

Survey Research Interviewing

Planned Papers

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Abstract

This document contains (draft) abstracts of some papers I'm working on. It's incomplete and rough, but readable. The topics cover a lot of ground, and the papers are pretty ambitious, so some of the abstracts will require substantial revision and refinement, but I think the basic ideas should be clear enough.

Drafts of some of the papers (in various states of completion/disorder), as well as the source for this document, can be found at <https://github.com/SurveyResearch>.

The general background for all of these papers is: 1) survey research strikes me as extraordinarily parochial, and 2) the past several decades have seen a great deal of ferment in the human (including social) sciences, to the point that some argue that we are in the middle of a real sea change in our thinking about the sciences, philosophy, and their relations to each other and to human phenomena. In particular, pragmatist philosophers (Rorty, Putnam, Brandom, Price, etc.) have elaborated compelling and detailed alternatives to many of the concepts that have dominated Western thought since at least the 17th century, and cognitive science has moved far beyond the naive computationalist models that dominated its early "classical" phase. So the overall theme of these papers is an attempt to rethink survey research in light of these developments.

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1 Pragmatism and Survey Research

Abstract

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2 Mensuration without Representation

Abstract

There are two basic problems with measurement in the social sciences. One is that the relentless drive to emulate physics as the model science has lead to an overly-narrow focus on metric structure at the expense of algebraic (and other) mathematical structure. The other is that measurement has been construed almost exclusively in positivistic, representationalist (etc.) terms. But representation is not essential to measurement; we can discard it without compromising effective measurement. A pragmatic conception of measurement has no need of the concept of representation, but works just as well. But pragmatism about measurement is not just another option; rather, we intend to show that the pragmatist perspective provides the most compelling account of measurement as actually practiced (and conceptualized) in the sciences.

This paper is organized as follows. It begins with an overview of the major themes of contemporary pragmatism, with particular focus on inferential semantics and linguistic expressivism. Conceptual content is viewed as inferentially articulated, as opposed to representational. The inferential structure is instituted by proprieties of practice. The representational dimension of language use is to be explained in terms of the social structure of the discursive practices that institute meaning. The role of language is to express rather than represent; it allows us to say what we can otherwise only do. Pragmatism turns away from metaphysical questions like “What *is* measurement, *really*?” in favor of questions like “What role does measurement play in our lives? How is it used? What counts as *doing* it?”

We then lay the groundwork for a revised concept of measurement by examining mathematical foundations. We examine basic concepts such as number, magnitude, counting, ordering, etc. with special attention to practice; for example, we characterize counting in terms of what one must be able to *do* in order to count as counting (put things into one-to-one correspondence). We then provide a brief overview of algebraic concepts, and show that the mathematics of measurement can be viewed as primarily a matter of algebraic rather than metric structure. From this perspective, metric measurement comes out as a species of a more general notion of mathematicization.

Then we move to an overview of the major theories of measurement on the contemporary scene, especially in the human (behavioral) sciences, with special focus on the role of measurement in Survey Research.

Next, we debunk the Myth of Measurement Levels, which plays such a key role in social scientific research. Stevens’ famous model of four “levels” of measurement construes the four scales as elements of a hierarchical structure in which each “level” (scale) subsumes those preceding it. But it does not fit the (mathematical) facts. For example, mathematically, the concept of an order (ordinal scale) does not presuppose the concept of a countable set (nominal scale). Furthermore, the concepts of line (infinite in both “directions”) and ray (infinite in one direction) are distinct; a ray is not a kind of line, nor vice-versa, so ratio scales (rays) do not subsume interval scales (lines) – nor vice-versa. Historically and conceptual, the notion of a ray (“line” with absolute origin) precedes the notion of a line (“line” with arbitrary center), so if there is a hierarchy of levels here, it is the reverse of Stevens’ hierarchy: ratio scale precedes interval scale. The way to remedy this situation is to recognize that what matters is not quantitative (mathematically: metric) measurement, but algebraic structure.

