SCIENTIFICALLY BASED RESEARCH IN EDUCATION AS A REGIME OF TRUTH: AN ANALYSIS USING FOUCAULT'S GENEALOGY AND GOVERNMENTALITY

by

JESSICA LYNN VAN CLEAVE

(Under the Direction of Elizabeth St. Pierre)

ABSTRACT

This poststructural analysis used Foucault's theories of genealogy and governmentality to examine the discourse of scientifically based research as it is produced, maintained, and regulated within the federal government, the National Research Council, and the American Educational Research Association in order to demonstrate that it functioned as a regime of truth. The analysis demonstrated that there were significant discontinuities in the discourse of scientifically based research that unraveled its history and revealed it as a construct rather than as the truth of high quality science in educational research.

 INDEX WORDS:
 Scientifically Based Research, Genealogy, Governmentality, Michel

 Foucault, Poststructuralism, Non-innocent Science
 Foucault

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JESSICA LYNN VAN CLEAVE

AB, The University of Georgia, 2001

MEd, The University of Georgia, 2004

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JESSICA LYNN VAN CLEAVE

Major Professor: Committee: Elizabeth St. Pierre Mark Faust Nate Kohn Kathryn Roulston Michael Feuer

Electronic Version Approved:

Maureen Grasso Dean of the Graduate School The University of Georgia August 2012

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CHAPTER ONE

SPEAKING SCIENCE

What types of knowledge do you want to disqualify in the very instant of your demand: 'Is it a science'? Which speaking, discoursing subjects—which subjects of experience and knowledge—do you then want to 'diminish' when you say: 'I who conduct this discourse am conducting a scientific discourse, and I am a scientist'?

~Foucault, 1980a, p. 85

Introduction

In 1999, the Reading Excellence Act (REA) passed unceremoniously and was signed into law by President Bill Clinton. This law was intended to improve reading skills and instructional practices through the findings of scientifically based reading research. Consequently, a definition of scientifically based research, now commonly referred to as SBR, was written into that federal law. Within the span of a few years, the definition was repeated in additional federal laws, was used for prioritizing federal funding for educational research, and was administered by national organizations established for that purpose. SBR also influenced standards of research and reporting research within educational research organizations. In other words, SBR became instrumental in the practices of government institutions, requiring education researchers and others who wish to conduct so-called scientific research to discipline themselves according to the rationality of the discourse of SBR. The practice of SBR has persisted and expanded and at present, SBR is a discourse that claims to be science and disqualifies other ways of knowing. In short, SBR defines the truth of high quality research in education. Responses to the critique I provide here claim that it was never the intention of SBR to disqualify other ways of knowing. However, I looked not at intentions but at effects. As Foucault (as cited in Dreyfus & Rabinow, 1983) explained in an interview, "people know what they do; they frequently know why they do what they do; but what they don't know is *what what they do does* [emphasis added]" (p. 187). Examining the effects of claiming science is one focus of this dissertation.

In the words of Foucault (1983), "modes of inquiry [like SBR] which try to give themselves the status of the sciences" (p. 208) are actually engaging in games of truth, "whether they be games of truth which take on the form of science or which refer to a scientific model, or games of truth like those that can be found in institutions or practices of control" (Foucault, 1997a, p. 281). These games of truth are not just definitions or individual reports but are "an ensemble of rules for the production of truth" (p. 16) that became available at certain moments in time as solutions to problems of the present. Collections of institutions and subjects who are produced with and through discourse enact these rules for the production of truth, producing a regime of truth. As a regime of truth, SBR is not "naturally occurring" but is instead "historically constituted," and cannot be thought in the same way outside of that historical moment (McWilliam, 2000, p. 166). This dissertation addresses the question of how SBR became "something that can and must be thought" in the present (Foucault, 1984/1985, p. 7). In the remainder of this chapter, I describe how I came to SBR as an object of knowledge and provide the statement of the problem and research questions that guided this study. I also explain the broad theoretical framework for the dissertation, postmodernism/poststructuralism, and provide an introduction to the Foucauldian analyses I employed in this study. I end the chapter with a brief description of the structure of the dissertation.

Coming to SBR

When I entered my doctoral program in the mid 2000s, I almost immediately recognized how the discourse of SBR permeated educational research. SBR in education was a commonplace term in coursework, conferences, and hallway conversations, but I was not initially interested in it. I did not understand how discussions of abstract concepts written into federal legislation were relevant to my work or life. However, as I moved through my doctoral program and began studying qualitative research and postmodern theories¹, the relevance of SBR to qualitative inquiry and my work became increasingly clear. The federal legislation that produced SBR, which will be discussed at length in Chapter Five, *mandated* educational research methodologies that are enabled by positivism², an epistemology incommensurable with the postmodernism in which I live and work. This marks the first time that research methodology has been legislated, and that determination was made by individuals who, often, were neither educators nor educational researchers.

Conceptions of the self, power, reality, truth, knowledge, and so on rest on completely different assumptions in positivism than they do in other epistemologies. Yet, according to

¹ I use postmodern theories here to designate work by scholars who have been categorized as postmodern or poststructural. Postmodern theories include theories about subjectivity (e.g. Foucault, Butler), power (e.g. Foucault), deconstruction (e.g. Derrida, Spivak, Caputo), and so on.

² Positivism, broadly speaking, is a philosophy of science that privileges the accumulation of knowledge and believes that only statements that can be verified by empirical data are valid. Positivism claims to be objective, theory-free, and value-free. For this study, I treat positivism as one of the enabling conditions of SBR, which I discuss in Chapter Four.

federal mandates, only positivist assumptions can count as science and can influence theory, practice, and policy in education. According to definitions of SBR, which will be discussed later, and the policy written from those definitions, research grounded in theories and methodologies that are not positivist cannot count as science and cannot be worthy of influencing theory, practice, or policy in education. Discussions and publications about SBR specifically point to postmodernism as the scapegoat for the problems with educational research: "[postmodernism] lacks rigor, fails to adhere to widely-accepted principles for the conduct of science, and fails to provide a solid evidence base that can guide real-world decisions that policymakers and practitioners make" (Walters, Lareau, & Ranis, 2009, p.17). For example, I became aware that institutional powers such as the U.S. Institute of Education Sciences (IES) and National Research Council (NRC) committees questioned the validity and the scientific value of poststructural and postmodern research. I began to realize that SBR did indeed have a direct impact on my work and that I needed to investigate SBR to understand how it came to carry the force of truth.

This dissertation uses Foucauldian genealogy and governmentality to examine the discourse of SBR to understand how it came about (i.e., what were its enabling conditions?), how it was maintained and perpetuated (i.e., what were its conditions of existence?), how it became the truth of high quality educational research (i.e., how did SBR function as a regime of truth?), and how it could be made visible as a discourse (i.e., what discontinuities exist in the history of SBR?). In order to do this work, I analyzed documents from the federal government, e.g., legislation and the Congressional Record; the National Research Council, e.g., the 2002 and 2005 consensus reports; and the American Educational Research Association, e.g., the Council minutes and its two sets of standards for reporting on research. I also interviewed scholars

involved in the debates about SBR in order to document their present understanding of that discourse.

Statement of the Problem

In this poststructural study, I used Foucauldian genealogy and governmentality to examine documents and interview scholars associated with the Federal government, the National Research Council, and the American Educational Research Association in order to understand how scientifically based research functioned as a regime of truth and to identify discontinuities that disrupt the history of scientifically based research and make it visible as a discourse.

Research Questions

- 1. What were the enabling conditions that made SBR possible?
- 2. How is SBR maintained, regulated, and resisted?
- 3. How does SBR function as a regime of truth?
- 4. What discontinuities exist within the discourse of SBR that make it subject to critique?

Poststructuralism/Postmodernism

Poststructuralism, a term often used interchangeably with postmodernism, is the name given to a set of critiques that questions foundations and transcendental signifieds. One way to distinguish postmodernism and poststructuralism is that postmodernism deals with the *avant garde* in culture (e.g., art, music, architecture), and poststructuralism involves academic critiques of structuralism. However, because culture and philosophy cannot be thought separately, some descriptions of poststructuralism and postmodernism may be helpful. Peters (2004) described poststructuralism as a "specifically philosophical response to the alleged scientific status of structuralism" (p. 8). Poststructuralism critiques particular descriptions of reason and science privileged in Enlightenment thought. Poststructural theories argue that it is not only impossible

but would be undesirable to separate the discourse of rationality from the outside interests that shape it. Flax (1994) wrote the following about postmodernism:

Postmodern discourses are all deconstructive in that they seek to distance us from and make us skeptical about beliefs concerning truth, knowledge, power, the self, and language that are often taken for granted within and serve as legitimation for contemporary Western culture. (p. 465)

In other words, postmodern theories shift the focus from asking essentializing questions about the meaning of things, what something essentially *is* (e.g., what *is* scientifically based research in education?) to asking questions like Bové's (1995): "How does it [e.g., scientifically based research] function? Where is it to be found? How does it get produced and regulated? What are its social effects? How does it exist?" (p. 54).

Poststructuralism also offers answers to those kinds of questions, so the distinction between postmodernism and poststructuralism is not always useful. Additionally, theorists like Foucault have been labeled both as postmodernists and poststructuralists. Postmodernism is often used as an umbrella term for theories variously called postmodern and/or poststructural because, as Rajchman (1987) wrote, postmodernism is "a motley and elastic range of things" (p. 49). Major figures of postmodern theory include Jacques Derrida, Michel Foucault, Judith Butler, Jacques Lacan, and Jean-Jacques Lyotard. Butler (1992), in particular, warned that some might use the term postmodernism to gather together very different scholars doing very different work in order to "dispense with them all at once" (p. 5). I will use the term *poststructural* in discussing the theory that grounds my work. However, because the literature on SBR uses the term *postmodernism* in the aforementioned umbrella fashion, I will also use that term when discussing SBR.

Introduction to Foucault's Theories

In the latter half of the 20th century, the French philosopher and historian Michel Foucault developed and used various poststructural analyses to study topics in which he was interested, including madness, the prison, and sexuality. He called his historical analyses archaeology and genealogy, depending on the analysis itself. Archaeology was chiefly concerned with the relationship between truth and knowledge while genealogy was concerned with the relationship between truth and power. Foucault called his ethical analysis of the construction of subjectivity care of the self. He also used a power/knowledge analysis in all of his work. In his later work, Foucault became interested in the state and government and developed the analytical concept of governmentality. Rather than asking what government is or should be, governmentality focuses on how government functions and how practices form within governments to produce certain effects. This work extended Foucault's genealogical work and shifted the focus from the body of the individual to the body of the population. In short, governmentality is Foucault's genealogy of government. Because SBR is a discourse that emerged within the government, a combination of genealogy and governmenality enabled an analysis in this study that questions SBR's claims to the truth about quality educational research.

Dissertation Structure

Using genealogical analysis to make the present conception of SBR seem less inevitable requires a different kind of thinking about history, which I will describe in detail in the next chapter. I represent this different kind of thinking in the structure of the dissertation, which is divided into eight chapters instead of the traditional five. In this chapter, I have introduced the concept of SBR—the problem of the present I analyze—and this dissertation study. In the next chapter, I explain the theories of genealogy and governmentality in detail, as well as how they

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connect with one another and to SBR. In Chapter Three, I explain how I used two key concepts, *discontinuities* and *power/knowledge*, to analyze the documents and interviews that served as data in this study. Chapters Four through Seven provide the findings of my study: Chapter Four describes SBR in the present as it functions as a regime of truth; Chapter Five is a discussion of the enabling conditions for SBR, Chapter Six chronicles the supposed origin of SBR and its conditions of existence, and in Chapter Seven, I describe discontinuities within the discourse of SBR. In Chapter Eight, I provide implications of this study for educational research.

CHAPTER TWO

Foucault's Theories

Introduction

Foucault, in descriptions of his various analyses, encouraged others to take them up in whatever fashion would be most useful. Each iteration, he wrote (1971/1972), "question[s] knowledge in a different direction and describe[s] it in a different set of relations" (p. 195). Tamboukou and Ball (2003), drawing on Foucault's generous invitation for researchers to use his theories "as a tool of analysis rather than as a closed theoretical framework," asserted "the need for new genealogies to be written" (p. 1). In other words, they call on researchers to use Foucauldian genealogy to inform their work, expanding on the possibilities of genealogical analysis. This study takes up that call by producing a genealogical analysis informed by governmentality that questions how SBR became the truth about high quality research in education within three key institutions: the federal government, the National Research Council (NRC), and the American Educational Research Association (AERA).

In this chapter, I describe the theoretical underpinnings that enable this genealogical work. I begin by situating scientifically based research (SBR) as a discourse that is deeply imbricated with power and truth, specifically in the production of knowledge and in the institutions and subjects that maintain and regulate that knowledge. I then describe the Foucauldian theories that I used in this study to analyze how the discourse functions genealogy, a historical analysis that begins with a problem in the present, and governmentality, Foucault's genealogy of government.

Discourse

Scientifically based research (SBR) in education is not just a concept or definition. It is a discourse. Foucault used the concept *discourse* differently from the way it is commonly used in linguistic theory—it is not a simple speech act or collection of speech acts. Foucault (1971/1972) wrote, "discourse is not the majestically unfolding manifestation of a thinking, knowing, speaking subject, but, on the contrary, a totality, in which the dispersion of the subject and his discontinuity with himself may be determined" (p. 55). As Hacking (1986) explained, "[d]iscourse, then, is to be analysed not in terms of who says what but in terms of the conditions under which those sentences will have a definite truth value, and hence are capable of being uttered" (p. 32). In this understanding of discourse, terms such as language, thought, knowledge, and power are situated squarely within the material conditions of life, rather than being abstract philosophical concepts.

Foucault (1981) wrote about the influence of institutional power on discourse, claiming that "discourse is not simply that which translates struggles or systems of domination, but is the thing for which and by which there is struggle, discourse is the power which is to be seized" (pp. 52-53). Discourse is more than a just way to think and make meaning; it is productive as well as descriptive. For example, discourse produces subjects and "constitute[s] the 'nature' of the body, unconscious and conscious mind and emotional life of the subjects" it produces (Weedon, 1987, p. 108). According to Bové (1995), discourse studies " the organized and regulated, as well as the regulating and constituting, functions of language" and "its aim is to describe the surface linkages between power, knowledge, institutions, intellectuals, the control of populations, and the modern state as these intersect in the functions of systems of thought" (pp. 54-55).

Consequently, discourse produces its own truth that, within its structure, begins to seem real and true, normal, everyday, and taken-for-granted. Discourse is not simply linguistic; the discourse of SBR, for example, has produced the world of educational research and the people who live in it. Genealogy, then, is a useful analysis of discourse because it examines how discourse has come to be seen as true. I describe genealogical analysis below.

Genealogy

Genealogy is a history of the present that attempts to create a counter-narrative that consists of the "the erudite knowledge and local memories which allow us to establish a historical knowledge of struggles and to make use of that knowledge today" (Foucault, 1980a, p. 83). This is a type of inquiry that according to Meadmore, Hatcher, and McWilliam (2000), "seeks to inquire into processes, procedures, and techniques through which truth, knowledge, and belief are produced" (p. 463). Genealogy identifies a problem in the present and through analysis of historical documents, traces how it was possible to think that problem. In other words, genealogy requires the simultaneous study of both the object of knowledge—in this case, SBR and the systems of thought that enabled its emergence.

Emergence, however, is not an origin. That is to say, while an origin is a unitary, lofty, intentional beginning, emergence is contingent, "the moment of arising" (Foucault, 1977, p.83). According to Prado (2000), who wrote extensively about Foucauldian genealogy, "[e]mergence is appearance or advent enabled by collision of forces, some of which enhance, nullify, or redirect others, and some of which combine with others to form new forces" (p. 37). In other words, emergence is somewhat like Derrida's (2003) event—a singularity, something that falls upon you, the thing that arrives. Emergence, then, can take a variety of forms, whether values, institutions, subjects, or conceptual constructs. Although some concepts emerge and disappear

again, others, like SBR, grow in strength, are maintained and perpetuated in power relations, and structure our lives. Prado (2000) explained:

What emerges and gains dominance then looks to be predetermined and is legitimized by its apparent inevitability. The first task of adherents of what emerges, whether it is an idea, a value, a discipline, or an institution, is to establish it as natural, as inevitable, as truth that has been discerned. (pp. 37-38)

Genealogy, then, must counteract the seeming inevitability of a concept like SBR by seeking its lowly beginnings. Drawing on the genealogical analyses Foucault developed, one can examine the effects of history, of power, and of discourse, beginning with a problem that exists in the present.

Foucault's Genealogies

Although Foucault invited others to take up and use genealogy, it is important and necessary to explain how Foucault himself conducted genealogy. Genealogy produces a counterdiscourse "capable of opposition and of struggle against the coercion of a theoretical unitary, formal and scientific discourse" (Foucault, 1980a, p. 85). For example, Foucault identified an issue, such as sexuality or punishment, that had come to be seen as natural, normal, and "without history" (Foucault, 1977, p. 139) and problematized it. That identification is the first step in Foucauldian genealogy. Foucault (1984a) explained problematization as "the development of a domain of acts, practices, and thoughts that seem to me to pose problems for politics" (p. 384). He claimed to "ask politics what it had to say about the problems with which it was confronted...[and] question it about the positions it takes and the reasons it gives for this" (p. 385). Identifying an idea that has become normal but is crumbling under its own weight and then making it a problem, an object worthy of examination, produces a problematization. Foucault said a problematization should be conceived "not as an arrangement of representations but as a work of thought" (p. 390). The work of genealogy is to identify a problematization in the present and then trace its emergence.

Foucault developed and applied his genealogical analysis in several projects. In one he looked at the history of discipline and punishment in France and in another the history of sexuality in France. In *Discipline and Punish: The Birth of the Prison* (1975/1977), Foucault focused on criminality and punishment, especially on how the body itself was disciplined and punished in his culture, primarily through incarceration during the *ancien regime*. He explored how we had come to believe that reform is the ultimate goal for the criminal and how the prison both sought that goal and made it impossible. However, Foucault also demonstrated that how we have understood criminals and punishment differently throughout history provided a counter-history of the constitution of criminality. In *The History of Sexuality: An Introduction, Volume One* (1976/1978), Foucault examined sexuality and questioned the perception that sexual behavior was out-of-control and in need of discipline. Everyday discourse of sexuality promoted the idea that desire, sexual acts, and deviance were repressed by society at large. This repression, much like the discipline of the prisons, had both its intended effects and the opposite effects in that sexuality proliferated in discourse.

Foucault demonstrated that the presumed truth of the nature of discipline and sexuality was, instead, the result of political, cultural, and historical forces. He demonstrated this by identifying and describing subjugated knowledges he found in various documents such as laws and police registers that he read while doing archival research to see what was said when, when laws were made, and how subjects were constituted through documents. He claimed that traditional history "is now trying to define within the documentary material itself unities,

totalities, series, relations," and his archaeology and genealogy attempt instead to disrupt the totality of history by accessing subjugated knowledges. Those subjugated knowledges— "historical contents that have been buried or masked in functional coherences or formal systematizations" or "naïve knowledges, hierarchically inferior knowledges, knowledges that are below the required level of erudition or scientificity" (Foucault, 1997b, p. 7)—demonstrated that ideas of sexuality that seemed natural and rationally inevitable were instead the result of contingent turns of history and illustrated the association between sexuality and power structures in modern society.

Traditional History v. Effective History. Foucault (1977) explained that genealogical work, or *effective history*, contrasted with what he termed *traditional history* in several ways. Traditional history "aims at dissolving the singular event into an ideal continuity—as a teleological movement or a natural process" (p. 154) while effective history "deals with events in terms of their most unique characteristics, their most acute manifestations" (p. 154) and assumes no rationality or progress to the order of events. Further, "[h]istory becomes 'effective' to the degree that it introduces discontinuity into our very being" (p. 154). History is not orderly, argued Foucault, "it is a profusion of entangled events" (p. 155).

Put simply, "[t]he purpose of history, guided by genealogy, is not to discover the roots of our identity but to commit itself to its dissipation. It does not seek to define our unique threshold of emergence, the homeland to which metaphysicians promise a return; it seeks to make visible all of those discontinuities that cross us" (Foucault, 1977, p. 162). Foucault (1971/1972) wrote, "discontinuity was the stigma of temporal dislocation that it was the historian's task to remove from history" (p. 8). Consequently, discontinuities are an important analytic tool in effective history. I describe my use of discontinuity as an analytic tool in Chapter Three.

Genealogists are not interested in producing a tidy history that reinforces a present concept, like SBR. Instead, genealogical work undermines the self-evidence of a concept by undermining the idea of an elevated rational origin and describing instead the enabling conditions for the emergence of a concept.

Origins. Contrary to traditional history, which claims an origin point from some period in the past and works its way forward, genealogy begins with the present, focusing on an idea that has come to be true. The object of genealogical analysis is an idea, such as SBR, that has been taken up and reified through discourse resulting in the development of a web of power and control that perpetuates the truth and validity of its existence.

As mentioned earlier, traditional history searches for the first instance of an idea or problem. Genealogy instead examines its *enabling conditions*. For example, a genealogist might ask what allowed a concept like SBR to be spoken into existence. Foucault described those enabling conditions in terms of the emergence described above. Prado (2000) explained that "[w]hat emerges is not the culmination of anything but is a consequence of an accumulation of factors with no inherent interrelatedness. It is only the retrospective imposition of some historical interpretation that makes those factors appear to be more than coincidentally related" (p. 37). In other words, the occurrences of the past possess no grand design that leads teleologically to some conclusion in the present. Rather, the *interpretation* of history produces history.

Counter-Memory. To eschew appeals to an origin, Foucault asked, "[h]ow is it that at certain moments and in certain orders of knowledge, there are these sudden take-offs, these hastenings of evolution, these transformations which fail to correspond to the calm, continuist image that is normally accredited?" (Foucault, 1980b, p. 112). In *The Archaeology of Knowledge and the Discourse on Language*, Foucault (1971/1972) described history as a dispersion of

events, only some of which were related. However, when a historian traces those events through time, he creates a straight line rather than a dispersion. This is why genealogy is appropriately called counter-memory. Human memory functions in personal experience much as it does in traditional history. Memory creates out of seemingly disparate events a narrative from which subjects can produce their subjectivity. The new history enabled by genealogical analysis is constituted primarily of events that have either been left out or devalued in traditional histories of the same topic, and it "fragments what was thought unified" (Foucault, 1977, p. 147). That is, genealogy sifts through the metanarrative produced by traditional history and searches for what has been forgotten, what might disrupt, interrupt, or rupture the tidy story of the past and leave in its place "an unstable assemblage of faults, fissures, and heterogeneous layers" (p. 146).

Genealogy for SBR

As described above, the discourse of SBR has come to be seen as natural, normal, and true. Consequently, Foucauldian genealogy is helpful in disrupting the self-evidence of that discourse. In taking up the invitation for researchers to reinscribe genealogy in their projects, I used structural elements of Foucault's genealogy—lines of descent, enabling conditions, and conditions of existence—to make the discourse of SBR visible. Below, I briefly describe how I used these concepts in this study.

Lines of Descent. Like family genealogies, Foucauldian genealogy traces lines of descent. However, the lines of descent in genealogy are not direct, linear linkages; instead, they follow unplanned paths of dispersion. Lines of descent function similarly to a citational trail. To follow a citational trail, a researcher begins with a document and examines the sources that document cites. She follows the trail of citations from one document to the next in order to trace

emergence or enabling conditions. Likewise, the genealogist traces lines of descent from the problem of the present to the ideas that made them possible.

Enabling Conditions. Enabling conditions, as described above, are the conditions of possibility of a discourse. The enabling conditions of a discourse like SBR are the set of circumstances that enabled it to be thought, described, written into federal law, funded, accomplished, reported in the literature, discussed at conferences, and so on—all the conditions that enabled it to become an object of knowledge and practice, a reality, a truth.

Conditions of Existence. The discourse of SBR is produced by positivist science, but it is maintained by its rules of formation. Foucault, (1971/1972) explained,

the conditions to which the elements of this division (objects, modes of statement, concepts, thematic choices) are subjected we shall call the *rules of formation*. The rules of formation are conditions of existence (but also of coexistence, maintenance, modification, and disappearance) in a given discursive formation.

(p. 38)

Rules of formation establish what is meaningful within discourse. In other words, they are the discursive regularities that maintain a discourse. Therefore, a description of the conditions of existence of a discourse would describe the *life* of that discourse—its history.

Governmentality

In the following sections, I describe Foucault's theory of governmentality. First, I situate this theory historically within Foucault's larger body of work. Then I provide various definitions of governmentality both from Foucault and from other scholars who have taken up his work. Finally, I explain important concepts in the context of governmentality—discourse, government, and discipline.

