to Fleck, concepts have no foundational meaning, but only work within a collectivity.

This problem is actualized within the community of Uppsala sociologists. Segerstedt wrote in 1956:

> In order to facilitate the formulation of hypotheses, for instance on groups or societies, there must be some agreement on the usage of certain words. Therefore, it is necessary to introduce one's linguistic usage, before one proceeds to formulate the hypotheses. For this decision on terminology we use the term

> theoretical definition. It proves rather difficult to present a list on such generally accepted theoretical definitions i sociology. In that respect it can be said that sociology is a young science, since the sociologists show a tendency each by himself to create a system of individual theoretical definitions. (Segerstedt 1956: 87)

The problem of a heterogeneous conceptual apparatus in sociology is expressed widely among the early sociologists. For example, Zetterberg argued is his *On Theory and Verification in Sociology* that sociologists must reach a consensus for an empirical science to be able to advance, because:

> [...] [at] present [time] there are so a many different competing definitions for a key sociological notions such as "status" and "social role" that these terms are no more valuable than their counterparts, "position" and "social relation" in everyday speech. (Zetterberg 1965: 30)

According to what I previously called the Kuhnian history of Swedish sociology, in the aftermath of 1968 Marxism impacted the homogeneous era of structural functionalism, and the role of sociology as social engineering was, at least intellectually, challenged. Gradually the programmatic set of theories and methods of the Uppsala School were replaced, or challenged in a heterogeneous milieu of disciplinary thinking.

Fridjónsdóttir's research, which allows Swedish sociologists to reflect back on their own history, provides explanations as to why the Uppsala School failed to preserve their programmatic research. Johan Asplund argued that the group around Segerstedt had become too dependent on direct contract research from the state, becoming an "[...] appendix to the even pace of society" (Asplund 1987: 136)[xxxviii] rather than pursuing sociological

research.   Other sociologists, such as Bengt Abrahamsson, ex-
plained the decline in programmatic sociology to be associated
with the increased pluralism in the late 1960s, when the empiri-
cist and objectivist approach was criticized by a rapid growth in
students (Abrahamsson 1987: 177-183).

   From my perspective, however, an important detail is easily
overlooked if a tradition or paradigm is declared deprecated. In
1955, contemporary with the surveys of Westerståhl and Särlvik,
the pollster *Sifo* was founded. As I will argue in Chapter 5 *Sifo*
can not be understood merely as dwelling in a "non-academic
Swedish context." (Eyerman and Jamison 1992b: 17) For the
quantification of society, in my perspective, what matters are
not primarily schools or paradigms but rather the process of
assembling surveys.

4.3   Assembling a local research center

The proposed expansion of the social sciences, as outlined in the
1947 investigation, was implemented over the following years,
and in retrospect, Westerståhl concludes that this was "an as-
tonishing year in the history of the [Swedish] universities" (West-
erståhl 1990: 65).[xxxix] Instead of researchers working individu-
ally, the new type of department structure was turned into a *col-
lective* enterprise, as the surveys required co-operation between
researchers.   The survey also involved rigorous methodological
training and the development of the survey technique measuring
social phenomena. A key element in the production of sociolog-
ical facts involved certain standardized practices of constructing
surveys, which enabled quantification and calculation.

   Already from the beginning of the Gothenburg tradition of
surveys, it was customary to work in research groups, rather than
individually. Westerståhl described this approach as "[...] an ab-
solute necessity, to work many of us together, in especially des-
ignated premises" (Ibid.). The notion of organizing research de-

partments in special buildings was commissioned already in the 1940s and was modeled after Anglo-Saxon departmental structures, which had already been proved successful in social research. The commission of 1946 even suggests constructing a new building to house the material presuppositions of social science, including libraries and apparatuses of counting. The 1950s marked a time of expansion in the social sciences, and the commission, which Westerståhl himself participated in, put considerable effort into investigating what the future of social research would look like by comparing with "their [the social sciences] position in foreign countries" (SOU 1946:74: 13).

The Gothenburg research group, similar to the Uppsala School, successfully developed relations with the state apparatus and a political thrust towards a planned society. The post-war sociologists managed to translate the general interests in a rationalization of social institutions and governance, thus receiving both funds and legitimacy through the interface configuration that was stabilized during the 1950s.

The theories and methodological approaches in both Gothenburg and Uppsala displaced sociological articulations from a US-derived context and realized them in a local practice. Theories, methods, concepts, and attitudes regarding scientific research were imported from one of several traditions of American sociology. As they materialized in a Swedish setting, their influence on social planning became perhaps even greater than in the US because of the strong interface between science and the state. Unlike in the US, the social sciences in Sweden were included as "big science," notably through the political position of prime minister Tage Erlander (see Eyerman and Jamison 1992b: 20). In retrospect, however, Westerståhl describes the primary task of the social sciences as "[...] presenting facts and contexts in the political analyses and not in suggesting measures" (Westerståhl 1990: 68-69).[xl]

In the 1960s through the 1980s, the Gothenburg research

in political science was dominated by large projects evaluating municipality reforms and voting behavior, most of which were funded directly by government subsidies (Westerståhl 1990: 65). Additionally, surveys on the correlation between newspaper readership and voting behavior were initiated, and mass media research was established in the late 1970s with a broader perspective on the uses and the credibility for radio and television (Holmberg and Weibull 1987: 1), as well as attitude research among the members of the parliament, which was unique in global comparison.

Blackboxed components for the future

The surveys of the *SOM Institute* have large statistical samples, and certain questions in their questionnaires have persisted regarding previous research dating back to the 1950s. Hence, the Institute's time-series are often unique in international comparison.

There are more features beyond scales that are being imported in to the *SOM Institute*. The statistical thinking, with independent and dependent variables, is quite plastic in the sense that it is rendered universally applicable throughout the social sciences (and other sciences as well). Thus, these statistical methods are naturally an important component of the foundation of the institute. Now, as with the scales mentioned, there is more to statistics than meets the eye. Scales and measurements necessarily limit the modes of representation, while shaping a positive ground of knowledge. For example, the Rokeach Value Survey reduces the complexity of the notion of human values because it contains a pre-defined set of 18 value statements. Combined with for example exploratory factor analysis, a notably productive assemblage is formed, which allows such entities as *morality*, *sociality*, *integrity*, and *aspiration* to emerge as constitutive traits of the nature of man in relation to society. As with the Uppsala School, the SOM researchers also turned to the

American tradition of social surveys. However, now there was no need to perform much theoretical work because the scales and measurement techniques were already pre-configured, as was the newly introduced machinery programmed for computing social factors in numbers.

4.4   Conclusion

The state-science interface that was created Sweden after the Second World War poses an interesting paradox according to Merton's ideals, as mentioned above. On the one hand, the social sciences serve as promoters of a democratic society, and on the other hand, they are a tool for social planning. This paradox, serves to create productive translations between science and the state. In the coming decades, this collaboration translated the facts of quantitative social sciences both as useful data for the state and as high-quality scientific surveys, especially through the works of Westerståhl and the Uppsala School.

Alain Desrosiéres describes the special changes in statistics that took place during these times, not only in Sweden, but also in other countries:

> The nature and the purpose of these surveys changed completely as of the 1940s, under the joint effect of the birth of the welfare state and, following that, of the Keynesian macroeconomic policies. From then on, surveys addressed the totality of the population in order to describe inequalities between classes, on the one hand, and on the other, to quantify global consumption. It was in this context that the method of representative sampling appeared and progressively replaced the monographic method. (Desrosières 2011: 50)

The random samples of the early Election Surveys from 1956 and onward addressed the "totality" of the Swedish population. These surveys were also produced by a team of researchers, rather than single authors, who carefully filled tables with numbers.

The interface, when utilized to facilitate surveys, has led to an active black box that points forward in history. The 1954 and especially, the 1956 surveys are still today referred back to as *data* half a century later, which means that the "Lazarsfeld style" social sciences have produced a time series, the impact of which can be compared to the Celsius scale temperature series made in Uppsala since 1722.[5] Both "Lazarsfeld" and "Celsius" styles contain time series that depend on standardized measurement procedures. A number of parameters, scales, technologies and interpretation models need to be kept static (blackboxed). Consequently, positive facts are allowed to become historical in the sense that they can describe historical changes in temperature or voting behavior. These facts also serve as methodological calibration devices. If the next measurement deviates (in Kuhnian terminology we could say if there is an anomaly), there are a whole series of debugging options already at hand.

There is, however, one other feature of the long time series that has been overlooked in this story. Being able to refer back to a long tradition of research may, under certain circumstances, convey epistemic authority. When social scientific facts become controversial, black boxes are sometimes cracked open. In the next chapter, I will examine what difference is made by the historicity of black boxes when they are disputed in another type of interface, namely, a mass media frenzy concerning election forecasts in 1985.

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[5] See the Swedish Meteorological and Hydrological Institute; http://www.smhi.se/klimatdata/meteorologi/temperatur/1.2855, accessed 2012-03-01.

Chapter 5

1985 — Epistemic Authority

In this chapter, I will look more closely at a type of controversy that took place in the mid-1980s between academically identified researchers, of whom some went on to form the SOM Institute *a year later, and commercial opinion polls, in particular a pollster called* Sifo. *I will argue that this debate became a part in constructing a clear boundary between science and non-science for the future, both on epistemic grounds and concerning the political, economic and social goals of different types of research. In this chapter, I will pay special attention to how the accuracy of opinion polls was handled during a specific time in history, which in turn will allow me to better determine how the border between science and non-science is manifested.*

5.1   Introduction

This [*Sifo*] survey you refer to, it is pretty much in high emotions. I really think that the *SOM Institute* that actually is here in Gothenburg are doing quite a few surveys about the attitudes to energy-related

> issues... I think they... well... - Kjell Jansson, Exec-
> utive Director, Svensk Energi (Swedenergy)[1] [xli]

The quote above is from the largest televised debate program *SVT Debatt* in Sweden. After the Fukushima nuclear accident in 2011, a debate was framed to discuss the future of nuclear energy in Sweden. The debate featured invited experts in nuclear safety, politicians and commentators of various backgrounds. A common strategy in journalism is not only to collect expertise and stakeholders but also to 'invite' the public opinion. Consequently, a survey was purchased from *Sifo*, one of the largest and most established pollsters in Sweden, and their results were presented as a point of departure for the debate. Twenty-five per cent of the population expressed that they wanted to decommission nuclear power, 55 per cent said they wanted to keep it and 21 per cent replied that they did not know. However, Kjell Jansson, the executive director of Swedenergy, which is an industry organization for companies involved in electricity, was skeptical and voiced a concern on the accuracy of the *Sifo* poll, as quoted above. He argued that it was the result of an affected opinion, which deviated from what was the average attitude toward nuclear energy. Not only did he question the accuracy of the *Sifo* poll, but he also mentioned a "counter-laboratory" called the *SOM Institute*.

In this chapter, I will go back in time to the mid-1980s to determine how this distinction could be made and how it was possible for an actor, such as Jansson, to contrast *Sifo* with the *SOM Institute* in two sentences during a very swift and intense televised debate. In the quantification of society, all numbers do not share a perfectly equal status. It appears as though they are modulated by *whomever* is making the surveys. In other words, what Jansson did in the TV-show *SVT Debatt* was, if only for a swift moment, to open the lid of a black box.

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[1] Transcribed from SVT Debatt, broadcasted on 20110317.

Boundaries of science

From 1986 and onward, the *SOM Institute* was founded and progressively shaped into an all-encompassing research center. The *SOM Institute* covered core social scientific topics, such as culture, values, and social trust — traits that were measured by translating and importing international models. For example, the notion of human values and culture were, in one respect, reproductions of the works of Milton Rokeach and Ronald Inglehart. Reproduction enabled the SOM-researchers to test their findings and compare them with other surveys, and they conveyed theories and concepts, such as *terminal human values* and *post-materialism.* As these notions were imported into the SOM survey, they were conserved because longitudinal studies must ask the same questions every year to be able to account for changes over time. The methods and statistical measurements were previously tested and evaluated, and the surveys were, as mentioned earlier in Chapter 3, mutually reinforced by previous data. In an international comparison, these characteristics would associate the *SOM Institute* with disciplinary social sciences and render them compatible with the academic debate as it was shaped abroad.

The *SOM Institute* did not depart from nowhere. In contrast, the history of survey-based research in Gothenburg played an active role in quantifying both the state, and the population, as described in Chapter 4. Moreover and more importantly for this chapter, the *SOM Institute* was not only composed of a pure academic science, but as the following controversy will show, it also contained elements that in theory and epistemology were closely related to opinion polls of a commercial character. To a certain extent, the SOM- surveys asked the same questions as the commercial opinion polls had, and still do today. Questions dealing with which political party people would vote for, and what their opinions about particular issues were (e.g., nuclear energy) were measured both by academic institutions and the

so-called pollsters, who sold the results to newspapers and other organizations.

Another twist to this story, a twist that has been overlooked, is the heritage line back to Lazarsfeld, which is shared by both the *SOM Institute* and *Sifo*. Eyerman and Jamison write,

> Lazarsfeld's individualist model was also transferred to Sweden — and institutionalized outside the university — in *Sifo* [...] Eventually Zetterberg left the [Tercentenary] fund [of the Bank of Sweden] and started *Sifo* which applied American opinion and marketing research in the non-academic Swedish context (Eyerman and Jamison 1992b: 17).

What Eyerman and Jamison do not mention is that the Lazarsfeld model, "individualist" or not, had already been imported to academic research in the 1950s, as I described in Chapter 4. In the 1980s, both the Election Surveys and *Sifo* were large-scale producers of quantitative facts, and if we were to select a common theoretical denominator, they share the same roots in "Lazarsfeld-style" surveys. However, the Westerståhl and Särlvik surveys identified themselves as academic research, whereas *Sifo* is primarily described as a non-academic pollster. Over time these identities may be weakened or strengthened, sedimented or sent drifting and, especially during times of controversy, become quite unstable. Consequently, the Lazarsfeld model had been uprooted from its origin in American sociology and transferred into pollster research and political science.