But Stevens’ model is doubly pernicious: not only is it beset by mathematical confusion; more damaging is its narrowness of vision. By assuming that *quantitative* measurement is the name of the game, Stevens’ model excludes entire classes of mathematical structure from consideration.

But many non-metric mathematical structures play key roles in the natural sciences; for example, chemistry relies on Group Theory to describe symmetries. Whether symmetries characterizable in terms of Group Theory (or other algebraic structures) are to be found in social and psychological phenomena is an empirical question, but Stevens' model excludes the possibility from the beginning. Or more accurately, it places the possibility outside of the researcher's field of vision.

This leads to the notion that empirical measurement should be viewed in terms of assigning *algebraic* structures to empirical systems; this is a broader notion than the classic idea of applying *metric* structures to such systems.

Having examined the theories, we step back and address the more general issue of criteria of adequacy for any theory of measurement. Any account of measurement must address the three fundamental aspects of measurement: mathematical vocabulary, empirical vocabulary, and their relation to each other and to the world. In other words, measurement always involves at least two vocabularies: a vocabulary of mathematics and an empirical vocabulary, and the task of the theory is to align them and make the latter "match" the world. This section of the paper examines the pragmatic dimensions of these aspects: what features must be exhibited by practices using these vocabularies in order for those practices to count as measurement practices?

Then we proceed to the critical part of the paper. I show how the pragmatist perspective exposes problems in the popular accounts of measurement, with special focus on the survey research. In particular, I show that some of the most basic measurement-related doctrines of orthodox survey research do not answer to the facts of the matter. For example, I show that the idea that a question is an instrument of measurement, and that asking a question and recording an answer measures something, is based on deep confusion about the nature of measurement and discursive practice.

Finally we move to the constructive part of the paper. I show how an acceptable account of measurement can be constructed out of purely pragmatist materials, and how survey interviewing can be used to produce scientifically useful information even without the positivistic models that has dominated it throughout its history..

3 Deflating Validity

Abstract

In philosophy and logic, validity and truth are closely related. Truth is a property of sentences (propositions); validity is a property of inferences. In recent decades, “deflationary” (or “minimalist”) accounts of truth have become increasingly popular among philosophers. Broadly speaking, these accounts deny that truth is a substantial property, and instead treat the term “truth” as a kind of expressive device; it adds nothing significant to the expressions in which it appears, but it makes the language significantly more powerful. It allows us to say things we otherwise could not say, or could only say in cumbersome ways. For example, with a locution like “... is true”, we can endorse claims by naming them (e.g. “Fermat’s last theorem is true”); without such a locution, we would have to explicitly repeat the theorem as a claim (e.g. “There is no integer z greater than 2 such that ...”). And some things we can say with “... is true” would be practically impossible to express without it, such as “everything the policeman said is true” (since it would not be possible to repeat everything he said) or “the theorems of group theory are true” (since there are (I assume) infinitely many such theorems).

A third aspect: sentences contain referring components. To the truth of a sentence corresponds the “referentiality” of its components. “Snow is white” is true; it is true because “Snow” refers to the famous cold stuff, and “white” refers to the famous color. We need (but generally speaking do not have) a technical term to refer to the property of such referring relations that corresponds to the property of truth of sentences. It is a category mistake to say “‘Snow’ is true”, but we would like to say “‘Snow’ is x ” in order to bring attention to this truth-like referential condition. In Survey Research (and social science in general), the term “validity” is often recruited to serve this need in measurement vocabulary. The inadvisability of this becomes obvious when you move from measurement to description: “2.3 meters is a valid measurement of the length of x ” is a common way to talk, but “‘Snow’ is valid” sounds decidedly off-key.

This paper has two goals. The theoretical goal is to do with validity what deflationists have done with truth. The more practical goal is to examine the use and role of the concept (term) validity in Survey Research.

The first part of the paper thus explores the plausibility of a deflationary or minimalist concept of validity. Not just logical (inferential) validity, but validity as used by the social sciences, as a property of referential relations.