History of Governmentality

In the aftermath of the political upheaval of May of 1968 in France, Michel Foucault was hired as a professor of philosophy at an experimental university, Paris VIII, Vincennes, where, as department head, he hired a politically active group of young academics. Not long afterwards, Foucault left that position to become Chair of the Department of History of Systems of Thought at the prestigious Collège de France, a position created for him so that he could report on his own research and thinking, which did not fit neatly into the traditional academic disciplines. Beginning in 1970 and ending with his death in 1984, Foucault gave a yearly lecture series at the Collège de France that was recorded, transcribed, and subsequently published in eight volumes. Those eight volumes have since been translated from the original French into English: *Psychiatric Power: Lectures at the Collège de France, 1973-1974, Abnormal: Lectures at the* Collège de France, 1974-1975, "Society Must Be Defended": Lectures at the Collège de France, 1975-1976, Security, Territory, Population: Lectures at the Collège De France 1977–1978, The Birth of Biopolitics: Lectures at the Collège de France, 1978-1979, The Hermeneutics of the Subject: Lectures at Collège de France 1981-1982, The Government of Self and Others: Lectures at the Collège de France 1982-1983, and The Courage of Truth: Lectures at the Collège de France 1983-1984. The 1978 and 1979 lectures focused on a new domain of research that Foucault called governmentality or governmental rationality.

As Gordon (1991) observed, it was no accident that Foucault was interested in government at this time, because "Foucault's 1978 course overlapped with an unexpected defeat in French parliamentary elections of an alliance of Socialist and Communist parties" and "his 1979 course ended a few weeks before Margaret Thatcher's election as British Prime Minister" (p. 6). Though the focus of his 1981-1982 lectures shifted to topics such as subjectivity and care of the self, Foucault maintained his interest in governmentality.

In the two collections of lectures titled, *Security, Territory, and Population* and *The Birth* of *Biopolitics*, Foucault outlined his concept of governmentality, most notably in the lecture of the same name (published first in Italian in 1978, in English in 1979, in French in 1986; then in a new English translation in 1991). In the lecture, he described various modes of state power, including sovereignty, discipline, and governmentality. Importantly, Foucault did not see the state as having an original essence or permanence; rather, he believed that the state is produced by the *practices* of government. Therefore, Foucault's notion of governmentality included not only the practices employed by governments to produce their desired citizens but also the practices subjects use to govern themselves within the discursive and material structures of the state. Foucault continued his work on governmentality until his death, while concurrently working on his ethical analysis, care of the self. Although care of the self primarily concerned government in the personal domain—the self's government of itself—governmentality dealt with government in the political and social domains. Foucault planned to publish a book on governmentality at some point but did not.

Definitions

Simply put, governmentality is a way of thinking about the practice of government that includes, "who can govern; what governing is; what or who is governed" (Gordon, 1991, p. 3). In his essay, "Governmentality," Foucault (1978/1991a) claimed that governmentality emerged in the 18th century, and he described it as follows:

By this word I mean three things:

- the Ensemble formed by the institutions, procedures, analyses, and reflections, the calculations and tactics that allow the exercise of this very specific albeit complex form of power, which has as its target population, as its principal form of knowledge political economy, and as its essential technical means apparatuses of security.
- 2. The tendency that, over a long period and throughout the West, has steadily led toward the preeminence over all other forms (sovereignty, discipline, and so on) of this type of power—which may be termed "government"—resulting, on the one hand, in the formation of a whole series of specific governmental apparatuses, and, on the other, in the development of a whole complex of knowledges [savoirs].
- 3. The process or, rather, the result of the process through which the state of justice of the Middle Ages transformed into the administrative state during the fifteenth and sixteenth centuries and gradually becomes "governmentalized." (pp. 102-103)

That is, Foucault considered the art of government as it is currently and was historically in addition to how governmentality has been made thinkable and practicable.

His theory of governmentality has been taken up and described in various ways by numerous scholars, many of whom believe that the critique offered by the theory is a practical and useful force for political action and change in attitudes and assumptions. Allen (1998) wrote that

"Governmentality" is a neologism Foucault introduced to combine the idea of *government*, or the power to direct conduct, with the idea of a peculiar *mentality* with which the activity of government has been approached in modern times: the presumption that "everything" can, should, must be managed, administered, regulated by authority. (p. 179)

Scheurich (1994) defined governmentality as "a word that Foucault used to denote the emergence of a kind of governance mentality that expands its reach into all aspects of the lives of its citizens" (p. 306). Davidson (1994) described governmentality more in terms of power relations, explaining, "Foucault wanted to analyze power as a domain of strategic relations between individuals and groups, relations whose strategies were to govern the conduct of these individuals" (pp. 118-119). These definitions demonstrate that governmentality was a new way of thinking about governmental power in which governing is not just about sovereign power (e.g., power conferred on a monarch or elected president) but is instead a complex set of power relations in which "the conduct of conduct" (Foucault, 1982, pp. 220-221) is multidirectional. Not only does a sovereign act on his subjects, but the subjects also act on the sovereign, each other, and themselves.

Discourse and Governmentality

Governmentality places the practice of government on the level of discourse; therefore, practices within government can be conceived as discursive formations. In other words, governmentality makes the practice of government "thinkable and practicable both to its practitioners and to those upon whom it was practiced" (Gordon, 1991, p. 3). The discourse of SBR, in the wake of NCLB and the NRC reports, is a space in which power/knowledge is in play. Consequently, the discussion about the discourse of science produces both science and scientists in the pursuit of scientifically based research.

One might argue that these questions of discourse are just language, or an issue of semantics, but language produces people and institutions (Foucault, 1971/1972). Lincoln and Cannella (2004) provided examples of the shift in the language of education from that of equal opportunity to that of "blame and punishment," switching the discourse of education from a

social justice focus to a process of "demonizing and labeling" (p. 9). Shifts enabled by language have real, material implications for people, and the effects can be quite damaging.

The implications of SBR are "part of a much larger fabric which is being woven, a whole cloth of methodological purity strongly attached to an elitist patriarchal belief system supporting a particular view of the state, of the federal government, and more important, of what constitutes 'truth'" (Lincoln & Cannella, 2004, p. 10). The conservatism of this discourse has produced more institutions, such as the Institute of Education Sciences and the What Works Clearinghouse. Perhaps most disconcerting is that this "conservatism assumes first and foremost a *monoculture*—a single discursive and methodological community that speaks the same language and more important, takes as its concerns the same issues from the same perspectives" (Lincoln & Cannella, 2004, p. 8). Or, as Bloch (2004) explained,

The role of power in the naming of rigor, truth, and science is denied. Although it is "true" that the federal government wants guidelines for determining what is valuable in educational research, the conception of science, as discussed in the [2002 NRC] report, eliminates many potential ways in which to look at the intersection of theory, policy, science, pedagogy, and power. (pp. 101-102)

To speak or work outside SBR "is to endanger one's professional standing, if not entire career" as the regime of truth "forms a 'web of power' in which all who wish to participate...are ensnared" (Lincoln & Cannella, 2004, p. 8).

One of the marks of this ensnarement was what Lather (2004) described as "web scrubbing' where the U.S. Department of Education is deleting research, including ERIC digests, that it deems unsupportive of Bush administrative agendas" (p. 15). Lather (2004) explained that the current state of educational research seems to ignore both 30 years of "the social critique of

science" (p. 17) as well as the constantly changing definitions and understandings of science as one field of discourse among others.

Government

In order to understand governmentality, it is useful to explain what Foucault (1982) meant by government, which he described as the "conduct of conduct" (pp. 220-221). Dean (1994) defined it as follows:

Government is any more or less calculated and rational activity, undertaken by a multiplicity of authorities and agencies, employing a variety of techniques and forms of knowledge, that seeks to shape conduct by working through our desires, aspirations, interests and beliefs, for definite but shifting ends and with a diverse set of relatively unpredictable consequences, effects and outcomes. (p. 11)

Governmentality is a set of practices of the self on the self and on others. Or, as Gordon (1991) wrote, "government as an activity could concern the relations between self and self, private interpersonal relationships involving some kind of control or guidance, relations within social institutions and communities, and finally, relations concerned with the exercise of political sovereignty" (p. 3). What this means is that government includes not only how a subject relates to and disciplines others but also how she relates to and disciplines herself. That is, governmentality relies on self-discipline in order to function.

In his lecture "Governmentality" (1978/1991a), Foucault discussed the art of government in relation to his reading of Machiavelli's (1910/1992) *The Prince*. He explained how the conception of government described by governmentality developed in opposition to the ultimate sovereignty presented by Machiavelli. Foucault did not believe the prince was external to his principality, as Machiavelli claimed, but that the art of government and the practices of government are inextricably linked to that which is governed and those who govern. For example, the prince's object of government is his principality, where power is exercised. In the art of government, "power is exercised over a complex of men and their 'relations to things'" (McNay, 1994, p. 115). "Things" in this case are money, resources, weather, reproduction, and so on, as well as occurrences such as accidents, arguments, and bad luck.

Discipline

McNay wrote that "in this sense, government resembles discipline, in that disciplinary techniques aim to maximize the utility and productive output of the human body" (p. 115). Governmentality, then, also relates to Foucault's genealogy of prisons, as the discipline of the body of the individual person expands to become discipline of the body of the state. Foucault (1975/1977) explained that "discipline produces subjected and practised bodies, 'docile' bodies. Discipline increases the forces of the body (in economic terms of utility) and diminishes these same forces (in political terms of obedience). In short, it dissociates from the body" (p. 138). In the case of governmentality, the object of study is the population rather than the individual body, a population that is supposed to remain cognizant of its own desires while simultaneously "ignorant of what is being done to it" (Foucault, 1978/1991a, p. 100).

Butler (2004) wrote that governmentality is "a mode of power concerned with the maintenance and control of bodies and persons, the production and regulation of persons and populations, and the circulation of goods insofar as they maintain and restrict the life of the population" (p. 52). In other words, rather than thinking of government as a sovereign who is the sole arbiter of the rule of law, governmentality focuses on sets of practices that are dispersed through people, goods, services, and land and how they discipline and control both individuals and populations. Like power, these practices come from everywhere. Governmentality, then, is

the set of practices that disciplines populations and produces people, practices, beliefs, and so on. Rather than thinking of government as that body established to reinforce the power of the prince, governmentality is intended "to reinforce the state itself" (Foucault, 1988b, p. 150). That is, the practices of government enact a particular ideological perspective, and in that way, the state reinforces itself.

Foucault, however, did not see governmentality as progress, as if sovereign power were somehow out-of-date or stale, because progress would imply both improvement over sovereign power and the abandonment of sovereignty as a form of government. Foucault believed that both sovereign power and governmentality continue to co-exist in Western culture. Butler (2004) suggested that it might be possible for them to exist simultaneously, citing the Bush administration and its policies as one example. She explained that, in reference to the suspension of prisoners' rights in the name of combating terrorism, "whereas the suspension of law can clearly be read as a tactic of governmentality, it has to be seen in this context as also making room for the reemergence of sovereignty, and in this way both operations work together" (p. 55). Rather than asking what government is or even should be, governmentality asks how government functions, specifically, how practices form together within governments to produce certain effects. As explained above, in order for the practices of government to be successful, citizens must simultaneously believe in their own freedom and be willing to discipline themselves according to the ideology of the state employing the practices the government privileges that reinforce the values and dominance of the state.

Dean (1994) described this paradox of agentive self-discipline within governmentality as "the idea that the one governed is, at least in some rudimentary sense, an actor and therefore a locus of freedom" (p. 13). That is to say that responsibility for controlling certain behaviors is

seen as the responsibility of the individual, a feature that allows the government to be at once totalizing and individualistic. It follows then that if citizens would just hold themselves to certain standards, the need for discipline from above would not exist. As a result, "the margin of the exercise of freedom is of course extremely narrow" (Dean, 1994, p. 15). In sum, governmentality is useful because SBR functions as a regime of truth. That is, it continues to both be taken as true by proponents of SBR and also resisted as only one description of good social science by its detractors.

Using Genealogy and Governmentality for SBR

The governing standards of scientifically based research in education attempt to normalize science and discipline difference into sameness—in this case, the sameness is positivist social science. Maxwell (2004) believed our responsibility is to try to speak across this difference, though that has often been impossible in the SBR debates. However, a fear of erasing difference persists, not only because erasure is a violent act, but also because a critique that erases or dismisses difference and dissent can create an epistemological holocaust where each side is determined to eliminate the other. Rather than eliminate other descriptions, this dissertation demonstrates "historically how the effects of truth are produced within discourses [like SBR] which themselves are neither true nor false" (Foucault, 1980b, p. 118).

Because governmentality *is* a genealogy of government, the analysis I used in this study is a genealogy of governmentality in SBR. In short, genealogy structured my analysis because I examined enabling conditions, conditions of existence, and discontinuities. However, the modes of power, the institutions, the practices, and the subjects of SBR operate within the government—that is, the space that organizes the conduct of conduct of educational research and SBR's effects are written not only on the individual subject as in genealogy but also on the population as described in governmentality. Therefore, both theories were useful and necessary to understand how SBR came to be seen as the truth of high quality educational research.

Conclusion

In this chapter, I have explained Foucault's theories of genealogy and governmentality, how they relate to one another, and how they relate to SBR. In the following chapter, I describe the documents and interviews I used as data in this study, as well as key Foucauldian concepts with which I analyzed that data using writing as a method of inquiry.
CHAPTER THREE

A Genealogy of Governmentality in SBR

Introduction

In the previous chapter, I explained Foucault's theories of genealogy and governmentality in detail. In this chapter, I explain how I used these theories to guide the data collection and analysis processes that enabled me to address my research questions. To do this, I describe my data sources and the reasons these data were necessary for this project. I also describe two key analytic concepts, *discontinuities* and *power/knowledge*, which I used to analyze data. I explain how those concepts enabled me to focus on when and how SBR began, who was involved, how SBR was defined, where it could be found, how it was maintained and perpetuated, and how it came to be seen as true. In short, I analyze how the truth of SBR became possible and was "hardened into an unalterable form in the long baking process of history," as well as the material effects of this discourse (Foucault, 1977, p. 144). In this chapter, then, I explain how I used a genealogy of governmentality in SBR to produce an alternate history of SBR, one that does not assume its truth but instead looks for the effects of power/knowledge in discourse that produce discontinuities.

Data

Before explaining my data sources, I describe what I mean by data in this study. For the purposes of this study, I define data as whatever I use to think and write with as I attempt to answer my research questions. Because documents are the primary data source Foucault used in his genealogical analyses, I collected and produced documents to represent "the network of

power relations...that passes through apparatuses and institutions" in the discourse of SBR. I describe the collection and production of documents in the following sections.

Although I believe that all data collection is in fact production because it involves choices of what to include and exclude, I use these two distinctions to signify documents that were already available to the public and documents I created. I describe the documents I collected, such as legislation, meeting minutes, and journal articles, which were public documents I could easily access through government websites, library databases, and academic journals. I also describe the documents I *produced*, namely, the transcripts of interviews I conducted with people who were involved in the debates about SBR, who became, in effect, subjects of SBR.

Document Collection

As described in the previous chapter, Foucault (1971/1972) searched historical archives for documents, such as "books, texts, accounts, registers, acts, buildings, institutions, laws, techniques, objects, customs, etc." (p. 7) that helped him write a history of the present of the problematization he had identified. These kinds of documents provide a "knowledge of details" that is key to genealogy and allow the genealogist to get a sense of the life of a discourse—its emergence, enabling conditions, and conditions of existence (Foucault, 1977, p. 76). Consequently, I collected documents that provided detail about the life of the discourse of SBR. To document SBR's emergence, I identified documents that defined or championed SBR to find the justification for it. Then, I followed citational trails from those documents to trace the supposed origin of SBR. In other words, I collected documents that were cited as evidence that SBR was necessary. Those documents were my lines of descent because they allowed me to discover the "events through which...[SBR] [was] formed"—its enabling conditions (Foucault, 1977, p. 81). Additionally, I collected documents that were either entirely left out of the traditional history of SBR, such as the minutes of AERA Council meetings, or partially excluded from traditional history, such as the oblique reference to qualitative methods in the legislative definitions of SBR. These documents were useful because they provided the minute details of SBR, the "local knowledges" necessary for genealogical work (Foucault, 1980a, p. 85).

As I collected those documents and began analysis, I quickly noted that SBR was repeated most often in documents that governed educational research. Consequently, I used Foucauldian governmentality to gear my collection of documents toward three primary institutions—the federal government, the National Research Council, and the American Educational Research Association—that organized the conduct of conduct in educational research. That is, those institutions reproduced SBR and exercised its power on the body of the population of educational researchers, a "body totally imprinted by history" (Foucault, 1977, p. 83).

Documents from the Federal Government. Because SBR emerged as a definition in federal legislation, I analyzed the three pieces of legislation that defined SBR. The first piece of legislation was the *Reading Excellence Act of 1999*, which was introduced by the Clinton administration and became federal law after more than a year of drafting and debate. The bill represented a bipartisan effort to improve reading skills and instruction in K-12 classrooms by supporting scientifically based research in reading *Excellence Act (REA)* provides competitive grants to states to improve reading skills of students and the instructional practices of teachers of reading, by using the findings from 'scientifically based reading research''' (para. 2). The goal of this law was to be a "major catalyst in helping to turn back the rising tide of illiteracy, and [ensure] that

reading instruction is based on scientific research" (para. 1). As a result, it defined scientifically based reading research in order to determine which grant applications would qualify for funding.

The second piece of legislation I analyzed was the *No Child Left Behind Act of 2002*, which was introduced by the George W. Bush administration shortly after he took office, and which enjoyed wide bipartisan support. *NCLB* supported standards-based education reform, arguing that establishing accountability through high and measurable standards would improve educational outcomes for all students. This law once again required that programs, curricula, and teaching methods be based on scientifically based research, and consequently, it repeated and expanded the definition of SBR established in *REA*.

The third and final piece of legislation I analyzed is the *Education Sciences Reform Act of* 2002, introduced by the George W. Bush Administration. This law repeated and expanded the definition of SBR once again and established the Institute of Education Sciences, an institution that championed SBR and created a methodological hierarchy for educational research.

In addition to these three pieces of legislation, I also analyzed the *Congressional Record* from 2001-2002 for debate and discussion surrounding *NCLB* and *ESRA* in both the House of Representatives and the Senate. These records document the presentation and description of the federal laws mentioned above, the discussion and debate prior to their passage, and they serve as the representation of federal subjects of the discourse of SBR.

Finally, I read and analyzed documents from the websites of the What Works Clearinghouse, the Institute of Education Sciences, and the Department of Education, including the Procedures and Standards Handbook of the What Works Clearinghouse and the 2008 Report to Congress by the former director of the Institute of Education Sciences, Grover J. Whitehurst. **Documents from the National Research Council.** The two primary documents I analyzed from the National Research Council (NRC) are the 2002 consensus report *Scientific Research in Education (SRE)* and the 2005 consensus report *Advancing Scientific Research in Education (ASRE). SRE* was published after the passage of *NCLB* and before the passage of *ESRA*. The purpose of *SRE* was to understand how the tenets of science could be brought to bear on educational research. *ASRE* made recommendations for implementing scientific educational research and targeted "federal agencies, professional associations, schools of education, and journals" to advance the field (NRC, 2005, p. 14). I also used documents from the National Research Council website, including "Our Study Process" and "Guidelines for the Review of Reports," in order to become familiar with the process a report goes through from committee to publication.

Documents from the American Educational Research Association. There were three categories of documents I used from the America Educational Research Association: standards, Council minutes, and website documents. The primary documents I used were AERA's Standards for Reporting on Empirical Social Science Research in AERA Publications and its Standards for Reporting on Humanities-Oriented Research in AERA Publications. These two sets of standards provided guidelines for how to report high quality research according to AERA. I also read the AERA Council minutes for meetings occurring between 2002-2009 to find instances of discussion of SBR or SBR-related topics. Finally, I used documents on AERA's website, such as its definition of SBR and its statement of purpose and descriptions of educational research.

Documents from Academic Journals.

Several special issues of journals were published in response to *SRE*, which I also analyzed as documents for this study. The special issues I focused on were the 2002 special issue of *Educational Researcher*, two special issues of *Qualitative Inquiry* published in 2004, and a special issue of *Educational Theory* published in 2005. There were also special issues of *Teachers College Record* in 2005 and *Adult Education Quarterly* in 2006 that influenced my analysis. Additionally, academic journals such as *Educational Researcher* published occasional articles about SBR that I used for this study.

Document Production

The discourse of SBR produced not only the three institutions identified and discussed here but also subjects within those institutions, subjects those institutions produced who conformed to SBR, and subjects who resisted SBR. Thus, as I explained earlier, I studied documents produced by the institutions as well as documents produced in face-to-face and telephone interviews with subjects who seemed to both support and resist SBR. The transcripts I produced from those interviews became further documentary evidence of the discourse of SBR. According to Sanford (2009), "[b]ecause genealogy has been understood to rely on documents as sources of data, how genealogy as a methodology could function within and for a qualitative interview study becomes a legitimate concern" (p. 4). Because an interview occurs in the present, it does not seem to fit the description of historical documents studied by a genealogist. However, Sanford posited that interviews could indeed be useful in genealogical work because the qualitative interview has a different function than "those popularized by news reporters and talk show hosts where interviews take place in the moment for immediate consumption" (p. 16). That is, the qualitative interview is *intended* to be documented and archived by being recorded and transcribed. Sanford therefore claimed that the interview is neither "more nor less reliable than other documents named as acceptable for genealogical work" (p. 16) such as diaries and journals because they all provide the illusion of fixing a moment in time. In other words, the transcript makes the interview usable as a document in the same way that the interaction and flow of a meeting are reduced to minutes. Consequently, the interview becomes an appropriate method for genealogical work.

For this study, the goal of this document production—the interviews—was to demonstrate the depth and breadth of the discourse of SBR by focusing on the subject positions available within that discourse and the particular effects of SBR on real, living people. As described in Chapter Two, different discourses enable different subject positions and different subjects. For example, the discourse of SBR produces proponents and resistors and those who don't care too much either way—those who support and agree with the science described by SBR and those "extreme postmodernists" who critique it-as well as subjects who administer the discourse of SBR, such as the director of the IES or NRC committee members who promoted and disseminated the findings of Scientific Research in Education (2002). The participants I interviewed were scholars active in the debates about SBR who had a connection either to the federal government, the NRC, or AERA. Consequently, the participants were subjects of the discourse of SBR. I also followed citational trails from my interviews. That is to say, I asked participants for suggestions of additional participants and documents that might enhance my work. For example, one participant suggested I confirm his information in the AERA Council minutes, which led me to use those documents as data for this study. The interviews I conducted took place in two parts, which I will describe below. I also provide more detail about the sample selection criteria for each set of interviews.

First Interviews. For the initial interviews, I selected participants who were active in the conversations about SBR and who had contributed to the NRC reports or to the special issues of journals that responded to those reports. In some cases, they had participated in both. I invited participants by email, and the interviews took place during the 2008 Annual Meeting of AERA in New York City for mutual convenience. I selected participants based on my interest in their work related to SBR and the connections I had that would improve the likelihood of their participation. I asked all participants to forgo confidentiality so that I could connect their public and academic speaking and writing to the data I collected from interviews. That way, I could analyze documents they had written that I had collected and documents produced by the interview transcript together. I did not see how I could use the interview data without revealing the identities of my participants as I had chosen them precisely because of their work in the SBR debates. Kelly (2009), in his brief review of critiques of anonymity and pseudonyms, explained how a determined reader could uncover the identities of participants based on contextual information. Still, deciding to reveal the names of participants is fraught with peril, which influenced my analytic choices.

The following six scholars agreed to participate in the study: Dr. Michael J. Feuer, then Executive Director of the Division of Behavioral, Social Science, and Education of the National Research Council at the National Academy of Sciences; Dr. Margaret Eisenhart of the University of Colorado at Boulder, who served as a member of the 2002 NRC committee that produced *SRE* and the CORE committee that produced *Advancing Scientifically Based Research* in 2005 and had written several journal articles about the topic as well; Dr. Tom Schwandt of the University of Illinois, who contributed to the special issue of *Educational Theory*, as well as writing other articles about SBR in the *International Journal of Qualitative Studies in Education* and other journals, and subsequently was asked to serve on an NRC committee on the topic of the nature of scientific evidence; Dr. Ken Howe of the University of Colorado at Boulder, who had edited the special issue of *Educational Theory*, contributed to the special issue of *Qualitative Inquiry*, and was at that time beginning work on the Humanities Standards for AERA; Dr. Patti Lather³ of The Ohio State University who contributed to the special issues of *Teachers College Record* and *Qualitative Inquiry*; and Dr. Robert Slavin, Director of the Center for Research and Reform in Education at Johns Hopkins University, part-time Professor at the Institute for Effective Education at the University of York in England, and Chairman of the Success for All Foundation, who had written several articles about the quality of educational research in general and SBR in particular.

