Field Research Methods

Elizabeth Levy Paluck

Princeton University

Robert B. Cialdini

Arizona State University

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Introduction

"... In that Empire, the Art of Cartography attained such Perfection that the map of a single Province occupied the entirety of a City, and the map of the Empire, the entirety of a Province. In time, those Unconscionable Maps no longer satisfied, and the Cartographers Guilds struck a Map of the Empire whose size was that of the Empire, and which coincided point for point with it. The following Generations, who were not so fond of the Study of Cartography as their Forebears had been, saw that that vast Map was Useless…"

Jorge Luis Borges, "On exactitude in science," 1999

Theories are like maps. Social psychologists typically use theories as maps to find phenomena that are worth investigating, or to deduce unexplored pathways between those phenomena. After formulating their hypotheses, they design a laboratory experiment that simulates the local conditions described in their theoretical map.

Social psychologists often do not see a place for field research in this cycle between theory and laboratory experimentation. Field research, they fear, will bloat their theoretical maps with too many added variables, distorting causal pathways. Observational fieldwork used to identify interesting phenomena, or experimentation outside of the highly controlled laboratory environment, so the argument goes, will produce unwieldy theoretical maps akin to Borges' maps that were the exact size and scale of the Empire.

Years ago, one of us (Cialdini, 1980) pointed out that on the contrary, field research can help social psychologists draw accurate theoretical maps that identify the most consequential social psychological phenomena. While theoretically driven laboratory experimentation can produce accurate maps, they may not tell social psychologists about the most interesting or important locations. Furthermore, it is by cycling through field observation, experimentation, and theory that social psychological theories can become precise as well as meaningful. This claim is rooted in a longstanding call for more fieldwork

in social psychology (Campbell, 1969; Lewin, 1947; McGuire, 1969), and in Cialdini's own "full cycle" research program, which used field observation, field experiments, laboratory experiments, and theoretical deduction to develop a framework for social influence (Cialdini, 2009a).[1]

This chapter will not cover the uses of field research for the *application* of social psychological theory. Instead, this chapter describes the use of field research for *development* of psychological theory. Field research fosters theoretical development in many ways. Field research helps to identify which phenomena are most psychologically and behaviorally consequential. Operationalizing independent and dependent variables and choosing the right setting in the field compels researchers to specify and make concrete their theoretical constructs of interest. Field research allows investigators to observe some possible boundary conditions of their theory, and to examine how different theoretical constructs relate to one another. The field is also a good setting for testing causal predictions. Of course, theory that has been developed from field research stands a better chance of successful application to real world issues. However, in this chapter, we will focus on the kinds of theoretical insights afforded by research in field settings.

Most students of social psychology are drawn to the discipline because of an interest in the world around them, but in the course of study, their eyes are retrained to find inspiration

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[1] Although a full-cycle approach to social psychology is not an exclusively field-based orientation, field investigation is a prominent and important component. The advocated process typically involves (1) recognizing a powerful and interesting phenomenon in the natural environment, (2) conducting an initial scientific test of the validity of the phenomenon, usually in the field, (3) conducting further scientific investigation of the mediating processes and theoretical underpinnings of the effect, often in the lab, and (4) looking back to naturally occurring situations to assess the match between the characteristics of the effect as it appeared in the studies versus how it appears in the real world. As an upshot, one can better determine the presence, reliability, and force of psychological phenomena in the real world while identifying the psychological mechanisms underlying these phenomena. Naturalistic observation, field research and laboratory research become symbiotic, with strengths and weaknesses that complement one another (Cialdini, 1980; Mortensen & Cialdini, 2010).

in abstract theory and to observe and test these theoretical processes in laboratories. Many important figures in social psychology, themselves experts in laboratory experiments (e.g., McGuire 1969), have lamented this rather myopic methodological focus. There are a few reasons why field-based observation, measurement, and experimentation have not historically been as prominent as laboratory work (Cialdini, 2009b). One simple reason is that social psychologists are not typically trained as a matter of course in field methodology.

We have designed this chapter to be a systematic treatment of various options in field research, so as to redirect students' and researchers' eyes toward these methods. It is our hope that awareness of the uses and advantages of these field methods, paired with an understanding of when and how they can be implemented, will promote more social psychological research in the field.

The chapter is laid out as follows. We first explain what we mean by field research as opposed to laboratory research, and discuss advantages that come from finding and testing ideas in the field. We explore the range of theoretical goals that can be accomplished with field research. We point out strengths and weaknesses of various field research techniques, and some best practices of each one. We conclude with practical suggestions and reasons for researchers at various stages of experience to engage in field research.

Laboratory and field research

What is field research?

"Field" research is, of course, not defined by its physical locale, but by the work's degree of naturalism. Defining field research as relatively more naturalistic elements allows for a continuum-based (rather than dichotomous) conceptualization of the approach. After all,

the laboratory can be the site of very realistic interventions, and conversely, artificial interventions may be tested in a non-laboratory setting. When assessing the degree to which studies qualify as field studies, one must consider the naturalism of four aspects of the study: (a) participants, (b) the intervention and its target, (c) the obtrusiveness of intervention delivery, and (d) the assessed response to the intervention.