To understand how the *SOM Institute* was created, it is important to explore how this line between science and non-science is negotiated in the public during the years before the institute was launched; or to use the vocabulary of Thomas F. Gieryn[2],

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[2] The concept of *epistemic authority* appears in the beginning of Gieryn's *Cultural Boundaries of Science - Credibility on the Line* (1999: 17). One

how struggles for epistemic authority are played out and what their role for the credibility of the social sciences are in a local Swedish context.

Boundary work is always taking place in time and space. According to my actualist position, as mentioned in Chapter 2, I need to identify a proper interface in which my study objects can be situated. A controversy between pollsters and academic researchers seems to be an appropriate way in, and in 1985, it was played out in the news media for anyone to follow.

5.2 The 1985 controversy

In 1985, the polls were published in the major newspapers and in television broadcasts throughout Sweden, and the polls had a high impact on the different analyses and comments were frequent in the mass media the days before the elections. Most of the opinion polls predicted that the *Social Democrats*, together with the left-wing parties would win. However, one poll deviated; *Sifo*, led by Hans Zetterberg, showed in one survey that only days before the elections, the conservative party *Modera-*

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type of "boundary work" is the *protection of autonomy*, which "results from efforts of outside powers, not to dislodge science from its place of epistemic authority, but to exploit that authority in ways that compromise the material and symbolic resources of scientists inside." As the inside boundaries of science are trespassed, another strategy, that of *expulsion*, may take place, thus "cleaning" the pure inside by clearly placing certain actors on the outside of its borders. Gieryn's work then contains a seductive set of cartographic metaphors, where the boundaries of science and non-science are laid out on a two-dimensional plane or map. For the present case, I choose not to follow this line of thinking, partly because boundary work is not the main topic of my work and partly because there is always a risk of theoretical over-determination that leads to reductionism. Instead of fulfilling a "Mercator projection" of a scientific landscape, my approach in this chapter is to follow an event that becomes important for another event (the founding moment of the *SOM Institute*). For this reason, Gieryn's idiom is treated as a conceptual tool without a theory and not as a perspective.

terna had scored high enough in the polls to win a majority
along with the other right-wing parties. To elaborate on what
is at stake as polls are published several times a week, how the
quantification of the public opinion is rendered into a fierce field
of interpretative struggles and how these surveys may fall back
on epistemic controversies, I will first work my way through two
weeks of media reports.

News media as an interface for controversy

As expected, the mainstream media reported heavily on the elec-
tions of September 1985. The editorial columns argued on polit-
ical issues, representatives of various organizations and parties
voiced their views and political personas were interviewed. To
make forecasts, however, newspapers relied heavily on the data
provided by the various pollsters, from which *Sifo* stood out as
the most productive by serving several papers. The tabloid *Ex-
pressen*, the local broadsheet *Göteborgs-Posten*, the local *Arbetet*
and the large national broadsheet *Svenska Dagbladet* all sub-
scribed to *Sifo*'s polls and wrote articles, editorials and graphics
based on the *Sifo* election forecasts called the *Election Barom-
eter* (Väljarbarometern). In contrast, the broadsheet *Dagens
Nyheter* subscribed to the pollster IMU.

Predictions about the outcome of the election were big news
in September 1985. *Expressen* published their *Sifo* polls in minute
detail with comments and analyses. The polls were promoted,
and Zetterberg was interviewed as a spokesperson for *Sifo*. He
explained the success as "I found it amusing that it was said on
the television news broadcast [*Aktuellt*] that SCB had done a
Sifo-poll, he said and chuckled." (*Expressen* 1985-09-01)[xlii]. The
brand name of *Sifo* had become very strong, and *Expressen* also
published a special *Express*-Sifo, a survey that was conducted af-
ter the major television debates between the politicians running
for parliament. On September 7th, *Sifo* published a controver-
sial poll that predicted a right-wing majority, and Zetterberg

Figure 5.1: *Expressen*, 14th of September, one day before the elections. Prime minister Olof Palme is reading the *Sifo* data. Headline "The loser".

stated "The big TV-debate meant that Ulf Adelsohn put an end to the advance of the *Social Democrats*" (*Expressen* 1985-09-07).[xliii] The *Sifo* poll was then criticized by the powerful *Social Democrat* minister of finance, Kjell-Olof Feldt, who threatened that "*Sifo* needs to watch out" (*Expressen* 1985-09-09)[xliv]. On September 12th, the IMU poll was published in *Dagens Nyheter*, but the *Expressen* political journalist Erik Månsson was skeptical and claimed that IMU was "overrating the predictions for the Green party" (*Expressen* 1985-09-12).

The other nation-wide tabloid, *Aftonbladet*, did not subscribe to a poll at the time. Consequently, their reports were less frequent, but there were nevertheless many interesting comments about the polls that were originally published elsewhere. The

Sifo polls were contested to a higher extent than in *Expressen*.
The party secretary of the *Center party* (Centerpartiet), which
had scored low in the *Sifo* polls, commented "We have gotten
used to *Sifo*, we do not take their polls seriously. In 1982 they
predicted [we would reach] 11 per cent, we got 15.5. If the same
thing happens again, one has to question whether there are sys-
tematic errors in the *Sifo* [polls]" (*Aftonbladet* 1985-09-01).[xlv]

As the controversial *Sifo* poll was published, methodology
also came into discussion. The party secretary of the *Social
Democrats* stated, "Why does *Sifo* change their survey method
from one week to another? [...] Last week they [*Sifo*] made house
calls to the homes of the respondents, and this time they are
doing both visiting interviews and telephone interviews. Such
shifts adds only even more suspicion against the measurement"
(*Aftonbladet* 1985-09-07)[xlvi]. Additionally, Sören Holmberg was
interviewed, who warned that surveys may influence the actual
election result and argued that a pluralism of surveys is better
to minimize such an effect (*Aftonbladet* 1985-09-11). Two days
before the elections, *Aftonbladet* claimed, based on the new data
from IMU, that the *Social Democrats* would stay in power and
that "*Sifo* is a pollster institute led by Professor Hans Zetterberg.
The results of their polls are published in several dailies. *Sifo*
is also used by *Moderaterna* for their internal opinion analyses"
(*Aftonbladet* 1985-09-13).[xlvii]

Subscribing to the less-frequent IMU poll, *Dagens Nyheter*
contained fewer numbers, graphs and tables to predict the elec-
tion. In a broadsheet fashion, *Dagens Nyheter* instead discussed
politics with in-depth analyses. In an editorial column, they de-
scribed the situation as "[...] As then *Sifo* published their last
election forecast in several papers saying that the difference be-
tween the [party] blocks was only one per cent. A measly per
cent, that is within the famous margin of error. Everything is
in flux, anything can happen[xlviii]" (*Dagens Nyheter* 1985-09-02).
As the controversial *Sifo* poll was published, they interviewed

Paper	Type	Pollster subscription
Dagens Nyheter	Broadsheet	IMU
Svenska Dagbladet	Broadsheet	Unknown *Moderate*
Expressen	Tabloid	*Sifo*
Aftonbladet	Tabloid	none
Arbetet	Local Broadsheet	*Sifo*

Figure 5.2: Newspaper pollster subscriptions during the 1985 elections.

the chief editor of the paper *Arbetet*, Lars Engqvist (see below), who reported he was confident that "[...] Hans Zetterberg was driven by a political agenda when he chose to to make the poll public" (*Dagens Nyheter* 1985-09-10).[xlix] Briefly before the election, *Dagens Nyheter* published their IMU poll, which contrary to the *Sifo* poll, held that the *Social Democrats* would remain in power.

The local *Social Democrat* newspaper of Malmö, *Arbetet*, also subscribed to the *Sifo* polls. However, their interpretation of the results differed greatly from that of the liberal newspapers, although they initially seemed to agree on the accuracy of the polls. On September 1st, they published a *Sifo* poll, in which they argued that the *Social Democrats* "have thus continued recovering from the record lows in December, when they had 39 per cent of the voters. Now they are getting 45 per cent, which is an increase by 1.5 per cent since June" (*Arbetet* 1985-09-01). Additionally, editor-in-chief Lars Engqvist wrote that "The *Sifo* numbers allow a definitive interpretation: the *Social Democrats* have been extremely successful during 1985" (Ibid.). This can be contrasted with the comments on the same poll in *Expressen*, where Erik Månsson had reported that "Ulf Adelsohn is probably happy for the *Sifo* numbers. The fact that the [political] middle is getting higher numbers and that the *Moderaterna* are getting lower, contributes to his chances of becoming prime minister" (*Expressen* 1985-09-02).

As the controversial *Sifo* poll is published, Lars Engqvist at *Arbetet* makes the decision as editor-in-chief not to publish its "diagrams and tables". This points to the prediction that the *Social Democrats* would lose power, but Engqvist's line of arguing is scientific: "The reason for not publishing diagrams and tables this time is that the survey was not made the same way as the ones we previously ordered [subscribed to]. It contained too many factors of uncertainty to be valued as the others. A 'real' *Sifo* poll will come a couple of days before the elections". Moreover, the *Green party* spokesperson Per Garton is interviewed and states, "[it is] a commercial false play" (*Arbetet* 1985-09-08). A week later, as the last *Sifo* poll was published, the diagrams are back again, showing a small leading position for the *Social Democrats.*

As we can see, the numbers were used to promote various political agendas in the very even elections of 1985. The pollster data were interpreted quite differently in the newspapers I selected and analyzed. Simultaneously, it became an issue for the pollsters to remain neutral, keep rigorous methods intact and only present unbiased data. Numbers could even be viewed as dangerous, as in the case of *Arbetet*, who chose not to publish unfavorable results.

This is, however, nothing new. The press in Sweden in the 1980s had had a tradition of siding with their party and political affiliations, and the interpretive flexibility of numbers is less subtle in editorial columns than in scientific controversies. A thorough analysis of the opinion journalism is made in Holmberg and Petersson's *Inom felmarginalen* (1980), where the relation between the pollsters and the media are discussed, among many other things, the political 'tendencies' of *Sifo* (Ibid.: 180). Zetterberg, however, defends against such claims: "Since the press in Sweden (in the editorial columns) have a strong conservative ("borgerlig") dominance, the impression sometimes is that *Sifo* is right wing, and always support the right wing par-

ties" (Zetterberg 1978: 158).

Credibility questioned

Zetterberg was an Uppsala School sociologist who had been trained by Torgny Segerstedt and had worked for 20 years in the US at respectable universities, such as Columbia University, before returning to Sweden and becoming the executive director of *Sifo* in 1975 (Nordfeldt 1981). Zetterberg's prediction that the *Moderaterna* were about to win the election later turned out to be incorrect as the final results were determined. What would follow was a heated debate on the accuracy and methodology of polls as well as on the impact of polls on the actual elections as they were mediated and commented. However, Zetterberg's political motivations were also questioned, especially by comments made by the left-wing press. Immediately after the elections, Zetterberg was held responsible for the errors in the controversial poll. Johan Schück, a columnist at *Dagens Nyheter*, wrote,

> He [Zetterberg] should now consider whether *Sifo* has become a factor of power on false premises. This is why the next *Election Barometer* will be highly expected: will the *Moderaterna* once again be predicted to get high numbers and be applauded in the party headquarters? Or will *Sifo* until then decide to correct their measurements in accordance with reality? (*Dagens Nyheter* 1985-09-29)[1]

Sifo's prediction had been a major theme in the newspapers, and the optimism of the right-wing coalition parties was directly related to the figures that were circulating in the media. This optimism-effect could possibly have influenced the final results; at least, this was alleged by several authors in the election aftermath. If we instead turn to the left-wing press, Zetterberg's credibility was questioned in the worker's union paper

LO-tidningen concerning his possible political affiliations with the *Moderaterna*. Ivar Ivre wrote,

> The great failure of *Sifo* was the systematic predic-
> tion that the *Moderaterna* would score high in the
> elections. Zetterberg's and *Sifo*'s close cooperation
> with the party [*Moderaterna*] makes it all look even
> more dubious. (*LO-tidningen* 1985-11-22)[li]

The *who* of surveys is consequently highlighted, and the non-
personal numbers suddenly have a human face, i.e., that of cer-
tain (alleged) affiliations that Zetterberg had had with a politi-
cal party. Political views and the suspicion that these affiliations
may influence the outcome of quantitative surveys, are, however,
weak lines of argument and are mostly found in the publications
of the left-wing press, such as *LO-tidningen*. What is more inter-
esting and important are the epistemological struggles that are
ignited.

Both Schück and Ivre had read Holmberg and Petersson's
book *Inom felmarginalen* written five years earlier. Schück used
one of the arguments in this book to highlight that there was
"a type of systematic bias in *Sifo*'s statistical sampling or, more
likely, in their analysis of the collected data" (*Dagens Nyheter*
1985-09-29)[lii]. Ivre, in contrast, made a reference to Holmberg
and the "group of election researchers around him at the De-
partment of Political Science at Gothenburg university" (*LO-
tidningen 1985-11-22*).[liii]" who together with Peterson already
had already noted the weaknesses with the method used by pri-
vate pollsters.