These first interviews took place primarily during a short visit to New York City for the 2008 AERA Annual Meeting, but the interview with Robert Slavin took place by telephone prior to that conference. Each of the interviews at the Annual Meeting was face-to-face and lasted between forty minutes and an hour and a half. In those interviews, I asked broad questions about the discourse of SBR and its effects on educational research. I also asked questions that specifically related to my participants' publications and other work related to SBR.

Second Interviews. After completing and transcribing the first interviews, I began analyzing these documents while writing, revising, and defending my dissertation prospectus. In the process, I developed different research questions, and I decided to conduct additional interviews to include participants who worked with each of the institutions that served as the

³ This interview was scheduled to occur during the Annual Meeting in New York, but Dr. Lather was unable to make it to the interview. We were able to reschedule the interview during a visit she made to the University of Georgia two years later.

primary foci of SBR's power: the federal government, AERA, and the NRC. This required follow-up interviews with some participants and interviews with new participants. Some people I would like to have interviewed were unable to participate, and so I relied on their publications in order to include their ideas in my study.

I conducted a follow-up interview with Dr. Michael J. Feuer, now Dean of the Graduate School of Education and Human Development at George Washington University, and I interviewed Dr. Gerald Sroufe, Director of Government Relations and Senior Advisor of the American Educational Research Association. I made several unsuccessful attempts to schedule interviews with Congressional aides for the Senate and House majority and minority leaders. Instead, I chose to read the Congressional Record during the time that the three key pieces of legislation (i.e., REA, NCLB, and ESRA) were debated, 2001-2002.

I conducted the additional interviews via telephone, each lasting between twenty minutes and an hour and a half. In those interviews my questions were more pointed and specific than in the first interviews. Rather than asking general questions about SBR, where it started, how people saw their work contributing to the conversation, and what they thought the role of government should be in determining methodological preferences or defining science, I asked questions about the timeline of SBR and how it functioned to privilege experimental research. I also asked questions about the work of the federal government, the NRC, and AERA in the maintenance and proliferation of SBR.

The Subject and Power in the Dissertation Interviews. The conventional qualitative interview puts the researcher and the researched (or the interviewer and interviewee) in a hierarchical power relationship and views power as top-down with the researcher ultimately controlling the truth of the interview. According to Kvale and Brinkman (2009), this power

asymmetry can take a variety of forms. First, the interviewer defines and controls the interview situation, designs the questions, and decides when to end the interview. Second, the conventional interview is not dialogic. That is, the interviewer asks the questions, and the interviewee answers them. There is very little, if any, two-directional conversation. Third, the interview is an instrument for collecting data. In other words, "[a] good conversation is no longer the goal in itself, but a means for providing the researcher with descriptions, narratives, texts—to interpret according to his or her research interests" (p. 33). Additionally, the researcher may have a "hidden agenda" for the interview that is not revealed to the participant. These forms of power asymmetry are inherent in the conventional qualitative interview. Because of the structure of the conventional interview, the researcher is in control of the flow of the interview and has the power to represent the knowledge produced by the interview in her research report. In terms of governmentality, the description of power in the conventional qualitative interview is the sovereign power Foucault described in which power can be held, given, or taken away, in the case of the interview, by the researcher.

But the goal of researchers is not necessarily to try to eliminate the power asymmetry but instead to attend to it and examine the potential epistemological and ethical issues raised in the interview setting. For example, in my interviews with participants, I positioned myself as a novice researcher who sought the knowledge and expertise of more experienced scholars, which resulted in my tendency to feel uncomfortable attempting to get the interviewees to answer my research questions. Instead, I followed the cues of my participants, a condition common in elite interviewing.

Elite interviews, according to Dexter (2006), are interviews "with *any* interviewee...who in terms of the current purposes of the interviewer is given special, nonstandard treatment" (p. 18). He defines "special, nonstandard treatment" as follows:

- 1. stressing the interviewee's definition of the situation,
- 2. encouraging the interviewee to structure the account of the situation,
- letting the interviewee introduce to a considerable extent...his notions of what he regards as relevant, instead of relying upon the investigator's notion of relevance. (p. 18).

Kvale and Brinkman (2009) wrote, "elite interviews are with persons who are leaders or experts in a community, who are usually in powerful positions" (p. 147).

Dexter (2006) noted that elites might not accept the assumptions of the project. They may be more interested in presenting their interpretation of issues or events rather than following the lead of the researcher, which is generally the reason for interviewing that person in the first place. Kvale and Brinkman (2009) argued that the "prevailing power asymmetry of the interview situation may be canceled out by the powerful position of the elite interviewee" (p. 147). That is to say that although in a standard interview the researcher defines the problem and creates the questions, in an elite interview, the researcher expects the interviewee to provide expertise. In an elite interview, the power flows back and forth between interviewer and interviewee and is not necessarily top-down with the researcher in charge.

In Foucault's description of power, power comes from everywhere and circulates during the interview, so it does not necessarily reside only in a powerful participant or in a powerful researcher. Even though I interviewed people who hold powerful positions in society, I still ultimately determined what data to use and how to interpret it. However, there were important considerations in interviewing these elite participants. For example, time was a significant concern. In this study, participants had full schedules and could sometimes provide me with only twenty or thirty minutes. Further, elite interviewees are not necessarily interested in or available for member checks and other standard validity practices of conventional qualitative inquiry. In the end, though, power is not a negative consideration in the interview. Rather, as Foucault, (1977) claimed, "[w]e must cease once and for all to describe the effects of power in negative terms: it 'excludes', it 'represses', it 'censors', it 'abstracts', it 'masks', it 'conceals'. In fact power produces; it produces realities; it produces domains of objects and rituals of truth" (p. 194). Because I interviewed subjects who had been produced by the discourse of SBR, power relations during interviews were an important concern. I concluded that following the lead of an elite participant, rather than derailing the interview, gave me access to information I otherwise would not have found. As a result, the power relations within interviews in this study produced useful and relevant data that strengthened my analysis.

Analytic Terms

As I analyzed the data to find SBR's emergence, enabling conditions, and conditions of existence, I continued to read Foucault and consequently saw examples of power/knowledge and discontinuities in the data that helped me think about how the discourse of SBR functioned as a regime of truth and how it was ruptured by dissonance. In the following sections, I operationalize power/knowledge and discontinuities and explain how I used writing as a method of inquiry to analyze data using those concepts.

Discontinuities

Foucault (1971/1972) explained that discontinuities often occur when "things are no longer perceived, described, expressed, characterized, classified, and known in the same way" (p.

217). That is to say that a discontinuity occurs at a moment when ways of knowing collide, which often come at moments of resistance to the disciplinary practices of a discourse. In that way, a focus on discontinuities makes discourses visible. Discontinuities in the history of SBR represent "tell-tale moment[s]" in which SBR comes undone (Spivak, 1974, p. xxxv).

Moments of discontinuity in the discourse of SBR are the snags that cause its fabric to unravel and reveal that SBR is not a truth to be taken as whole cloth but instead a complex narrative sutured together using subjects, institutions, practices, and techniques that form a regime of truth. A discontinuity, then, is any event, idea, subject, institution, practice, or process that disrupts the accumulation of knowledge that serves to constitute a discourse as truth. This accumulation of knowledge is documented and codified through the creation of traditional histories. In other words, a discontinuity ruptures the "appearance of continuity and unity" (Foucault, 1966/1970, p. 50) of a discourse or way of knowing. Simply put, a discontinuity is a contradiction (e.g., not having scientific evidence to justify the application of SBR), an incompatibility within a discourse that "pos[es] the problem of its [the discourse's] own limits" (e.g., the view that RCT is the gold standard for science despite contradictory evidence), a moment at which a discourse implicates itself (e.g., drafting two sets of standards because it was easier than starting over, the effect of which is the reinforcement of the discourse of SBR) (Foucault, 1971/1972, p. 117).

Power/Knowledge

Foucault claimed that power is based on knowledge, the use of knowledge, and the perpetuation of knowledge, though power and knowledge are not synonymous or equivalent. As Foucault (1976/1978) explained, "[p]ower is everywhere; not because it embraces everything, but because it comes from everywhere....power is not an institution, and not a structure; neither

is it a certain strength we are endowed with; it is the name that one attributes to a complex strategical situation in a particular society" (p. 93). That is, power is localized and produced in strategic relationships. Or put another way, "power is not an institution, a structure, or a certain force with which certain people are endowed; it is the name given to a complex strategic relation in a given society" (p. 93).

Knowledge, then, is a product of these power relations. Gordon (1980) explained that "power/knowledge belongs within a version of the Nietzschean project of genealogy" (p. 236) because it deals with "tracing the mobile systems of relationships and syntheses which provide the conditions of possibility for the formation of certain orders and levels of objects and forms of knowledge of such objects" (p. 236). Consequently, power and knowledge are decentralized and in constant motion and work together simultaneously. As a result, Foucault created the concept *power/knowledge* (sometimes written *power-knowledge*). Because power and knowledge are intertwined, knowledge is never neutral. Gordon (1980) wrote, "what is at issue is indeed a certain series of historical connections which become visible and intelligible in terms of power" (p. 237), but this is not a result of any kind of overthrow of previous knowledge through the violent exertion of power. Rather, "if certain knowledges of 'Man' are able to serve a technological function in the domination of people, this is not so much thanks to their capacity to establish a reign of ideological mystification as to their ability to define a certain field of empirical truth" (p. 237). In other words, knowledge is not power; instead, certain knowledge can be imbued with power if produced within a discourse that achieved dominance and is consequently viewed as true.

Foucault's work on power/knowledge and the subject also enabled his work on governmental rationality. Foucault wrote that it is not the subject who initiates the exercise of

power—for example, a sovereign who ubiquitously disciplines his citizens—but, rather, it is individual practices (e.g., legislation, institutions) that, when taken together as regimes of practices, join in a piecemeal fashion to produce governmentality. As such, there is no grand, rational intentionality behind a particular government's or administration's actions. Instead, an incidental collection of practices exercised within a given ideology and epistemology construct truth, because as Foucault (1980a) explained, "we cannot exercise power except through the production of truth" (p. 93). As a result, in considering the practices of government, Foucault was most interested in the "conditions of their [statements'] singular emergence; their correlation with other previous or simultaneous events, discursive or otherwise" (Foucault, 1972/1991b, p. 59). Therefore, from Foucault's perspective, truth is always discursively produced and contingent.

In his study of government, Foucault examined power/knowledge on a state level in order to develop his concept of governmentality. Foucault (1978/1991a) considered government from a variety of perspectives, including "how to govern oneself, how to be governed, how to govern others, by whom the people will accept being governed, how to become the best possible governor" (p. 87). Foucault traced various governmental rationalities, such as pastoral power, the reason of the state, the police state, liberalism, and neo-liberalism. Within each of these rationalities, Foucault noticed a simultaneous desire of government to, at the same time, "totalize" and "individualize" (Gordon, 1991, p. 3). The analysis of power/knowledge in a discourse that circulates in government, such as SBR, demonstrates how that discourse has functioned as a regime of truth whose power is inscribed on the body of the educational research population.

Writing as a Method of Inquiry

In this section, I describe how I analyzed data using writing as a method of inquiry, similar to what Richardson and St. Pierre (2005) described as a way of knowing and thinking that allows a researcher to write herself into spaces she might not have occupied with more common ways of sorting, categorizing, and coding data, positivist approaches to data analysis used even in interpretive and critical qualitative research. In short, I used writing to make sense of data. As Cixous (1993) wrote,

[t]he thing that is both known and unknown, the most unknown and the best unknown, this is what we are looking for when we write. We go towards the best known thing, where knowing and not knowing touch, where we hope we will know what is unknown. Where we hope we will not be afraid of understanding the incomprehensible, facing the invisible, hearing the inaudible, thinking the unthinkable, which is of course: thinking. Thinking is trying to think the unthinkable: thinking the thinkable is not worth the effort (p. 38).

The space of "rigorous confusion," (Lather, 1996, p. 15) where a researcher must write in order to think because writing is thinking, is a fruitful space of data analysis. An analytic practice such as coding does not leave space for the unthinkable, the invisible, and the inaudible, so I had to trust that writing would lead me to the interstitial space where the known and the unknown touch. Like Derrida (1991/2001), "[e]ach time I begin a new text, however modest it may be, everything falls apart in the face of the unknown or the inaccessible, a crushing feeling of clumsiness, of inexperience, and of powerlessness. Anything I had already written is instantly annihilated, or rather, as if thrown overboard" (p. 64). This is the work of "get[ting] lost" that Lather (2000) described—work which begs for interruptions, lines of flight, and the proliferation

of data in order to "open up present frames of knowing to the possibilities of thinking differently" (p. 288).

The analysis I did in writing was entangled with reading. I read and reread my field notes, research journal, and interview transcripts repeatedly, immersing myself in that data. In addition to studying the interviews, I carefully reread the definitions of SBR in REA, NCLB, and ESRA; I reread the special issues of academic journals dedicated to the topic; the Congressional Record; AERA Council minutes; and my other data sources. I also read a great deal of Foucault' work, returning again and again to his writing about governmentality and genealogy to help me think. As I sat down to my computer to write, my desk piled high with documents and texts such as those listed above, the words of Foucault, my memories of the interviews, and the continued reading of other SBR documents mingled in my head and on the page. Sometimes this writing took the form of a sort of word association game as the words of all of my data sources mingled on the page. For example, when writing the introduction to the dissertation, I reread a text by Foucault in which he described science, and I came across the quote that now opens the dissertation. As I wrote that Foucault proposed that asking "Is it a science?" disqualifies other ways of knowing, I was listening to Pandora, and the lyrics of Coldplay's *The Scientist*— "running in circles/chasing tails/coming back as we are...questions of science/science and progress....I'm going back to the start"-ran through my head. I returned to the interview transcripts in which participants discussed whether the federal government had any business defining science. Examples such as this one occurred frequently as I wrote.

At other times, this writing involved work with my writing partner, Sarah, as we thought and wrote together in person, over the phone, by text, and in email. She offered me questions, quotations, comments on drafts, and interpretations of philosophy—*her* Foucault—that I used in writing (Van Cleave & Bridges-Rhoads, 2012). For example, while Sarah was attending the Ninth International Congress of Qualitative Inquiry Conference in Urbana-Champagne, IL and I was working in my office in North Carolina, we had an hour-long conversation during which I explained my thoughts and struggles in describing Foucault's concept *discontinuity* and how I used it in the dissertation. Because she was on the campus of the University of Illinois and did not have her computer with her, Sarah suggested I "go find the Spivak quote about 'threatens to collapse the system'" because the image evoked by this quote continually popped into her head as I talked to her, and she thought it might be useful to move my thinking. Consequently, I reread Spivak's Preface to Derrida's *Of Grammatology* and found the quote. I read all around it, thought and wrote about it in connection with my problem of discontinuity, and I ultimately included the quote in my discussion of discontinuity in Chapter Six.

Everyday life often interrupted this so-called academic writing, and I cannot pretend I did not think with and through all of this and more because, once again, St. Pierre (2011) encouraged me to "think about [my] topic[s] with all [I] can muster" (p. 622). Consequently, writing "ha[d] nothing to do with signifying" and instead was a process of "surveying, mapping, even realms that are yet to come" (Deleuze & Guattari, 1980/1987, p. 4-5). Writing could not represent the real because the real does not exist, and all I could hope for was "substituting sign of the real for the real itself" (Baudrillard, 1988, p. 167). Instead, I wrote "toward an experimentation in contact with the real" that is always "susceptible to constant modification" (Deleuze & Guattari, 1980/1987, p. 12), so I could "work [my] way somewhere in thinking" (St. Pierre, 2011, p. 622).

The more I wrote, the more I began to focus on the key concepts I described above, which helped me understand how SBR came to be seen as the truth of good science. The circulation of power and the production of knowledge in the discourse of SBR were frequent topics of my writing, and I continued to get caught at moments that disrupted the truth of SBR. Genealogy and governmentality helped me write through those issues, providing a framework for my thinking in writing, and I began to methodically focus on the play of power/knowledge that produced SBR as a regime of truth as well as on instances of discontinuity in the narrative of SBR that pointed to its limits and failures. Though I would wager that many researchers experience analysis as I did, they seldom describe it in such terms, instead choosing to say that they "coded" data or did "thematic" analyses. Again, I used writing to think, to analyze, to write my way into an understanding of how SBR functioned as a discourse of truth and how people were caught up and produced within the power/knowledge of that discourse.

Conclusion

In this chapter, I have described the data sources for this study and the key analytic concepts I used to analyze those data. I also described writing as a method of inquiry. In the following chapter, I take the first step in genealogical analysis and describe the problem of the present—how SBR has become a regime of truth.

CHAPTER FOUR

Enabling Conditions

Introduction

Enabling conditions are the conditions of possibility for the emergence of a discourse. In this chapter, I explain the supposed origin of scientifically based research (SBR) and present some of the enabling conditions that allowed SBR to emerge and become a significant discourse structuring educational research. That is, I explain how SBR began to insinuate itself into the discourse and practices of educational research and how the following perceptions influenced SBR's emergence: the supposed poor quality of educational research, the deprofessionalization and feminization of education as a field, and the invocation of science as the cure-all.

The Origin of SBR

In this section, I argue that there was no point of origin for the discourse of SBR. Origins imply a lofty beginning to a concept when instead, according to Foucault (1977), most beginnings are lowly and accidental. SBR was mandated in educational research based on perception. That is, the *idea* that educational research was of poor quality served as the rationale for SBR. Consequently, SBR has no rational, scientific foundation on which to stand, and although it has garnered truth-value, the discourse of SBR is neither real nor true.

Subsequent to the publication of the 2002 NRC report, The Use of Scientifically Based Research in Education Working Group Conference (2002), sponsored by the Office of Elementary and Secondary Education of the U.S. Department of Education, took place during which "leading experts in the fields of education and science discussed the meaning of

scientifically based research and its status across various disciplines, including reading, math, safe and drug-free schools, and comprehensive school reform" (Saxton, Winters, and Kickbush, 2002, para. 1). The presentations in the session were as follows: "Welcome and Introduction" by Susan Neuman, Assistant Secretary for the Office of Elementary and Secondary Education (OESE); "The Logic of Scientific Research" by Valerie Reyna, Senior Research Advisor to the Assistant Secretary of the Office of Educational Research and Improvement; "The Basic Principles of Scientific Based Research" by Michael Feuer, Executive Director of the Division of Behavioral, Social Science, and Educational research of the National Academies of Sciences, and Lisa Towne, NRC Study Director for the NRC's 2002 report, Scientific Research in Education; "Identifying Scientifically-Based Research in Education" by Stephen Raudenbush, Professor at the University of Michigan; "The Use of Scientifically Based Research: Math Instruction" by Russell Gersten, Eugene Research Institute, University of Oregon; "What Scientifically Based Research Means for Reading Instruction" by Eunice Greer, Reading Consultant; "Scientifically Based Research and Safe and Drug Free Schools" by Judy Thorne, Senior Study Director, Westat; and "Scientifically Based Research and the Comprehensive School" by Becki Herman, American Institutes for Research⁴. Their work included the examination, description, and application of the methodologies that should be used in SBR. Michael Feuer, then executive director of the Behavioral, Social Science, and Education Division of the National Research Council, operating arm of the National Academies of Sciences and consultant on the 2002 NRC report, explained the impetus for the discussion of SBR:

⁴ Professional affiliations of conference contributors reflect their positions at the time of the conference in 2002.

We don't have any evidence and we didn't try to get evidence to support or refute the claim of the overall quality of educational research being poor. But we did take as a datum that the perception that it is poor is important (2002).

Feuer's words highlight one of the key issues currently plaguing educational research that it is perceived to be of poor quality. However, as Feuer (2002) stated at the working group conference, the NRC, and indeed the federal government, had no empirical evidence that indicated the necessity of SBR and its resultant stranglehold. Still, perception has been enough to warrant a narrowing of what counts as high-quality educational research. The lack of a sound rationale for the invention of SBR is a prime example of Foucault's trouble with origins, which he explained are more about historical accident than importance or necessity. Or, put another way, "[t]here is no essence or original unity to be discovered; there are simply the conditions of its inception, its continuation, and its effects on real people" (Davidson, 1986, p. 224). That is, the way a concept is born, or becomes able to be spoken, is more about power relations than a line of logic or system of rational thought. SBR's beginning was not rational or intentional, but rather "[was] rooted in domination, subjugation, the relationship of forces—in a word, power" (Davidson, 1986, p. 255). For example, the perception of a lack of quality in educational research was transformed into a truth when the federal government, an institution imbued with a great deal of power, decided to mandate a particular kind of research methodology to ensure that educational research is scientific and rigorous. By accepting the premise that such research is, indeed, of poor quality, the federal government, the NRC, and AERA could all set out to remedy that condition.

That there is ever a clear origin to be found at the beginning of discourse and practice is doubtful. According to Foucault (1980b), "[w]hat makes power hold good...is that it traverses

and produces things, it induces pleasure, forms knowledge, produces discourse" (p. 119). The idea that something is real, true, or central and that it has a rational beginning is more productively understood as simply a description that we have come to accept as the truth of things. According to Spivak (1974), "[t]he most that can be said ... is that a certain view of the world ... has been accepted as the correct one," (p. xiii), so any description of the world is contingent. The unsteady beginning of SBR in general and the work of the NRC in particular prompts several questions: why did the NRC not question its charge, which was officially commissioned by the U.S. Department of Education and influenced by a political climate that valued evidence-based rhetoric; and how and why was SBR taken up by other institutions, including the American Educational Research Association, a professional organization that prides itself on "the broad range of disciplines represented" by its membership (AERA, para. 3)?

The 2002 working group conference is not the only example of a group's assuming that educational research was of poor quality. Senator Judd Gregg of New Hampshire, in debate in Congress over the passage of ESRA in 2002 October, claimed that

[t]hough significant Federal involvement in educational research dates back to the 1950's, we are still without a strong body of high quality educational research to guide education policymaking. Yet the need for sound, rigorous educational research that is free of political bias and useful to educators has never been more important (Congressional Record-Senate, 2002, p. 20721).

Gregg further argued that the Education Sciences Reform Act, which defines scientifically based research and translates this definition into mandated methodologies for research, "establishes more rigorous research standards, which all Institute-funded educational research will have to meet" (p. 20721). He decried any research that falls outside of this definition, claiming,

"[e]ducation fads that masquerade as science will no longer be acceptable" (p. 20721). Senator Jack Reed of Rhode Island echoed those sentiments during the same proceeding, and similar statements could be heard in the debates that took place in the House of Representatives at the same time. In 2002 October, Rep. Howard P. "Buck" McKeon of California claimed, "[b]y requiring that research be based on valid scientific findings, H.R. 5598 will greatly improve the quality of federal scientific research in education" (Congressional Record-House, 2002, p. 20355). This statement not only presumes that "scientific research" is a concept that is intelligible to all people in the same way but also presumes that educational research was not already scientifically based. Those three congressmen, as well as several others, spoke about the need for high quality research. They claimed that the new legislation that required that educational research to meet the standards of science outlined by SBR would put an end to the poor quality educational research that had been produced for years. However, none of the representatives had ever been an educator, and none were experts in educational research. Nonetheless, they joined the clarion call to improve education and educational research in response to the supposed poor quality of educational research.

From a genealogical perspective, it is important to note that there was no grand scheme, no intentionality, no conspiracy to eliminate all research that is not positivist. Origins of powerful discourses like SBR "are often fabricated" (Scheurich & McKenzie, 2005, p. 851). Instead of finding a rational origin at the beginning of things, what one often finds is "randomness, piecemeal fabrications, dissension, disparity" (Scheurich & McKenzie, 2005, p. 851), "details and accidents," "petty malice" (Foucault, 1977, p. 144), "chance," "surprises," "unsteady victories, and power" (Davidson, 1986, p. 224). As Foucault (1977) explained, the origin of things is more about historical accident than rational deliberation and necessity. He wrote:

devotion to truth and the precision of scientific methods arose from the passion of scholars, their reciprocal hatred, their fanatical and unending discussions, and their spirit of competition -- the personal conflicts that slowly forged the weapons of reason ... What is found at the historical beginning of things is not the inviolable identity of their origin; it is the dissension of other things. (p. 142)

Consequently, in the effort to make educational research more scientific and more rigorous, NCLB and subsequent policy has narrowed the definition of science such that much educational research and the theories that provided us with insight into teaching and learning over the last thirty years no longer counts. As Spivak (1999) explained,

If we want to start something, we must ignore that our starting point is, *all efforts taken*, shaky. If we want to get something done, we must ignore that *all provisions made*, the end will be inconclusive. This ignoring is not an active forgetfulness; it is, rather, an active *marginalizing* of the marshiness, the swampiness, the lack of firm grounding in the margins, at beginning and end. (p. 175)

So the SBR debates emerged from politics and power, and the shaky foundation of SBR was ignored, if it was ever acknowledged in the first place. In the following sections, I describe the enabling conditions for the discourse of SBR.