For example, a study on the effects of interpersonal empathy might involve (a) undergraduate psychology majors, (b) written instructions aimed at the participant's perception of a sad story, which vary systematically according to whether (c) instructions to empathize with the protagonist of the story are included or not, and (d) outcome measures such as empathy and willingness to help scales. Relatively more naturalistic versions of each aspect of this study are (a) non-psychology major young adults or citizens of the local town, (b) a television station broadcasting a sad story, which varies systematically in terms of its (c) language and imagery that encourages or does not encourage the participant to take the perspective of the protagonist, and (d) measures such as the participant's facial expression as they watch the screen, or their response to a nearby confederate who disparages the protagonist. Note that this experiment could be conducted in a laboratory that has been outfitted to look like a waiting room with a television, which would make the laboratory more naturalistic.

Cronbach (1982) suggests an acronym, UTOS, to use when assessing the naturalism of a study: Units, Treatments, Observations, and Setting. To this list of considerations, Reis and Gosling (2010) add that non-laboratory research settings differ from the laboratory in the goals that are likely to be activated by the setting, the setting's correspondence with the

behavior under study, and the degree to which the setting is natural and appropriate for the research question.

Advantages of the laboratory

Before we detail some of the disadvantages of laboratory settings that are addressed through research in field settings, it is important to recognize the many advantages of the laboratory for social psychological research. The laboratory is singular for its precision and control, which produces low error variance and nuanced and well-targeted measurement. Control over variations in treatment allows the investigator to fully stage-manage and test the interaction of the setting with participants' individual differences. Investigators are free to eliminate or include any variables they determine to be extraneous or potentially influential. In this way, investigators can test theory from all angles, probing mechanisms and counterintuitive predictions of the guiding theory (Smith, this volume; Wilson, Aronson, & Carlsmith, 2010).

Laboratory research is also convenient for university investigators. Laboratories can be located next door to the investigator's office, for easy supervision of research assistants and the research site itself. Undergraduate psychology majors are efficiently exposed to psychological research in on-campus laboratories, and critically, they serve as participants in laboratory research so that investigators can execute multiple studies per semester.

Disadvantages of the laboratory

What is the cost of the predominance of the laboratory research in social psychology? It is theory's "close relation to life," according to Kurt Lewin, one of the founders of modern social psychology (Lewin, 1944/1997, p. 288). Relating back to the idea of theories as maps,

Cialdini (1980) wrote that theory-driven experimentation without attention to the real world could result in an accurate but less consequential map, or even a misleading map. As is often the case, the greatest strength of laboratory research—its control—is also part of its weakness. We elaborate on this point below in terms of Cronbach's (1982) scheme of Units, Treatments, Observations, and Settings, and in terms of psychologists' concerns about culture, complex systems, and identification of "extraneous" influences.

Units. Undergraduate students are predominantly used as participants in psychological laboratory settings because of their convenience, tradition, and financial discount for academic investigators. For the purposes of building widely applicable theories, undergraduates present several troubling bias. Their developmental stage and their particular social and educational backgrounds may exaggerate some effects and diminish others, or restrict the range of variation on the dimensions being studied (Henry, 2008; Sears 1986).

Treatments. The treatments administered in laboratory settings are typically weaker, briefer, and less varied than the naturally occurring phenomena in the world that they are designed to simulate. The fact that treatments are weaker is often due to experimenters' ethical obligation to avoid intense or distressing events, such as authority coercion, severe disappointment or sadness, or sexual harassment (cf. LaFrance & Woodzicka, 2005).

Laboratory treatments are brief to accommodate the typical hour-long sessions allotted to participants. As a consequence, researchers use a "reactive or acute form" of a variable to stand in for longer-term phenomena. For example, when studying low self-esteem, laboratory experimenters must lower self-esteem with negative feedback or an experience of failure, rather than observe the process of erosion of self-esteem over a longer term. Unfortunately, "an occasion of low self-esteem may have nothing to do with a lifetime of low self-esteem"

(Ellsworth, 1977, p. 607). Short-term states may not operate under the same underlying processes as chronic states, which are almost impossible to study experimentally in the laboratory over long time periods (cf. Cook's [1978] months-long laboratory studies of interracial workgroups).

Finally, there is often little variety in the types of laboratory treatments used in the laboratory. Investigators rely on a few established paradigms to study a variety of outcomes, and very rarely translate their abstract theoretical ideas into new operationalizations. "The mental dexterity demonstrated in dealing with abstractions often seems to vanish at the translation stage, as the old standard treatments and measures are used and reused with little consideration of their suitability for the task at hand (i.e., choosing a concrete version of an abstract question)" (Ellsworth, 1977, p. 604; see also Webb, Campbell, Schwartz, & Sechrest, 1966).

Observations. Very rarely are the behaviors measured in the laboratory commensurate with the behaviors that investigators wish to explain in the real world. First, many outcomes examined in the lab, such as reaction times, are rarely important outcomes in and of themselves in real world settings. Second, lab-based pseudo-behaviors like deciding the salary of a fictional person, or assigning a sentence to a fictional criminal in a jury vignette, may not result from the same interpersonal and intrapersonal processes that produce these behaviors in the world. This difference is troubling for theory testing and building. Most often, laboratory investigations measure self-report rather than behavior (Baumeister, Vohs, & Funder, 2007).

Settings. Social psychologists strive to study the interaction of the person and the situation, but there is very little work that describes situations themselves (cf. Kelley, Holmes,

Kerr, Reis, Rusbult, & Van Lange, 2003; Reis, 2007), or innovates different situational paradigms in the laboratory. As a result, investigators cannot observe and catalogue the situations that are most frequent or consequential for individual or group behavior.