The second part of the paper examines the notion of validity as used in Survey Research. Suffice it to say that vocabulary of validity in the social sciences, especially psychology and education research, is very, *very* confused. Generally speaking, the term is used to refer, not to inferences and their properties, but to referential relations. Classic definitions of validity in the social sciences usually say something like “measures what it purports to measure”, which is to say, measurement expressions (e.g. “2.3 meters”) *refer* to entities (properties, relations) in the world. But it is also used to refer to inferences and a variety of other concepts.

The connection between the first and second parts is that the social sciences usually treat validity as a substantial property. Theories of validity often take on a metaphysical hue; they attempt to say what validity *is*, as if it were some kind of entity or substance – validity stuff – that referring terms “have”, possibly in greater or lesser degrees. On the deflationary view, this is a mistake that inevitably leads to unresolvable problems.

4 Why 'True 'Values' Are Not Important in Survey Research

Abstract

See Robert B Brandom, [“Why Truth Is Not Important In Philosophy”](#)

DRAFT

5 Reliability

Remark 1 *This abstract needs a lot of work.*

Abstract

This essay argues against the use of statistical concepts of reliability in Survey Research. Such concepts only describe the past - data already gathered. But the notion of reliability essentially involves present and future; to call something “reliable” is implicitly to make a prediction about the future.

So the critical question is how we can make decisions about the reliability of instruments, procedures, practices, etc. Statistical analysis has a role to play here, but cannot decide the issue; statistical measures of variance in past observations cannot by themselves say anything about the likelihood of reliability of future observations.

Reliability judgments are about the future. We want to know “Can I rely on this the next time I use it?”

The Standard Model of survey research seeks to show that questions are reliable; that is, that a given question can be relied on in future uses. More specifically, a question is reliable to the extent that, when administered properly (usually this means use of “Standardized Interviewing” methods), it *will* measure what it purports to measure, so that it yields good, “comparable” data. The standard means of establishing this sort of reliability involves statistical analysis of *past* performance; lower variance means higher reliability [FIXME?]. But this is not enough; what is missing is a theory that links past to future.

Compare temperature measurement. Here too statistical analysis is used to provide evidence of reliability, but predicatability is only available by virtue of a theory of heat that explains temperature measurement. That is what provides the basis of projecting future from past performance. Generally speaking, this sort of theoretical basis goes missing in survey research reliability studies.

But that is not all. All measurement involves a vicious circle. In the case of temperature measurement, a good theory of heat is necessary but not sufficient to prove that temperature is in fact quantitatively measureable. Such a theory only provides presuppositions.

Remark 2 *TODO: summary of how the circle works in temperature measurement (H. Chang, [Inventing temperature](#), Sherry, “[Thermoscopes, thermometers, and the foundations of measurement](#)”).*

What’s missing is an account of the essentially pragmatic nature of measurement. The way we arrive at an acceptable notion of temperature measurement is by repeated cycles of hypothesis-test-revise, not by deductive proof. This cycle never yields proof or truth; the best it can deliver is usefulness (etc.). The reason we think temperature is quantitatively measureable is because we have managed to create *effective* concepts and measurement procedures - that is, concepts and methods that have proved successful in describing and manipulating the world – and that have continued to improve. *Proof* of a theory of quantifiable temperature has always been and will always remain beyond our grasp. Such a proof would require a variety of questionable ontological commitments that we simply do not need.

The second part of this paper is constructive. It attempts to construct an alternative notion of reliability in survey research that is anchored in acknowledgment of the essentially pragmatic and open-ended nature of measurement. It proposes that statistical analyses of past performance

be complemented by a concept of reliability as an intentionally constructed feature of survey interviews. The basic idea is analogous to concepts of quality assurance used in manufacturing: reliability (and other quality attributes) are viewed as something that can be guaranteed by construction, rather than merely measured after the fact. In manufacturing, this translates into efforts to identify and remove causes of (and opportunities for) defects in the production process. In survey interviewing, this translates into efforts to structure the questionnaire and the interview such that the respondent's grasp of the meaning of questions is actively constructed, rather than left to chance.