The Enabling Conditions of SBR

There seems to be consensus that education in the United States is broken and needs to be fixed. Most of all, there is a rage to know what will work in schools and classrooms unequivocally, what will solve the problems of public education once and for all and will serve all students equally. Rage may seem a strong word, and it is, but there is real anger behind some of the accusations leveled at teachers, researchers, and education as a whole. People ask why those involved in education haven't gotten it right, after all this time, all this research, and all this money. The fact that we do not seem to have *the* answers, the education cure-all, as it were, the general public and policy makers to believe that the research informing our decisions about education must be flawed, resulting in the perception that educational research is of poor quality (e.g., Mayer, 2001; Sroufe, 1997).

In a 1993 article in *Educational Researcher*, "The Awful Reputation of Educational research," Kaestle described the troubled state of educational research, and his paper is often cited by those who endorse SBR (e.g., Feuer, Towne, & Shavelson, 2002; Shavelson, Phillips, Towne, & Feuer, 2003; Slavin, 2002). In fact, Kaestle did not argue that educational research is awful; instead, he attempted to account for its bad reputation. Kaestle (1993) conceded that educational research "suffer[s] from a widespread verdict of uselessness" and maintained that "the impression of disarray stems from three related conditions: constant reorganization, habitual timidity, and lack of consensus" (p. 27; p.28). He noted that these conditions are not unique to educational research, however, and that "all social science research faces daunting skepticism and dubious reputation in Washington" (Kaestle, 1993, p. 30). In other words, policymakers in Washington want "quick, measurable results" (Kaestle, 1993, p. 23) that social science cannot always provide. Consequently, quantitative results are often sought because numbers and metrics appear unequivocal and can easily be written into policy. The push for more scientific research is, in many cases, a push for the *appearance* of rigor in place of an honest attempt to produce new and useful knowledge that might not point to a simple solution to complex problems.

Weird Science

Science has long been a contested term. Educational research experienced the paradigm wars of the 1980s in which objectivist-positivist science was rejected in favor of interpretivist and critical approaches among others (Gage, 1989). Those paradigm wars, sometimes referred to as the science wars, continued through the 1990s, and skirmishes erupt even now. The point here is that discussions of what constitutes high quality science are nothing new. This ongoing conflict is playing out in educational research now as a result of the return of positivist social science that claims that an external reality exists waiting to be discovered; that human beings are capable of understanding and describing that reality accurately; and that multiple observers of the same phenomenon can agree on what they see. The privileging of knowledge produced by science above knowledge produced by other means—science worship—is referred to as *scientism*.

Scientism

Coined by Hayek (1952), scientism describes the belief that the methods of the natural sciences can be applied to the human sciences. Hayek (1952) explained that "[t]he methods which scientists or men fascinated by the natural sciences have so often tried to force upon the social sciences were not necessarily those which the scientists in fact followed in their own field, but rather those which they believed that they employed" (p. 22). In other words, the perception of what constitutes science and scientific practice rather than the actual practice of science in the natural sciences produces a "slavish imitation of the method and language of Science" (p. 24). According to Widdowson (2011), scientism "began as a label for the doctrine that truth is fixed, *a priori* and universal; that inductive science is the only means to its discovery and certainty is a realistic outcome" (para. 3). Further, Margolis (2003) claimed that

'Scientism' signifies the assured possession of a privileged methodology or mode of perception, or even the assured validity of a metaphysics deemed ineluctable or overwhelmingly favored by the self-appointed champions of 'Science' even in the face of insufficient evidence or substantive doubt or their proclaimed opposition to cognitive privilege or Cartesian foundations. (p. 6)

He further argued that scientism had been unable to adequately respond to its critics and specifically maintained that scientism's "refusal to admit the viability and reasonableness of a constructivist and historicist reading of science and practical life" signaled the end of "deceptions by which to prolong scientism's hegemony" (p. 13). Haack (2003) agreed, stating that "[s]cientism is an exaggerated kind of deference towards science, an excessive readiness to accept as authoritative any claim made by the sciences, and to dismiss every kind of criticism of science or its practitioners as anti-scientific prejudice" (pp. 17-18). Sorrell (1991) noted that "the belief that science, especially natural science, is much the most valuable part of human learning—much the most valuable part because it is much the most authoritative, or serious, or beneficial" (p. 1) is major tenet of scientism. Because of that, social science research that does not emulate the methods of the natural sciences is often not accepted as being scientific. Ruccio and Amariglio (2003) explained as follows:

If the growth of scientific knowledge is the key accomplishment of the past three centuries in the West, it has been accompanied by an elaborate philosophical defense of a variety of exclusionary practices by which those deemed to be untrained in or unreceptive to such science are shunted aside or even denied opportunities to speak (since they are considered to be the voice of unreason). (p. 42) Here, rationality is a feature only of scientific thought, and all other thought and the knowledge produced by such thoughts have no claims to reason.

Further, scientism is "science's belief in itself" (Habermas, 1968/1971, p.4), which means that science is seen as the only way to produce knowledge; science *is* knowledge. Stenmark (2001) noted that "[w]hat is characteristic of Scientism is that it works with a *narrow* definition of science" (p. 5). Conversely, those critical of scientism adhere to the belief that there are multiple truths and multiple ways of knowing and producing knowledge, many of which become subjugated knowledges in the face of scientism. Critiques of scientism (e.g., Kuhn, Feyeraband), according to Cullenberg, Amariglio, and Ruccio (2001),

have promoted the idea that "agreement" (voluntary, forced, and every combination in between) in science is what needs to be understood and investigated, and that those theories that often succeed at any given moment in time in shaping a field of thought are either bound to more general social institutions and patterns of status, wealth, and power, or are able to hegemonize the field by "normalizing" the conditions under which that theory arises, and maybe both. The postmodern critique of scientism is close as well to the view of Feyerband that there are no singularly exceptional methods that are productive of science, and even that actual scientific " progress" is a result of scientists' refusal to codify and obediently follow any philosophically prescribed road toward truth. (pp. 28-29)

Such critiques call into question the sacred nature of science.

Haack (2003) explained that because there is no singular method of scientific inquiry, the so-called scientific method is a myth maintained by a scientistic culture. The myth of the scientific method enables a religious fervor in science that is both fascinating and perplexing.

Scientism's supporters are much like those of Francis Schaeffer's school of Christianity⁵ that contends that spiritual beliefs, and specifically biblical teachings, should be integrated into all aspects of life, including politics, legislation, and self-governance. Scientism installs positivist science into all inquiry, regardless of domain, in an effort to make a science of everything, including, in the SBR debates, education. In the next section, I discuss positivism, which is closely related to scientism.

Positivism

Positivism is an approach that attempts to impose the rules, practices, and methodologies of the natural sciences on the social sciences. Positivist social science claims that the goal of inquiry is to predict and control phenomena through the discovery of knowledge. Auguste Comte, the French theorist who invented sociology, first described positivism in the mid-nineteenth century. According to Ryder (2005), Comte held "an extreme view of empiricism, insisting that true knowledge of the world arises only from perceptual experience" (para. 1). Comte criticized "ungrounded speculations about phenomena that cannot be directly encountered by proper observation, analysis and experiment" (Ryder, 2005, para. 1). Pickering (1993), in her biography of Comte, explained that positivism is based on the belief that a statement is scientific only if it can make claims that can be put to experimental test. Comte is credited as having off-

⁵ Schaeffer (1981) said in an address at Notre Dame, "Christianity is not a series of truths in the plural but, rather, truth spelled with a capital "T." Truth about total reality, not just about religious things. Christianity, biblical Christianity, is Truth concerning total reality — and the intellectual holding of that total Truth and then living in the light of that Truth."

repeated the aphorism, "from science comes prediction; from prediction comes action" (Pickering, 1993, p. 566).

According to Baez and Boyles (2009), "the main features of positivism include an insistence that science has the only basis for obtaining valid knowledge and observable facts are the only valid objects of knowledge" which requires "scientific techniques in the social sciences because assertions about the world must be 'verifiable through experience and observation'" (p. 49). McClosky, (1983) agreed, explaining that positivism rests on modernist assumptions, such as:

- (1) Prediction (and control) is the goal of science.
- (2) Only the observable implications (or predictions) of a theory matter to its truth.
- (3) Observability entails objective, re-producible experiments.
- (4) If (and only if) an experimental implication of a theory proves false is the theory proved false.
- (5) Objectivity is to be treasured; subjective 'observation' (introspection) is not scientific knowledge.
- (6) Kelvin's Dictum: 'When you cannot express it is numbers, your knowledge is of a meager and unsatisfactory kind.'
- (7) Introspection, metaphysical belief, aesthetics, and the like may well figure in the discovery of an hypothesis but cannot figure in its justification.
- (8) It is the business of methodology to demarcate scientific reasoning from nonscientific, positive from normative.
- (9) A scientific explanation of an event rings the event under a covering law.

(10) Scientists, for instance economic scientists, have nothing to say as scientists about values, whether of morality or art. (p. 484).

Such rationality "reduce[s] complex social phenomena to manageable, observable units that are tested through experimentation" and therefore "denies the existence of forces or substances that go beyond the facts and laws ascertained by and through scientific methods" and "opposes ... any procedure that is not reducible to the scientific method, where that method presumes objectivity and value neutrality" (p. 49). Muller (2009) further described tenets of positivism:

that information which is numerically measurable is the only sort of knowledge necessary; that numerical data can substitute for other forms of inquiry; and that numerical acumen can substitute for practical knowledge and the underlying assets and services. (p. 4)

Therefore, mathematical language, statistics, and experiment are the tools of positivist social science. St. Pierre (in press) noted that proponents of positivism are "encouraged by neopositivsm's and neoliberalism's cult of accountability that links logic and measurement to knowledge" (p. 4).

Positivist social science is presumed to be a unified science that can produce unified knowledge that can explain all social phenomena. Or, as Hacking (1983) explained, science is successful because it, supposedly, is converging on truth. He went on to say,

The phenomenon of growth is at most a monotonic increase in knowledge, not convergence. This trivial observation is important, for "convergence" implies somewhat that there is *one* thing being converged on, but "increase" has no such implication. There can be heapings up of knowledge without there being any unity of science to which they all add up. There can also be an increasing depth of understanding and breadth of generalization without anything properly called convergence. Twentieth-century science is a witness to this. There are numerous merely sociological explanations of the growth of knowledge, free of realist implications. (p. 55-56)

The heaping up of knowledge does not indicate improved understanding. In order to make that argument, the knowledge that is produced must come together to support one point or idea. "The accumulation of knowledge in science," according to St. Pierre (in press), "presumes that, over time, one bit of knowledge builds on another and then another, correcting errors along the way, until there is a solid foundation of scientific truth" (p. 18). Though the theory of the accumulation of knowledge is key in positivist thought, it is unclear what, exactly, is accumulating.

According to positivists, experiment is the only clear method to achieve scientific goals. Scholars such as Hayek (1952) argued that such a limited understanding of empiricism and science "leads to an abuse of reason that transforms a rational philosophy of science into an irrational dogma" (Ryder, 2005, para. 1). Habermas (1968/1971), warned that positivism spurns self-reflection and public scrutiny. More significantly, Feyerabend (1975) argued that any move to standardize and mandate the scientific method would stifle scientific progress. Nevertheless, qualitative research remains dependent on positivist terms and proponents of SBR in education continue to promote a positivist social science.

Qualitative research and positivism. Positivism "offers assurance of unambiguous and accurate knowledge of the world" (Crotty, 1998, p. 18), a definitive answer to questions of, for example, what works in the classroom, an orientation towards reality that is problematic within other theoretical frameworks such as social constructivism and postmodernism. Those who do positivist work "assume a fixed measurable reality external to people" (Glesne, 1999, p. 5).

Research produced in other frameworks has different goals and different understandings of reality. For example, social constructivists believe that "reality is socially constructed, complex, and ever changing" (Glesne, 1999, p. 5) rather than objective and "out there" to be discovered.

Qualitative research has long critiqued the limits of positivism. Kvale and Brinkman (2009) claimed, "positivism takes as its starting point the elimination of human subjectivity in research" (p. 58). In 1985, Lincoln and Guba explained that positivist inquiry had become insufficient for answering questions that continued to be raised in social science research and that

cracks have begun to appear in science's magnificent edifice as new "facts" are uncovered with which the old paradigm cannot deal or explain. Normal science in the Kuhnian sense is becoming more and more difficult to sustain. Serious challenges are being mounted from the perspective of alternative paradigms that suggest new and different answers. (p. 7)

Lincoln and Guba offered a possible explanation for the resistance to qualitative research by positivist researchers who attempted to discredit it as unscientific. They wrote, "[w]e are all so imbued with the tenets of science that we take its assumptions utterly for granted, so much so that we almost cannot comprehend the possibility that there might be other ways of thinking" (pp. 8-9). It is difficult to disrupt the dominance of the discourse of conventional positivist social science. Even those who are open to other ways of thinking continue to use concepts that they critique (e.g., particular descriptions of data, validity, bias).

SBR and Positivism. Tracing SBR back to the 1890's, Beghetto (2003) and others (e.g., Franco, 2007; Smith, 2003; St. Pierre, 2006) argued that the term was the federal government's tactic for transforming a "soft" social science (education) into a "hard" science, like physics, in effect, making education a science. Positivism has long been part of the social consciousness of

education. The transmission or banking model of education, which views curriculum as little more than a means of transmitting skills to students, follows this rationality. Freire (1970/2000) described the banking model of education as turning students "into 'containers,' into 'receptacles' to be 'filled' by the teacher" (p. 72). Consequently, this view of education provides a justification both for measuring student outcomes and teacher effectiveness using standardized testing, which ostensibly quantifies the amount of knowledge a teacher has transmitted to students. The reduction of teaching and learning to the transmission of skills and the measurement of that transmission through standardized testing allows for the supposed prediction and control of learning outcomes and by extension, student success.

It is also important to understand that the experimental method privileged by SBR is produced in and through the discourse and theories of positivism, not outside theory as is sometimes implied. Because the federal government during the Whitehurst years both supported and promoted a positivist science in educational research to the exclusion of other sciences described in other theoretical frameworks, the range of possible sciences recognized as valuable by the federal government was greatly diminished. The government promoted the standardization of methodology as a move towards some "common good," with particular meanings of "science," "standards," "quality," and "evidence" that are only thinkable within positivism.

Shifting Science

Kuhn (1962/1996) argued that there is no singular truth to science but that the idea of science is determined in any given moment by the consensus of the scientific community. Kuhn used the term *paradigm* to describe the consensus of the scientific community at a given historical moment. Through changes in thinking, technological developments, cutting edge
research, and other means, scientific beliefs change over time. When a rival paradigm becomes strong enough to challenge the current, established paradigm, a paradigm shift may occur. Kuhn believed that science is always temporal and contextual, described by the values and culture of the historical period. Further, he recognized that only a certain set of intellectual options, technological tools, terminology, and so forth are available to science at any given time, which always restrict what it is possible to think and know. Consequently, the goal of science according to Kuhn is not to find a universal truth but to create a theory or model that accounts for the greatest number of observations and phenomena in a given context. The idea that science is always contingent has been accepted by many including Feyerabend, Latour, and other scholars who work in the field of science studies.

Kuhn (1962/1996) also critiqued the concept, *objectivity* and argued that because science is conducted by humans, it is always subjective. The development of scientific measurement tools was an attempt to remove human error from observation. However, when both the observer and observed are human or a human phenomenon, subjectivity is always present. Importantly, much that relates to human beings simply cannot be measured.

Gage (1989) defined science as "a network of laws that would hold forever everywhere" (p. 143), and his definition clearly aligns with a positivist understanding of social science. However, he claimed that educational research would be better off if instead of perceiving a conflict among the paradigms producing research in education, educational researchers were to explore how those paradigms might work together to produce knowledge about education. This approach, he argued, might come closer to something we could call science. After all, much has been gained from positivist research, interpretivist research, critical research, and indeed, postmodern research. One need not lay claim to the label of science to the exclusion of all other ways of knowing. However, as St. Pierre (2011) argued,

some descriptions have great purchase in the social sciences, especially positivist approaches that mimic ideas no longer supported in the natural sciences. Ideas of absolute time, linearity, and sequenced progression enable positivist ideas such as cause and effect, the accumulation of knowledge, and so on. (p. 619)

If we view positivist social science as one description among others, we honor the openness and drive to inquiry that guides much scientific work. Mandating research methodology in federal law is absurd because it promises easy solutions to complex problems. Diverse and perhaps incommensurable knowledges produced using different research methodologies and theories are more like to address the complexity of social life, what might be called a science-to-come⁶, a science we have not yet been able to think and do. However, whether or not educational research is scientific is only one factor that contributes to its poor perception.

Perception of Educational Research

That educational research lacks merit and is useless is an accusation that has been leveled again and again in the SBR debates, but it is interesting that the accusers chiefly cite Kaestle's (1993) paper, discussed above, as a warrant for that claim. They also cite Lagemann's (2000) history of educational research that does an elegant job of explaining the challenges, both real and perceived, that the field has faced. Lagemann described "the history of education scholarship not as the isolated history of an intellectual field, but rather as an ongoing story about larger

⁶ I use science-to-come in the Derridean sense to signal the importance of science while also acknowledging it may never be. Concepts like science, democracy, justice, and so forth are, according to Derrida, necessary but impossible. That is, these are concepts we cannot do without, that we must remain open to, but they are not attainable.

constellations of social values and views that have often found their clearest manifestations in debates about education, including educational research" (p. xiii). She also discussed issues and concerns within the educational research community such as the difficulty in training practitioner/researchers and the marginalization of education as a field. She did not, however, provide a condemnation of research in education. One of her strongest warnings echoed Dewey's (1929) that "excessive quantification, combined with a diminished emphasis on history and philosophy, would significantly diminish educational study" (Lagemann, 2000, p. 235). Although Lagemann did not laud educational research, her critique certainly does not support the narrow definitions of science used by those who cite her. In fact, she reserved some of her strongest criticism for research using narrow definitions of science such as those found in the SBR debates. Lagemann (2000) defined science as "systematic, empirical investigation" (p. 19), which would include a variety of methodologies.

Four Lines of Descent

Lagemann proposed four main reasons for the poor perception of educational research, which Foucault (1977) described as *lines of descent*. Lines of descent, as explained in Chapter Two, trace an idea to the conditions that made it possible. Lagemann's first reason for the poor perception of educational research is the poor perception of teachers. She, along with Labaree (2004), argued that the feminization and deprofessionalization of teaching marginalized it as a field. As a result, those who choose to study education are likewise relegated to the lower rungs of academia. Further, Lagemann argued that it is difficult to categorize educational research because of its diversity. She explained, "[n]either singular in focus nor uniform in methods of investigation, educational research grew out of various combinations of philosophy, psychology, and the social sciences, including statistics" (p. ix). Consequently, some of the distinctions between these philosophies and methodologies prove impenetrable to the general public. For example, there is research about curriculum (descriptive work that looks at curriculum theory), but there is also research that evaluates curricula (study of the implementation of specific curricula). Likewise, Labaree (2004) wrote of the conflict that can occur in schools of education that are responsible for both "preparing teachers and producing educational researchers" (p. 83), particularly when schools of education are such low-status institutions. Even now, EdD and PhD programs are administered in the same college and by the same departments, but the goals of the two degrees can be quite different—EdD programs prepare so-called classroom practitioners and PhD programs prepare educational researchers. Many PhD students in education are former classroom teachers who have left teaching in order to become educational researchers, which is also unusual. There are also issues of gender (i.e., that education is a female-dominated field), professionalism (i.e., the contention that education is a vocation rather than a profession), race (i.e., African Americans are awarded PhDs in education more than any other field) and so forth that undermine the perception of the quality of the work in the field.

The second reason that Lagemann (2000) proposed for the poor perception of educational research is that those who studied education early on developed "a sadly narrow problematics for the field" (p. xii) in which early educational researchers emulated the natural sciences and "tr[ied] hard to be 'hard'" (St. Pierre, 2006, p. 240). Consequently, they "failed to realize that their goals might have been better served by instead pondering what distinctive characteristics might comprise rigor and relevance in this particular domain of scholarship" (Lagemann, 2000, p. xii). That is to say, rather than seeking to identify characteristics that were unique to education and that required a different methodological approach, early educational research suffered from the same scientism as much other social science.

A third reason Lagemann (2000) proposed for why educational research is seen as inferior is "the multifaceted relationships that have existed between scholars of education, on the one hand, and the society that has sustained them, on the other" (p. xii). Everyone who has been to school believes they are experts on education. Further, "people who study and practice education are engaged in low-status work" (p. xii), and in many ways, it is this low status that has undermined the possibility of developing a professional community. The attitude persists that, "he who can, does; he who cannot, teaches" (Shaw, 1903/1922, p. 230). Unfortunately, Shaw's comment has become an accepted truth. Lagemann noted that this view contributes to the negative view of expertise and the value of education in America, which she described as follows:

a compound of all the qualities Hofstadter saw in anti-intellectualism—especially a skepticism toward intellect and a preference for instrumental knowledge, know-how, over less purposive reflection, speculation, or pondering. Antieducationism also encompasses assumptions concerning the lack of knowledge, skill, ambition, and competence needed and possessed by educators...it encompasses as well assumptions concerning the simplicity, sterility, and, more often than not, irrelevance or pointlessness of the educational process. Antieducationism thus allows one to believe that excellence can be achieved in and through education even when investments in personnel, research, materials, and equipment are limited. As one can see throughout the history of education scholarship, antieducationism has helped to undermine the effectiveness of all aspects of education. (pp. xii-xiii)

The fourth reason Lagemann (2000) proposed that educational research suffers from poor perception is that education is impacted by a variety of factors, for example, politics and policy,

and it operates across multiple disciplines. As a result, "education [is] a field of study and professional practice that is illuminated by a wide variety of disciplinary and multidisciplinary approaches" (p. xiv). Consequently, the field seems scattered and fractured as if there is no unified purpose or organizing principle. Indeed, that notion is true, but it is also true of other fields such as sociology and anthropology. Once again, the difference for education is in the perception, or as Lagemann stated, "many of the most difficult educational problems that exist in the United States today are related to ways in which the study of education has been organized and perceived within universities" (p. xv).

The Link Between Teaching and Educational Research

Teaching, by the end of the 19th century, had become, by and large, women's work. Thought to have the appropriate disposition to guide young minds and willing to accept much lower salaries than their male counterparts, women were seen as the ideal teachers in the United States. At that time, the belief that women were intellectually inferior to men negatively impacted men's perception of education as a profession, which extended to educational research. Further, because of the assumed intellectual limitations of women and the perception that women were appropriately affectionate and naturally inclined to nurture young people, women were seen as fit to teach only in the lower grades. Higher education remained largely dominated by men. Additionally, as universities and institutions of higher education created schools of education, the association of education with women's work served to marginalize those schools within the larger university, delegitimizing education as a profession and undercutting the value of research focused on education. By the turn of the 20th century, education and educational research had already been established as inferior fields lacking the rigor of other disciplines. As a result, professors of education sought to combat their position in the university by creating a science of education:

Between roughly 1890 and 1920 educational research emerged as an empirical, professional science, built primarily around behaviorist psychology and the techniques and ideology of quantitative measurements. Inevitably, as this approach gained acceptance, other approaches to the study of education—most important, the approach developed by John Dewey at the University of Chicago between 1894 and 1904—were pushed to the margins, though they were never totally eclipsed and would reappear from time to time. From the first, contests tinged with issues of gender and professional status, as well as with a host of more immediate circumstantial factors, were central to the history of educational research. (Lagemann, 2000, p. 16-17)

The negative perception of educational research has done a great deal of damage. Thorndike (1906) claimed that "what the expert in the science of education deems scientific has the greatest probability of being so" (p. 81), but professionals in this field have never been granted the status or trust, public or otherwise, to make this determination for their own work.