The criticism of *Sifo* also attracted other actors to join in the
debate. Jan Hagberg, a statistician at *Statistics Sweden* (SCB)
voiced the issue of the inherent bias in commercial institutes in
LO-tidningen:

> The pollsters do not have a scientific status to loose.
> They have already sold their souls, since they are on

> the market. [...] Acting as they were, only describing
> an opinion with an allegedly neutral tool, they have
> attempted to influence the opinion. (*LO-tidningen*
> 1985-10-25)

Hagberg, who was employed by *Statistics Sweden*, argued
that the problem is that *Sifo* is in a "market", which would ex-
plain the errors of the pollster. However, it is not just any mar-
ket (see also Chapter 4 to see how *Statistics Sweden* was defined
to conduct field work), but rather, the speedy sale of surveys
to newspapers days before the election that is the cause of the
faulty predictions. Hagberg further suggested that the solution
would be that pollsters handed over their raw data material to
an authority that could audit the statistical methods used.

A couple of months after the elections Zetterberg was inter-
viewed in *Svenska Dagbladet* where he defended the neutrality
of *Sifo*:

> - Why do not *Sifo* present the method of their polls?
> Why this secrecy?
> - This is completely false — even though it is repeated
> over and over again. The methodology for *Sifo*'s
> *Väljarbarometer* is public in all parts — except the
> party affiliations of particular individuals. Detailed
> statistics on the functions of our questionnaires, our
> visiting interviews and, nowadays, our telephone in-
> terviews are available every month. (*Svenska Dag-
> bladet* 1985-11-03)

The direct surface of the controversy thus contains three dis-
tinctions that are at work in the demarcation and defense of epis-
temic authority. The first distinction is that between a passive
and an interactive survey. Ideally, a survey should only describe
the opinion, not engage in it. *Sifo* was accused of doing precisely

this by publishing uncertain results only weeks before the elections. Second, the objectivity of the surveys was at stake in case certain personal alliances influenced the agenda, the timing and the interpretation of the results. Third, the commercial nature was described as a logic standing in opposition to the scientific ideals required to perform a qualified measurement of the public opinion.

The boundary between science and non-science is, however, not yet fully explored, and to determine where this boundary it is drawn, I must open more black boxes.

5.3   Borders of science

The controversy made the credibility of pollsters in general, and *Sifo* in particular, open to debate in public, outside the academic journals and institutional method talk. As the black box of swift surveys was opened in 1985, its complex components were laid out in the public sphere, and a number of actors took turns to comment on these components after the elections. By taking a closer look at how the academic reception of the controversy was shaped, it is possible to further describe what is at stake and how to define the space in which the SOM Insitute appeared only one year later.

After the election, the political scientist from Gothenburg Mikael Gilljam deeply criticized *Sifo*'s credibility. The poll had made a large impact in the newspapers, and Gilljam argued that this impact had given opinion polls a great deal of power to influence the outcome of democratic elections. Gilljam completely disqualified the methods used by *Sifo*, from how they randomized their populations to how they conducted the actual surveys. In a *Dagens Nyheter* column, he argued the following:

> For the future it is, moreover, my hope that the methodology is presented fully, already when the polls

> are published. Can you already now, gentlemen poll-
> sters, promise that we will see more rigid polls in the
> 1988 elections? (*Dagens Nyheter* 1985-09-29)[liv]

The methodological criticism, already mentioned in the pre-
election weeks, targeted the small populations of the surveys,
especially the telephone interviews made in connection to the
televised debates between politicians. Moreover, there had been
a suspicious attitude toward *Sifo* and the other pollsters because
they published their full methodological accounts together with
the survey. According to Sören Holmberg, the surveys of the
commercial pollsters had to have dealt with populations below
1000 respondents, which in turn meant that the number of uncer-
tain voters in the population was approximately 60-70 persons
(Holmberg 1986: 45). The polls were thus unreliable both for be-
ing too small to provide statistical accuracy and to hide this fact
by not being transparent enough. As mentioned above, however,
this is not something with which Zetterberg agreed.

The controversy, however, goes deeper than the analyses and
criticism right after the elections. The overlapping of epistemol-
ogy/methodology and political claims to knowledge in the public
sphere had been at stake in academic journals before the 1985
elections. One such aspect, which would later constitute a defini-
tive trial of strength as the *SOM Institute* was founded in 1986,
is the question of whether postal surveys actually work. In a
1978 article, Zetterberg was very skeptical:

> Postal surveys sent to the population produce far too
> large deviations to be used in serious polls. After re-
> minding and promising rewards to the respondents,
> there are still drop off rates of 40 to 50 per cent.
> Usually postal surveys are considered to show the
> opinions of the "crossword people", i.e., persons who
> enjoy solving crosswords and submit the solutions.
> Crossword people think differently, and have differ-

ent opinions, than others, and no corrective measure-
ments could ever make them resemble other people.
(Zetterberg 1978: 146) [lv]

As we observed in Chapter 3, there were methodological in-
quiries developed by Anders Ohlsson that could be used to de-
termine how the postal survey should be made to work. Until
1985, the commercial polls mainly relied on visiting interviews,
sometimes combined with telephone interviews. To identify the
traces of this methodological debate, however, we must go *for-
ward* in history (to use the vocabulary of Thomas Kuhn)[3], even
though we are sometimes moving back a few years. As we ob-
served in Chapter 4, the circumstances for survey methods have
changed over time, and how they are treated may very well differ
from time to time.

As mentioned above, in 1980, Sören Holmberg and Olof Pe-
tersson published the book *Inom Felmarginalen*. This book thor-
oughly examines the methodology and political impact of polls
and surveys in relation to elections. The book contains a de-
tailed technical evaluation of *Sifo*'s methodology, which criti-
cizes Zetterberg's 1978 article as quoted above on epistemological
grounds. Moreover, Holmberg and Petersson (1980) thoroughly
inspect the methodology of *Sifo*, even the political affiliations
of the interviewers (Ibid.: 68), *Sifo*'s economy (Ibid.: 14) and
its history. *Inom felmarginalen* can be read plainly as a critical
inquiry to the role of opinion polls in society and their method-
ology. However, as it is a critique, *Inom felmarginalen* is also a
type of boundary work:

Playing with the margin of error is a seductive game
of numbers. It leads to an unreflected acceptance of

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[3] The distinction between backwards and forwards history appears in the
chapter "The Invisibility of Revolutions" in *The Structure of Scientific Rev-
olutions* (Kuhn 1996 [1962]: 138-139).

> the main results of opinion polls, and reduces skep-
> ticism to a few per cent up or down. And that is
> not good. There are many other sources of error be-
> sides the statistical uncertainty that we also ought to
> be cautious about. (Holmberg and Petersson 1980:
> 41)[lvi]

Surprisingly, it is Zetterberg who, in 1986, defines the border
between scholarly, academic research and the research performed
by the pollsters. He writes that "The institutes [pollsters] are on
the one hand part of "the research community" (Zetterberg 1986:
26)[lvii]. However, "research" is defined here as a wider concept
than "scientific and scholarly research" (Ibid.)[lviii]. He continues,

> Those who direct *Sifo's* projects are called leaders of
> investigation [undersökningsledare], not researchers,
> to make it clear that it is about "research".[4] They are
> similar to university scientists, in that they have an
> academic degree, belong to scholarly societies, keep
> up with the international journals of their subject ar-
> eas, go to scientific conferences concerning their sub-
> ject matters and are encouraged to present results
> of methodological interest at international meetings
> and at Swedish occasions (Ibid.)[lix]

Thus, Zetterberg himself defines a border between "scientific
and scholarly research" and simply plain "research". The space
for pollster research was occupied by several private actors in
the mid-1980s. However, what about the scientific- or academic
research?

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[4] The translation of Zetterberg is particularly difficult here. He uses the
word "research" in English, to denote the type of activities in *Sifo*. He
makes this distinction in contrast to "scientific and scholarly research" (also
English in original), which in turn is somewhat loosely defined as academic
and university based research.

In 1985, Uppsala sociologist (and International Sociological Association President 1978-1982) Ulf Himmelstrand expressed that sociology not only had lost but also had to reclaim a space occupied by the commercial pollsters:

> Swedish academic sociologists have since the mid-1960s, with some few exceptions, been utterly uninterested in [public] opinion and attitude surveys and their methodology. Such surveys have in turn become an important matter of concern for mass media, political parties and commercial pollster institutes. (Himmelstrand 1985: 59) [lx]

Here, Himmelstrand refers to the work of the Uppsala School sociologists, who had declined in importance, as described in Chapter 4. He continues to argue not only that pollsters need to become more scientific but also that academic research needs to engage more widely in opinion research. Himmelstrand's main complaint is exemplified by recent claims by *Sifo* and the research organization SNS[5] that the Swedish population is becoming more skeptical of the (large) size of the public sector. He argues that if one "compiles the results of a number of recurring *Sifo* surveys and Election Surveys conducted at the Department for Political Science at the University of Gothenburg [...] you will in all reach the following conclusion. The support of the Swedish population for the public sector remains" (Himmelstrand 1985: 65).[lxi]

Himmelstrand's request for academic opinion research points toward a perceived vacuum, a position yet to be taken by someone — at least implicitly — that assumes methodological responsibility on a stricter level than the pollsters. I write "per-

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[5] Today, SNS is referred by the English name *Centre for Business and Policy Studies* and describes itself as "an independent network of leading decision makers from the private and public sectors who share a commitment to social and economic development in Sweden"; see http://www.sns.se/english, accessed 2012-02-16.

ceived" here because the absence of academic research is relative to how history has been written. Himmelstrand acknowledges the Gothenburg scientists, whereas a few years earlier Zetterberg argued that both theory and method in opinion research had been primarily developed outside the universities and that this type of knowledge had to be integrated in academic research (Zetterberg 1978: 148). Referring to the Uppsala School sociology, he concluded,

> Toward the end of the 1950s, the young sociology in Sweden came to be confused with opinion surveys. This was deeply unfortunate for sociology, which in fact is a large, wonderful tent that houses very much more than opinions, attitudes and [interview] methodology. (Zetterberg 1978: 154)[lxii]

What Zetterberg argues here is somewhat of a contradiction. While he describes opinion research as driven by pollsters when it comes to methodology and in the capacity of conducting large surveys, he also argues that the Uppsala School came too close to adopting the design of the pollsters. On the one hand, there is great value in opinion research data for the social sciences. However, on the other hand, the social sciences should not attempt to do what pollsters do.

It appears as though this question cannot be resolved purely on epistemic grounds. What the roles of social scientists and pollsters should look like is defined in much stronger terms when it refers to their role in a democratic society.

5.4   Conclusion — Science and democracy

While the border between the social sciences and pollsters is quite blurred when it comes to their epistemic practice, their role in a democratic society is richer in contrast. The title of Jan

Hagbergs article, quoted above is *Demokrati är varken* Sifo*krati eller dIMUkrati*, in which "Democracy is neither *Sifo*cracy nor dIMUcracy" is meant to contain the names of the *Sifo* and IMU polls. Already making the point that pollsters are private companies acting within the logic of the market has a strong bearing in Hagbergs rhetoric in a left-wing unionist paper, such as *LO-tidningen*.

However, the role of opinion research is not clear-cut as simply as left-right issues. Additionally, Zetterberg reflects critically on his own work but uses another word:

> A disadvantage of opinion polls about the match between leaders and the led, lies in that they encourage a "Gallup Democracy". A benefit is that they provide the political parties with information of where they have failed in their activities to educate the public. (Zetterberg 1978: 162)[lxiii]

The Swedish Gallup Institute was owned by the *Social Democratic* party in the mid-1980s, whereas *Sifo* and IMU were privately owned pollsters. Gallup kept their polls secret and only revealed the results to the political campaign planners of the party (see also Holmberg and Petersson 1980: 108).

What everyone seems to agree on is that opinion research conducted solely by pollsters, especially when dubious actors do not reveal their methodology, may lead to a serious failure for democracy. The question then becomes, is there another position, a neutral one, from where surveys can be made?

In this chapter, I have, on one level, analyzed the positions, arguments and identities of several actors that attempted define how proper surveys should be made, what responsibilities quantitative facts entail in a democracy, and in what way a surveyor of society should strive toward a neutral political position.

However, when the 1985 debate is connected to my previous empirical chapters, there is a higher theoretical significance

that goes beyond a classic controversy. For quantitative facts to circulate widely, they first need to be translated by one or several interfaces (see Chapter 2). In the mid-1980s, pollsters had already assembled a successful interface, whereby newspapers subscribed to their data, which in turn were produced with swift visits and telephone interviews. In 1985, you could follow the election forecasts almost on a daily basis immediately before the election, and the "numbers" were considered to be "big news".

The criticism of the pollsters that was pursued by academic researchers such as Holmberg, Peterson, Himmelstrand and Gilljam created a *problematization* in the circulatory system of quantitative facts about the public opinion.

A type of indispensability was achieved by claiming the epistemological precedence of university-based social science, combined with the neutral and non-political character associated with the academic pursuit of knowledge. The harsher the critique of the pollsters turns, the more open the space of a solution to the problem becomes. There are many problems of the pollsters: they operate under the logic of the market, their methods are insufficiently accurate, they are hired by political parties and organizations with political agendas and they interact with the public opinion to the extent that they may influence democratic elections. To these problems, university-based research is portrayed as an anti-thesis: funded by public means, performed with methodological rigor, hired only by the pursuit of knowledge and making their results public in a non-sensational manner that minimizes the risk of interacting with ongoing political issues. The distinction and demarcation between science and non-science, which was reinforced during the mid-1980s, makes it possible for Westerståhl in 1990 to write,

> Another trait of Swedish political science is that the subject [discipline] has *not* to the same large degree as other social sciences *made claims to represent so-*

> *cial engineering.* After the victory of value relativism
> it has come to appear evident that the substantial
> task [of political science] consists of presenting facts
> and contexts in the political analyses and not in sug-
> gesting measures. (Westerståhl 1990: 68-69, italics
> in original)[lxiv]

The reconfiguration of the interfaces that enabled scientific facts to enter the public debate thus paved way for another type of knowledge, which, unlike pollster data, was considered to be unaffected by inaccuracies and political bias. Neither did this type of academic research represent the old figurative of "social engineering", where it would draw its legitimacy from its useful-ness in social planning.