The culture of the laboratory. One presumed advantage of the laboratory is that it is a "culture free" setting—one that is not tied to any particularistic traditions, scripts, ideologies, or standards of reference. Adams and Stocks (2008) argue that this assumption is misguided, and has given rise to theories that are incorrectly portrayed as universal processes of human cognition and behavior. Examples of cultural elements of the laboratory are Likert scales featuring implicit standards of reference that are culturally specific (e.g., comparing oneself to another individual or to another group; Heine, Lehman, Peng, & Greenholtz, 2002, cited in Adams & Stocks, 2008), and exercises that rely on culturally specific ideas of relationships (e.g., trust building exercises that involve self-revealing information; Aron, Melinat, Aron, Vaollone, & Bator, 1997).

Related to these points, investigators have found that participants harbor social scripts and expectations for laboratory situations that affect their behavior and thus threaten the external validity of the research (Shulman & Berman, 1975). For example, Bator and Cialdini (2006) argue that certain features of the laboratory as well as its scientific atmosphere stimulate research participants to respond in more logically consistent ways than outside of the laboratory. Other researchers worry that investigators rarely implement methodological solutions to prevent artifacts like experimenter bias, subject motivation, and meta-processing of the situation by participants (West & Graziano, in press).

Complex systems in the laboratory. The laboratory is often an inappropriate setting for studying complex systems, which is troublesome given the complexity of human behavior.

"More and more, we are coming to recognize that [variables'] interrelations may be causal but much more complicated than we can assess with our usual methods" Ellsworth (1977) asserts; "[i]t is in just these instances [of complex relationships] that the typical laboratory experiment is weakest; so much is held constant that there is no opportunity for this sort of complex causation to manifest itself" (p. 614).

Moreover, there are many unobservable variables operating in a real world context, variables of which investigators may not be aware when they set out to simulate that context in he laboratory. Consequently, a relationship uncovered between two variables in the laboratory may not exist or may occur rarely in the world because of the interference of this unobserved variable. Preceding, co-occurring, or proceeding variables in the real world may diminish the relationship identified.

While we have catalogued many disadvantages of laboratory settings and research paradigms, these critiques should not be taken solely as arguments to incorporate fieldwork into a research program. These preceding points can also be used to inspire more rigorous laboratory experiments that test and produce theoretical maps that are, in Lewin's words, "closer to life."

Advantages of the field

The most obvious advantage of the field is that the investigator does not always have to work as hard to make the units, treatments, observations, or settings of a study naturalistic. Participants are those people involved in the treatment or who come from the social group of interest; treatments can be more high impact and lengthy than a laboratory intervention; outcome measures can be those that already occur in that setting.

Definition of constructs. Selecting the location and the participants for a field-based study helps investigators to precisely define the nature and scope of their theoretical constructs. Take the following example. Suppose that you are interested in studying cooperation. You understand that your choice of setting (e.g., households of married couples, a cheese cooperative in Berkeley, a financial trading floor) and your participant population (e.g., adults, kindergarteners, residents of a small-scale agriculturalist society) change what you mean by "cooperation" and how you will measure it. As you eliminate certain types of settings and populations, you refine your concept of what kind of cooperation you will be able to describe and theoretically map in relationship to other constructs. Leaving the laboratory's standardized paradigms and generating a list of possible settings, participants, and measurements reveals implicit assumptions or theoretical confusions about your construct (Ellsworth, 1977).

Inductive power. Another advantage of research in field settings is that it can provide an inductive approach to theory that begins with facts about cognition, emotion, and behavior in the world, rather than a deductive approach that begins with abstract theory. Cialdini (1980) described the inductive capacity of field research as a "steadily developing sense of which of our formulations account not just for aspects of human behavior, but also for aspects of the behaviors that matter" (Cialdini, 1980, p. 26). Moreover, to generate ideas in the first place, McGuire (1969) suggested that investigators would spend more productive time observing field settings as opposed to reading the top journals. Using fieldwork to establish the strength, frequency, and surrounding conditions of an effect is a powerful approach to assembling the building blocks of a new theory or to modifying an existing theory.

Causal testing. As we discuss later, the field is not just a setting for observational research. Field settings provide a powerful stage for causal tests. Just as laboratory experimentalists use stagecraft to import various conditions of the real world into the laboratory, field experimentalists export experimental control from the laboratory into the field. Testing causal relationships in the field allows investigators to identify whether the relationships hold up in the presence of other social and situational factors. Field experiments also indicate plausible boundary conditions of an effect across different time periods, settings, varying numbers of people, and other important contextual factors.

Test of a theory's pragmatic worth. Saying that field experiments reveal whether causal relationships hold up in real world settings is one way of saying that field experiments test the *pragmatic* worth of a theory. By pragmatic, we do not simply mean applied. We use pragmatic in the way William James (1907/1981) defined pragmatism, specifically that theories are pragmatic when their predictions "cash out" in the real world—when they predict behavior occurring in the "rich thicket of reality" (p. 517; see also Fiske, 1992). Field settings invite psychologists to be concerned not just about their stock in the marketplace of ideas, but also about their stock in the marketplace of observable effects.

Relevance. Relatedly, field research renders social psychological theory and research more valuable to members of important nonacademic communities. Because it takes place in natural, everyday settings, field research makes transparent the relationship between the obtained data and everyday lives. That clear relevance allows those who have paid for the work (e.g. taxpayers and research purse-holders) and those who would want to employ it (e.g., policy- and decision-makers) to view social psychologists as a credible source of information about the issues of concern to them. Some evidence that social psychologists have yet to be

viewed in this way by certain important individuals comes from a pair of experiences of one of the authors. At two separate meetings of high-level government officials, he was labeled not as a social psychologist but as a behavioral economist because, he was informed, it was judged to be more palatable to the participants.