Conclusion

Assumptions about the poor quality of educational research were the rationale for the imposition of federal definitions of science and methodology at the beginning of the 21st century. Those assumptions were also cited as the impetus for the NRC's reports about scientific research in education, and furthermore, those assumptions drove AERA to write and publish standards for reporting research in its journals, draft its own definition of scientifically based research, and convene annual meetings around the issue of quality in educational research. In sum, powerful

institutions with the authority to influence and regulate the conduct of research in education have contributed to a regime of power based on a faulty assumption. It appears that those institutions did not question too much the assumption that educational research was of poor quality before determining to improve it by re-instituting positivist social science. The form those actions took—legislation, reports, standards, and so forth—questioned ideas of what counts as science, interrogated notions of methodology, and troubled issues of epistemology, yet ironically, none seriously questioned the underlying hypothesis that educational research was in serious need of improvement. Importantly, they did not produce sound evidence to warrant their claims, the very standard used in SBR. In this chapter, I have explained some of the key enabling conditions for the formation of the discourse of SBR. In the following chapter, I describe SBR's conditions of existence as a regime of truth.

CHAPTER FIVE

A Problem of the Present: The Conditions of Existence of a Regime of Truth Introduction

In this chapter, I explain the conditions of existence for the discourse of scientifically based research (SBR) in the last decade. Conditions of existence are the conditions that enable a concept, like SBR, to exist in a field of discourse; that is, the conditions of existence describe the life of a discourse-its history and rules of formation. These conditions of existence demonstrate how the overlapping practices of the federal government, the National Research Council (NRC) and the American Educational Research Association (AERA) produced SBR as a regime of truth. Foucault (1980b) claimed, "[t]ruth is a thing of this world: it is produced only by virtue of multiple forms of constraint" (p. 131). The federal government, the NRC, and AERA are powerful institutions that influence the direction of educational research and their practices provide examples of multiple forms of constraint that disciplined educational research and attempted to limit the kinds of knowledge that could be produced. Although these institutions claim to be independent of each other in both their political motivations and the topics they engage, even a cursory examination of the events that took place between 2002 and 2010 demonstrates that the practices of those three institutions overlapped and implicated one another in regard to SBR, both overtly and implicitly. I provide a detailed analysis of the conditions of existence of the regime of truth of SBR below.

The Work of the Federal Government

In the following sections, I explain some of the key events related to SBR that occurred within the federal government. I demonstrate how these events contributed to the federal government's being a locus of power for the discourse of SBR. I begin by describing SBR in education as it has been variously defined in federal legislation including the Reading Excellence Act (1999) (REA, Public Law 105-277), the No Child Left Behind Act (2002) (NCLB, Public Law 107-110) and the Education Sciences Reform Act (2002) (ESRA, Public Law 107-279). I discuss those definitions and how they evolved through those three pieces of federal legislation. Finally, I describe some of the federal institutions that were established as a result of those federal laws. Those conditions of existence of SBR in the federal government demonstrate how SBR began its web of power.

Definitions of SBR in Federal Legislation

Beghetto (2003) wrote that the government's involvement in defining science for education could "be traced from the Cooperative Research Act of 1954 to the creation of the National Institute of Education in the early 1970s, which was later subsumed by the Office of Educational Research and Improvement (OERI)" (para. 2). However, scientifically based research as a concept emerged between 1999 and 2002 when the U.S. Congress passed three pieces of federal legislation that defined scientifically based research in education. The original description of SBR first appeared in REA, was repeated in NCLB, and then was reinforced and expanded by ESRA. I have included below the complete definition of SBR from each of the three federal laws as well as a brief discussion of the changes in the definition of SBR in the three laws. The purpose of REA was to provide early literacy intervention for all students in order to identify deficits, improve reading skills, and increase teacher expertise in the area of early literacy "through the use of scientifically based reading research" (Title VIII, Part C, §2251, para 3). In order to enforce the mandate of scientifically based research, it was necessary to define SBR. REA defined SBR as follows:

The term "scientifically based reading research"-

(A) means the application of rigorous, systematic, and objective procedures to obtain valid knowledge relevant to reading development, reading instruction, and reading difficulties; and

(B) shall include research that—

(i) employs systematic, empirical methods that draw on observation or experiment;

(ii) involves rigorous data analyses that are adequate to test the stated hypotheses and justify the general conclusions drawn;

(iii) relies on measurements or observational methods that provide valid data across evaluators and observers and across multiple measurements and observations; and

(iv) has been accepted by a peer-reviewed journal or approved by a panel of independent experts through a comparably rigorous, objective, and scientific review. (Title VIII, Part C, §2252, para 5B)

Offering up a definition that seemed overly general and confused, scientifically based research began its emergence with REA.

Subsequently, according to Baez and Boyles (2009), in the summer of 2000, draft legislation was introduced "by United States Representative Mike Castle (R-Del) that pertained to the reauthorization of The Office of Educational Research and Improvement" (p. 6). Called "The Castle Bill," this legislation included a definition of scientifically based research that would determine the allocation of federal dollars for educational research. Eisenhart and Towne (2003) explained that "The Castle Bill" proposed standards for "scientifically based quantitative" and "scientifically based qualitative" research (p. 32). The reauthorization process for OERI continued for some time, and The Castle Bill was revised and redrafted multiple times, sparking debate about the nature of scientific educational research and the need to define it. Although the definitions proposed in The Castle Bill did not ultimately make it into law, they influenced the conversation about the federal influence on educational research.

According to Eisenhart and Towne (2003), SBR itself did not have much impact until the passage of NCLB, which was the far overdue reauthorization of the Elementary and Secondary Education Act that had expired in 1997. NCLB was passed by The House of Representatives in May of 2001, passed by the Senate in June of 2001, and signed into law on January 8, 2002 by President George W. Bush. It lifted the definition of SBR from The Reading Excellence Act of 1999 (REA) and expanded it to include preferences for certain kinds of research methodologies. NCLB defined SBR as follows:

The term "scientifically based research"-

(A) means research that involves the application of rigorous, systematic, and objective procedures to obtain reliable and valid knowledge relevant to education activities and programs; and

(B) includes research that—

(i) employs systematic, empirical methods that draw on observation or experiment;

(ii) involves rigorous data analyses that are adequate to test the stated hypotheses and justify the general conclusions drawn;

(iii) relies on measurements or observational methods that provide reliable and valid data across evaluators and observers, across multiple measurements and observations, and across studies by the same or different investigators;
(iv) is evaluated using experimental or quasi-experimental designs in which individuals, entities, programs, or activities are assigned to different conditions and with appropriate controls to evaluate the effects of the condition of interest, with a preference for random-assignment experiments, or other designs to the extent that those designs contain within-condition or across-condition controls;
(v) ensures that experimental studies are presented in sufficient detail and clarity to allow for replication or, at a minimum, offer the opportunity to build systematically on their findings; and

(vi) has been accepted by a peer-reviewed journal or approved by a panel of independent experts through a comparably rigorous, objective, and scientific review. (Title IX, Part A, §9101, para 37A)

It has been reported that the term "scientifically based research" appears in NCLB 111 times (Neuman, 2002; Shavelson & Towne, 2002), representing unprecedented federal legislation of research methodology. Described by Beghetto (2003) as "the most sweeping reform of the Elementary and Secondary Education Act (ESEA) since it was enacted in 1965, [NCLB] redefines the federal role in K-12 education" (para 2). The definition of SBR in NCLB includes

conceptions of reliability and validity that are not appropriate for qualitative research, and further, the definition privileges a peculiar "what works" mentality that gives preference to causal research that uses "random-assignment experiments" (Title IX, Part A, §9101, para 37Biv) and replicability. As Walters, Lareau, and Ranis (2009) commented, "[i]n effect, NCLB gave the federal government an unusual degree of authority for setting standards for what constitutes good science and an unusual degree of control over the conduct of 'science''(p. 6).

SBR, nonetheless, was mandated in federal law and functionally became federal policy, as a variety of federal (e.g., U.S. Institute of Education Sciences) and national (e.g., NRC) institutions and professional associations (e.g., American Educational Research Association) took it up and variously enforced it. In 2002 November, The Education Sciences Reform Act (ESRA) became law and the research arm of the Department of Education, OERI, became the new Institute of Education Sciences (IES), whose very title emphasized the importance of science in education.

Baez and Boyles (2009) argued that ESRA was particularly important because it was the "first explicit attempt to establish a science for educational research," whereas REA and NCLB dealt with issues of education at large. Consequently, ESRA and its definition of SBR disciplined the field of educational research, solidifying the discourse of SBR in documentary and juridical form. ESRA used the definition of SBR from NCLB, applied it specifically to educational research, and mandated preferred methodologies and research designs that would constitute high-quality science. ESRA defined SBR as follows:

The term "scientifically based research standards" means research standards that-

(i) apply rigorous, systematic, and objective methodology to obtain reliable and valid knowledge relevant to education activities and programs; and

(ii) present findings and make claims that are appropriate to and supported by the methods that have been employed.

(B) The term includes, appropriate to the research being conducted—

(i) employing systematic, empirical methods that draw on observation or experiment;

(ii) involving data analyses that are adequate to support the general findings;

(iii) relying on measurements or observational methods that provide reliable data;

(iv) making claims of causal relationships only in random assignment experiments or other designs (to the extent such designs substantially eliminate plausible competing explanations for the obtained results);

(v) ensuring that studies and methods are presented in sufficient detail and clarity to allow for replication or, at a minimum, to offer the opportunity to build systematically on the findings of the research;

(vi) obtaining acceptance by a peer-reviewed journal or approval by a panel of independent experts through a comparably rigorous, objective, and scientific review; and

(vii) using research designs and methods appropriate to the research question posed.

(19) SCIENTIFICALLY VALID EDUCATION EVALUATION.—The term "scientifically valid education evaluation" means an evaluation that—

(A) adheres to the highest possible standards of quality with respect to research design and statistical analysis; (B) provides an adequate description of the programs evaluated and, to the extent possible, examines the relationship between program implementation and program impacts;

(C) provides an analysis of the results achieved by the program with respect to its projected effects;

(D) employs experimental designs using random assignment, when feasible, and other research methodologies that allow for the strongest possible causal inferences when random assignment is not feasible; and

(E) may study program implementation through a combination of scientifically valid and reliable methods.

(20) SCIENTIFICALLY VALID RESEARCH.—The term "scientifically valid research" includes applied research, basic research, and field-initiated research in which the rationale, design, and interpretation are soundly developed in accordance with scientifically based research standards. (Title I, §102, para 18)

ESRA placed a high priority on the randomized control trial (RCT)⁷, explaining that it was the only adequate method for producing causal inferences and thereby implying that causal research is superior in solving educational problems. The discussion of SBR within those three federal laws began "a whole new 'régime' in discourse and forms of knowledge" (Foucault, 1980b, p. 112).

⁷ RCT is an experimental design in which a sample is randomly selected and assigned to both control groups and experimental groups. An intervention is applied to the experimental group, and data is collected based on the effect of the intervention.

The emergence of a definition. What is immediately obvious in looking across the three pieces of legislation is that the definition of SBR was expanded in each succeeding law. In fact, in REA, the definition was 111 words long; in NCLB, it was just over 200 words; and in ESRA, the definition topped 330 words. However, what is not explicitly stated is how the definitions in the laws were developed. Eisenhart and Towne (2003) researched the genesis of the definition of SBR and documented their findings. They reported that Robert Sweet, then professional staff member for the majority members of the House Education and Workforce Committee, was tasked with creating a definition of SBR to be included in REA. According to Eisenhart and Towne,

To craft this definition, which would be the starting point for all of the now numerous definitions that appear in major federal education laws, Sweet visited the websites of several DC-based research institutions (including the NRC, although this took place well before the committee was convened to produce SRE [NRC, 2002]), consulted with numerous university-based researchers (primarily with backgrounds in cognitive psychology), and shared drafts with these researchers (he estimates approximately 20-25 of them). The language that emerged from the several-months-long process was inserted into REA (1999), and passed without fanfare. (p. 32)

Of particular note is that the researchers Sweet consulted by and large had backgrounds in cognitive psychology, a field heavily influenced by positivism, so it would follow that their definition of science would be positivist. Research grounded in other epistemologies would have different methodologies, different standards, and therefore, different definitions of science.

The nod to qualitative research. Importantly, each definition of SBR included a reference to *observational methods*, though the intended meaning the term in each instance is

unclear. In the SBR definitions, observational methods are set in opposition to experimental methods (i.e., each law states "empirical methods that draw on observation or experiment" Title IX, Part A, §9101), seeming to signify that observation stands for any methods that are not experimental. This may be a gesture towards the inclusion of qualitative methodology⁸, but those who conduct qualitative research likely would have used different language. In his qualitative research textbook, Patton (2002) described qualitative research as "people-oriented inquiry" (p. 27) and explained that "[q]ualitative findings grow out of three kinds of data collection: (1) indepth, open-ended interviews; (2) direct observation; and (3) written documents" (p. 4).

In Patton's definition of qualitative research, observational methods constitute only one of the methods that can be employed by qualitative researchers. He also pointed out that observation and interviewing are rarely separate and distinct methods within qualitative research. The definitions of SBR in the three pieces of legislation completely ignore interviewing, which is arguably as important, if not more important, than observation in many qualitative research studies. Patton (2002) explained that interviewing is especially important in qualitative research because "we cannot observe everything" and as a result "[w]e have to ask people questions about those things" (p. 341). Thorne (2008) wrote that "interviewing has become the primary source of data in so many fields of clinical qualitative inquiry" (p.79), and Bogdan and Biklen (1992)

⁸ It is also possible that "observational methods" are a reference to standardized observation protocols that commonly exist in experimental research and single-subject design research. However, the use of observational methods, and specifically observation procotols, is commonly addressed in qualitative research textbooks (e.g., Creswell, 1998; Fraenkel and Wallen, 2005; Patton, 2002). Because this term can be taken up in both communities, its use in federal definitions of SBR is both confusing and problematic.

pointed out that interviews "may be the dominant strategy for data collection, or they may be employed in conjunction with participant observation, document analysis, or other techniques" (p. 96). What these descriptions all highlight is the importance and prevalence of the interview in qualitative research, which is not an observational method. Thus, the way the word "observation" is used in the laws does not make sense, and indicates a lack of knowledge about research methodologies that are not experimental.

Further, the definition of SBR in REA stipulated that data must remain valid "across...observers and across...observations." In other words, high quality studies must be replicable. According to Patton (2002), however, in qualitative research "observational data must have depth and detail. The data must be descriptive—sufficiently descriptive that the reader can understand what occurred and how it occurred" (p. 23). Each observer necessarily notices and describes different things, all of which are "correct" and "valid" though different. Multiple observers do not and cannot agree on what they see because as Patton (2002) explained, "what people 'see' is highly dependent on their interests, biases, and backgrounds. Our culture shapes what we see, our early childhood socialization forms how we look at the world, and our value systems tell us how to interpret what passes before our eyes" (p. 260). Further, Bogdan and Biklen (1992) claimed, "becoming a [qualitative] researcher means internalizing the research goal while collecting data in the field. As you conduct research you participate with the subjects in various ways" (p. 90). Thus, qualitative studies cannot be replicated because each is specific to a particular site, to particular participants, and to a particular researcher. In other words, qualitative research deals with "particular problems and must deal with local conditions that limit generalizations and theory building" (Berliner, 2002, p.18). Observational data in qualitative

research cannot be replicated across observers or observations nor does qualitative inquiry aim for such replication.

NCLB also included the term "observational methods" in its definition of SBR, but again it is unclear what that means. Additionally, NCLB added "a preference for random-assignment experiments, or other designs to the extent that those designs contain within-condition or acrosscondition controls" (Title IX, Part A, §9101, para 37B4). Randomized trials are an experimental method, not a qualitative method, and the design preferences all but eliminate any acceptance of qualitative methodology in the definition of SBR.

Although ESRA also gestures towards observational research and observational data, the privileging of causal inference and random assignment, concepts and practices used in experimental research, undermines this gesture. While it is generally accepted by qualitative researchers, and even law enforcement officers, that "when looking at the same scene or object, different people will see different things" (Patton, 2002, p. 260), the definition of SBR in ESRA contradicts that assumption because it simultaneously includes a method of qualitative research, observation, while also requiring replication, which is not possible in qualitative research.

In summary, the implications for qualitative researchers are unclear in the three federal laws because the language used to define SBR demonstrates little understanding of qualitative research practices and purposes. What is clear is that the insistence on "rigorous scientific standards" (U.S. House, 2002, p. 2) led to inadequate definitions of science that had a real material effect on who could receive federal funding for educational research. Instead of responding to the confusion created by the definition of SBR in REA, NCLB and ESRA continued and elaborated that confusion.

Federal Institutions Produced by the Discourse of SBR

Those three federal laws and their resultant definitions of SBR are not an example of the discovery of new knowledge. Instead, as Foucault (1980b) explained, this was a historical moment in which there was "a modification in the rules of formation of statements which are accepted as scientifically true" (p. 112). The federal government did not discover and then represent the truth about what constitutes scientifically based research in education; it created a truth. Further, the establishment of institutional bodies such as IES that supported and enacted that truth created disciplinary structures that regulated and proliferated the supposed true discourse of SBR.

For example, the replacement of the Office of Educational Research and Improvement (OERI) with IES⁹, now the research arm of the Department of Education, was pivotal, under the leadership of Grover J. Whitehurst, in the expansion of SBR. It became clear that Whitehurst, from educational psychology, a field rife with positivism, found value only in experimental

(C) the effectiveness of Federal and other education programs. (Title I, § 111, ¶ b1)

⁹ The mission of the Institute is to provide national leadership in expanding fundamental knowledge and understanding of education from early childhood through post-secondary study, in order to provide parents, educators, students, researchers, policymakers, and the general public with reliable information about—

⁽A) the condition and progress of education in the United States, including early childhood education;

⁽B) educational practices that support learning and improve academic achievement and access to educational opportunities for all students; and

research. Now there was an institution created by federal legislation that, through policies and practices, promoted and privileged positivist science. IES, along with the Education Sciences Reform Act of 2002, established the definitions of SBR that would be used in determining federal funding for educational research and programs (Eisenhart & Towne, 2003).

The most significant project of the newly established IES was the creation of the What Works Clearinghouse (WWC). The goal of WWC was to create a synthesis of research on different topics, based on a review process that determined whether programs were supported by SBR, that could provide educators with solid and irrefutable strategies that will work in classrooms and schools (www.whatworks.ed.gov). Director Whitehurst also intended the WWC to serve as a resource to aid school districts in making curricular decisions by providing a review of research on various curricula and curriculum materials in order to determine whether they have been "proven" effective (IES, nod). IES (2007) explained that it "will support research, conduct evaluations, and compile statistics in education that conform to rigorous scientific standards, and will disseminate and promote the use of research in ways that are objective, free of bias in their interpretation, and readily accessible" (para 1).

During his tenure, Whitehurst and IES created a hierarchy of methodology that constituted SBR, and the only research included in the Clearinghouse under Whitehurst adhered to those methodologies. According to Franco (2007), the first methodological preference, as outlined by IES on its website at that time, (ies.ed.gov), was RCT, followed by quasiexperimental design, co-relational studies with statistical controls, co-relational studies without statistical controls, and finally, case studies. RCTs became the "gold standard" for educational research in the Whitehurst years.

In order to help meet the goals of the WWC and out of concern that the American

Educational Research Association was not promoting high quality scientific educational research, IES provided a \$750,000 grant to help organize a new educational research association, the Society for Research on Educational Effectiveness (SREE), intended to disseminate scientifically based research findings to practitioners and educational leaders. According to its website (www.sree.org), SREE

aims to: (1) increase the capacity to design and conduct investigations that have a strong basis for causal inference, (2) bring together individuals investigating cause-and-effect relations in education, and (3) promote the understanding and use of scientific evidence to improve education decisions and outcomes (para 1).

Within IES, the discourse of SBR produced definitions of "scientific" research, methodological and epistemological privilege, a new educational research organization, and a particular kind of "scientific" educational researcher. To restate, SBR proliferates and produces human subjects whose work within institutions further enables the establishment of SBR as a discursive and material formation. Here, an entire governing body began to take form, producing the discourse of SBR as truth.

In 2009, President Barak Obama appointed John Easton to be the new Director of the Institute of Education Sciences, and, as Viadero (November 30, 2009) reported in *Education Week*, Easton explained "that while promoting rigorous research through randomized experiments will be an important part of that agenda, it won't be the agency's guiding star as it was under his predecessor, Grover J. 'Russ' Whitehurst" (para 3). Instead, Easton stated that he planned to focus more on the usability of educational research and therefore include policymakers and practitioners in the research process. Viadero wrote that rigor was being replaced by another R-word—relevance. Easton agreed that RCTs are useful, but he maintained that understanding the reasons for research findings requires different questions, a wider range of research methodologies, and a more robust definition of rigor that includes, for example, relevance. Still, SBR has become entrenched in educational research, practice, and policy, and the change in presidential administration has only served to fortify it.

What began as an idea—scientifically based research—was documented as a definition in federal legislation and grew exponentially in very short order. As Foucault (1980b) explained, "it is a question of what *governs* statements, and the way in which they *govern* each other so as to create a set of propositions which are scientifically acceptable ... in short, there is a problem of the régime, the politics of the scientific statement" (p. 112). I have demonstrated that federal legislation created institutions and human subjects with particular epistemological and methodological allegiances. Those subjects produced additional institutions, additional subjects, additional practices, and codified all in documentary form, positioning the federal government as a locus of activity and power within the discourse of SBR. Consequently, the documentary accumulation allows SBR to function as truth and the federal government to contribute to SBR as a regime of truth.

The Work of the National Research Council

The federal government painted the first broad strokes of SBR, but the National Research Council contributed to an immediate "hastening of evolution" (Foucault, 1980b, p. 112) of the discourse of SBR by producing reports that validated and reinforced a contested discussion already in process. In the following sections, I discuss two consensus committee reports written by the National Research Council (NRC) committees on the topic of SBR in order to demonstrate how the NRC became an institution that defined science for education and disciplined researchers according to the truth of SBR.

Scientific Research in Education

To respond to the ongoing discussion about SBR and to the perception that educational research was weak, and also to respond to growing concern that "narrow definitions of *research* or *science* might trivialize rather than enrich our understanding of education policy and practice" (Feuer, Towne, & Shavelson, 2002, p. 4), the U.S. Department of Education (DOE) commissioned the NRC to form a committee to investigate SBR (Baez & Boyles, 2009, p. 14). The NRC did not question whether the call to take up the question of SBR for education was necessary. Rather, as set forth in its founding charter, the NRC responded to the request of the federal government, thus validating and reinforcing the discourse and setting into motion additional practices that produced SBR as a regime of truth. This is not to say that the NRC *intended* to join and reinforce this discourse. However, this dissertation was not concerned with intentions and examined instead effects—"where the speech goes and what it does there" (Alcoff, 1991, p. 26).

The NRC is the operating arm of the National Academy of Sciences, and, according to its website, "the mission of the NRC is to improve government decision making and public policy, increase public education and understanding, and promote the acquisition and dissemination of knowledge in matters involving science, engineering, technology, and health" (para 2). In the wake of the federal legislation that introduced and supported SBR and in an effort to include educational researchers in the conversation, "Kenji Hakuta, the chair of the National Educational Research Policy and Priorities Board (NERPPB), turned to the NRC to inject the voice of researchers into policy initiatives of this kind" (Eisenhart & Towne, 2003, p. 32).

As a result, in late 2000, the NRC organized a committee to investigate what constitutes scientific research in education—the Committee on Scientific Principles in Educational

Research. In March of 2001, the committee "hosted a workshop on science, evidence, and inference in education" (SRE, 2002, p. ix) in order to hear from a variety of speakers on the topic of scientific research in education. The members of the committee were as follows: Richard J. Shavelson (Chair), School of Education, Stanford University; Donald I. Barfield, WestEd, San Francisco; Robert F. Boruch, Graduate School of Education, Wharton School Department of Statistics, and Fels Center for Government, University of Pennsylvania, Philadelphia; Jere Confrey, Department of Curriculum and Instruction, University of Texas at Austin; Rudolph Crew, Stupski Family Foundation, Mill Valley, California; Robert L. DeHaan, Department of Cell Biology, Emory University, Atlanta, Georgia; Margaret Eisenhart, School of Education, University of Colorado at Boulder; Jack McFarlin Fletcher, Department of Pediatrics, University of Texas, Houston; Eugene E. Garcia, Graduate School of Education, University of California, Berkeley; Norman Hackerman, Robert A. Welch Foundation, Houston, Texas; Eric Hanushek, Hoover Institution, Stanford University; Robert Hauser, Center for Demography of Health and Aging, University of Wisconsin-Madison; Paul W. Holland, Educational Testing Service, Princeton, New Jersey; Ellen Condliffe Lagemann, The Spencer Foundation, Chicago, Illinois, and New York University, New York; Denis C. Phillips, School of Education, Stanford University; Carol H. Weiss, Graduate School of Education, Harvard University; Lisa Towne, NRC Study Director; Tina Winters, NRC Research Assistant; and Linda DePugh, NRC Senior Project Assistant.