A consequence of the 1985 controversy was that a space of epistemic authority, distinct from pollster research, had been opened, a space that could be entered by the *SOM Institute* in 1986. However, to make that happen, Zetterberg's claim that postal surveys were only responded to by "crossword people" and had "drop off rates of 40 to 50 per cent" (as quoted above) had to be *disproved*, as we observed in Chapter 3.

Chapter 6

1999 — Center of calculation

In this chapter I will empirically synthesize the different negotiations and uncertainties presented in previous chapters and show how the SOM Institute *has become an established center of calculation that has been able to expand its surveys. I will show how the institute achieved stability and how this is reflected in the history of its surveys. Moreover, I will analyze how nonrespondents continue the to render problematic the blackboxing process for the postal survey, creating the need for analysis and debugging even when facts are generated in normal science.*

Prelude

In a press release from 2011 the *SOM Institute* announced the following:

> Close to 450,000 Swedish citizens belong to the often neglected but growing group of expatriate Swedes. A group that is relatively unexplored. The *SOM Institute* now seeks collaboration partners to conduct an

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> *Abroad-SOM* survey (Utlands-SOM) with unique op-
> portunities to investigate the living conditions, media
> habits, social trust, health, life styles, opinions and
> values and views on corruption of [these] expatriate
> Swedes.[1] [lxv]

Since 1986, the *SOM Institute* has progressively grown in size.
In 1992, regional surveys were launched alongside the national
survey. The sample populations have increased, allowing more
detailed statistical measurements, and the reports written every
year are now hundreds of pages long. Moreover, today, special
surveys are performed with students and in smaller municipal-
ities and for further developing methodological techniques. As
the quotation above indicates, the *SOM Institute* even attempts
to quantify the Swedish expatriates who are difficult to reach via
conventional postal surveys.

For the this research institute to arrive at a point at which
it may cover new territories, regularly gather facts and attract
partners, prior negotiations must have taken place. In Kuh-
nian terminology, the quotation above presents an instance of
puzzle-solving normal science. To me, however, there are black
boxes that are being assembled and successful interfaces that
are translating large numbers into useful scientific knowledge.
The difference here is very important: even though normal sci-
ence may have been sedimented and stabilized on a general level
(paradigm), as a century-old tradition of conducting quantita-
tive surveys, this is only valid for ready-made science. Science
in the making needs to be assembled and re-assembled for ev-
ery new experiment, each new laboratory site, or, as in my case,
during every new survey that measures society. The degree of
stability is determined not by a paradigm, but by the degree of
blackboxing. This way, the durability of an epistemic assemblage

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[1] See http://www.som.gu.se/aktuellt/Nyheter/Nyheter_detalj//samverkanspartners-
sokes-till-utlands-som.cid1047557, accessed 2012-02-28.

depends on how well black boxes can be made to work together, and how smoothly interfaces translate and mediate in between them.

In this chapter, I will depart in the 1999 survey to determine if the *SOM Institute* qualifies as a center of calculation, as suggested in Chapter 2. Moreover, I will analyze to what degree the postal survey has stabilized and whether it has required updates, patches and fixes. Whereas the previous chapters were focused on often controversial scientific work that was unstable or had to be performed for the first time, I have in this chapter chosen a point in history at which, at least on a superficial level, everything seems to work smoothly.

6.1 Hard social facts

Every year since 1986, the *SOM Institute* has published a volume in which social scientists present the latest results and trends revealed by yearly postal survey. The the report of the 1999 survey is called *Det nya samhället* (*The New Society*, see Figure 6.1), which is a 400-page volume that also includes the questionnaire that was sent out in 1999, and a methodological appendix that describes field work conducted as part of the survey.

An epistemic assemblage

To determine the strength of the facts, we may approach the preface of the report and already here find interesting results. It is written by Sören Holmberg and Lennart Weibull (2000), who state that the report is based on a survey of two randomized populations, each with 2,800 respondents and fairly equal response rates. The fieldwork took place between October and December 1999, and the private contractor *Kinnmark DM AB* was hired to collect the data. The methodological appendix is referenced as a source to which the interested reader can turn.
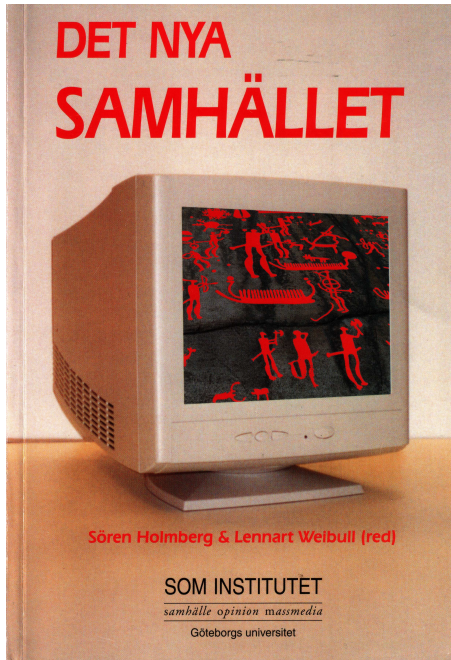
Figure 6.1: Book cover of *Det nya samhället*, the results of the 1999 SOM survey presented in the institute's report series; see Holmberg and Weibull 2000.

It is also mentioned that the survey has been conducted every year since 1986. The reader of the report is rapidly entangled in technicalities, and even though it is full of unopened black boxes (populations, field work, data, response rates), the experienced social scientist can still make a judgment about its qualities, and recognize it as a typical scientific survey based on its methodology presented on the first page of the book. Compared with the average media reports (see Chapter 2 there is more information than only the output, more than just numbers produced by the *SOM Institute*. The curious reader may ask what is a

"randomized population"? How many is two times 2,800? Who
is "Kinnmark DM AB"? What is "field work"?

The methodological appendix in the back of the report pro-
vides answers to these questions. It is stated that this is the
fourteenth survey and that each survey is performed "under al-
most identical circumstances" (Lithner 2000: 395). A detailed
description of the survey follows. The "randomized population"
mentioned in the preface is now characterized as a "simple ran-
dom sample".[2] The concept of a simple random sample is not
further explained (another black box); instead the population
further described as including "two times 2,800" individuals be-
tween the ages of 15 and 80 years, including foreign citizens.

What is stable and what is unstable? In Chapter 4 I tracked
down the discussion of sampling techniques all the way back to
the 1940s, when Särlvik and Westerståhl chose to perform sim-
ple random samples rather than the quota samples that Gallup
Sweden had utilized. Since then, random samples became a stan-
dardized way of sampling, which was regarded to be the most
accurate method. In 1999, random samples need no further elab-
oration. The black box of simple random samples is closed, which
means that they are unproblematic to build upon and to function
as a keystone upon which further components may be securely
assembled. To find instability, the reader must go further.

The two questionnaires are introduced and are included in
another appendix. It is indicated that the questionnaires have
been developed in cooperation between the *SOM Institute* and
affiliated research projects, which in turn are listed in another
appendix. There are 21 such projects, most of which are research
projects at the University of Gothenburg but which also include

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[2] The use of simple random samples (obundet slumpmässigt urval) means
that each individual has the same probability of being included in a sample.
This form of sampling is considered superior to cluster sampling or stratified
sampling in the social sciences (see Esaiasson et al. 2002: 195-201). To open
this black box, one would have to follow statistical sampling back in time,
as in Chapter 4.

Year	Response rate	Contractor
1999	67%	Kinnmark
1998	69%	*Sifo*
1997	69%	Gallup
1996	69%	Gallup
1995	65%	Temo
1994	67%	Gallup
1993	70%	*Sifo*
1992	71%	*Sifo*
1991	67%	IMU-*Testologen*
1990	66%	IMU-*Testologen*
1989	66%	IMU-*Testologen*
1988	68%	SCB
1987	70%	*SOM Institutet*
1986	68%	SCB

Figure 6.2: List of external contractors performing field work for the *SOM Institute*. Based on Lithner (2000: 398).

participants from Uppsala University, the Institute of Public Affairs in Warzaw and Stiftelsen Institutet för mediestudier.[3] The purpose of using two questionnaires is to be able to ask more questions without making the questionnaire too long, and to ask identical questions on certain topics. The latter makes the population large enough that it can be broken down in smaller groups without decreasing the statistical validity of the study.

However, the most interesting part of the methodological appendix is the description of the field work that was conducted during the late fall of 1999. Again, the private contractor *Kin-*

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[3] The name of the organization, which lacks an official English title, can be translated as "The Institute for Media Studies Foundation". Its purpose is to promote scientific research on the media (see http://www.mediestudier.se, accessed 2012-03-01).

nmark DM och Distribution[4] is mentioned as having been commissioned to perform the actual data collection process. The delegation of the "field work" to a private contractor, seems to be unproblematic at this moment. As figure 6.1 shows, the *SOM Institute* has hired private pollsters such as *Sifo* over the years, even though, as shown in Chapter 5, the methods of the pollsters were seen as highly problematic from an academic perspective in the mid-1980s. However, in 1999, there is no need for this debate, at least not in a methodological appendix.

Then a list of equipment is presented that includes questionnaires, post-cards, telephone reminders and follow-up letters, each of them dated. Taking a closer look at these dates, one can re-construct what happened during the last few months of 1999:

29 Sept. 1999 Dispatch of advance letter[5].

4 Oct. Dispatch of questionnaire, follow-up letter, brochure and return envelope.

11 Oct. Dispatch of greeting and reminder letter[6].

20 Oct. Dispatch of reminder letter.

20 Nov. - 5 Oct. Dispatch of extra greeting- and reminder letter for Riks-SOM I.

5-16 Nov. Telephone reminder.

17 Nov. Dispatch of postal reminder.

23-29 Nov. Telephone reminder.

2 Dec. Dispatch of postal reminder.

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[4] Kinnmark, as of 2010, sells survey field work to several other university-based research centers and state agencies. The partnership between that organization and the *SOM Institute* is indicated on the web-site of the former (http://www.kinnmark.se, accessed 2011-02-15) as one of its merits.

[5] "Aviseringsvykort" here can be translated as "advance letter", which is the terminology used in the survey method literature, although "vykort" actually means "post-card".

[6] "Vykort" (post card) is translated as "letter" here.

7-13 Dec. Telephone reminder.

14 Dec. Dispatch of postal reminder.

23 Dec. Dispatch of mini-survey.

18 Jan. 2000 Field work is terminated.[7]

If we open the black box "postal survey", which is practiced as "field work", we find this list of components. Each postcard, telephone call and batch of questionnaires is dated precisely. This information probably satisfies most readers of the report as to the degree of scientific rigor and openness of the information regarding the survey. This is the Hubble Telescope or the Large Hadron Collider of the *SOM Institute*, and although it is not as costly or famous, I will argue that it should be put on an equivalent level as scientific laboratories in the natural sciences. This means, once again, that more black boxes must be opened. But why is this aspect of the methodological appendix still open? Why are the descriptions so accurate and detailed?

In "Give Me a Laboratory and I will Raise the World", Latour (1999b) discusses the role of Pasteur's laboratory in Paris in the 1880s and its relations to an outside society. Latour suggests that the notions of inside/outside and micro/macro need to be abandoned or at least drastically revised if we are to understand what was at stake when Pasteur discovered the anthrax vaccine. For Pasteur to create the vaccine, he had convince a number of actors to participate via series a of negotiations. For instance, farmers had to be convinced that the vaccine would save their cows and income and, thus, to let Pasteur take samples from them for his laboratory. Once inside the laboratory, the anthrax bacilli were cultivated (made stronger) and are progressively turned into a vaccine. Then, the vaccine could eventually be brought back to the farmers and their cows, fulfilling the promise that Pasteur had made to the former (Latour 1999b: 208-274).

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[7] Based on Lithner (2000: 397).
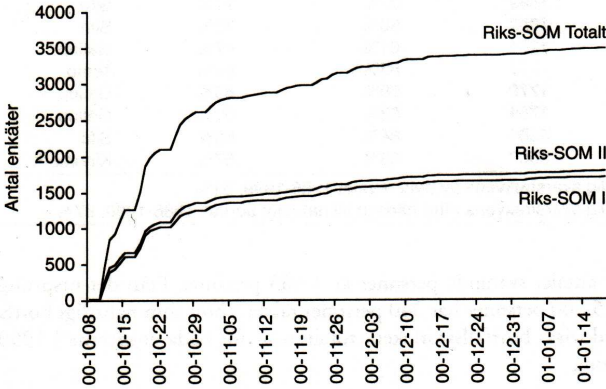
Figure 6.3: "Daily inflow of questionnaires in the 1999 Riks SOM survey". X-axis: "Number of questionnaires", Y-axis: Dates. (Lithner 2000: 397).

Similar to how the anthrax bacilli needed to be brought into and then transported out of Pasteur's laboratory, as shown in Figure 6.1, the questionnaires also needed to be dispatched to the respondents returned completed to the *SOM Institute*. The respondents are the microbes within their society, and it is not until they are captured and counted that they can be transformed into macrobes that speak in the name their society as a whole. Whereas Pasteur had to travel to the countryside to obtain his samples, the *SOM Institute* utilizes mediating interfaces; the questionnaires are sent via the postal system, after which they are gathered by the Kinnmark contractor and turned into computer-readable data. The data are then returned to the institute and sent to the researchers working on the projects. If everything goes as planned, articles and reports can be written, press conferences can be held, news media can print articles cov-

ering the findings and circulate them back to anyone who is interested in society, as described by the social sciences. Using the vocabulary of Latour, we might suggest that *to state something about the macro-society, the* SOM Institute *needs to amplify a sample of the micro-society through strength trials.* Because response rates have to be conquered each year, because they are hard to collect and amplify, this section of the epistemic assemblage constituting the 1999 survey is left open for anyone to inspect.