Field observational methods

Observation in the field is not a supplement to empirical work—it is empirical work. For example, Cialdini's (1980) full-cycle model endorses "a more empirical science that is based firmly in the observation of everyday worlds." From this perspective, observation and experimentation are each "but one tool in the social psychologist's repertoire," and each is "better suited to some tasks than others" (Adams & Stocks, 2008, p. 1900).

Observational methods can be put to many important purposes in field settings. Observation of individual and group behavior can generate hypotheses and theoretical insights, or point researchers toward phenomena that are powerful and prevalent in the community (Cialdini & Mortensen, 2010). Or, as Solomon Asch once pointed out, observation can help researchers to become more familiar with the phenomenon of interest: "[b]efore we inquire into origins and functional relations, it is necessary to know the thing we are trying to explain" (Asch 1952, p. 65, cited in Reis and Gosling, 2010). This includes observing and describing the types of situations that give rise to the phenomenon (Kelley et al., 2002). In addition, observational measurement techniques like interviews or behavioral trace indicators, described below, can be used as outcome measurements for field or laboratory experiments. All observational measures, particularly those that are highly unobtrusive, can serve as strong verification of self-report data.

Qualitative methods

Qualitative methods that are used to explore or describe phenomena include personal observation, participant observation, structured interviews, and ethnography. Notes that are produced by these methods can be coded and written up for publication (see Emerson, Fretz, & Shaw, 1995). If the data were collected in a systematic manner, qualitative outcomes may be used as outcome measurements in a study by quantitatively coding and analyzing the data as events or ratings (see Paluck, 2010).

Personal observation. Personal observation is a time-honored tradition of hypothesis generation in social psychology. A classic example is Festinger's (1957) observation that catastrophes are followed by rumors of further disaster rather than reassurances of relief, which led to his formulation of cognitive dissonance theory. Although many social psychologists report that ideas and counterintuitive notions were inspired by real world observation, observational skills are not often recognized as part of the social psychologist's official toolkit. McGuire (1973) urged psychologists to "[cultivate] habits of observation that focus one's attention on fertile aspects of natural experience," adding "[we should] restructure our graduate programs somewhat to keep the novice's eye on the real rather than distracting and obscuring his view behind a wall of data" (p. 453). Keeping one's "eye on the real" could involve training oneself to be more alert in everyday life, delving into written accounts of everyday life through various peoples' eyes in blogs or newspapers, or using more systematic observation techniques like participant observation.

Participant observation. Participant observation involves observation while participating in an institution, social group, or activity. For example, investigators can participate in skilled practitioner trainings, as did Cialdini (1993) in his observation of sales

trainings. These observations helped him to formulate important underlying principals of compliance tactics that salespeople had honed over years of work. Some psychology departments send students out to participate in community organizations, to observe and analyze where social psychology can contribute (Linder, Reich, Braver, in press). "Being on the scene often means a necessary exposure to a large body of irrelevant information" Webb et al. (1981) caution, but "the payoff is often high" (p. 240).

Ethnography. Ethnographers spend concentrated amounts of time in a particular place or following a particular group of people or event (e.g., a neighborhood, or a traveling political rally). Rather than seeking to measure the frequency of a behavior in a setting, ethnographic methods are aimed at understanding the social psychological *meaning* of that behavior in the context. And rather than collecting a representative sample of people or places, ethnographers focus on one or a few "cases," such as individuals, classrooms, teams, or towns. Ethnographers try to get to know their subjects and to become part of their lives and contexts for a period of time (Lareau & Schultz, 1996). For example, Erving Goffman wrote the foundational text *The Presentation of Self in Everyday Life* (1959) after one year of living in and observing a Shetland Island subsistence farming community.

Interviews. Field research can also benefit from structured or in-depth interviews with individuals, called "key informants," who have specialized experience with the phenomenon or community under investigation. For example, Huggins, Haritos-Fatouros, and Zimbardo (2002) interviewed Brazilian police who had tortured and killed citizens during Brazil's military rule, to understand the process by which they were convinced to commit atrocities on behalf of the state, and how they justified this violence to themselves and their peers. Adams

(2005) combined interviews with field observation to uncover the concept of enemyship (a personal relationship of hatred) in West Africa and North America.

Observation-based estimates of individual or population characteristics

Individual and population characteristics can be inferred from observational field methods such as daily diary techniques, trace measures, ambulatory assessment, and social network mapping (see also Reis, Maniaci, & Gable, this volume). These types of observational data can be collected in person, in archives, or can be harvested from the Internet.

Individual characteristics. Daily diary methods "capture life as it is lived" (Bolger et al 2003, p. 95). Participant are asked to fill out reports about their behavior, affect, cognition, and/or surroundings at regular intervals or when prompted at random times by a PDA or a cellphone. Diary methods serve the important descriptive purpose of cataloguing information about the prevalence, the chronological timing, and the co-occurrence of events and situations (Reis & Gosling, 2010).