By November of 2001, the committee released in prepublication form its consensus report *Scientific Research in Education (SRE)*, "a report designed to articulate the nature of scientific educational research and to guide efforts aimed at improving its quality" (*ASRE*, 2005, p. vii). It was, indeed, "no small task" to deliberate on the nature of science in education, "particularly in less than a year" (*SRE*, 2002, p. xii). In the introduction, the committee wrote, "[o]ur report is specifically intended to provide an articulation of the core nature of scientific inquiry in education from the research community" (NRC, 2002, p. 21). Interestingly, the word "core" in that sentence is an obvious marker of positivism, because it is only positivism that claims that science is essentially the same across all instances of its occurrence.

SRE acknowledged that the political climate within which it was written was marked by a "rising enthusiasm for evidence-based education policy and practice," (NRC, 2002, p. 1). The U.S. Department of Education commissioned the NRC to take up the question of what constitutes scientific inquiry in education and whether education is different than other disciplines in this regard. The charge read as follows: "This study will review and synthesize recent literature on the science and practice of scientific educational research and consider how to support high quality science in a federal educational research agency" (NRC, 2002, p. 22). The committee that authored SRE translated its task into three basic questions: "What are the principles of scientific quality in educational research? How can a federal research agency promote and protect scientific quality in the educational research it supports? How can researchbased knowledge in education accumulate?" (NRC, 2002, pp. 22-24). The first question represents an increasing focus on accountability in the 21st century in general and in education in particular, what some have called "the audit culture" (Hodkinson, 2004, p. 16). The question about the "accumulation of knowledge" is positivist and an alert that a particular theoretical framework guided the committee.

The executive summary of *SRE* (2002) stated that "at its core, scientific inquiry is the same in all fields" (p. 2), another statement that reflects the positivist claim that all knowledge is unified. The committee further claimed that even with a core understanding of scientific inquiry,

"[a] wide variety of legitimate scientific designs are available for educational research" (NRC, 2002, p. 6). The report stated, "rarely does one study produce an unequivocal and durable result," and its authors agreed that "multiple methods, applied over time and tied to evidentiary standards, are essential to establishing a base of scientific knowledge" (2002, p. 2). While these concessions about research and its limitations seem to provide some latitude for different kinds of research that might qualify as scientific, the report, nonetheless, reinforced a narrow and positivist understanding of science.

SRE presented six guiding principles of scientific inquiry:

1) Pose significant questions that can be investigated empirically,

2) Link research to relevant theory,

3) Use methods that permit direct investigation of the question,

4) Provide a coherent and explicit chain of reasoning,

5) Replicate and generalize across studies,

6) Disclose research to encourage professional scrutiny and critique. (2002, pp. 3-5) Principal number four mobilizes a particular description of reason as coherent and explicit—one that smacks of positivism. And principle number five employs positivism because, as explained earlier, only experimental research claims replicability. *SRE* (2002) also claimed that "to make progress possible, then, theories, hypotheses, or conjectures must be stated in clear, unambiguous, and empirically testable terms" (p. 18). The phrase "empirically testable" is problematic because it is only positivism that relies on the testing of hypotheses¹⁰. While

¹⁰ I use *hypothesis* here in contrast to the term *theory* as used by Baez and Boyles (2009), following Culler (2009). They explained that theory "is a hypothesis but of a particular kind. It must involve complex relations of a systemic kind among a number of factors, and it cannot be

positivist research surely produces knowledge, many scholars within the education community do not practice positivist science. So, once again, while providing nods to qualitative research and claiming an interest in a wide variety of scientific research methods, the report uses language that specifically limits those possibilities.

Advancing Scientific Research in Education

In an attempt to further the discussion and provide specific recommendations for the implementation of SBR in various contexts, including the preparation of educational researchers, the NRC convened a second committee to continue the work of *SRE*. The Committee on Research in Education (CORE),

was convened to advance an improved understanding of a scientific approach to addressing education problems; to engage the field of educational research in actionoriented dialogue about how to further the accumulation of scientific knowledge; and to coordinate, support, and promote cross-fertilization among NRC efforts in educational research. (*ASRE*, 2005, pp. vii-viii)

CORE produced three reports culminating in the 2005 summary consensus report, *Advancing Scientific Research in Education (ASRE)*. Some members on the *SRE* committee were also members of the CORE committee, whose members were as follows: Lauress L. Wise (Chair), Human Resources Research Organization, Arlington, VA; Linda Chinnia, Baltimore City Public School System; Kay Dickersin, Department of Community Health, Brown University, Providence, RI; Margaret Eisenhart, School of Education, University of Colorado, Boulder; Karen Falkenberg, Division of Educational Studies, Emory University, Atlanta, GA; Jack

obvious or easily confirmed or disproved" (p. viii). Consequently, a theory is an overt "act of interpretation" (p. viii) and not a hypothesis that is empirically testable and falsifiable.

McFarlin Fletcher, University of Texas-Houston Health Science Center and Center for Academic and Reading Skills; Robert E. Floden, College of Education, Michigan State University, East Lansing; Ernest M. Henley, Department of Physics, University of Washington, Seattle; Vinetta C. Jones, School of Education, Howard University, Washington, DC; Brian W. Junker, Department of Statistics, Carnegie Mellon University, Pittsburgh, PA; David Klahr, Department of Psychology, Carnegie Mellon University, Pittsburgh, PA; Ellen Condliffe Lagemann, Harvard Graduate School of Education; Barbara Schneider, Department of Sociology, University of Chicago; Joseph Tobin, College of Education, Arizona State University, Tempe; Lisa Towne, NRC Study Director; and Tina M. Winters, NRC Research Associate.

ASRE was the culmination of a year-long series of workshops convened by the committee to "engage a range of education stakeholders in discussions about five key topics" (*ASRE*, 2005, p. viii), which were as follows: peer review in federal educational research programs; understanding and promoting knowledge accumulation in education: tools and strategies for educational research; random assignment experimentation in education: implementation and implications; journal practices in publishing educational research; and education doctoral programs for future leaders in educational research.

ASRE reinforced the definition of SBR outlined in *SRE* and stated three objectives: "1. promoting quality, 2. building the knowledge base, and 3. enhancing professional development" (2005, p. 17). To this end, *ASRE* provided twelve recommendations in its summary report as follows:

Recommendation 1. In federal agencies that support educational research, the criteria by which peer reviewers rate proposals should be clearly delineated, and the meaning of

different score levels on each scale should be defined and illustrated. Reviewers should be trained in the use of these scales.

Recommendation 2. Federal agencies that support educational research should ensure that as a group, each peer review panel has the research experience and expertise to judge the theoretical and technical merits of the proposals it reviews. In addition, peer review panels should be composed so as to minimize conflicts of interest, to balance biases, and to promote the participation of people from a range of scholarly perspectives and traditionally underrepresented groups.

Recommendation 3. In research conducted in educational settings, investigators must not only select rigorous methods appropriate to the questions posed but also implement them in ways that meet the highest standards of evidence for those questions and methods. Recommendation 4. Federal agencies should ensure appropriate resources are available for educational researchers conducting large-scale investigations in educational settings to build partnerships with practitioners and policy makers.

Recommendation 5. Professional associations involved in educational research should develop explicit ethical standards for data sharing.

Recommendation 6. Educational research journals should require authors to make relevant data available to other researchers as a condition of publication and to ensure that applicable ethical standards are upheld.

Recommendation 7. Professional associations and educational research journals should work in concert with funding agencies to create an infrastructure that takes advantage of technology to facilitate data sharing and knowledge accumulation in educational research. Recommendation 8. Educational research journals should develop and implement policies to require structured abstracts.

Recommendation 9. Schools of education that train doctoral students for careers in educational research should articulate the competencies those graduates should know and be able to do and design their programs to enable students to develop them. Recommendation 10. Schools of education that train doctoral students for careers in educational research should design their programs to enable those students to develop deep substantive and methodological knowledge and skill in a specialized area. Recommendation 11. Schools of education that train doctoral students for careers in educational research should provide those students with a variety of meaningful research experiences.

Recommendation 12. Peer review panels in federal agencies that fund educational research should be composed to promote the participation of people from a range of scholarly perspectives and traditionally underrepresented groups and provide

opportunities for professional development. (NRC, 2005, pp. 3-7)

It seemed that the committee responsible for writing *ASRE* had not read, or if they had, either did not understand or take seriously, the sustained and serious critiques of *SRE*. Rather than responding to the critique of *SRE*, *ASRE* claimed that the 2002 *SRE* report was not only correct in its recommendations but that it did not go far enough in its limitations on educational research. In fact, the report summarily disregarded the concerns of academics and researchers who had taken issue with definitions of SBR and governmental encroachment on methodology, arguing, *"scientific research in education could be improved, and the field should focus its energies on doing so"* (NRC, 2005, p.11), according to the definitions established in *SRE*. Ideas promoted in the twelve recommendations made by *ASRE*, such as data sharing and the preparation of educational researchers, worried those who critiqued the entire discursive and material formation of SBR as defined by federal law, *SRE*, and *ASRE*. Unsurprisingly, this report also prompted backlash and resistance from those in the educational research community who do not do positivist research and are aware of its limits in knowledge production (e.g., Franco, 2007; St. Pierre, 2006; St. Pierre & Roulston, 2006).

In conclusion, with the publication of those two reports, the NRC promoted and expanded the discourse of SBR. Once again, the "effects of power circulate among scientific statements" (Foucault, 1980b, p. 112), producing subjects that maintain and perpetuate the discourse of SBR, for example, committee members whose responsibility was to come to consensus and disseminate their findings. Those findings were also codified into documents—the reports themselves—as well as journal articles written by committee members that explained their work and expanded the reach of SBR. As Foucault, (1976/1978) wrote, the "general design or institutional crystallization is embodied in the state apparatus, in the formulation of the law, in the various [political] hegemonies" (p. 93). When the practices and technologies of the federal government and the NRC are taken together, they produced SBR as a discourse with the kind of sovereign power that Foucault described. That effect is only increased when the practices of AERA are taken into account.

The Work of the American Educational Research Association

The connections among the federal government, the NRC, and AERA are also evident in AERA's practices. In this section, I explain how the discourse of SBR circulated in AERA. I further explain how AERA produced a variety of technologies—practices, committees, documents—that both took up and reproduced the discourse of SBR.

The AERA Council

In June of 2002, after NCLB became law and after the publication of *SRE*, discussions about SBR began to take place officially in the AERA Council, which is the legislative and policy body for AERA composed of the President, the President-Elect, the Immediate Past President, the Vice-Presidents of Divisions, the six At-Large Members, a Graduate Student Representative, a Special Interest Group Representative, and the Executive Director serving ex officio, without a vote. Gerald Sroufe, AERA Director of Government Relations and Senior Advisor, introduced the topic of the reauthorization of OERI and brought relevant issues to the table.

In January of 2003, the Council discussed the possibility of allocating a large number of sessions at the 2003 AERA Annual Meeting to the topic, "Evidence and Warrants in High Quality Educational research." The Council minutes noted that the proposal to allocate sessions to that topic was important to AERA but did not warrant devoting the time and resources to "facilitating and sustaining singular conversation throughout the Annual Meeting" (AERA, 2003, p. 40). However, Council member Bruce Thompson, Professor of Educational Psychology at Texas A&M University, pointed out that it was important for AERA to take and maintain a leadership role in the discussion about evidence-based research, and then-President-elect Hilda Borko, Professor of Educational Psychology at Stanford University, indicated that the "importance of examining the warrants of evidence-based research" (AERA, 2003, p. 41) would be reflected in the program of the annual meeting the following year. Although the proposal to develop a specific strand at the Annual Meeting devoted to evidence-based research was voted down, there was strong support for revisiting that idea after additional data had been collected. Nevertheless, discussion of evidence-based research and SBR dominated much of the talk at this

meeting.

In that same meeting in January 2003, Sroufe reported that AERA would be involved in writing the definition of scientifically based research to be used for research programs funded by IES. The Council then "devoted considerable time to discussion of the essential elements of scientifically based research," noting that it appreciated the Department of Education's "commitment to quality scientific knowledge" (AERA, 2003, p. 42). Although it was noted that Council members valued the emphasis on experimental design, Council members expressed concern that methods be appropriately connected to the theoretical frameworks used and the problems being studied. Additionally, some members of the Council were troubled by the fact that "the Department of Education seemed to be devaluing the strengths of non-experimental modes of inquiry in its emphasis on controlled experimental work" (AERA, 2003, p. 42). AERA President Robert Linn, professor of Educational Psychology at the University of Colorado at Boulder, appointed a committee to draft a resolution to address those concerns.

Linn, who served as AERA president when NRC's 2002 report was published, raised the idea that AERA needed to consider the essential elements of scientifically based research. Notably, Linn was also appointed by the NRC to oversee the review of both *SRE* and *ASRE*, so he was intimately involved in the process of shepherding those consensus reports through the peer review process to publication. According to a National Academy of Sciences document entitled, "Our Study Process: Ensuring Independent, Objective Advice," all reports produced by the National Academies "must undergo a rigorous, independent external review by experts whose comments are provided anonymously to the committee members" (n.d., p. 2) in order to ensure quality and objectivity. Further, reviewers "are asked to consider whether in their judgment the evidence and arguments presented are sound and the report is fully responsive to

the study charge, not whether they concur with the findings" ("Report Review", n.d., p. 2). Although AERA maintains that it operated independently according to its own interests and professional credibility, the activities of the NRC and the activities of AERA were connected because they had at least one common participant in Linn.

In June of 2003, Council members began to voice concerns that grant funding was increasingly unavailable for qualitative research because of SBR, especially from agencies like National Center of Education Statistics (NCES), which collects, analyzes, and distributed data related to education, and IES. From 2003 April to 2005 January, according to the AERA Council Minutes, the Council seemed to pay little, if any, attention to the growing debates surrounding SBR. However, in January of 2005, then-President Marilyn Cochran-Smith, Professor of Teacher Education for Urban Schools at Boston College, addressed the topic of *ASRE* with the Council and "indicated that she had asked the AERA Research Advisory Committee to review the [2005 NRC] report and offer relevant guidance to Council" (AERA, 2005a, p. 48). Executive Director Felice Levine distributed copies of the report to Council members and asked them to pay particular attention to the recommendations made by the authoring committee of *ASRE*.

In April of 2005, President Cochran-Smith charged a new task force, the Task Force on Reporting of Research Methods in AERA Publications, with "examin[ing] requirements for reporting the results of empirical research" (AERA, 2005b p. 38). By June of 2005, a calendar of meetings, as well as specific goals for the task force had been established. According to Sroufe (25 February 2011), this task force was organized to address a perceived lack of quality in research reports submitted to AERA publications. He explained that he did not remember specifically how the Council began the discussion about how to improve publications, but he did explain the general pattern of events that led to the formation of the task force: "the council has a
discussion about an issue that's raised with them either by the central office or by a council member, frequently by the president" (personal communication).

By January of 2006, the task force had produced the "Draft Standards for Reporting On Research Methods," and the discussion at the Council meeting on January 21, 2006 focused on the proposed standards. AERA Director Felice Levine carefully pointed out that "the Task Force was seeking to specify standards that would help to ensure that articles published in AERA journals are transparent and well warranted" (AERA, 2006, p. 54). Although Council members raised several topics for discussion and expressed concerns such as the omission of historical, theoretical, and philosophical work from the Draft Standards for Reporting on Research Methods, "overall, Council thought that it was important for the Association to have standards for reporting, as they are beneficial to authors, editors, and readers" (p. 54). A great deal of discussion during the January meeting concerned those "other domains of scholarship" that were not addressed in the Draft Standards, which resulted in a tentative decision to "develop complementary standards for domains of scholarship not covered" by the Task Force on Reporting of Research Methods (p. 55).

At the January 2006 meeting, several other issues related to scientific research in education were also addressed. Director Levine told the Council that the new Society for Research on Educational Effectiveness (SREE), founded by Mark Constas and Larry Hedges, would be announced publicly, and a website would be launched. SREE, as noted above, is the professional organization created by a \$750,000 IES grant to fill a perceived gap in practical research related to educational effectiveness that focuses on causal questions. Then Presidentelect Eva Baker, Distinguished Professor in the divisions of Psychological Studies in Education and Social Research Methodology at UCLA, announced that the theme of the following year's annual meeting would be "The World of Educational Quality," a marker that once again AERA was aware of and responsive to pressure to improve the quality of research in education.

Additionally, the Research Advisory Committee on Data Sharing, an AERA body tasked with advising the Council on the data sharing recommendations of *ASRE*, presented a report to the Council, which provided "an overview of activities undertaken by the [*ASRE*] Committee as well as specific recommendations on data sharing" (AERA, 2006, p. 53). The Research Advisory Committee further recommended that "the Council support, in principle, Recommendations 5, 6, and 7 on data sharing addressed by the NRC report" (p. 53), but instead the Council voted to form a task force to "look more carefully at the NRC recommendations, specifically with regard to data sharing" (p. 53).

AERA's Standards for Reporting Research

In 2006, *Educational Researcher* published AERA's "Standards for Reporting on Empirical Social Science Research in AERA Publications," directed at editors, authors, reviewers, and readers of AERA publications. The publication of the social science standards was the result of a lengthy period of deliberation by the task force that created them, and there was immediate objection to the standards from many AERA members because they seemed narrow and exclusionary, capitulating to the federal government's definitions of science as defined by SBR.

The Social Science Standards define empirical work both by the units of study—"sites, groups, participants, events, or other units"—and by the data collected—"participant and nonparticipant observations; unstructured or semi-structured interviews; documents and other artifacts; audio- or video-recordings; and standardized instruments like surveys or tests, structured interview protocols, and categorical demographic information that permit aggregation

of data across cases or units of analysis"—for the study (AERA, 2006, p. 35). Further, the Social Science Standards emphasized that research reports must be warranted and transparent, demonstrate adequate evidence, a clear line of inquiry, and a description of practices used in the conduct of research. Although those requirements do not seem problematic, the concern is that imposing standards on research will limit the possibilities for knowledge production.

Standards are not the issue *per se*; rather, standards become worrisome when they are created without regard to methodology or epistemology. In fact, the standards adopted by AERA are remarkably free of epistemological concerns, such as how epistemology produces methodology. As Foucault (1980b) wrote, "'Truth' is linked in a circular relation with systems of power which produce and sustain it, and to the effects of power which it induces and which extend it. A 'régime' of truth" (p .133). AERA, NRC, and the federal government are all systems of power within the discourse of SBR and, as a result, they are responsible for producing a particular truth about science for education.

The White Paper

The production of that truth continued when, in 2007, The Governing Board of the American Educational Research Association Grants Program (Grants Board) published a think tank white paper titled, "Estimating Causal Effects Using Experimental and Observational Designs," which was the result of a "think tank" meeting on causal inference held at the behest of National Science Foundation (NSF) and the National Center for Education Statistics (NCES), the funders of the Grants Board. The white paper stated,

[t]here is a general consensus in the educational research community on the need to increase the capacity of researchers to study educational problems scientifically. This report considers key issues involved in selecting research designs that allow investigators to draw valid causal inferences about treatment effects using large-scale observational datasets. It addresses why issues of establishing causal inference are of particular interest to educational researchers, provides a brief explanation of how causality is commonly defined in the literature, and describes some of the tools that analysts use to approximate randomized experiments with observational data. (Schneider, et al., 2007, p. 109)

The white paper equates causal research with scientific research, thus implying—with the federal government and the two NRC reports— that other kinds of research, such as interview research and historical research, are not scientific. It also overtly acknowledges its stance in the conversation, stating, "[g]overnment funding agencies in the United States and elsewhere are at a critical juncture as they seek to determine what types of research studies to fund in an era of declining resources" (Schneider, et al., 2007, p. 109). Although the authors do not exclusively define causal research as scientific, they privilege causal research as the only work that should influence policy, especially in the context of scarce resources. Further, the white paper claimed that the push to "test theories" and "examine rival explanations" (Schneider, et al., 2007, p. 112) was important, thus demonstrating its reliance on the positivist science privileged by the NRC and the IES.

The white paper refers to NCLB, explaining that its enactment, "in conjunction with other evidence-based movements internationally" (p. 116), cemented the importance of RCTs and explained that the federal government should fund studies that use RCTs. The authors of the white paper do cursorily mention that RCTs can "complement qualitative data on best practices such as interviews and classroom observations" (p. 117), but it is clear that they privilege studies that use RCTs.

AERA's Definition of SBR

In 2008, at the behest of Congressional aides who expressed an interest in including AERA in the processes of deliberation that would contribute to the reauthorization of the Elementary and Secondary Education Act, now known as NCLB, AERA published the following definition of SBR on its website:

I. The term "principles of scientific research" means the use of rigorous, systematic, and objective methodologies to obtain reliable and valid knowledge. Specifically, such research requires:

- A. development of a logical, evidence-based chain of reasoning;
- B. methods appropriate to the questions posed;
- C. observational or experimental designs and instruments that provide reliable and generalizable findings;
- D. data and analysis adequate to support findings;
- E. explication of procedures and results clearly and in detail, including specification of the population to which the findings can be generalized;
- F. adherence to professional norms of peer review;
- G. dissemination of findings to contribute to scientific knowledge; and
- H. access to data for reanalysis, replication, and the opportunity to build on findings.
- II. The examination of causal questions requires experimental designs using random assignment or quasi-experimental or other designs that substantially reduce plausible competing explanations for the obtained results. These include, but are not limited to, longitudinal designs, case control methods, statistical matching, or

time series analyses. This standard applies especially to studies evaluating the impacts of policies and programs on educational outcomes.

III. The term "scientifically based research" includes basic research, applied research, and evaluation research in which the rationale, design, and interpretation are developed in accordance with the scientific principles laid out above. The term applies to all mechanisms of federal research support, whether field-initiated or directed. (www.aera.net)

That definition does not differ significantly from those that had already been published in federal legislation and in the 2002 NRC report. Interestingly, AERA stated that the request from Congressional staffers for this definition "derived from an interest in averting the inconsistencies and at times narrowness of other SBR definitions used in legislation in recent years" (www.aera.net). The Council adopted the definition on July 11, 2008. Indeed, the definition was consistent with the *Standards for Reporting on Empirical Research in AERA Publications* that the Council had approved two years earlier, both in its emphasis on methodological consistency and in its exclusion of other ways of knowing and producing knowledge. AERA claimed to provide a more inclusive, less confusing definition that could be written into federal legislation, but instead, it once again preserved positivist science as science. In 2009, AERA published the Standards for Reporting on Humanities-Oriented Research in AERA Publications in its journal, *Educational Researcher*, cementing the distinction between empirical research and research in the humanities, and again creating an opposition between work that is science and work that is *interesting*, perhaps, but not scientific.

Here, I have provided an overview of the some of AERA's activities that demonstrate how AERA joined and extended the discourse of SBR in education. Whether in Council meetings, committees and task forces, standards, definitions, or annual meetings, AERA supported the necessity and truth of SBR. AERA responded to both the activities of the federal government and the NRC in taking up SBR and, consequently, became another institutional force in the regime of truth established around SBR.

The Resistance: Special Issues of Journals in Response to the NRC Reports

Despite ongoing, persistent critique, SBR remained strong. Much of that critique occurred in special issues of key academic journals in the field of educational research including *Educational Researcher 31*(8) in 2002, *Qualitative Inquiry 10*(1) in 2004, *Educational Theory 55*(3) in 2005, *Teachers College Record 107*(1) in 2005, *Educational Researcher 37*(9) in 2008, and *Educational Researcher 38*(6) in 2009 devoted to the topic. Those special issues constitute a reaction formation to SBR as a regime of truth. I pay particular attention to the special issue of *Educational Researcher* because it was the first special issue devoted to this topic and because it included comments not only from those who critique SBR but also from those who supported and helped produce it.

Educational Researcher

In 2002, *Educational Researcher (ER)* published a special issue about the 2002 National Research Council report. The editors at that time, Evelyn Jacob and Stephen White, wrote in their introduction to the issue that they had invited Michael Feuer, Executive Director of the Division of Behavioral, Social Science, and Education of the National Research Council in the National Academy of Sciences; Lisa Towne, the *SRE* study director; and Richard Shavelson, chair of the *SRE* committee, to write the lead paper for the special issue to present their interpretation of *SRE*. "To foster dialogue" (Jacob & White, 2002, p. 3), the editors also invited scholars from a range of approaches in educational research to comment on the paper and then

provided Feuer, Towne, and Shavelson an opportunity to respond to those commentaries. The issue consisted of the lead paper by Feuer, Towne, and Shavelson (2002), four commentaries by Erickson and Gutierrez (2002), Berliner (2002), St. Pierre (2002), and Pellegrino and Goldman (2002), and concluded with Feuer, Towne, and Shavelson's (2002) response to those commentaries.