To a large extent this is a matter of reconceptualizing the survey interview. It moves away from the standard "laboratory model" of survey research, in favor of a collaborative model that recognizes the fundamentally social and constructive character of discursive practice. Question-answer sequences in an interview are *always* constructed by the participants, but the standard model pretends otherwise. The approach suggested here merely recommends that researchers acknowledge the constructive, collaborative character of interviewing and use that knowledge to achieve their goals.

6 Error

Abstract

A pragmatic perspective on survey interviewing requires that we rethink the concept of “survey error”.

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7 Speech, Discourse, Language: a survey of contemporary models and their relevance to Survey Research

Abstract

A common lament in the Survey Research literature is the lack of a good model of what is variously referred to as the “survey interview process”, the “question-answer process”, the “response process”, or the like. Where researchers do articulate an explicit model, they tend to rely on the sort of cognitivist model exemplified by Tourangeau, Rips, and Rasinski, *The Psychology of Survey Response*.

But today we have a great variety of distinct models of speech, discourse, and language. The purpose of this paper is to critically survey some of the best known such models and examine their relevance to survey research interviewing.

The first part of this paper provides background. It begins with a brief overview of the critical distinction between the natural “space of laws” and the cultural “space of reasons”. Discourse is obviously dependent on the causal realm; it’s hard to talk without a body, or to imagine a mind without a brain. Yet the *intelligibility* of discursive behaviour (speech, language) seems to call for a distinctive order of explanation, one that swings free of cause and effect and instead appeals to notions of normativity and rationality. This first section examines the tension between these orders of explanation and provides a general overview of some of the distinct (and sometimes incompatible) ways of addressing it. It concludes that any model adequate to the needs of survey research should disregard the causal realm of the states and processes underlying discursive practices, and instead focus on the rational structure of those practices - what Wilfrid Sellars dubbed “the space of reasons”.

This section also provides a brief overview of the dominant modes of linguistic thought in the 20th century, with particular attention to Chomskyism. The main purpose of the overview is therapeutic: Chomskyism is basically dead, but, zombie-like, it refuses to die, and many survey researchers accept it (or some variant) uncritically. Many of its tenets (competence v. performance, language acquisition v. language learning, Universal Grammar) are still defended by some specialists (e.g. Pinker) and continue to enjoy uncritical acceptance by non-specialists (including in particular survey research methodologists). So one purpose of this section is to expose the problems with 20th century “scientific” linguistics in general and Chomskyism in particular.

Finally, it sketches some of the main relevant themes from cognitive science, neuroscience, and the philosophy of language.

The second part examines three (four?) distinctive approaches to the study of discursive practice.

Ethnomethodology and its offshoot *Conversation Analysis* seek to understand such behavior in terms of the local, accountable order actively produced by participants in discourse. It reverses the standard sociological order of explanation, which seeks to understand the doings of individuals in terms of causal forces exerted by social entities and processes. CA instead examines the fine detail of actual situated episodes of discursive behavior in order to discover how participants (“members”) manage to produce and sustain discourse as a local, situated phenomenon.

Dialogism the rubric adopted by a number of scholars (mainly northern Europeans in psychology departments) for a framework or collection of doctrines traceable (mainly) to Mikhail Bakhtin but also indebted to e.g. G. H. Mead. It is largely motivated by Bakhtin’s observation that *utterance* is essentially dialogical; it always presupposes not only a speaker, but also *responsivity* and

addressivity. This approach categorically rejects the atomistic, monological perspective that usually characterizes cognitivist approaches. To understand discursive episodes, one must understand a complex whole in which the parts (individual utterances) are always essentially interrelated.

Integrationism is an approach to linguistics advocated by the linguist Roy Harris in reaction to the sort of structuralist, cognitivist conceptions of linguistics that have dominated the field since the days of de Saussure. In particular, it rejects what Harris calls the “telementation model” of language, according to which discourse is reducible to the encoding, transmission, and decoding of thought. Although this is primarily a model of linguistics, it is closely related to dialogical models and has direct relevance to the study of discursive practices.