Trace measures bring out the Sherlock Holmes in social psychologists. To unobtrusively track psychologically meaningful behavior, psychologists seek out systematically or automatically recorded traces of behavior in official archives or unofficial spaces of everyday life. The advantage of these measures is that the subjects of study are unaware that they are being watched. For example, from official public records, investigators can study government voting or hospital immunization records and yearbook photos. Investigators might even obtain data from retail stores on customer loyalty card activity showing individuals' purchases of fruits and vegetables, cigarettes, or other products. From the "unofficial" records, social psychologists have mined trash cans to measure alcohol

consumption (Rathje & Hughes, 1975), and counted the number of vinyl tiles that needed to be replaced around various museum exhibits as a measure of interest in the exhibit (Duncan, personal communication, cited in Webb et al., 1981). Analyzing the composition of personal Internet profiles on social networking websites is one of the latest ways to use trace measures (Reis & Gosling, 2010). Online social networking sites also help investigators to identify individual's network of potential social influences (e.g., Goel, Mason, & Watts, 2010).

Another individual-level measurement technique is ambulatory assessment, which uses electronic equipment to measure an individual's movement and states of being throughout their daily lives. This includes blood pressure monitors, sound recording (Pennebaker, Mehl, Crow, Dabbs, & Price, 2001), and GPS tracking devices located in individuals' mobile phones. Some tracking devices can even assess which people in a social network, such as a school social network, interact the most frequently, and for how long.

The advantages of these observational methods is that they capture daily experiences as they occur in the stream of natural activity across different situations. Many of these methods produce time series data, which means they can evaluate hypotheses regarding the chronological ordering of a particular process and within-person processes (Reis & Gosling, 2010, pp. 96-97). Depending on the way they are collected, observational measures can overcome biases of self-report. For example, in the garbage trace measures collected by Rathje and Hughes (1975), 15% of households reported at the front door that they drink beer, while beer cans were found in the trash can at the back door in 77% of the same group of households.

Disadvantages of individual observational methods include noncompliance or misreporting, in the case of daily diaries. In addition, there may be imprecise translation

between trace measures and psychological constructs or behavior. For example, did individuals actually drink the beer from the cans found at the back door, or did they use the beer to bake bread? This may be an overly generous interpretation of their trash cans, but for observational measures the general principle holds that measures are strongest when they are deployed alongside different types of measures that can corroborate their findings.

Population characteristics. Some observational methods cannot connect observations to specific individuals, but can draw a picture of a community as a whole. For example, a linguistic analysis of online journals before, during, and after the events of 9/11 revealed average social psychological reactions to trauma among United States residents (Cohn, Mehl, & Pennebaker, 2004). The lost letter technique is another extensively used population-level observational method. Throughout the streets of a community, investigators drop stamped, addressed letters (while the name varies, the address sends the letter to the investigator). The proportion of letters that are picked up and delivered to a mailbox serves as a measure of average community helpfulness. Investigators have extended the purpose of this technique to measuring social bias. The names listed on the letter's address are randomized such that half feature typically Anglo American names and the other half African American names. Investigators measure whether the proportion of re-delivered letters differs depending on the presumed race of the recipient (Crosby, Bromley, & Saxe, 1980).

Population-level observation is the most unobtrusive of the research methods reviewed here, avoiding completely the "speak clearly into the microphone, please" aspect of other approaches (Webb et al., 1981, p. 241). In this sense, such observation holds an advantage over laboratory settings in which participants know that their behaviors are being examined, even if they don't know which behaviors those are. Population-level observations do,

however, prevent the investigator from connecting individuals to behaviors, which means that these outcome measures are best used for description, hypothesis generation, or for an experiment in which the community is the unit of randomization and analysis.

Observation of situation characteristics

Despite the stated importance of the situation in social psychological analysis (Rozin, 2001), very little observational work has been devoted to establishing a taxonomy of different situations. The work of Kelley and colleagues (2003) is a notable exception, in which the authors classify and describe 21 of the most common everyday situations thought to influence various aspects of interpersonal behavior. We believe that psychologists could make greater use of this work, and expand upon it with situational taxonomies relevant to other types of behavior, cognition, or emotion.

Ultimately, observational research in field settings involves detecting "pieces of data not specifically produced for the purpose of comparison and inference but available to be exploited opportunistically by the alert investigator" (Webb et al., 1981, p. 5). We now turn to experimental research in the field that is explicitly designed for the purpose of comparison and causal inference.

Field experimental methods

Experimentation in field settings can be just as rigorous as in a laboratory setting. Treatments can be randomly assigned and delivered in a standardized manner to individuals, groups, or institutions, and standardized outcome measures and evidence speaking to the process of change can be collected. Causal inference in field settings has greatly improved over the years, mostly through innovations in field experimental design that address

challenges particular to field settings (Green & Gerber, in press; Rubin, 2005; Shadish, Cook, & Campbell, 2002; West, this volume).

Randomization and control in field settings

Psychologists who conduct laboratory experiments may approach field experimentation with two types of trepidation. One concern is that ongoing activity in field settings will destroy pure randomization and segregation of experimental and control groups. A second is that many things are simply impossible to randomize in a field setting. On the first point, psychologists might be pleasantly surprised to find the many varieties of experimental designs that field experimentalists have developed to preserve the integrity of experimental design against special situations that arise in the field.

On the second point, canvassing the types of psychological field experiments conducted over the last decade reveals few limits on the kinds of treatments that have been randomized. Social psychologists have randomized a variety of interventions, in national parks (Cialdini, 2003), on national radio (Paluck, 2009), in schools (Blackwell, Trzesniewski, & Dweck), and amusement parks (Gneezy, Gneezy, Nelson, & Brow, 2010), targeting and measuring psychological phenomena from perceived norms, beliefs, implicit theories, to social welfare concerns, and connecting them with real world behavior. For most social psychologists, whether their interests lie in basic or applied research, these are important and worthy investigations.