The lead paper, "Scientific Culture and Educational Research" (Feuer, Towne, & Shavelson, 2002a), provided an interpretation of *SRE*. The authors asserted their concern that the use of SBR in federal legislation "inches dangerously toward a prescription of methods and a rigid definition of research quality" (p. 4). They also acknowledged the concerns of educational researchers that "the splendors of unfettered scholarship will be eroded by creeping tides of conformity and methodological zealotry" (p. 4), and they described themselves as being in the unique position to allay those fears by emphasizing their goal of diversity within educational research.

The authors of the four response papers complimented the 2002 NRC committee for its intention to be inclusive in its definition of SBR, but the authors of three of the four responses (Erickson & Gutierrez, Berliner, and St. Pierre) did not believe *SRE* had succeeded in doing so and was instead grounded in one particular epistemological approach to social science, positivism. Pellegrino and Goldman's (2002) response, on the other hand, was less critical of the report's findings and suggested that the recommendations and calls to action in the 2002 NRC report be taken further and serve as "a wakeup call to the field as a whole for how we address issues of quality, rigor, and community" (p.16). Alternatively, Berliner (2002) cautioned against attempts to define science and wrote, "it is not clear to me that science means the same thing to all of us who pay it homage, nor do I think that the distinctions between educational science and

other sciences have been well made in either report" (p.18). With Berliner, Erickson and Gutierrez (2002) explained that educational research differs from research in other fields because education is locally constructed and, consequently, definitions of scientific research must be appropriate to the field of education. They argued that the actual administration of treatments and the contexts of educational research could only be determined with qualitative methodologies. St. Pierre (2002) focused on the rejection of postmodernism in the 2002 NRC report. She wrote, "though the NRC report initially seems well intentioned and claims to resist the narrow view of science proposed by those in the federal government who would legislate educational research, it fails to achieve the inclusiveness its rhetoric promises" (p. 25). In their response to the four commentaries, Feuer, Towne, and Shavelson (2002b) reasserted their claim that they supported diversity. They explained, "we did not mean that all research, scholarship, and inquiry must be scientific, and indeed have noted many times the value of different forms of education scholarship" (p. 28). In that statement the authors actually belie their earlier claims to inclusiveness by assuming that their definition of science, the positivist definition described in SRE, is the only legitimate one and rejecting other conceptions of science as "different forms of educational scholarship." That is a common stance of positivist social science.

Other Special Issues and Response

In the years after the publication of *SRE*, *ASRE*, and the special issue of *ER*, the debate about SBR continued in special issues of other journals. For example, in 2004, *Qualitative Inquiry* (*QI*) published two special issues about the 2002 NRC report. Editors Lincoln and Cannella (2004) explained that "the authors in this issue use analyses of the NRC report to demonstrate how regimes of truth are being established that produce and are produced by a new methodological conservatism" (p. 6), and papers in the issue looked at the disciplinary power of

such regimes of truth. In 2005, *Teachers College Record (TCR)* and *Educational Theory (ET)* both published special issues on the topic of SBR and the 2002 NRC report. *TCR* continued to explore the limitations on research caused by narrowing the definition of science to positivism. Howe (2005), then editor of *ET*, argued that the push for scientific educational research is "retrograde" and renders "qualitative methods auxiliary and epistemologically second-rate" (p. 235). The other authors in that issue also grappled with various epistemological issues evident in the debate.

What the critiques pointed out, again and again, is that research questions and science itself are produced by epistemology, which determines methodology. Some questions and statements, and not others, are possible in a given epistemology, so limiting social science to positivism restricts knowledge production in educational research rather than strengthening it. Proponents of SBR attempted to install a positivist epistemology and methodology as the gold standard of educational research. The debates surrounding SBR continue to define what counts as science, produce institutions to circulate and enforce these definitions, produce subjects, only some of whom count as "scientists," and create practices that maintain positivism as the gold standard for quality educational research and imbue that description with the force of truth.

Given that Director Whitehurst of the IES and the 2002 NRC report both explicitly rejected postmodernism, it might be expected that postmodern researchers have been at the center of resistance to SBR. However, there has been consistent and persistent critique from researchers who use a variety of epistemologies and methodologies. For example, the aforementioned special issues of academic journals contained articles written by authors whose work is grounded in both critical and interpretive theories in addition to poststructuralism. Further, researchers from various content areas in education such as mathematics have also resisted the imposition of SBR on their work. For example, in 2008, the National Math Advisory Panel released a report that outlined how mathematics teaching and learning could be improved. In response to this report, *Educational Researcher* published a special issue in which both panel members and those who critiqued the panel's report discussed its implications. As Kelly (2008) wrote:

the significant implications of this report...include not only the policy recommendations and potential funding implications for mathematics education but also the Panel's adoption of a strict and narrow definition of "scientific evidence" and an almost exclusive endorsement of quantitative methods at the expense of qualitative approaches. (p. 561)
Kelly also referred to several other publications that critiqued the Panel's report (e.g., *The New York Times, The Washington Post, Teachers College Record,* and Canada's *Globe and Mail*), noting that the report had garnered both national and international attention. Interestingly, the critique of SBR in the 2008 *ER* issue about the math panel's report almost mirrors the critique of *SRE* in the 2002 *ER* issue.

Since its initial special issue on the topic in 2002, *Educational Researcher* has regularly published papers on issues related to SBR (e.g., Shavelson, Philips, Towne, & Feuer, 2003; Slavin, 2004; Olson, 2004; Chatterji, 2005; Seigel, 2006; Freeman, deMarrais, Preissle, Roulston & St. Pierre, 2007; Slavin, 2008; Howe, 2009; Tillman, 2009; Pearson & Hiebert, 2010; Luke, 2011), and several books have been published on the topic (e.g., Walters, Lareau, & Ranis (2008), *Educational research on Trial: Policy Reform and the Call for Scientific Rigor*; Baez & Boyles (2009), *The Politics of Inquiry: Educational research and the "Culture of Science*"; and Hyslop-Margison & Naseem (2010), *Scientism and Education: Empirical Research as Neo-liberal Ideology*.)

The conversations about SBR have continued and the effects of those conversations have appeared in a variety of places, for example, in the topics of educational research conferences, in the publication of books on the topic of SBR, and especially in the continuous flow of SBRrelated journal articles published in *Educational Researcher*, an AERA journal. Further, SBR has appeared in other institutional documents, such as the National Math Advisory Council Report, in which the definition of "scientific evidence" was narrow and positivist. SBR has insinuated itself further into educational research, which produced effects documented and reified by institutional forces.

Although critique was persistent, and although the resistance to SBR continued to produce the conditions of existence of that discourse, it did not shift the decisions of policymakers, institutions promoting SBR, or even educational researchers who were proponents of SBR. Instead, the federal government's intervention continued to support a narrow, positivist definition of science, which was taken up and reproduced by the NRC and AERA. Advocates of SBR continued to classify research grounded in other epistemologies and methodologies as lacking in quality and rigor.

Conclusion

Government, as Foucault described it, is not necessarily simply the governing body of the state. Rather, government is the "conduct of conduct" (Foucault, 1982, pp. 220-221). That is, it encompasses the control of self and control of others in a variety of contexts. In this chapter, I demonstrated how the federal government, the NRC, and AERA all serve as governing institutions for educational research because they implement and sanction laws, standards, and practices that control the conduct of conduct in educational research. In other words, by engaging in practices that produced official documents securing SBR as the truth about high quality

educational research, each organization created a documentary trail for SBR, endowing it with truth-value. The federal government, for example, defined science for education through legislation in response to the perception that educational research is of poor quality. The NRC reinforced the need for this definition by producing a report at the request of the U.S. Department of Education that defined what counted as scientific research in education. AERA validated those claims by crafting its own definition of SBR at the request of congressional staffers and then by creating standards that maintained a false distinction between scientific research in education and "other" work. Because their respective contributions overlapped and reinforced each other, the federal government, the NRC, and AERA formed "a chain or a system" (Foucault, 1976/1978, p. 92) that produced SBR as a regime of truth.

In this chapter, I followed the lead of scholars like Howe (2009), Hacking (1999), and Bové (1995) who suggested that the educational research community cease its attempt to define scientifically based research in education and instead ask questions such as, "how does [SBR] function? Where is it to be found? How does it get produced and regulated? What are its social effects?" (Bové, 1995, p. 54). Those questions enable an analysis that makes discourse visible and subject to critique. My work in this chapter addressed these questions by describing SBR's conditions of existence. Further, I explained how SBR has produced a regime of truth in the government of educational research. In the following chapter, I continue the critique of the discourse of SBR by explaining how the federal government, the National Research Council, and the American Educational Research Association, as significant institutions that perpetuated the discourse of SBR, served as sites of ruptures within that discourse.

CHAPTER SIX

Discontinuities

Introduction

In this chapter, I describe three discontinuities I identified in the discourse of SBR. In this study, I use the term discontinuity to describe a "moment that genuinely threatens to collapse the system" of SBR (Spivak, 1974, p. lxxv). In the case of SBR, when a rupture occurred that threatened the structure of SBR, its truth was reinforced instead of questioned. The ruptures in the discourse of SBR illustrate that it, like any discourse, is discontinuous and available to transformation at any time because of shifting power/knowledge relations. Highlighting such moments reveals that the discourse is not a secure structure but is contingent, unsteady, and unstable. Certain "truths" in the discourse of SBR seemed to fit its particular rationality but failed and were snags that could have unraveled it but didn't. The discontinuities reflect the consistent, ongoing failures of positivist social science since its inception in the 19th century. That many of the chief players in establishing the structure of SBR were trained in positivist social science no doubt helps to account for the fact that the discontinuities discussed here—very serious failures of reason-were glossed and ignored. However, those who critiqued SBR from other social sciences approaches, including interpretive, critical, and postmodern social science, recognized the discontinuities immediately. I have described their resistance that can be found in special issues of journals devoted to critique. The three discontinuities I identified and that I discuss here are (1) the privileging of RCTs in SBR, (2) the work of the What Works Clearinghouse, and (3) AERA's standards for reporting educational research in its journals.

Discontinuity: Privileging Randomized Controlled Trials

The discontinuity in the discourse of SBR addressed in this section is the assumption that a particular methodology, randomized control trials (RCT), which I have briefly discussed earlier, produces the best science. According to Screven, (2008), an RCT is,

an experimental design involving at least two groups of subjects, the control group and the experimental group (a.k.a. study group, or treatment group), between which the subjects are distributed by a strictly random process (i.e., one with no exceptions), and which are not further identified or distinguished by any common factor besides the application of the experimental treatment to the experimental group. (p. 11)

The assumption that RCTs produce the best social science constitutes a discontinuity because no research methodology can guarantee high quality, rigorous, scientific truth in the social world. In this section I question that assumption. I note that, to a great extent, early proponents of SBR in education simply copied positivist evidence based research (EBR) in medical research that used RCTs to predict, control, replicate, generalize, and scale up. I conclude by demonstrating how appeals to the discourse of the "common good" have served to support a reliance on RCTs in the public's perception of high quality science.

The federal government's involvement in education is nothing new, but until SBR, it had never mandated research method in federal law. In my interview with Howe (2008) about the government's intervention in educational research methodology, he commented, "this is kind of unprecedented, isn't it? I haven't looked into it, but I doubt they tell physicists what kind of methodology they should be using, or even physicians" (personal communication). Indeed, the federal mandate of methodology for educational research is unique to this field. The privileging of experimental design and RCT, in particular, in the SBR debates illustrates the lure of randomization and generalization, measures linked to objectivity and fact, that many contend ensure quality, rigor, and validity. However, RCT designs are thinkable only within positivism, a particular social science approach that fell out of favor decades ago because of the limited knowledge it can produce. In order for a study to meet the criteria of SBR, its findings must be generalizable (a marker of external validity) and replicable (other researchers must be able to reproduce the study and achieve the same findings). (NRC, 2002; Slavin, 2003). When I interviewed Slavin (28 February 2008), he pointed out that "NCLB ... placed a lot of reliance on the idea that you could define very carefully and very narrowly what was scientifically based research," but he explained that within NCLB and other legislation, such as The Reading Excellence Act of 1999, educational programs could be simply "based [emphasis added] on scientifically based research" (personal communication). He claimed that this distinction undercut the value of SBR, commenting, "[t]he net effect was to make no difference at all. There's never been a program on the face of the planet that you couldn't justify in some form as having some scientifically based research that kind of sort of sounds like it should support it" (personal communication). According to Slavin, this led to an even more simplified understanding of SBR. For example, in the Reading First program, if a curriculum used phonics, it was considered scientifically based simply because there have been a variety of experiments on the use of phonics. The purpose of defining scientifically based research is to ensure that interventions in education are based on scientific evidence and not the latest fad. However, the unintended consequence is that if an experimental study supports any part of an intervention, researchers can claim that the study is scientifically based. This example demonstrates that those who wrote SBR into federal law did not understand the research methodologies they mandated and relied on the RCT research design to produce high quality scientific findings. In the

following section, I explain how a key feature of RCTs, randomization, constitutes a discontinuity in the discourse of SBR because it is not only difficult to achieve, but it also does not produce the effects it promises.

Randomized (Control Trials)

As mentioned earlier, randomization is a critical element of RCT. Randomization refers to either random selection, when participants are chosen at random from a larger population, or random assignment, when participants are randomly assigned to either the treatment or control group in an experiment. At The Use of Scientifically Based Research in Education Working Group Conference (2002), several speakers touted the importance of randomization, criticizing other experimental research that did not have true random samples.

Randomization is also a prerequisite to generalizability, both of which are validity measures in experimental research. Generalizability refers to the extent to which the findings of a study can be applied to a larger population. On the surface, randomization appears to be a sensible practice because it is a key factor in whether a study is generalizable. However, generalizability is itself problematic because "what may be correctly generalized about human institutions and practices changes over time. A generalization that is now false could have been true at an earlier time and place" (Howe, 2004, p. 51).

Randomization can also be problematic because of context. In the "real world," groups of people are not random. People are organized and organize themselves by identity categories (such as race and class), interest, and other variables. Ironically, a truly random sample, particularly one large enough to warrant generalization, does not reflect what exists in real life and real human interaction because people organize themselves in ways that are not at all random. Because of that, the use of randomization raises questions of validity, in particular, whether findings from RCTs can be applicable outside the laboratory-like settings they require. Howe (2004) explained as follows:

Among the major drawbacks of randomized experiments are problems with external validity, including inconsistency in implementing interventions across contexts ("dispensing a curriculum" is not quite the same as "dispensing a pill"). There is a trade-off between internal and external validity: The more investigators restrict the population and the treatment to achieve internal validity, the less external validity the study will have. (p.45)

In this way, randomization weakens the possibility of accurate and useful generalizability. Researchers generalize from one population (the randomized one) to a larger population that can never be the same. Further, in order to have a truly random sample based on the factors that address education and achievement, many variables would have to be controlled that cannot be. "SBR can control for the easily measured factors but may not be able to successfully account for the constructs that further contribute to the uniqueness of students and achievement," (Franco, p. 3, 2007) such as how students exist at the intersections of identity categories such as race, class, gender, and so on. Consequently, SBR produces RCTs, with their reliance on randomization and generalizability, as a normalizing technology, which privileges statistical evidence and assumes that any educational anomaly can be corrected by an appeal to positivist science. Rabinow (1984) explained that normative technologies "are purportedly impartial techniques for dealing with dangerous social deviations" (p. 21). This kind of objectivity has been critiqued for years in educational research and in science, including the "hard" sciences (e.g., Haraway, 1991; Harding, 1991; Howe, 2003; Howe, 2004; Kuhn,

1962/1996). Further, the "objective' knowledges produced as a result of such inquisitions become part of a 'web of control' of the state bureaucracy" (Kenway, 1995, p. 135).

In addition to those concerns, RCTs "[provide] no fresh answers to long-standing criticisms of classical experimentalism's penchant to...oversell randomization, and to oversell the ability of randomized experiments to provide causal explanations" (Howe, 2004, p. 43). The methodology does not ensure research quality; it is simply a process for doing research within a certain social science approach, namely positivism. No methodology is inherently valid, rigorous, or reliable nor can it guarantee the truth.

For example, what many RCTs must do is "forgo random *selection* and make do with random *assignment*" (Howe, 2004, p. 46) because random sampling is much more difficult and time consuming. Researchers must then ask for volunteers for their studies. Random assignment creates challenges for researchers in generalizing to a larger population because "the resulting estimates [of the experiment], however unbiased, are thus restricted to a population of volunteers," (p. 46) and volunteers do not necessarily reflect the population at large. In this situation, the benefits of reducing bias by employing randomization are negated by the inability to reliably generalize results. A final problem with randomization is accounting for participants who drop out of a study for various reasons. In schools, this can result in policy decisions being based on faulty, or at least inconclusive, data.

In addition to the problems with randomization and generalizability is the intervention within the RCT. In an experiment on the effectiveness of a teaching strategy or program, a teacher must implement the intervention. Franco (2007) explained that in order for an RCT to work as designed, it must assume a standard intentionality of the teachers and the students and proceed as if the teachers of the control groups and treatment groups are administering materials in exactly the same way and that the materials are being received and put to use in the same way by the students. Obviously, this is impossible. Undoubtedly, however carefully a researcher controls for variables, variables will still exist. According to Franco (2007):

Student populations vary widely in their ethnicity, socio-economic status, and family backgrounds. These factors can be controlled for. The student motivation, the intentionality of students and teachers and the variety of administrative philosophies make it impossible to control for all variability. Many of these constructs cannot be measured or scaled as a data point; however, each can strongly influence educational achievement. (p. 3)

A person's mood, the events of the day, the weather, and other variables also impact teaching and learning, making education a field that resists research methodologies aimed at randomization, generalization, and predicting and controlling outcomes.

In conclusion, RCTs—grounded in a positivist social science— use one research method that is not only prohibitively expensive but also impossible to implement in schools. RCTs produce only one kind of knowledge, and if SBR limits knowledge to that produced by RCTs, then it excludes a broad range of knowledge produced by other research methods. Introductory research textbooks (e.g., Creswell, 2002; Frankel & Wallen, 2005; Glesne, 1999; Patton, 2002; Tuckman, 1994) explain that research methodology is determined by the questions that a researcher attempts to answer, and those questions dictate the methods that can be employed. But even that claim is insufficient because methodology is completely imbricated with epistemology and ontology. The discursive and material formation produced in that grid of intelligibility—e.g., positivist social science, interpretive social science, critical social science, postmodern social science—enables the questions we can think and ask. So it isn't just methodology that produces research questions but the entire structural formation produced by the combination of a particular epistemology, ontology, and methodology that produces our questions. A postmodern researcher is highly unlikely to ask the questions a positivist would ask. But SBR is so focused on method and methodology that it ignores epistemology and ontology, and that is its fatal flaw. RCTs are thinkable only in positivist social science, so every other kind of social science and the knowledges they produce are excluded from SBR.

The Medical Model

The use of the RCT as the "gold standard" of educational research and virtually the only methodology that can be scientific in the definition of SBR raises several concerns. Smith (2003) explained that this particular methodology was born out of evidence based medical research (EBR) where specific problems, interventions, and outcomes can be identified. In fact, the link between RCT and medical research is frequently cited by IES (e.g., 2007) as the proof of the rigor of RCTs. Determining the efficacy of a drug through clinical RCTs in which the "treatment" is standard is not the same as determining the effectiveness of the learning relationship in the classroom in which there is no "typical student."

Educators know that two children of the same age, gender, race, social class, and sexual orientation can never be equivalent learners, and so they can't be randomized into a control and an experimental group as an RCT would require. According to Riehl (2006), "in education, a significant portion of research simply cannot be conducted with randomized clinical trials" (p.25). Of course, RCTs provide only a limited kind of knowledge, certainly not all the knowledges needed to make informed decisions about the variety of questions educational research explores (e.g., descriptive questions, causal questions, explanatory questions). Further,

according to Eisenhart (2005), RCTs are only sometimes effective in addressing causal questions.

The invocation of medical research using RCTs as the model educational researchers should emulate was common in the SBR debates, but even medical researchers understand the limits of RCTs. Riehl (2006) explained, "in these discussions, examples for medical research are sometimes invoked as models to which educational research ought to aspire, the implication being that educational research fares poorly in comparison" (p. 24). Riehl completed a survey of medical research, including the medical equivalent of SBR, evidence-based medicine (EBM), which is controversial and under assault because it can produce only a certain kind of knowledge. Thus, the call in the SBR debates for educational researchers to emulate their colleagues in medicine is not based on sound science, and that false assumption constitutes a discontinuity. Practicing physicians, like practicing teachers, base much of their work on experience, intuition, and relationships with people that cannot be measured, quantified, predicted, or controlled.

The appeal to RCTs and positivist social science is an example of what Foucault (1982) called normative rationality. Rabinow (1984) explained that normative rationality is "an increasing appeal to statistical measures and judgments about what is normal and what is not" (p. 21) in order to discipline science. A positivist rationality that claims to be able to predict and control can become a disciplinary force over a population—in this case, educational researchers. Further, a belief that a RCT is the best research design is an effect of the "centralising powers which are linked to the institution and functioning of an organised scientific discourse" (Foucault, 1980a, p. 84). That is to say, the discourse of SBR and its function as a regime of power enable the invocation of science through a particular methodology.

Proponents of SBR in education seem only interested in the "evidence" that supports their ends despite the critiques of RCTs. For example, in medicine, RCTs are conducted for new medications before they are approved for public consumption. However, as Franco (2007) noted, after several years of longitudinal studies, those same medications are taken off the market when the initial findings are invalidated by studies of the long-term effects of the medication (e.g., Vioxx, ibuprofen, Tylenol). In addition, the results of studies using RCTs can conflict with the findings of studies using other research designs. Riehl (2006) wrote that, in medicine, conflicts among findings of studies with different research designs cannot be ignored; that is, RCTs cannot be presumed to produce the best findings. However, in education, this has not been the case for those who claim the supremacy of positivist social science.

Riehl (2006) explained that in medicine, as in any other field of research, "many different research designs are used ... Each of these methods is matched to the kind of research questions for which they are appropriate" (p. 24). There is no methodological elitism with the RCT as the gold standard in medical research; rather, the appropriate methodology is borne out of the research questions, which are determined by the epistemological stance of the researcher. Again, the RCT is not always the best or most cost-effective research design. In fact, in medical research, "the RCT typically comes at the end of the laborious, time-consuming, and expensive progression of research" and "it is the cumulative knowledge gained from this extensive research, not just the result of the last, large RCT, that lends weight to a finding of effectiveness for treatment" (p. 25). Even the Salk vaccine, whose discovery SBR advocates have been fond of citing, was not created solely through RCT. Erickson (2005) recounted the experience of speaking with a medical researcher working at the Salk Institute who claimed, "if knowledge development in polio research had had to depend only on conclusive findings from experiments,

research on polio would today consist mainly of studies of the treatment effects of the iron lung" (p. 9). Riehl proposed that "given this model for medicine, educational researchers ought to have similar opportunities to conduct extensive conceptual and exploratory research," (p. 25) that would undoubtedly use a variety of research designs. There are, of course, well-designed and useful experimental studies within medicine, and they have provided valuable information in the medical field. However, Riehl argued that a randomized trial does not guarantee sound research.

Additionally, the purpose of the RCT is often at odds with the intentions of educational researchers. Riehl (2006) wrote, "experiments cannot explain the complexity of a phenomenon associated with learning in specific cases and cannot provide useful guidelines for local action" (p. 25). Instead, the RCT is appropriate for producing estimates of the probabilities of certain outcomes for a population level. The field of medicine relies heavily on physicians to conduct anecdotal research day-to-day and adjust their practices accordingly. This implies a level of professional confidence that is not generally afforded to educators and educational researchers. If the results of research conducted using EBM do not make sense in terms of a doctor's experience, she has the professional leeway to choose an alternate course, one that is specific to her patients in context. Riehl posited that

one source of reluctance or resistance to evidence-based research in education is the suspicion, accurate or not, that it may be just another tool to deprofessionalize teaching and deemphasize the knowledge work that teaching requires. (p. 27)

Or as Lather (2004) said, it "is a way to manage quality issues by displacing professional judgment with promised effectiveness via the procedural production of evidence" (p. 20).