Pragmatism begins by asking what counts as discursive *practice*. Instead of asking “what is it?”, it asks questions like “what role does it play in our lives?” and “what must one *do* in order to count as deploying it?”, etc. (Huw Price, et al.) The most thoroughly worked out pragmatist model of discursive practice is the *deontic scorekeeping* model of the philosopher Robert Brandom. This is a very sophisticated account of the pragmatic foundations of discursive (and thus conceptual) practice, elaborated by Brandom in his 1994 masterpiece *Making It Explicit: Reasoning, Representing, and Discursive Commitment* and many other works.

One striking fact emerges: these approaches may employ distinctive vocabularies, and may be incompatible in various ways, yet they are all clearly taking similar approaches to more-or-less the same sorts of things. They recognizably involve variants on a few master concepts: the primacy of practice (and hence of empirical investigation over speculative theorizing); the situated or context-dependent nature of meaning; the essentially social nature of language, thought, and communication; etc. (Another way to put this might be to say that they share a common enemy, one that appears in a variety of guises: monologism, cartesianism, atomism, representationism, etc.)

The final section of the paper explores the relevance of such models (which I group under the general rubric of “pragmatism”) for Survey Research.

8 The Conduct of the Survey Interview: Models and Protocols

Abstract

This paper analyzes and compares three models of survey interviewing. That is, models of the conduct of survey interviewing, rather than models of the structure of questionnaires, interviews, etc.

The first is the Laboratory Model, which is motivated by a desire to mimic the experimental physical sciences, paradigmatically physics. The paradigmatic example of this sort of model is the "Standardized Survey Interview". Analysis of this model exposes a variety of (usually) unacknowledged commitments to theoretical/philosophical doctrines, which are shown to be untenable.

The second model is the Extended Laboratory Model. This is a modification of the Laboratory Model. It acknowledges that, due to the interactive nature of the interview, the interviewer inevitably makes a contribution. But it retains the basic structural commitments of the laboratory Model. An example of an Extended Laboratory model is Maynard et al's "alternating model".

The third model is The Theatrical Model. This model is similar to the Laboratory Model, in that it recommends that the interviewer read the questions exactly as written, avoid probes, etc., but it involves a very different conceptualization of the nature of interviewing. Like the Extended Laboratory Model, it acknowledges that the Field Interviewer makes a substantial contribution to the survey interview, due to the fundamentally interactive and collaborative nature of discursive practice. But it stresses that interviewing essentially involves role-playing. This may or may not result in interviewer behavior that is different from what it would be under the Extended Laboratory Model, but either way it would suggest different approaches to interviewer training. This model is based on a more realistic picture of the nature of surveys and survey interviewing as traditionally practiced, but it also has some weaknesses, which we analyze.

Finally, the fourth model is The Collaborative Model. This model is driven by a closer and more realistic analysis of the nature of the survey interview. It demystifies aspects of the interview that the other two models take for granted or ignore, such as the various asymmetries involved in interviews, the fact that completion of a survey questionnaire is the joint responsibility of the interviewer and the respondent, and so forth. It discards the fictions that are at the core of the other models discussed. Most critically, motivated by considerations of the nature of discursive practice and the production of meaning, it denies that survey interviewing involves measurement. In summary, this model recommends that survey interviewing be construed as collaborative or joint action, and that the demystified facts of the matter be openly acknowledged in the conduct of interviews. This means, among other things, that the field interviewer should serve as an assistant to the respondent, rather than a proxy for the researcher; that interviewer and respondent are jointly responsible for completing the questionnaire; and that the results of individual survey interviews should be viewed as a trace a kind of dialog between the individuality of the particular respondent and the stereotype presupposed by the questionnaire design.

9 A Critique of the Theory of Cognitive Interviewing

Abstract

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10 A Quality Assurance Model for Survey Research

Abstract

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Appendices

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