The questionnaires were safely flowing back to the *SOM Institute* during the fall of 1999 (Figure 6.1). The response rate according to the methodological appendix is 67 per cent, a number that the author suggests is sufficiently high. In a brief discussion of response rates, the author references "Ohlsson 1986"[8], who states that "the response rate of social scientific postal surveys are generally between 60 and 70 per cent, depending on geographical area and types of respondents" (Lithner 2000: 398)[lxvi]. What response rate is sufficient, and who decides? At what moment can a social scientist know when the survey works? As I showed in Chapter 3, we have to go back in time to examine the debugging, tinkering, patching and analyzing of these progressive steps that urged the researchers to assemble letters, postcards and telephone calls in these specific configurations. Even though the postal survey was made to work in 1986, it still comes with the problem of response rates, a problem that has no single solution, not even today (see the introduction of Chapter 1).

Configuration of the 1999 interface

In 1999, the *SOM Institute* needs not only to create interfaces that will allow it to interact with other entities but it has itself become an interface, one that is able to translate a multitude of

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[8] Anders Ohlsson's book *Att svara eller inte svara - det är frågan* is further introduced in Chapter 3.

social scientific research questions into powerful and convincing quantitative facts. Returning to the preface of the 1999 report, we can see that the 25 authors of the volume are social scientists. Each author is responsible for his or her own findings. The authors are listed in the last pages of the report, and almost all of them are affiliated with the Gothenburg Faculty of Social Sciences.

Moreover, there is a passage in the preface that addresses the organizational framework of the survey. The data are stored at SND[9], a national repository for the collection of data within the social sciences. The *SOM Institute* is presented as a collaborative endeavor undertaken by the departments of Political Science, Journalism and Mass Communication, and the School of Public Administration at the University of Gothenburg. The work of these departments makes the *SOM Institute* a "research and educational organization".

The 1999 SOM survey is the work of 25 individual social scientists, and 21 participating research projects (Holmberg and Weibull 401ff). The individuals and teams are able to complete and fulfill their research goals by conducting the survey, contributing funds and expertise and producing high quality results. Thus, the survey needs to be sufficiently plastic to encompass such heterogeneous topics as health risks, mass media and public opinion, gender and political violence, comparative research on stereotypes in the European Union, and others. The two questionnaires are large enough to mediate the research interests of more than 20 research projects. This way, the *SOM Institute* can function as an interface that enables quantification of a multitude of social scientific research questions.

However, the survey is not exclusively affiliated with research projects and institutional frameworks. The links of the scien-

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[9] Today SSD has been reorganized into *Svensk nationell datatjänst* (SND), including not only data from the social sciences, but also from the humanities and social medicine/epidemiology.

tists involved may include additional projects that only par-
tially use the SOM survey as an epistemic vehicle. For example,
Ulf Bjereld, a Gothenburg-based political scientist, published
the monograph *Gender and political violence* (Kön och politiskt
våld) in 1998. While Bjereld assisted with the 1999 SOM survey
via a research project with the same title as the monograph, the
latter publication also includes interviews with children and data
from surveys that pre-dated the *SOM Institute*. The SOM sur-
vey can thus function as a component of various social scientific
investigations by raising a smaller empirical study to a broader
societal level, if what is measured in the previous surveys is com-
patible with the focus of the new survey. In this case, Bjereld
compares the SOM data with the Election Surveys[10] dating back
to the 1960s and with newer, smaller surveys.

   This reinforcement, as I discussed in Chapter 3 in Collins
and Pinch's example, is itself part of an epistemic practice that
contributes to the shaping of the SOM survey. If it is not consis-
tent with the past and future research, the survey stands alone.
Debugging the survey such that it *interfaces* with other surveys
is thus a very important task. Moreover, the *SOM Institute* is
further reinforced for each successful research project or publi-
cation that uses their data. The more facts that circulate, the
more allies the SOM survey is able to associate with itself.

6.2   The limits of representation

The recurring survey of the *SOM Institute* is a territorial assem-
blage. It is situated in time and space and it works by rendering
particular aspects of Swedish society visible by way of quantifica-
tion. There are, however, insides and outsides of what is possible
to know about society. The limits of representation are defined
and reinforced through the process of blackboxing. The 1999
SOM survey is resting on a bedrock of sedimented practices that

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[10] For a brief introduction to the Election Surveys, see chapter 4

defines its borders, and it is worthwhile to reflect a bit deeper on where these borders are drawn.

In the social sciences, there are basic notions, such as that of norm systems, values and public opinions, general assumptions that help characterize the society. However, norm systems, for example, cannot be measured directly in the empirical domain; they cannot be objects of study as such. Instead, they are hidden or unconscious forces, and can only be described indirectly by measuring behavior, views, or judgments. In the positive domain of knowledge, quantitative sociology is able to use statistical tools to generate general measurements and statements. Consequently, in the modern social sciences, it is reasonable to say that underneath people's views on the importance of political engagement, there are underlying norms that *structure* how the individual will respond once surveyed.

Similarly, although more generally, one might say that behind people's behavior are routines that influence how the routines are patterned. For example, voting behavior may be influenced by one's position on the continuum from communitarianism to individualism, one's religious habits may depend on whether one subscribes to traditional or progressive norms, and trust in social institutions may be a function of changes in values over time. In other words, there exist theoretical social scientific explanations that must be operationalized and broken down into measurable yet coherent pieces. However, for such knowledge to become perceptible as scientific facts, researchers in the social sciences need to conduct surveys and take measurements, and the respondents must agree to participate in these surveys and interviews. It is inside these practices that we need to look if we are to find the limits of survey-based knowledge.

Domain of imperceptibility

In her book *Sick Building Syndrome and the Problem of Uncertainty*, Michelle Murphy introduces the concept of "domains of

imperceptibility" as follows:

> I call the regular and sedimented contours of percep-
> tion and imperception produced within a disciplinary
> or epistemological tradition its "regimes of percepti-
> bility" [...] Produced by assemblages that are an-
> chored in material culture, regimes of perceptibility
> establish what phenomena become perceptible, and
> thus what phenomena come into being for us, giving
> objects boundaries and imbuing them with qualities.
> Regimes of perceptibility populate our world with
> some objects and not others, and they allow certain
> actions to be performed on those objects. (Murphy
> 2006: 24)

What the *SOM Institute* has been able to do over the years is
to create a regime of perceptibility that "gives objects shapes",
boundaries and qualities. By studying the contours of these
regimes, we can carefully outline what Murphy calls "domains
of imperceptibility" (Ibid.: 9), those that falls outside of knowl-
edge produced under certain conditions. This domain, I argue,
is primarily produced by the blackboxing of surveys. Regimes of
perceptibility render visible which entities that are able to speak
and how they can have an opinion in the first place. Some el-
ements fall inside the scientific regime, and others fall into the
domain of imperceptibility. By returning to the problem of re-
sponse rates, the question of whether people are willing to re-
spond to actual questionnaires or not, becomes a critical point
in the functioning of these regimes of perceptibility.

When the questionnaires reach the respondents via mail, they
instruct or *command* the selected person to fill them out and
return them to the researchers. Deleuze and Guattari (2004)
called this an "order-word assemblage".[11]

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[11] See the chapter "November 20, 1923: Postulates of Linguistics" in Deleuze
and Guattari (2004).

The survey is assembled in such a way that it launches a program of action which may either be obeyed or disobeyed, as the authority of these commands is derived from other social bodies. Order-words, such as questionnaires (see Figure 6.2), are performative. For example, a judge who is convicting a suspected criminal is able to do so,only if the words "guilty" or "not guilty" have a particular meaning withing the judicial system, whose authority is generated by the power of the law and the state. The same is true of scientific assemblages. It is only meaningful to participate in science if science is trusted as an authority, and the social sciences can only produce facts if they are regarded as objective by other bodies. Thus, a questionnaire does not merely ask a person to fill out a form; it instructs the person to do so and asks him or her to participate in science as a member of society.

If a person responds to the questionnaire, an *incorporeal transformation* takes place; the respondent's daily life is interrupted such that he or she can respond to the survey. The *becoming study-object* is thus a struggle between many different regimes of everyday life, or as Callon puts it, a matter of "interdefinition", including devices of "interessement" and enrollment that always includes the risk of "betrayal" (Callon 1986). These series of negotiations, in which the social scientists must constantly persuade a population to accept the incorporeal transformations through which they become study-objects, are primarily productive; they create a regime of perceptibility in which it is possible to quantitatively represent the norms and values of the population. These negotiations take place when the questionnaires initially reach the respondents at the receipt of the reminder letters and during the telephone calls.

The notion of persuading respondents is a central theme in Sarah E. Igo's study of the pioneering Gallup surveys in the US. Igo argues that the early pollsters "install[ed] the opinion survey as a permanent technology of American democracy" (Igo 2011:

Figure 6.4: Questionnaire used for visiting interviews in the 1985 Election Surveys. Published in Holmberg, Gilljam, Oskarsson (1988: 335)

303) from 1936 and onward. Presenting an argument similar to mine, Igo writes:

> Gallup and Roper needed continually to persuade individuals to submit to their questioning. The foundation for individuals' willingness to answer pollsters' queries was, in Gallup's public statements anyway, "the average American's belief that what he thinks is important; that expressing himself is part of his birthright" (Gallup 1941, 3). Gallup claimed that "nine people in every ten" were willing to talk to pollsters and, moreover, were eager to furnish their opinions (1941, 3). (Igo 2011: 239)

However, Igo also discusses the problem created by the individuals who refused to answer the polls. "Nonresponse, a contemporary researcher reported, was 'most frequent among poor people, women, and in large cities'" (Igo 2011: 239). The territorial reach of Gallup and his pollster colleagues thus exhibited the same basic limitation (although they did not use random sampling): there is a certain domain withing the territory (or a certain part of the population) that is imperceptible to the survey.

The nonresponse problem manifested in a similarly in Sweden when the Gallup methodology was introduced in the 1940s and created ramifications for how public opinion was negotiated conceptually during the first major survey performed with large portions of the Swedish population. The 1950 study *Gallup och den svenska väljarkåren* (Gallup and the Swedish electorate), as previously mentioned in Chapter 4, is not a theoretical treatise on the nature of social reality. Rather, it is an methodological collection of essays that carefully struggle to create rigorous social science from the data collected by a pollster over several years.

One of the authors, social psychologist Torsten Husén, drew his definitions from the contemporary American social psychology of Louis Thurstone, Rensis Likert and Louis Guttman, scientists whose work would become paradigmatic in its use of scales and measurement techniques in quantitative (social) psychology. Husén wanted to use the concept of *attitude* rather than that of public opinion. "In American literature one ceaselessly find the expression 'public opinion' [English in original], as if that was something strictly defined and made clear" (Husén 1950: 108)[lxvii]. Instead, the basis of collective opinion is indirectly based on the subject expressing certain views. Husén provides an example:

> If a person advocates a return to religion to save society, if he is of the opinion that extra-marital sexual relations should be prevented by all means, if he is of the opinion that our children's upbringing and prison care are too lax, then we can conclude that this person is of a rather strong conservative disposition. (Husén 1950: 109)[lxviii]

Conservativism may thus be derived from a set of attitudes that supply responses to a set of questions, which in turn can be integrated into the epistemic assemblage of a survey. In this way, the indirect and underlying attitudes are expressed verbally as behavior, a *positivity* that can be measured empirically.

However, every such investigation also contains a negativity: the non-respondents, who determine the response rate of a sample. Because they cannot be in a definite, empirical sense, these individuals need to be interpreted. Husén continues, "What the response rates in the best cases mirror, are the reactions to a certain, verbally expressed question, and not the immediate reaction to the actual matter the question concerns" (Ibid.: 120)[lxix]. As the interviewer encounters his or her study objects, the latter react to his or her very presence: "[...] one must keep in mind the

particular social psychological constituted by the Gallup interviewing. The interviewee victim not only reacts to the question, but also to the interviewer" (Ibid.)[lxx]. As described in Igo's study of the American Gallup surveys, the Swedish researchers struggle with certain groups; for example, "the normal relation according to the average is thus that women to a greater extent constitute a relatively larger do-not-know group than men" (Edstrand 1950: 200)[lxxi]

This is how we can establish what is rendered visible, what falls into the domain of imperceptibility. It is more important, however, to see how this problem is progressively resolved once the epistemic assemblage works.

Statistics as a Tool for Response Rate Accumulation

The proper reconstitution of science and society through encounters with social reality depends on the willingness of people to respond to questionnaires. In the 1980s, the *SOM Institute* was very successful in conducting national surveys, easily attaining 70 per cent response rates. However, to achieve this number, which is crucial for every statistical measurement, the researchers at the *SOM Institute* had to apply their sociological skills to another type of work. In the early days of the institute, books were written on the relationship of surveys to the behavioral patterns of the respondents (Ohlsson 1986). To solve the puzzle of why some people never replied to surveys, the SOM researchers themselves applied a sociology of rational choice based on Talcott Parson's distinction between instrumental and expressive action. Combined with a model of mass communication, in which the actual survey was described as a message and the respondents as the receivers, this perspective allowed the SOM researchers to behave as Callon's engineers, using sociological skills to generate concrete innovations. Structural functionalism and theories of mass

communication were used to "get to the world" rather than describing it.