Many field experiments involve simple random assignment of a treatment to individuals, households, or communities. Investigators deliver the treatment, or they collaborate with an organization that is already intending to deliver the treatment. However, many types of treatments are impossible to package neatly and randomly deliver to some

individuals but not others. The following types of designs address some of the issues that arise for these types of experimental treatments.

Encouragement designs

One advantage of experimenting in field settings is the opportunity to study interventions that are very difficult to simulate in the laboratory, such as political movements. Political movements, however, exemplify the type of treatment that at first blush seems impossible to study experimentally. In the specific case of political movements, the "treatment" is broadcast to the general public, meaning there is no obvious control group. Moreover, joining a political movement is a highly individualized and rare decision, meaning the "treatment" group that joins a political movement is self-selected and small. One experimental design that can capture a treatment with these characteristics is a randomized encouragement design. An encouragement design randomly encourages some people and not others to engage with the treatment, and then measures reactions within the entire sample of encouraged vs. not-encouraged people.

Consider the following field experimental design to study the effect of joining a political movement on, for example, individual political perceptions and communal behavior. Suppose you identify an organization that has mounted a website calling for political change in a particular city. To measure the causal effects of joining this movement, you could randomly divide a list of city residents' email addresses in half, and send an email to one half of the sample. The email would encourage the recipients to visit the site and join their efforts. In your entire study population, there will be people who would have visited and joined without encouragement, people who will visit and join despite the fact that they weren't encouraged, and people who will never visit or join regardless of encouragement. However,

there is also potentially a group of residents who would *not* have visited or joined without encouragement. An encouragement design measures the causal effect of invitation on visitation, participation, and on their subsequent political perceptions and behavior among the encouragement group, compared to the equivalent types of people in the no-encouragement group (for an explanation of analysis of encouragement designs, see Green & Gerber, in press; Angrist & Krueger, 2001). Because obtaining informed consent is not a typical component of such designs, researchers need to ensure that the invitation and the encouraged activity would not violate participants' privacy or well being.

Randomized rollout designs

Some high-impact and theoretically relevant treatments are administered by governments or private companies who do not wish to exclude treatment recipients in the interest of forming a control group. In such cases, investigators can use randomized rollout designs to study the causal impact of these treatments. A randomized rollout eventually assigns the entire population to treatment, but over a certain time period, during which outcome measurements can be assessed among the treated and as-yet-untreated participants.

For example, a company that is struggling to attract a diverse workforce may be anxious to implement new hiring accountability measures or a set of diversity trainings. A strategic field experimentalist could explain to the company that, since an immediate implementation of diversity initiatives to *all* of the company's offices may not be possible for financial or scheduling reasons, a lottery would represent a fair procedure by which to "roll out" the new diversity trainings. Half of all randomly selected offices could receive the diversity trainings in the first year and the remaining half in the second. This kind of randomized rollout (or waiting list design) is ethical in addition to being practical, because it

allows the company to assess halfway through its implementation whether or not the intervention is having the intended effect (Campbell, 1969; see also Shadish et al., 2002).

Downstream field experimentation

One exciting opportunity that is born of a field experiment is downstream field experimentation, or analysis of the indirect effects of an experimental intervention. Policy experiments randomize high-impact treatments that can be expected to set off a chain of events, for example, educational opportunities to low-income students. Investigators can re-contact or gather publically available data on treatment and control students down the road to ask important theoretical questions, for example whether more education (attained through the college scholarship) changes a person's political ideology, their social values, or the ways in which they raise *their* children (Sondheimer & Green, 2010). Measurement of those outcomes will still represent a causal chain of effect because the educational opportunity itself was randomly assigned. Downstream effects created by pre-existing experiments are low-hanging fruit that can be gathered up by graduate students or other investigators with fewer resources.

Hybrid lab-field experiments

As already discussed, some field experiments are more naturalistic than others. Hybrid lab-field experiments are experiments in which elements of artificiality are introduced for purposes of better control over treatment assignment or delivery, or for more precise measurement. Hybrid models are useful when investigators are studying a high impact independent or dependent variable.

For example, in an experiment on media and interpersonal influence, investigators randomly selected groups of friends in neighborhoods of Juba, South Sudan, to listen for a few hours to a recording of a previously broadcast radio program (Paluck, Blair, & Vexler,

2011). The participants in this study were the target audience of the radio program, and they listened in their own neighborhood with their typical listening partners. The artificial elements of this study were the researchers who sat with the group as they listened, to take notes on group reactions and to separately interview each group member when the program was finished. The laboratory-like surveillance involved in this study detracts from the overall naturalism of the field experiment, but it allows the investigators to obtain precise measurements of attention, verbal and nonverbal communication among the friends, and individual reactions to the program.

Another type of hybrid lab-field experiment is one in which an intervention is delivered in the laboratory and outcome measures are gathered in the field, such as when Walton and Cohen (2007) treated a random half of their sample of minority university students to a belongingness intervention in a laboratory, and then followed all of the sampled students' grades over the course of the year. University grades are a high impact dependent variable, which persuades us of the power of Walton and Cohen's (2007) laboratory-based intervention.