The methodological text written by Ohlsson divides the respondents into segments based on age, profession, political interest, and other qualities. It then discusses the costs and benefits of responding to the survey and describes additional methodological obstacles that must be accounted for once the respondents have replied to ensure validity. For instance, young adults have unstable identities because they are seeking a position in society, middle-aged individuals have stable identities but are busy with work and reproduction, and, retired people often lose their stable identities because they feel empty as they leave their professional lives behind (Ibid.: 46ff). Thus, knowledge and practice are tightly interwoven, and using both creates *of* practice. However, the true trial of strength lies not in the methodology itself but in the real-world encounter between the SOM researchers, their approach and the reactions of the respondents.

In observing the composition and development of the instrument used by the SOM researchers (Figure 6.2), we see the engineering skills used withing sociology manifested in the response rates attained. A survey involves leg-work sociology, in which a direct interaction with the study object takes place. The survey, sent via mail, must be followed by progressive programs of action (Latour 1992c) that, include reminder postcards, more surveys, and finally telephone calls that encourage the respondents to complete the survey and send it back. The SOM-researchers' model of mass communication involves letters and postcards that are sent to 2x2,800 homes, and the rational-choice model is thus expressed in the flow of returned survey forms. In its entirety, this concrete assemblage, which is neither pure theory nor merely series of letters and postcards, generates a sufficient response rate for the researchers to produce reliable statistics. The 67 per cent response rate is sufficient according to the criteria used in quantitative social science, which had been active since 1947 in

Year	Freq											
1999	67%	AP	S	P	LS	P	TR	LS	TR	LS	TR	LS	MS
1998	69%	AP	S	P	LS	TR	TR	TR	LS			
1997	69%		S	P	LS	TR	TR	TR	MS			
1996	69%		S	P	LS	TR	TR	TR	LS			
1995	65%		S	P	LS	TR	TR	TR				
1994	67%		S	P	LS	TR	LS	TR	TR			
1993	70%		S	P	LS	TR	LS	TR	LS			
1992	71%		S	P	LS	TR	LS	TR	LS			
1991	67%		S	P	LS	LS	LS	TR	LS		TR	
1990	66%		S	P	LS	LS	LS	TR	LS			
1989	66%		S	P	LS	LS	LS	TR	TR			
1988	68%		S	P	LS	LS	TR					
1987	70%		S	P	LS	LS	LS	TR				
1986	68%		S	P	LS	LS	TI					

Figure 6.5: Programs of action. *Explanation*: S - Survey, P - Postcard, AP - Announcement postcard, LS - Letter and survey, TI - Telephonic interview, TR - Telephonic reminder, PC - Postcard, MS - Mini Survey.

Sweden.

Over time, this assemblage is radically expanded as, in order to achieve a response rate higher than 60 per cent, the *SOM Institute* had to load more and more actants into it. Although the 1999 survey was described as a success, the SOM researchers cannot take society for granted, and throughout the annual fieldwork the method is constantly evaluated.

Callon (1987) argues, rather provocatively, that engineers developing an electrical car in France in the 1970s were actually better sociologists than sociologists themselves, since they were doing social analysis as a matter of life and death as part of their engineering project, rather than having to defend their ideas in an academic arena. In order for the engineer-sociologists to succeed, their sociological assumptions on consumer markets, social stratification, and lifestyles needed to be absolutely correct. Oth-

erwise their technological project would fail and all investments would be in vain. In short, engineers are in Callon's view *reconstructing* society rather than merely describing and defending an interpretation of it.

In 1999, the response rate is 67 per cent; however, increasingly more programs of action are needed for the institute to achieve high response rates. While the engineer-sociologists in Callon's story failed in linking together consumers and fuel cells and electric vehicles, the SOM researchers are quite successful in terms of their survey response rates.

This progression of keeping up response rates each year, makes the *SOM Institute*, as a territorial assemblage, look more and more like a center. There is no need to treat the social sciences differently than the natural sciences. As scholars within STS have argued, laboratories (Latour 1983), bubble chambers (Pickering 1995) and laser-devices (Collins 1992) shape productive regimes of perceptibility by translating social interests, intermingling human and material agency and being using tacit knowledge to generate scientific facts. The quantitative social sciences do the same regardless of their apparent disembodiedness, with their reliance on statistical graphs and tables. Regimes of perceptibility render certain entities able to speak while leaving others behind.

6.3   Returning to centers of calculation

The assembly process, as described in previous empirical chapters, seems to have stabilized in 1999. However, when inspected more closely, we still find struggles and negotiations to keep response rates high enough. Consequently, what we have is a territorial assemblage, which repeatedly performs surveys. These surveys pile up and make possible a cumulative history of facts, used on the one hand to describe changes in society over time and on the other hand to reinforce each new survey through

methodological self-referentiality. However, each year the *SOM Institute* needs to perform a survey, which must be calibrated and debugged to function.

This assembly process indicates that the *SOM Institute* closely resembles what Latour calls "centers of calculation". This concept appears in the last chapter of *Science in Action* (1987: 215-257), where it is clarified that centers of calculation are logistic hubs that collect inscriptions from far away and then bring them back to a particular place where they can be stored, classified and calculated. The reach of a center of calculation is a function of the size of its networks. For example, a natural history museum is able to gather species from destinations as far away as the museum can send collectors or perhaps from even further away if they have the resources to buy them from a foreign country. The Hubble Space Telescope, in low orbit around the world, transmits images of stars to an antenna in New Mexico at which point the data are then distributed to the principal investigators and to other scientists, with some of the beautiful images circulated to the news media. Only when data are in a repository can it be quantified, compared, calculated and progressively transformed into scientific facts. A social science institute, a pollster and a census bureau are no different in the sense that they run the logistics of inscriptions back and forth. Their territories, however, are very different.

As previously mentioned, there is a large amount of research within history that has analyzed the link between statistics and the state. For example, Alain Desrosières places statistics in a central position as having a *unifying* and *administrating* function, especially in producing the norms and standards that facilitate the practical management of the state (Desrosières 1998: 147ff). In general, census bureaus, pollsters and social scientific institutes are national and have their networks that extend to the borders of the nation, although there are also global and regional surveys (such as the Eurobarometer and the World Value

Survey or Gallup International), which national centers of calculation may participate.

As of 1999, the *SOM Institute* has built successful interfaces that translate quantitative facts into newspaper articles and scientific articles, which make alliances possible for different research projects and are able to use pollsters and private contractors for field work. Moreover, the *SOM Institute* has established itself as a territorial assemblage that transforms the "microsociety" of a statistical sample into and indicator of norms, values, attitudes and public opinions on a "macro-society" level. The postal survey contains both black boxes, such as how to conduct a simple random sample. However, the box must be reopened each year to analyze the non-respondents, as previously outlined in the mid-1980s. Its identity as a territorial assemblage that translates scientific facts and blackboxes its methods qualify the *SOM Institute* as a center of calculation that exists still today, covering even wider territories and expanding into new areas of research.

Chapter 7

Reflections and conclusions

In addition to returning to my three aims, in this final chapter I will reflect upon the politics of quantification and the role of the social sciences in the constitution of Swedish society. Moreover, I will discuss the importance of studying the social sciences within Science and Technology Studies.

Introduction

Perhaps one of the most provocative pictures of the history of the social sciences appears in Latour's article "When things strike back: a possible contribution of 'science studies' to the social sciences", which was published at the turn of the millennium. It deserves a lengthy quotation because it is rare to find examples of this type of writing in STS.

> Most of the social sciences were invented, a century ago, to short-cut political process after many years of insufferable civil wars and revolutionary strife. If we have a Society that is already composed as one single whole and which can be used to account for

> the behaviour of actors who do not know what they
> are doing, but whose unknown structure is visible
> to the keen eyes of the trained social scientist, it
> then becomes possible to embark on the huge task of
> social engineering in order to produce the common
> good, without having to go through the painstaking
> labour of composing this commonality through polit-
> ical means. (Latour 2000: 117-118)

This line of argument is consistent with Latour's notion of a purification process in which science and politics appear dislodged from each other (as discussed in Chapter 2). The paradox can be explicated as follows: on the one hand, the social sciences construct a domain called society, in which facts are pure and positive as long as the epistemic means and methods are available to study them. On the other hand, through social engineering and reasoning toward a common good, these facts short-circuit traditional politics and return as highly political facts from which we can make decisions based on scientific rationality. Once facts are hard enough and durable due to blackboxing, they are able to circulate outside of scientific practice. When facts are disconnected from their original negotiations, they appear pure, at least until they are contested.

Throughout this thesis, I have attempted to describe how this double-edged process has occurred, with a special emphasis on quantification and the empirical case of the *SOM Institute*. It is time to present my results and reflect on where these findings may lead.

7.1   The quantification of society

The making of the first Election Surveys in the 1950s depended heavily on the interface between the sciences and the state. To be more precise, Westerståhl and Särlvik were successful in trans-

lating the interests of not only political parties but also other organizations. The first surveys required funds and resources; in exchange they provided facts on election behavior and propaganda.

The results of the 1954 survey were presented to Swedish society in the fairly remote town of Borås (Petersson 2011: 171) instead of in the capital. The 1954 survey was conducted according to the Lazarsfeld model. Although the survey was presented in Borås because this was the place where the participants had been interviewed, the similarities between Borås and the equally remote town of Sandusky, Ohio, where Lazarsfeld and his colleagues had conducted the survey for *The People's Choice* (1968: 3) in 1940, are quite striking.

At the time, it was considered unproblematic that Särlvik planned the election campaign for the *Social Democrats* while simultaneously performing social scientific surveys that measured the effects of such propaganda (see Petersson 2011: 185ff).

Another case that clearly illuminates this strong state-science interface in this historical era is the work on the Romani minority by sociologist Bertil Pfannenstill (1948). During the spring of 1944, the *National Board of Health and Welfare* (Socialstyrelsen) commissioned a census of "tattare", a term for the Romani minority that is currently considered derogatory. According to the census, there were 8,000 "tattare" living in Sweden. In this investigation, scientists were commissioned to conduct further research on this population. For example, racial biologists conducted "anthropological" investigations of 60 individuals. However, these results were considered inconclusive (Pfannenstill 1948: 226ff).

In Borås, Pfannenstill began to conduct interviews with the individuals registered in the *National Board of Health and Welfare* census. His results were critical of the very definition of "tattare" used by the census, and he suggested that the definition should be sociological. Pfannenstill stated that the "tattare"

shared a particular ethos that had social rather than "anthropological" causes. Moreover, he argued strongly against the forced sterilizations that had occurred for half a decade (and would not be abandoned until 1976; see SOU 2000:20: 15). The state had identified a social problem, and scientists attempted to solve it.

After the Second World War, the social sciences expanded in Sweden and became part of a general model of a planned society, a process sometimes referred to as *social engineering* (Fridjónsdóttir 1991). This integration is reflected in the often programmatic statements made by social scientists. Torgny Segerstedt of the Uppsala School of Sociology wrote the following in the first volume of *Acta Sociologica*:

> [...] because of the rapid change in social structure the need for sociological research is very great; but at the same time we find a lack of understanding of the fact that sociology as an empiric scien[c]e requires both men and money in order to carry out its research. (Christiansen et al. 1956: 1)

Thirty years later, however, the SOM researchers wrote,

> It is open to debate, the amount of resources which should be allocated in periodical surveys. However, the main idea of the SOM survey is the fact that yearly surveys provide the most valuable material for social scientific research. (Holmberg and Weibull 1987:4).

Two paradigmatically different arguments are made here. The Uppsala School claimed that society itself needed sociological research due to its rapid changes. The SOM researchers, in contrast, held that research needed to be conducted for the sake of the research itself.

In 1956, the department for finances established another commission to investigate the centralization and reorganization of

Swedish statistics. This commission is interesting in two major ways. First, it contained a plan to unite the production of statistics in Sweden in a very technical sense. It suggested strict unity in definitions of measurement, data collection and storage. It also outlined a computerization of Swedish statistics that would allow more efficient treatment of data and would facilitate the creation of sample surveys. The investigation began as follows:

> The development of our society could be described as an ambition within all activities towards increased rationality. What used to be traditional craftsmanship is replaced with carefully studied technologies, and the estimate judgment has moved on to precise measurement and quantitative correlations. Education is gradually steered towards training the population in increased rationality in their occupational lives. (SOU 1959:33: 13)[lxxii]

This development is similar to what occurred in France during the 1940s and 1950s (Desrosières 2005), when the state provided centralized statistics as an integral part of building a welfare state. The social sciences in Sweden were included in these structural changes. Fridjónsdóttir notes that the role of the social engineer was both an actual working condition and a common self-image among sociologists (Fridjónsdóttir 1991). Anna Larsson confirms this suggestion in a study in which she argues that the practical uses of sociology served as a legitimizing strategy in the post-war times (Larsson 2001). In many cases the tasks delegated to the social sciences were defined by attempts to make the developments of the welfare state as smooth as possible.

If we move on to the literature of the 1980s, we find something different than the rational social engineering of the earlier days. In 1986, the SOM researchers claimed epistemic author-

ity. In *Inom felmarginalen*, Holmberg and Petersson analyze the methods of the pollsters and argue:

> [M]easurements are not simply mirrors of fixed political views — public opinions are formulated in the very process of measurement [...] if these measurements were to become a central component in the democratic process, it is of uttermost importance to know who is asking the questions. In the "Gallup-democracy" the political power is not with the people, but rather with Gallup. (Holmberg and Petersson 1980: 244)

In the 1980s, the social engineering that was present at the birth of survey research had diminished. Instead, the authority of science was manifested by the creation of borders. Holmberg and Petersson's strategy was almost reversed compared to the era of social engineering. Pollsters were criticized because they were *too* political. During the 1985 elections (as described in Chapter 5), Gallup was owned by the *Social Democrats*, and *Sifo* was a private pollster whose chief, Hans Zetterberg, was accused of sympathizing with the *Moderaterna*. Following Latour's notion of purification, this situation marks a moment in which survey-based social science attempted to dislodge itself from politics and instead invented a position in which epistemic authority arose from academic science.

A center of calculation requires productive resources. Unlike commercial pollsters, centers conducting academic research can draw these resources from the state (via other mediators, such as research councils and departments), although they cannot as easily sell data to the private sector and preserve their autonomy. This state support requires that the output of the survey be regarded as valuable in a specific way. The output must be shaped to comply with external demands, such as scientific ideals and public access. To produce these scientific facts,

the surveys must (be made to) work, and the logistics of the questionnaires must function. Moreover, for these facts to be recognized specifically as scientific facts, the border between science and non-science and the between commercial and academic research must be safeguarded.

In 1985, the newspapers frequently featured opinion polls that attempted to predict the outcome of the coming September elections. The pollsters had been in Sweden since 1942, when the Swedish Gallup Institute conducted its first surveys, inspired by the popularity of election predictions in the United States. Already in the 1940s the Gallup pollsters and academic scientists showed a mutual interest in explaining voting behavior. The 1950 work *Gallup och den svenska väljarkåren* became an early example of how academic social scientists were allowed to use the pollsters' data, albeit with some skepticism. Westerståhl noted the potentially unscientific elements of the Swedish Gallup Institute: "Even if the Gallup method could not be used in any other contexts with satisfactory scientific accuracy, it appears that the possibilities it offers within this area makes its use motivated on its own." (Westerståhl 1950: 58)[lxxiii]. However, a great amount of logistics is involved in collecting data, as outlined in Chapter 3, even during the 1970s, the research project *Dagspresskollegiet* used the *Testologen/Sweden Now* survey to measure newspaper readership. Thus, social scientists have often shown an interest the activities of pollsters on a practical level and occasionally used pollsters' data.

This co-operation can also be identified during the later development of the *SOM Institute*, which, with a few exceptions, has relied on external contractors for the logistics of sending and receiving questionnaires. The border between academic science and pollster research is sometimes distinct. However, both areas share a common origin in the Lazarsfeld technique of quantification, and their results are often mentioned in the same public arenas.

7.2   Social sciences, STS and the aims of the thesis

In the field of STS, the natural sciences have been seen as co-produced with society at large, but what about the social sciences? Are they essentially soft sciences, saturated with politics from their inception? What about the use of statistics? In one respect, it would be unproblematic to think of statistics as an ordering device for the state, if we continue to call it political arithmetic. However, the uses of statistics have changed dramatically in another respect: statistics have become, at least in some forms of practice, the epistemic flagship of the modern sciences, in biology, physics, informatics, or sociology. Although there is a common sense type of criticism in the saying "anything can be proved with statistics", it may be an understatement to say that statistics in the modern social sciences convey "scientific" and "objective" work (Porter 1995). In an ideal form, which never exists in practice but has bearing on field research, statistics may be considered as "royal science" that is metric, ideal, and submits the world to constant laws (Deleuze and Guattari 2004). However, statistics represent science not only by symbolizing aspects such as traditions, methods, and results but also by standing as the material representative of institutions, as centers of calculation. Furthermore, statistics can be seen as influential in political decision-making (Fridjónsdóttir 1991) and in inscribing perceptions of social order that affect everyday conceptions of the composition of society.

However, there is more to the social sciences than numbers and charts; it also involves qualitative categories that describe the nature of humans, society, language, emotions, and behavior. These cannot be provided by statistics alone, although the results may differ when they are quantified.

Studying quantification in the making enables us to look more closely at the shaping of the social sciences. What we

see is not so much sociologists' description of social reality, but rather sociologists' reconstruction of social reality, which allows them to accomplish work, by debugging their research instruments. By studying the uses of certain methods and techniques, we are able to describe the relations between the social sciences as academic disciplines and the modes of knowledge that render certain social and historical contexts possible.

I have argued that the objects of study in social sciences emerge through epistemic practice rather than through a priori definitions or unmediated empirical experience. By assembling different regimes of perceptibility, society is constructed and rendered knowledgeable through certain conceptual, material and quantifiable devices. To understand how notions such as public opinions, norm systems or attitudes can be articulated in their generality, which is crucial to the function of the social sciences. I have argued that one must depart from this level of concrete epistemic assemblages, to be better equipped to analyze and reflect upon the role of the social sciences and their integration into the trajectories of social history.

Thus, I have returned to my three aims of this thesis. I have described a particular line of contemporary history concerning survey-based social science. I have used and further developed a set of useful theoretical tools to navigate through this type of knowledge production, and I have identified and engaged with critical elements in STS and the theory of science. Together, these aims have brought me closer to understanding the quantification of society.

7.3 1912 — Outro

In 1912, medical doctor and professor in psychiatry Bror Gadelius published two short essays entitled *The Spiritual Life of the Masses* and *Spiritual Contagion* (Massornas själsliv och andlig smitta). He presented a sociological imagery heavily inspired by

the works of Gabriel de Tarde (1846-1904) and Gustave Le Bon (1841-1931). This type of sociology was preoccupied with the problems of hysteria, the affects of the masses and the possibility of social conformity.

Gadelius argued that the "spiritual unity" of a mass was caused by "imitative suggestion" in modern societies that emerged in the form of hypnosis and led to an inferior, dangerous state of mass behavior and conformity. Drawing on historical anecdotes of the plague in Europe during the Middle Ages, Gadelius concluded that the irrational character of human actions caused "cowardliness" and "swarm behavior", in which people chose to follow leaders rather than thinking for themselves. Modern society was infected by sick, imitative social patterns and. Just as the Great Plague had caused death and destruction, spiritual contagion weakened society:

> The mass is a terrain, where the bacteria of evil germinates with ease, whereas the bacteria of goodness usually dies out of malnutrition. (Gadelius 1912: 21)[lxxiv]

Alhough the sociological problems discussed by Gadelius were typical of the time, they were never realized as an empirical science. Despite Gadelius' influence in the fields of modern psychology, which paved the way for reforms in clinic psychiatry, the introduction of the French sociological theories of Gabriel de Tarde and Gustave Le Bon failed to become established within academic traditions in Sweden. After the Second World War, the social sciences took another path.

What constitutes a social problem in a given historical context is not determined merely by the circulation of certain ideas or beliefs. Rather, as I have shown in this dissertation, the path of social scientific ideas depends on whether they can be embodied in surveys, questionnaires, research institutes and quantifying methodologies.

The sociological ideas of Gadelius have not been completely forgotten, even though the particular essays introduced above are found in very few university libraries. The "spiritual contagion" of the masses did not survive as an adequate social scientific concept.

Instead, the quantification of Swedish society took a different historical path. I have described one particular line that led to the *SOM Institute*. These centers of calculation, which fill our lives with numbers that describe the society in which we live, were created and have multiplied under certain historical conditions. Understanding these conditions — how they were composed, assembled and made to work — is an important aspect of understanding modern societies. However, it is equally important to understand how the quantitative social sciences and the knowledge they generate feed back and re-compose the modern societies.

Original Quotes

[i][...] och det syns ingen ljusning på den statistiska bortfallshimmelen. Dels har den moderna telefonin gjort det svårare att få tag i människor, och dels är vi numera ett enkättrött folk, berättar Åsa Nilsson som projektleder den stora SOM undersökningen, postenkäter till nio tusen personer. - Vad vi ser är någon typ av enkättrötthet i samhället. Det finns just så många undersökningar, det finns väldig mycket marknadsundersökningar.

[ii]Forskarna talar om en Göteborgseffekt och en långsam normförskjutning. Den innebär att en ökad utbredning av korruption tycks leda till en ökad acceptans för korruption. Det visar nya resultat i en ny rapport från *SOM Institutet* vid Göteborgs universitet, som presenterades vid en presskonferens i går.

[iii]"Här finns allting. *SOM Institutet* är ju ett vetenskapligt institut där vi verkligen jobbar med metodutvecklingen [...] Det är alltså 9000 i urvalet och vi gör alltså tre emissioner och frågeformulär. Ett lite mera politiskt, ett lite mera åt medier och kultur, ett lite mera mot livsstil och hälsa. Våra basfrågor i SOM-undersökningen finns med i alla dom här tre formulären [...] Det är inget ni behöver veta, det framgår här. Men är man även här intresserad utav att botanisera, så har ni [...] ni har alltså från sidan 595 dom tre frågeformulären in extensum redovisade i boken.

[iv]"Vi har inspirerats av amerikansk forskning omkring konsulter och politik när vi har hittat på våra mått, våra nyckeltal, som sagt var på sidan 109. Det är name recognition, man måste känna till en företeelse för att kunna ladda det här namnet med värden. Nummer två, bedömning, inte av förtroende nu som vi mäter i andra sammanhang, inte hur nöjd man är personligen, utan bedömning av job performance, hur man tycker att det jobb som man förväntas göra, hur man sköter det. Det kallas på amerikanska för job performance.

[v]"- Om vi vet mer om hur personlighetstypen ser ut kan vi ju både förebygga problemen och hjälpa personen som behöver stöd, säger forskaren John Magnus Roos.

[vi]Det har under senare år blivit allt svårare att nå en hög svarsfrekvens och därtill har SOM-undersökningarna successivt ökat i omfång. Årets siffra måste alltså betecknas som hög. I 1999 års undersökning blev svarsfrekvensen 67 procent, vilket motsvarar genomsnittet av de fjorton hittills genomförda Riks-SOM-undersökningarna [...]

[vii]"Från och med 2000-talet har nivån emellertid sjunkit. Om genomsnittet fram till och med 1999 var 68 procent, var resultatet för undersökningarna under 2000-talets första decennium 63 procent. 2008 års undersökning var den första med ett resultat under 60 procent, vilket även gällde 2009. Årets undersökning nådde dock åter 60 procent.

[viii]"Den sämre svarsfrekvensen hos unga grupper är särskilt tydlig bland unga män. Hos män i åldrarna 20–29 år är svarsfrekvensen 36 procent, att jämföra med 48 procent hos kvinnor i motsvarande åldersgrupp.

[ix]Beräkningen av Peasons r är minst sagt krånglig [...] Även om det kan vara nyttigt att någon gång själv genomföra beräkningarna [...] - det är inte så länge sedan forskarna var tvingade att anställa assistenter för att överhuvudtaget kunna genomföra sådana här beräkningar - är det förstås datorerna som beräknar Pearsons r åt oss.

[x]Personer med invandrarbakgrund har klart mindre förtroende för "statsmakten" och svenska myndigheter än vad svenskar utan invandrarbakgrund har. Skillnaden är mycket stor när det gäller förtroendet för polis, sjukvård och domstolar. Kungahuset och svenska kyrkan åtnjuter också ett lägre förtroende. Invandrargrupperna har däremot ett klart större förtroende för institutioner som står "fria" från staten som storföretag, banker, universitet och högskolor samt radio och TV.

[xi]"Vid undersökningar enligt gallupmetoden stöter man ofta på ett slags cirkelproblem. Innan undersökningen igångsättes måste man förutsätta, att det finns en opinion angpående det berörda spörsmålet, och för att kunna formulera en lämplig fråga måste man vidare göra vissa antaganden om denna opinions beskaffenhet. Men det är ju först sedan undersökningen slutförts, som man kan erhålla kunskap om opinionen existens och utseende!

[xii]När skall man nu anse att en opinion är för handen? Var går gränsen till vet-inte-gruppen, den grupp som inte har någon opinion? [...] Icke heller når man mycket längre genom att definiera begreppet opinion så - vilket i och för sig är möjligt och dessutom skett - att begreppet kommer att omfatta praktiskt taget alla slags tankar och meningsyttringar.

[xiii]Erfarenheten av enkätmetodik finns sedan länge vid statsvetenskapliga institutionen. Det ovan nämnda forskningsprogrammet Dagspresskollegiet använder postenkäter för sina lokala läsarundersökningar, liksom förvaltningsforskningen arbetat med enkäter i flera lokala kommunstudier. Institutionen har även haft kontakt med undersökningsorganisationer som *Testologen* och *Forskningsgruppen för Samhälls- och Informationsstudier* (FSI, Torsten Österman) samt Stiftelsen för opinionsstuier (Sten Hultgren) vilka alla anser sig ha goda erfarenheter av enkätmetodik.

[xiv]Helt oväntat var att "postenkätmetoden" gav ett pålitligare svarsmaterial än "besöksintervjuerna", i stället för att äventyra undersökningarnas representativitet.

[xv]Normalt är att 30 a 35% av de tillskrivna personerna aldrig svara vid enkätundersökningar [postal surveys] medan motsvarande andel vid intervjuer brukar ligga vid 15 till 25%.

[xvi][...] får nästan intrycket att något slags hyvel skurit jämna och nästan liktjocka skivor från norr till söder, frön öster till väster.

[xvii]Statistiska centralbyrån har tecknat avtal med Stiftelsen om att fortsätta enkäterna med samma uppläggning och genomförande som hittills och att överlämna materialet till SSD.

[xviii][...] ett par sidor av totalt 20 sidor [...]

[xix]De senare innehåller frågor om en rad andra företeelser, alltifrån användning av konsumtionsvaror till politiska åsikter. Det är svårt att bedöma i vad mån den blandning av frågor som förekommer i ett omnibusformulär har några direkta effekter på frågor om tidningsläsning.

[xx]Ett ytterligare problem gäller svarsfrekvensen i en postenkät av detta slag. Erfarenhetsmässigt ligger andelen svarande vid postenkät på omkring 70 procent. Detta kan påverka representativiteten hos studien. Denna kan dock prövas genom det slag av bortfallsuppföljning som exempelvis *Testologen* använder sig av.