Designs to address challenges in the field

Spillover refers to the problem when the treatment or treated participants influence untreated or control participants. For example, a random subset of an apartment building's residents who receive a message encouraging recycling may communicate those messages in passing conversation to the untreated control residents of the building. Attrition refers to the problem of participants dropping out of an experiment, or missing dependent measures for some participants. Attrition is especially problematic if it is differentially triggered by one of the experimental conditions. For example, in an educational experiment, students might drop

out of a solitary studying condition more frequently than a social studying condition. Spillover and attrition are two problems that arise more frequently in field settings compared to laboratory settings.

Because certain types of spillover can underestimate the true effect of the treatment, and more importantly, because standard statistical analyses assume that units of a randomized experiment are independent (see West, this volume), many field experimental designs are set up to prevent spillover between units. The underlying principle of these designs is to select units for your experiment (people, situations, or communities) that are spaced out geographically, or to space your randomly assigned treatments temporally. Alternatively, when it is too difficult to prevent participants from interacting, you can group them together, randomly assigning *clusters* of interacting participants to treatment vs. control. Of course, some experiments are explicitly interested in spillover effects, such as the spread of influence throughout a network, and so they use designs that can detect influence among units (see Green & Gerber, in press, chapter 8).

Attrition happens more often in field settings because researchers are more likely to lose track of participants—participants in their natural environments feel less obliged to comply compared to those in laboratories—and because outcome measures are unavailable or blocked by an intervening agent. Of course, attrition is sometimes interesting in itself to study, because it may reveal whether your treatment is viable for real world use. For the most part, though, attrition is an impediment to learning about your experimental effect.

Some statistical approaches to this problem are to make strong assumptions about the potential outcomes among those who dropped out of the study, to put larger bounds around the findings, or to launch a new data collection that attempts to fill in missing values for a

randomly chosen subset of the missing participants or outcome measures (Green & Gerber, in press, chapter 7). Increasingly, investigators use technology to minimize attrition in the field, for example by sending participants text message reminders to their mobile phones (e.g., Tomlinson et al., 2011).

Quasi-experimentation in the field

Randomization is always recommended for causal inference in the field, since observational studies will bias average treatment effects (Gerber, Green, & Kaplan, 2004; West & Graziano, 2011). However, when random assignment is not possible in the field, social psychologists have at their disposal many creative designs that are based on the principles of random assignment and causal inference (Shadish, et al., 2002). We mention two of the most prominent designs here.

Regression discontinuity

A regression discontinuity design is useful when there is no randomization but there is a clear decision point along a continuous measure of eligibility for treatment regarding who will receive the treatment and who will not. If the decision point regarding eligibility is monotonically measured and rigid (i.e., it is monotonically increasing, not nominal like ethnicity, and there are no exceptions to the cutoff), then people who fall just above and just below the decision cutoff are likely to be, on average, comparable. This expectation of average comparability is similar to but not as strong as the expectation of comparability between two groups formed by random assignment. Thus, for the sample of people whose scores fall around the cutoff, investigators can test causal inferences about the treatment received by those who qualified (see West, this volume; Shadish et al., 2011).

Interrupted time series analysis

Interrupted time series analysis is used to assess the impact of a treatment that occurs at an observed point during a sustained time period of consecutive observations made on one or a set of outcome measures. Thus, unlike the other designs covered thus far that track outcomes for comparison samples, this design follows one sample over time. A causal relationship between the treatment and the outcome measures is proposed when investigators show that the slope or level of the outcome measures was significantly changed after the treatment. The causal case is strengthened when investigators show that the slope or level of other continuously collected measures that are unrelated to the treatment were in fact unchanged when the treatment occurred. One example of this method is the study of online journaling before, during, and after 9/11, that shows a change in the way people keeping regular journals responded to trauma as a result of 9/11 (Cohn, Mehl, & Pennebaker, 2004).

The Internet as a site for experimentation

As psychologists come to agree that the Internet can be a site of meaningful social expression, interaction, and behavior, they have profited from Internet sites and samples for psychological experimentation and measurement (Gosling & Johnson, 2010). The Internet is an efficient way to conduct survey experiments that can, depending on the goal, deliver both representative samples of large populations (Berinksy, Huber, & Lenz, 2010) and selective samples of difficult to reach populations (e.g., Glaser, Dixit, & Green, 2002). An extensive treatment of Internet experimentation is provided by Johnson (this volume).

Advantages and disadvantages of field experiments

The advantages of field experiments build upon the advantages of fieldwork more generally. They reveal causal relationships that hold up in the "rich thicket" of myriad social influences. Field experiments can also serve multiple research goals at once; working in the field on an experiment provides opportunities for qualitative observation that can inspire or refine future hypotheses. On a more personal level, running experiments in the field can be very rewarding, thanks to the social interaction and engagement with practitioners with knowledge of and insight into the psychological constructs of interest to investigators.

Of course, it is important to keep in mind several disadvantages to field experimentation. Because behavior is constrained less in the field than in the laboratory, problems with participation, or "take up," of the randomly assigned treatment, treatment spillover, and attrition occur more frequently. Certain types of interventions are more difficult to manipulate in field settings, or may be manipulated less precisely, such as cognitive or emotional states. In light of this fact, it is important to keep in mind that validity and precision are properties of research programs as well as individual studies (Brewer & Crano, this volume), and so field experiments can be profitably combined with other studies to answer questions that may be further out of reach in the field.