[xxi]*Testologen*s urval av intressen bygger desvärre inte på någon typ av teoretiskt antagande om hur nya intressen sprids i samhället. Deras undersökningar är av kommersiell art och valet av intressen styrs i första hand av beställarna.

[xxii][...] ser anmärkningsvärt lika ut i SOM '86 och i Vu 85.

[xxiii]Ekonomibedömningarna var också mycket svagare relaterade till väljarnas partisympati i SOM '86 än i Vu 85 [...]

[xxiv]Även om de använda samplingsmetoderna icke tillåter en exakt beräkning av felmarginalen, synes den praktiska erfarenheten, icke minst från de svenska Gallupundersökningarna, peka på att vid alla de tillfällen är man kunnat göra jämförelser med känd statistik, felmarginalen visat sig uppgå endast till några få procent och oftast väl håller sig inom de av sigmaformeln tillåtna gränserna (2,5 – 3,5 %).

[xxv]statistiska slumpfelet

[xxvi]Samtliga ledande svenska opinionsundersökningsinstitut använder numera sannolikhetsurval.

[xxvii][...] skulle vara omkring 1,200 och intervjuobjekten skulle vara bosatta inom en stad, eventuellt storstad, och en landskommun.

[xxviii]Som tidigare konstaterats brukar omkring 70 procent av tillfrågade returnera enkätformulären då det gäller undersökningar som behandlar samhällsfrågor.

[xxix]En tredje viktig omständighet är att svenska folket än så länge ställer upp för intervjuer när *Sifo* och SCB knackar på. Andelen människor som vägrar låta sig intervjuas har visserligen ökat under 1970-talet men fortfarande ställer cirka 80 procent av urvalspersonerna upp för besöksintervjuer i hemmet. Motsvarande siffra i tex England och USA är i dag ungefär 65-70 procent.

[xxx]Är enkätens avsändare en offentlig myndighet eller liknande organisation torde detta öka den instrumentella motivationen att svara. Dels därför att respondenten kanske anser att offentliga myndigheter bör samla in den

typ av uppgifter som efterfrågas i enkäten [...] Men också därför att respondenten kanske befarar att det kan medföra obehag att visa ohörsamhet mot överheten genom att inte svara [...] Upprepade påminnelser från avsändare kan antas fungera på ett likartat sätt. För vissa respondenter som visserligen beslutade sig för att inte svara då det första brevet med enkäten kom kan de upprepade påminnelsebreven som kommer med jämna mellanrum upplevas som ganska obehagliga.

[xxxi]I mars 1954 utgick från Statsvetenskapliga Institutionen vid Göteborgs Universitet en framställning om bidrag till en undersökning av valrörelse under hösten samma år. Framställningen riktades till Folkpartiet, Högern, Landsbygspartiet Bondeförbundet och Socialdemokratiska partiet samt till Statens Samhällsvetenskpliga Forskningsråd och Radiotjänst.

[xxxii]Jag har här tidigare antytt att fakta "sparkade", och att dessa "sparkar" kan ha gett upphov till vissa "anomalier" inom den svenska sociologiska forskningen. [...] Naturligtvis underlättade "glasögonbytet" förändringarna inom ämnet då i och med att det tillät dimensioner att framträda som i stort sett varit osynliga tidigare.

[xxxiii]Frågan om valpropagandans betydelse har hittills endast i ringa grad gjorts till föremål för studium. Vissa undersökningar har dock företagits i USA och några uppgifter kan hämtas från svenska Gallupintervjuer.

[xxxiv]Westerståhl här just nu för att söka få mitt stöd för en undersökning av vilka agitationsmetoder som ger resultat under valrörelsen.

[xxxv]Framhållas bör slutligen det nära sambandet mellan det demokratiska styrelsesättet och samhällsvetenskapen. Det är icke någon tillfällighet, att denna vetenskap nått sin rikaste utveckling i två av de äldsta demokratiska och konstitutionella länderna, England och Förenta staterna

[xxxvi]Framhållas bör vidare, att den moderna socialvetenskapliga forskningen blivit avsevärt mer kostnadskrävande än tidigare varit fallet. Så länge de socialvetenskapliga undersökningarna inskränktes till teoretiska eller begränsade historiska studier, kunde dessa uppgifter i stort sett fullföljas av den enskilde forskaren ensam. När man numer strävar efter att vidga denna ram och beakta det utomordentligt omfattande empiriska material som här föreligger - ur samhällets synpunkt måste undersökningar av denna art te sig som de mest betydelsefulla - kommer den socialvetenskapliga forskningen att kräva helt andra personella och materiella resurser än förut.

[xxxvii]I ett modernt samhälle av västerländsk typ blir denna tendens särskilt framträdande. Ett rationellt handlande måste i stor utsträckning byggas på en uppfattning om mänskliga beteenden, individuellt och gruppvis. Tendensen i det moderna samhället går i växande grand - både inom enskilda företag och samhällsorgan - mot en ökad planmässighet, som samtidigt så smidigt som möjligt anpassas till den enskildes beteende.

[xxxviii][...] ett bihang till samhällets jämna gång.

[xxxix][...] ett enastående år i universitetens historia.

[xl][...] presentera fakta och sammanhang i de politiska analyserna och inte i att föreslå åtgärder.

[xli]Den här undersökningen som du relaterar till den är ju väldigt mycket i affekt /så att/ Jag tror faktiskt att *SOM Institute*t som faktiskt finns här i Göteborg gör en hel del undersökningar om inställningar till energifrågor... jag tycker att dom... ja... [...] Kjell Jansson, VD Svensk Energi.

[xlii]Jag tyckte det var mycket roligt att man i TV-aktuellt sade att statistiska centralbyrån (SCB) hade gjort en *Sifo*-mätning, sade han [Zetterberg] småskrattande.

[xliii]Den stora TV-duellen innebar att Ulf Adelsohn satte stopp för Socialdemokraternas framryckning.

[xliv]-*Sifo* får passa sig, hotar den mäktiga finansministern i en kommentar

[xlv]Vi har vant oss vid *Sifo*, vi tar inte deras undersökningar på allvar. 1982 spådde de 11 procent, vi fick 15.5. Blir det så igen måste man ifrågasätta om det görs systematiska fel i *Sifo*.

[xlvi]Varför byter *Sifo* undersökningsmetod från den ena veckan till den andra, undrar Bo Toresson, partisekreterae hos socialdemokraterna. - Förra veckan gjorde man hembesök hos de tillfrågade, den här gången handlar det om både hembesök och telefonintervjuer. Sådana växlingar bidrar bara till än större misstänksamhet mot mätningen.

[xlvii]*Sifo* är ett opinionsinstitut som leds av professor Hans Zetterberg. Resultatet av deras opinionsmätningar publiceras av flera morgontidningar. *Sifo* används också av *Moderaterna* för deras interna opinionsanalyser.

[xlviii]Ty då kom *Sifo* i flera tidningar med en valprognos som sa att skillnaden mellan blocken bara är en procent. En futtig procent, det ligger ju inom den berömda felmarginalen. Allt flyter, allt kan hända.

[xlix][...] Hans Zetterberg drevs av en politisk ambition när han valde att offentliggöra undersökningen.

[l]Han borde nu fundera över om *Sifo* kommit att spela rollen som maktfaktor på felaktiga premisser. Därför emotses nästa väljarbarometer med spänning: kommer *Moderaterna* åter att ta ett skutt uppåt och framgången applåderas i partiets huvudorgan? Eller har kanske *Sifo* till dess funnit det för gott att korrigera sina mätmetoder med hänsyn till verkligheten?

[li]*Sifo*s stora misslyckande låg främst i att man under hela året i undersökning efter undersökning överskattat *Moderaterna*s opinionssiffror. Att Zetterberg och *Sifo* nära samarbetat med partiet kom det hela att framstå än mer tvivelaktigt.

[lii][...] någon typ av systematisk snedvridning skulle förekomma i *Sifo*s statistiska urval eller, troligare, i dess efterbearbetning av insamlat material.

[liii][...] Holmberg och gruppen av valforskare kring honom vid den statsvetenskapliga institutionen vid Göteborgs Universitet.

[liv]För framtiden är det dessutom en förhoppning att metodredovisningen blir fylligare redan då undersökningarna presenteras. Kan ni redan nu, herrar opinionsundersökare, utlova att det i alla avseenden blir bättre opinionsmätningar i 1988 års valrörelse?

[lv]Postenkäter till allmänheten ger för stora avvikelser för att kunna användas i seriösa opinionsundersökningar. Efter påminnelser och löften om belöning flr insända formulär återstår ändå vanligen ett bortfall av 40 à 50 procent. Man brukar säga att postenkäter visar opinionen hos "korsordsfolket", alltså de människor som tycker om att fylla i rutor och skicka in lösningar. Korsordsfolk tänker och tycker annorlunda än andra och inga vägningar eller poststraftifieringar i världen kan göra dem lika andra.

[lvi]Spelet med felmarginalen är en förrädisk sifferlek. Den får oss att okritiskt acceptera huvudresultaten från olika opionsundersökningar och endast reservera vår skepticism till några frå procent upp eller ned. Och det är inte bra. Det finns många andra felkällor vid sidan av den statistiska osäkerheten som vi också borde vara vaksamma på.

[lvii]Instituten tillhör visserligen "the research community".

[lviii]Instituten tillhör visserligen "the research community". Men research är ett bredare begrepp än forskning ("scientific and scholarly research").

[lix]De som leder *Sifo*s projekt kallas undersökningsledare, inte forskare, för att markera att det gäller "research". De liknar universitetsforskare i det att de har akademisk examen, tillhör lärda sällskap, föjer med sina ämnens internationella tidskrifter, åker på vetenskapliga konferenser i sina ämnen och uppmuntras att lägga fram resultat av metodologiskt intresse på internationella möten och i svenska sammanhang.

[lx]Svenska akademiska sociologer har med några få undantag varit ointresserade av opinions och attitydundersökningar och deras metodik alltsedan mitten på 60-talet. Sådana undersökningar har i gengäld blivit en viktig angelägenhet för massmedia, politiska partier och kommersiella opinionsundesökningsinstitut.

[lxi]Sammanställer man resultaten av en rad återkommande *Sifo*-undersökningar och väljarundersökningar utförda vid Göteborgs universitets statsvetenskapliga institution från början av 70-talet till 1982 så kommer man till följande resultat i stora drag. Svenska folkets stöd för den offentliga sektorn kvarstår.

[lxii]Mot slutet av 50-talet kom den unga sociologin i Sverige att förväxlas med opinionsundersökningar. Detta var djupt olyckligt för sociologin som ju är ett stort, underbart tält, vilket rymmer lång mycket mera än opinioner, attityder och intervjumetodik.

[lxiii]En nackdel med opinionsundersökningar om överensstämmelse mellan ledare och och ledda ligger i att de uppmuntrar en "gallup-demokrati". En fördel är att de ger partierna upplysningar om var de brustit i sin folkbildningsverksamhet.

[lxiv]Ett annat karaktäristiskt drag för svensk statsvetenskap är att ämnet *inte* i lika hög grad som många andra samhällsvetenskaper har *gjort anspråk på att företräda samhällelig ingenjörskonst.* Efter värderelativismens seger har det framstått som självklart att den väsentliga uppgiften består i att presentera fakta och sammanhang i de politiska analyserna och inte föreslå åtgärder.

[lxv]Närmare 450 000 svenska medborgare hör till den ofta förbisedda men växande gruppen utlandssvenskar. En grupp som är förhållandevis out-forskad. SOM-institutet söker nu samarbetspartners för att genomföra en Utlands-SOM, med unika möjligheter att undersöka utlandssvenskarnas livsvillkor, medievanor, samhällsförtroende, hälsa, livsstilar, åsikter och värderingar och syn på korruption.

[lxvi]Svarsfrekvensen för samhällsvetenskapliga postenkätstudier ligger i allmänhet på mellan 60 och 70 procent, beroende på geografiskt område och typ av svarspersoner (Ohlsson 1986).

[lxvii]I amerikansk litteratur finner man ständigt och jämt uttrycket "public opinion", som om detta vore något strikt definierat och klarlagt.

[lxviii]Om en person förordar en återgång till religionen för att samhället skall räddas, om han anser att sexuellt umgänge utanför äktenskapets ram bör med all makt förhindras, om han anser att det daltas i vår uppfostran och vår fångvård, så kan vi sluta oss till att denne person är ganska starkt konservativt inställd

[lxix]Vad som svarsfrekvenserna i bästa fall avspeglar är reaktionerna inför en viss, verbalt formulerad fråga och inte den omedelbara reaktionen inför den konkreta företeelse frågan avser.

[lxx][För det tredje] måste man hålla den säregna socialpsykologiska situation, som gallupintervjuandet innebär, i minnet. Intervjuoffret reagerar inte bara på frågeställningen utan också på intervjuaren.

[lxxi]Den enligt genomsnittet normala relationen är alltså att kvinnor företer en relativt större vet-inte-grupp än män.

[lxxii]Den utveckling som äger rum i vårt samhälle kan sägas vara grundad på en strävan inom all verksamhet mot en ökad rationalitet. Det traditionellt hantverksmässiga ersätts av den studerade tekniken, det skönsmässiga omdömet av den precisa mätningen och ett studium av kvantitativa sammanhang. Utbildningen inriktas i växande grad på att skola människorna för en ökad rationalitet i yrkeslivet.

[lxxiii]Även om gallupmetoden icke med tillfredsställande vetenskaplig exakthet skulle kunna begagnas i några andra sammanhang, synes enbart de möjligheter den erbjuder inom detta område göra dess användning motiverad.

[lxxiv]Massan är en terräng, där det ondas bakterie lätt utvecklar sig, medan det godas bakteri nästan alltid dör av brist på näring.

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