Finally, field experiments are more logistically challenging to launch and to manage compared to most laboratory experiments. Field experimenters must become bureaucrats, politicians, marketers, and public relations managers in order to organize, interact with, and appease all of the various people involved in the enterprise (or, they must hire or collaborate with competent partners who can do so). Challenges include getting permission from IRBs and from participating organizations in the field, and identifying the participant samples and

means to reach out to them and measure their outcomes. Below, we provide some general practical tips for field research.

Practical issues of research in field settings

Permission from stakeholders in the field. The first practical hurdle to overcome when you have chosen a research site or a population is establishing a collaboration agreement with the relevant stakeholder. The stakeholder may be the administration of a park where you plan to observe people, the director of a prison where you plan interviews, or the CEO of a company whose services you would like to observe or randomly assign. In our experience, there are two cases for field research that you can present to these stakeholders. One concerns the value of contributing to scientific knowledge about their environment or enterprise. The second, and in our experience the far more persuasive case, concerns the research's potential for benefit to the stakeholder. For example, it may be that parks are interested to see a descriptive analysis of interactions in various park spaces; prisons want your insights into social dynamics inside the prison, and companies want to know whether your treatments can improve their sales or efficiency. Of course, you can only promise to share data that will not compromise the well being or privacy of your participants.

It is fair, particularly when the scientific investigator represents a tax on the stakeholder's resources or time, to offer learning in return. This can include writing a non-academic, brief summary of the study's findings. To alleviate anxieties that a study will be providing a "thumbs up / thumbs down" assessment of an organization's environment or services, it is also important to explain to the stakeholder that psychologists are interested in processes as much as they are interested in outcomes. Thus, even if a treatment is found to have negative effects, your research can provide clues as to why that may be happening,

thereby allowing the partner to address the problem productively. This point, and the point that it is ethical to test *whether* interventions are having a beneficial or harmful effect, is useful when partnering with organizations that seek to promote prosocial change in the world.

But, even with all these points addressed, you may need to convey another type of assurance to stakeholders. They may need to feel confident that the researcher is supportive of their purposes. For instance, a while ago, one of us led a research team seeking to test certain theoretically relevant request strategies on blood donations. Arranging for the tests necessitated the cooperation of the local blood services organization and required that we convince their officers that a collaboration would be worthwhile not just to us but to their organization's vitality. Although we thought that we had made a compelling case in these regards, the organization's chief administrator hung back from authorizing our project. It wasn't until a junior member of his staff quietly informed us of the reason for her boss's reluctance that we understood what we had left out of our persuasive approach. "None of you has given blood yet," she whispered during a break in one of our meetings. Mildly chastised but properly enlightened, we asked just before the meeting's close how we might contribute to the organization's important goals by donating a pint or two of blood ourselves. An opportunity was arranged, blood was drained, and full approval of our project followed within the week.

Memorandum of understanding. When you have come to an agreement with a field-based stakeholder to conduct your research, it is advisable to draw up a simple memorandum of understanding regarding exactly what the study activities will entail, your ownership of the data, your intention to strip any participant identity from the data, and your right to publish the data. Sometimes it is wise to include a clause that you will omit the identity of your field

site, if the stakeholder desires, and that you will share the data if the stakeholder has a use for it (with all identities protected as mandated by your Institutional Review Board).

Institutional Review Boards. IRB's are sometimes much more reluctant to grant permission to conduct research in field settings, although this is quite varied from institution to institution. Our advice, particularly if you hope to conduct research in a setting that involves some degree of sensitivity, vulnerable populations, or danger, is to do your homework. Contact researchers who have done work in similar settings and ask for a copy of their IRB application and approval. Propose similar safeguards in your own work, and cite previous work that was approved for that setting. If you can, contact your IRB board ahead of time, and ask if there are immediate issues that you should remember to address in your application. Potential issues include the possibility of obtaining fully informed consent, the question of how to recognize privacy rights in proposed observation, and the potential for embarrassing or compromising behavior to be recorded. Provide as much information as you can on the field setting, so that decisions are not made on the basis of too little information about the potential risks.

Conclusion

The prospect of adding field research to an existing program of laboratory research may trigger different reactions among social psychologists. Psychology students may worry about the reception of field research in their department or in journals where they hope to publish. Faculty may worry about the same issues, and additionally about the time or the learning curve involved in mounting a line of field research. To both groups of psychologists, we emphasize once more that field research

can be used for the *development* of psychological theory, not solely the (underappreciated) application of theory. Moreover, we expect theories developed with the aid of field research to be more psychologically and pragmatically consequential.

To students in particular, we add that the activity of choosing the right field setting and real world variables will compel you to describe and make concrete your theoretical constructs of interest like no other requirement in your training program. To psychology faculty, we point to William McGuire's (1973) once more relevant advice: "if the budgetary cutbacks continue, instead of running ever faster on the Big-Science treadmill, we [should]…rediscover the gratification of personally observing the phenomena ourselves and experiencing the relief of not having to administer our research empire" (p. 455).

Personally observing the psychological phenomena of interest may include creating laboratory simulations that are much more realistic. And certainly, all researchers should balance their research portfolios to include some fieldwork, some laboratory work, and some pure theoretical work. But we will end on a challenge: if we are correct, that fieldwork creates the most accurate and consequential theoretical maps for psychology, then our field today finds itself in a concerning state of imbalance. The volume of insights from laboratory work far outweighs those from the field. Conduct research in the field, and create theoretical maps that will bring psychological science into its next era.

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