In conclusion, these examples demonstrate that RCTs are not always the best research design in education or in medicine. Additionally, RCTs are assumed to be an epistemologically

neutral method, determined in advance of the research, that will produce valid, scientific results. On the contrary, basing design choices on questions rather than the intersection of epistemology, ontology, and methodology is vital to producing quality research that is scientific. Therefore, the choice to use RCTs as the gold standard despite its problems demonstrates a contradiction in the discourse of SBR, a failure of it supposed rational structure. Another contradiction can be found in SBR's claim to be a force for the common good.

The Common Good

The discourse of SBR claims that leaving no child behind is a goal for the common good. But Foucault (1978/1991) believed that appeals to the common good generally reinforce sovereign power and that the "common good" "refers to a state of affairs where all the subjects without exception obey the laws, accomplish the tasks expected of them, practice the trade to which they are assigned, and respect the established order" (p. 95). In effect, in his work on governmentality, Foucault (1980a) noted that the notion of the common good reinforces the power structure in place—in this case, SBR. Further, as Foucault (1980a) warned, "it is surely necessary to question ourselves about our aspirations to the kind of power that is presumed to accompany such a science" (p. 84), one in service of the common good. For what reason would we deem some kinds of research to be science and others not-science? And who has to power to do so?

SBR was established under the auspices of improving education and asserted that all children can achieve at high levels. If people believed that SBR serves the common good, then those who sponsored it could garner both public and political support. Feuer (25 March 2008) discussed the perception issue: the rhetoric, perhaps justifiably, sets out very noble and grandiose goals ... we're going to get every kid in America achieving above average by the year 2014 ... on the plus side of that, it energizes people, captures the imagination, gives people a sense that we're responding to an urgency, we're gonna really do what we can, we're gonna come together, it's the next Manhattan Project and by 2014, every kid will be above average, above proficient ... and I don't think you should underestimate the importance of occasionally grand visionary rhetoric. You need that. And people need to feel like there's hope ... so you have some hope. (personal communication)

But deploying that rhetoric does not guarantee that the visions *will* be or even *can* be achieved. Feuer described the letdown when the grand narrative set forth begins to fail:

but then reality strikes, and you realize, well, wait a second ... even if we accelerate the rate of achievement, it will still take a hundred years to get everybody there, so what are we talking about? So that's the problem. And then what happens is if you relax it and say we didn't mean 2014, we meant in 2014 years? You're in trouble because then you have taken the wind out of the reform sails, and you run the risk that you're going to demoralize a lot of people who really care about it. ... You can essentially strip yourself of any hope by setting the hope too high (25 March 2008, personal communication).

The contradiction between visionary rhetoric and material effects highlights a problem with basing educational policy decisions on perception rather than research. The failure of the grand narrative that all children can achieve at a standard high level if they have instruction and curricula based on scientific research produces greater disappointment than if those goals had never been set. Consequently, highlighting the disconnect between the goals and the effects of the discourse of SBR unravels the narrative of the common good. The discourse of SBR and specifically the implementation of RCTs "'regulates' who will conduct the studies, who will receive resources, what kinds of data will be collected, and what evidence will be considered" as well as what evidence will be considered "useful in policy recommendations" (Lincoln & Cannella, 2004, p. 7). Lincoln & Canella (2004) explained that power in the debates about scientifically based research,

serves to "police" the boundaries of the acceptable and declares whose work will be considered legitimate and whose will not. Power is embedded within a regulatory technology that appropriates and excludes, a system of governmental order that creates an illusion that there are no boundaries, only the laws of universalist science that would be followed for the common good (pp. 7-8).

That description illustrates Foucault's (1978/1991) governmentality at work, predicated on the ideas of progress, the common good, and a better future, with language as an intentional tool. A move towards this common good would mean "exercising towards its inhabitants...a form of surveillance and control as attentive as that of the head of the family over his household and his goods" (Foucault, 1978/1991, p. 92). Further, this understanding of power and science accepts "the tenets of modernity—the notion of science as cumulative, progressive, and knowable" and the "idea that rigorous research is defined best by a knowable, definable, and replicable standard for knowledge" (Bloch, 2004, p. 102). Even those in favor of SBR would do well to ask whether the discourse of scientifically based research is worth the perpetuation of the invisible surveillance and disciplinary structures on which it depends. It is important to remember that the cohesion of a research community that SBR privileges is possible only through a consensus that necessarily excludes its Other. Specifically, privileging the randomized control trial excludes other methodologies, and one might ask whether such exclusion should be the goal of science.

However, in my interview with Feuer (25 March 2008), he contended that backing off from the insistence on RCTs would also be seen as a capitulation, giving in to desires to not be rigorous or hold educators and educational researchers accountable for their work. Although he conceded, "one does not have to do a randomized trial to do really good work" (personal communication) he also recognized that it is a public relations issue—a matter of perception. Feuer explained, "the fact that we said seven years ago that you have to do a randomized trial and now we're saying you don't should not be understood as an erosion of standards. It should be understood as a natural, healthy evolution towards new meaning of what that's all about" (personal communication). However, Feuer continued, "[i]f you set the standard up here, it's very hard to get it down." In other words, if powerful groups establish RCTs as the most rigorous, scientific method for conducting research, a shift away from that standard could be interpreted as accepting lower quality research rather than a rational correction of a prior irrationality. Continuing to adhere to that prior rationality instead of modifying one's position produces a discontinuity. That is to say, making decisions based on worries about perception instead of on harmful material effects produces a rupture in the discourse of SBR. SBR is supposed to improve educational research and practices, but it is doubtful that it has or can.

Because SBR has been in circulation for over a decade and has been repeated and reinforced through a variety of practices, it has become invisible for some, especially for those who were not present at its fractious beginning and now believe it is real, true, and uncontested. Consequently, even those who can identify problems with SBR are sometimes still produced by it. Proponents of SBR remain persistently attached to positivist social science, which discredits other ways of knowing. Feuer (25 March 2008), said "if all you're going to do is tell me you had a dream and this is the way to go, I'm going to have to look somewhere for someone to give me something more to go on" (personal communication). However, he was quick to add, "I don't need a randomized trial. I don't need an experiment. I would like the best available evidence that pushes me in the right direction" (personal communication). He provided an example of a question that he felt would necessitate an experimental study:

For certain questions, under certain circumstances, I will press for an experimental design. ... Let's say the proposal is to reduce class size from an average of 28 to an average of 22, and I'm being pressed to this by the teachers' union or by whoever. I say you know what, this is a fascinating proposal. I can see where you're coming from. I really kind of like the idea because 28 sounds like an awful lot of kids. What the hell do I know? I'm a congressman. I'm not teaching. So if I'm good about this, I will say, do me a favor, put this thing on hold for a year and get a little experiment going and give me something more to work with. (personal communication)

This description of how research should drive policy is encouraging. Moreover, the idea that the question should determine the methodology is refreshing within the discourse of SBR. However, it is telling that the first inclination is to return to experimentation and the discourse of SBR. The language of SBR has become so pervasive that its invocation seems natural. In the end, practices that were intended to serve the public good ultimately served only to perpetuate SBR. In conclusion, the discourse of SBR is not about the intentional deception of the public, but because of discontinuities—for example, the slavish use of RCTs despite disconfirming evidence about their efficacy—one of the effects of SBR has been to create a disconnect between the rhetoric of SBR and the goals of education. Attributing the return of positivist social science to any one person or institution is both impossible and undesirable. As Foucault (1976/1978) explained, " there is no power that is exercised without a series of aims and objectives," but he

added that "this does not mean that it results from a choice or decision of an individual subject" (p. 95). Of course, the fact that the discourse of SBR is so pervasive does not make it true. Rather, the discourse of SBR is an effect of power and an exercise of governmentality.

Just as legislation and institutional policy are products of the discourse of SBR, so are proponents of SBR. Even critiques of SBR and RCTs cannot happen from a place outside the discourse. Because SBR has proliferated throughout institutions, it has become pervasive and imbued with truth-value, especially in those institutions. Only certain thoughts, certain experiences, and certain subjects are possible within the discourse. Consequently, SBR is concurrently a disciplinary and productive space. It is disciplinary because it creates practices and standards that perpetuate its existence and organize spaces and subjects in accordance with normative rationality. It is productive because it produces truths, human subjects, policics, policy, and resistance.

As Rabinow (1984) explained, the "aim of disciplinary technology, whatever its institutional form" is to produce docile bodies that may be manipulated according to a given discourse (p. 17). In this case, the bodies are not only the physical human bodies Foucault described in *Discipline and Punish: The Birth of the Prison*—in this case, educational researchers, teachers whose practices must be based on SBR, school principals who change test scores, and so on—but are also bodies of literature, knowledge, institutional bodies, and so forth. "This is done in several related ways: through drills and training of the body, through standardization of actions over time, and through the control of space" (Rabinow, 1984, p. 17). The discourse of SBR advocates the idea that enough standardization of research will result in standardization of educational interventions, which will lead to standardization of teaching and learning and, eventually, increased student achievement. However, the desire to predict and

control the docile bodies in education has not been realized. Schools everywhere are labeled as failing, which has led the Obama administration to create loopholes and exceptions to the mandates in NCLB in order for schools to continue to operate.

Discontinuities in the discourse are more than just glitches in the system, snags in the fabric of a discourse once thought seamless. Rather, discontinuities are evidence that room exists to work against the structure. SBR is not inviolable truth; deconstructive moments such as the reliance on RCTs beg to be unraveled. Likewise, the What Works Clearinghouse presents another opportunity to chip away at the discourse of SBR.

Discontinuity: The Work of the What Works Clearinghouse

In the following sections, I explain how the work of the What Works Clearinghouse (WWC) constitute another discontinuity in the discourse of SBR because problems with its review methods, its privileging of RCT over other methodologies, and an overreliance on perception rather than science demonstrates that it operates on a false assumption—namely, that politics and perception are paramount to science. I will demonstrate in this section that there is a breakdown in SBR because instead of providing a database of scientifically based research of "what works" in education, the WWC produced almost nothing at all under the leadership of Grover J. Whitehurst, its first executive director, an educational psychologist who staunchly supported SBR and denigrated those who resisted it. Additionally, rather than taking critiques of its review process seriously, the WWC has ignored and attempted to erase these concerns. The contradictions in the workings of the WWC destabilize the truth of SBR.

The WWC was meant to be a repository of information based on scientifically based research about effective practice in education, but it has very little to show for its ten years of work and is now considered "a disaster" (Slavin, 28 February 2008, personal communication) by

one-time supporters. Created by the Institute of Education Sciences (IES), established by the reorganization of the Office of Educational Research and Improvement (OERI) as mandated in the Education Sciences Reform Act of 2002, the WWC was established to be the "trusted source of scientific evidence of what works in education" (para. 2). Since its inception in 2002, the WWC has reviewed hundreds of thousands of research reports, rated those reports, and stockpiled those that "work." All education stakeholders are interested in what works, so it was a public relations boon for the federal government to create an agency devoted to that end. However, from the beginning, was been skepticism about the success of this endeavor.

In our interview, Eisenhart (25 March 2008) thought a synthesis of educational research was valuable but doubted the focus on "what works" because "what works is always dependent on what the context is" (personal communication). She went on to explain that public perception plays a significant role in the assumed credibility of a venture:

So, it's a catchy title and people kind of intuitively think that's a good idea. We should know what works. But it's really what works, when, how, under what conditions ... I'm sure if it was called something like synthesis of research in education, there's no way it'd

get twenty-five million dollars or whatever it is they have. (personal communication)

The disconnect between the idea of this enterprise and its execution is stark and recognized both by its one-time proponents and long-time critics. As Eisenhart explained, "I think the idea of doing syntheses of research studies is a great idea. I think that we don't do enough of that in educational research. We don't work hard enough at trying to understand across studies what it is that we know and don't know" (personal communication). Taking stock of knowledge that has been produced and attempting to make inroads into areas that have not been well explored is important work. However, when synthesis occurs at the cost of integrity, it is difficult to argue that the result has been anything other than a multi-million dollar smoke and mirrors campaign.

Perception is perhaps the overarching discontinuity in the discourse of SBR because it is impetus both for implementing SBR and perpetuating it, yet perception does not hold up to the standards of evidence outlined by SBR. Consequently, because the discourse of SBR has succeeded in making it *look* as if the improvement of education based on scientific evidence is the goal, the fact that the policies and practices produced by SBR (e.g., standardized testing, teacher accountability, labeling and sanctioning schools) are failing becomes a secondary concern. It seems to be easier and more politically advantageous to continue down the same path than to reevaluate SBR and make different decisions for education.

The Review Process

The WWC itself was an effect of the discourse of SBR because its purpose was to be a repository of research findings that resulted from employing scientifically based research in education. As Schoenfeld (2006) explained,

WWC does not conduct research. It scans the extant literature in search of studies that meet its very stringent methodological criteria, examines those studies, and reports on their findings. Ultimately, WWC's goal is to conduct meta-analyses to determine the effects of educational and other interventions. (p. 13)

The WWC served as the proving ground for how SBR could be translated into policy and practice within a government institution and how SBR could produce concrete results that would improve education immediately.

The WWC locates, screens, and reviews studies according to the following process:

1. Develop a review protocol. Protocols define the scope of studies that will be reviewed, the process through which studies will be identified, and the outcomes that will be examined. Protocols also specify the time period during which relevant studies will have been conducted, the outcomes to be examined in the review, and keyword strategies for the literature search.

2. Identify relevant studies, often through a systematic search of the literature.

3. Screen studies for relevance and the adequacy of study design, implementation, and reporting.

4. Retrieve and summarize information on the intervention studied, the study characteristics, and the study findings.

5. Combine findings within studies and across studies when relevant. (WWC, 2011, p. 2) After selecting a topic, a review team is chosen, which includes a primary investigator, a deputy primary investigator, and a content expert, who are all approved by IES, as well as a project coordinator and reviewers¹¹. In order to be considered for review, studies have to conform to one of the three following designs: randomized control trials, comparison group quasiexperimental design, regression discontinuity design, and single-case research design. If a study did not follow one of those designs, it was automatically rejected because it could not conform to the definition of SBR put forth by WWC. Additionally, regression discontinuity design studies and single-case research design studies are only reviewed individually and do not contribute to the WWC's syntheses of evidence.

If a study has not been deemed ineligible because of its design, two reviewers are assigned to "document[s] the study design, outcomes, samples and attrition, and analysis methods" (WWC, 2011, p. 11). They then hold what is called a "reconciliation meeting" during

¹¹ See the WWC Standards and Procedures Handbook for a detailed explanation of each of these positions, as well as the review process.

which reviewers meet with a senior reviewer, and any concerns or inconsistencies between the reviews are sorted out. From beginning to end, the review process typically takes two weeks. After being reviewed, a research report is rated as meeting the standard, meeting the standard with reservations, or not meeting the standard. In general, "only well-designed and well-implemented randomized controlled trials (RCTs) are considered strong evidence, while quasi-experimental designs (QEDs) with equating may only meet standards with reservations; evidence standards for regression discontinuity and single-case designs are under development" (p. 11). However, there are various reasons that an RCT would not meet the standard, including, as Schoenfeld (2006) explained, having a difference in attrition rate between the control group and experimental group that was too large. If a study meets the WWC standards, the implication is that the intervention (often a curriculum measure) is effective and can be implemented with the guarantee of success.

Critiques of the Review Process

Initial skepticism about the project of the WWC grew as the implementation of its review processes became clear and research deemed relevant to the question of "what works" became increasingly narrow. Even those working within the WWC had reservations about its goals and procedures. For example, Schoenfeld (2006), former senior content advisor for the studies of mathematics curricula, explained that "[he] had some misgivings, because the WWC agenda is very narrow; [he] believed that many factors other than those on the WWC agenda should be taken into account when examining curricular effectiveness" (p. 13). However, because he also "felt a moral and intellectual responsibility to help make sure that things would be done well" (p. 14), he continued to participate in the project.

Schwandt (2005) claimed that skepticism arose because of the overreliance on RCT

studies:

Here, there is an implied privileging. It doesn't matter that you say you think other things if the actions, practices, and effects of your work are all supportive of the argument of a singular science that values causal effect research over all and privileges RCT as the design that gets you there. (p. 288)

Slavin (28 February 2008) concurred, stating that the WWC "decided that random assignment was the only thing that mattered. ... they decided that if something involved random assignment, then all other aspects of it, no matter how transparently foolish, were going to be ignored" (personal communication). Consequently, the process of selecting studies for review challenges the notion that the WWC values any research besides RCTs. Further, as explained above, ignoring the problems of RCTs in order to maintain the guise of science contradicts the assertion that the goal of SBR is using science to improve education.

Aside from criticism of the general review process, there were also problems with the results of the reviews. According to Schoenfeld (2006), the WWC might review a study that employed RCT to compare two curricula and report no significant differences between the two "because [the review] would be insensitive to significant differences in conceptual understanding and problem solving between the two treatments," and he emphasized that "curricula must be assessed according to all of the relevant criteria" (p. 17). Standardized testing, often responsible for providing evidence in studies reviewed, does not take those issues into account. Instead, the reviews of curriculum studies assumed that an RCT, by design, would result in the kind of scientific data are desired by the "what works" mentality.

However, the issues that Schoenfeld described are not the only problems scholars have identified in the WWC review model. Slavin (28 February 2008) described some of the other

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problems in an interview:

The key factors that they've been ignoring are first, duration, so that some of the studies in some of their reviews have been extremely brief. They've ignored sample size, and so some of the studies that they've emphasized have been extremely small. And they, perhaps most importantly, they've ignored measures that are of skills that are inherent to the treatment, you know skills not taught in the control group. And so, as a result, a lot of the studies that they've accepted have been studies that you can look at and in ten minutes, see that this has nothing to do with anything that would be useful for educators ... and it just so happens that a lot of the studies that are the very worst on those aspects, also used random assignment, and so you frequently have a single study of 46 kids that finds a huge effect size because the only measure used was one of something the experimental group was taught and the control group was not. And that it trumps the findings of everything else, of every other study that was of higher quality, used better measures, didn't have those problems. It doesn't matter because the random assignment study trumps all the rest. And over and over again, the findings are just inherently ridiculous. (personal communication)

It is unclear what kinds of knowledge result from a synthesis of studies that have not been evaluated according to relevant criteria or that rely so heavily on methodology that they ignore implementation, but it is at least questionable that this kind of synthesis results in any information about "what works." Or, as Schoenfeld (2006) commented, "[a] failure to conduct content analyses of the outcome measures used in comparative studies undermines the very purpose for which WWC was created" (p. 19). Rather than acknowledging and addressing concerns that undercut the usefulness and efficacy of the WWC as an agency, leaders within the organization chose to continue the narrative that it had produced an accumulation of knowledge about what works.

Unfortunately, what Schoenfeld found is that the WWC and IES were consistently more interested in promoting the *perception* that their work was scientific and valuable than enacting practices and policies that ensured such claims were valid and defensible. Consequently, in many ways, the WWC has become the poster child for all that is wrong with SBR. Plagued by accusations of uselessness, agenda pushing, and concealing results (see: Shoenfeld, 2006), the WWC refused to acknowledge problems with the review process, even when presented with the arguments on multiple occasions.

Schoenfeld first attempted to address his concerns with the WWC in an introductory chapter for the technical description of mathematics curriculum review. After that chapter was cut by the WWC before publication, Schoenfeld was invited to retool this paper to be published in a special issue of the electronic journal *Research in Middle Level Education*, along with several other papers, some of which were supportive of the project of the WWC and one which was critical. However, IES insisted on reviewing the papers and signing off on them before the issue was published. After registering his concerns that this "sign off" might be tantamount to censorship and being reassured that censorship was not the intent, IES made the decision to pull the special issue. When Schoenfeld once again presented his concerns and was met with no response, he chose to resign from his position at the WWC.

Schoenfeld (2006) chronicled his experiences with the WWC in an issue of *Educational Researcher*, where members of the WWC (including those mentioned in Schoenfeld's article) were given the opportunity to respond. In their article, the representatives of the WWC asserted that the flaws identified by Schoenfeld were in fact beyond the scope of work of the WWC and

therefore not a useful expenditure of time and resources. Further, they explained that the rules for authorship for contractors within a federal agency differ from those in an academic setting, and it is fully within the purview and responsibility of the IES to publish (or not) work as they see fit.

Schoenfeld responded to these supposed clarifications, noting that the authors addressed an issue that he had not, in fact, raised and in so doing, skirted a direct response to his claims. He reiterated that in a review of 20 years of research, the WWC determined that only 10 research reports met the standards for evidence set by the agency, and some of those reports were "seriously flawed" (p. 23). As to the issue of censorship, Schoenfeld rejected the notion that the decisions were made based on a difference between federally funded contract work and academic work, claiming instead that the WWC was complicit in concealing problems in the review process. He argued that "the issue here is the suppression of a report that challenges the scientific underpinnings of the current federal policy agenda" (p. 23), which undercuts the WWC contention that it exists in order to provide the best scientific evidence available. He also speculated that this concealment was part of a series of efforts to suppress scientific evidence that runs counter to the political ideology and rationale that governs the WWC.

Schoenfeld's experience with the WWC demonstrates that, as Best and Kellner (1991) explained, "discourse is power because the rules determining discourse enforce norms of what is rational, sane, or true, and to speak from outside these rules is to risk marginalization and exclusion" (p. 57). That Schoenfeld questioned the norms of the WWC, and by extension, the discourse of SBR, resulted in a marginalization of his work and his eventual resignation from the WWC. Further, Schoenfeld's experience illustrates the opposing work of discourse: "it reinforces [power], but it also undermines and exposes it" (Foucault, 1976/1978, p.101). The discourse of SBR produced a power structure within the WWC that allowed those in positions of

power to draw on SBR and assert its infallibility instead of acknowledging problems and attempting to do their work differently—a clear contradiction. Because the workings of the WWC represent a discontinuity in the discourse of SBR, its practices are also evidence of its failure because they document its contradictions.

Unfortunately for both science and education, the WWC has been a monumental failure according to many. Classified as "the worst thing that's happened to evidence-based reform in a very long time" (Slavin, 28 February 2008, personal communication), the WWC placed greater value on the perception of action and of rigor than on actually achieving those aims. Schwandt (2005) suggested that "IES is a bit overzealous in its rhetoric promoting the WWC as a 'trusted source of scientific evidence of what works in education" and proposed that "[i]t might more accurately claim that what it has established is the 'What We Currently Think Works Clearinghouse' that provides the evidence to date that we believe we can trust" (p. 292). Even this arguably generous view maintains that the WWC is not and cannot be what it claims to bea clearinghouse for the irrefutable truth of what works in education. Feuer (25 March 2008) questioned the standards that the WWC had set, and posited, "you could be pushing for such high standards of inquiry that you end up believing that nothing works" (personal communication). However, if that is the case, perhaps it would be more accurate to say that the standards are not too high but instead are invalid for what they intend to regulate. As Feuer commented, "this was methodological realism at its worst. You can raise the bar to the point where nothing passes" (personal communication). Consequently, analyzing the practices of the WWC threatens to collapse the system of SBR.

Still, the interest in "what works" in education prevails, in both the policy arena and the public sphere. However, this discontinuity demonstrates the impossibility of working towards

any one science, particularly one whose definition rests so strongly on a methodological mandate. As Schoenfeld (2006) said, "I believed when I signed up for my stint at WWC, and I still do, that properly conducted quantitative research has an important contribution to make, as one of many ways to explore the impact of educational interventions" (p. 20). Determining "what works" without regard to time, implementation, and other contextual information is impossible, especially in an agency where there is disagreement about what constitutes science.

Discontinuity: AERA and Two Standards

In this section, I make the case that by publishing two sets of standards for the reporting of research, the American Educational Research Association (AERA) produced another discontinuity in the discourse of SBR because the two standards unintentionally codified the separation of science and not-science. In order to do this, I describe some of critiques of the quality of education research that prompted AERA to write standards for reporting research in its journals. I then describe the Standards for Reporting on Empirical Social Science Research in AERA Publications and explain how they align with SBR. I also describe the Standards for Reporting on Humanities-based Research. Finally, I argue that the separation of the two sets of standards was an effect of practical concerns such as time and ease of completion rather than a description of two exclusive fields of knowledge. However, the unintended consequence of the separation was the perpetuation of the science/not-science binary produced through the discourse of SBR. This constitutes a discontinuity because although this separation *appears* to support the discourse of SBR that defines science in a particular way, the separation occurred for convenience rather than science. Consequently, rather than being true, the science/not-science binary produced by the two sets of standards is an "irresolvable contradiction" (Spivak, 1974, p. lxxv).

Perceptions of AERA

According to its website, AERA is "concerned with improving the educational process by encouraging scholarly inquiry related to education and evaluation and by promoting the dissemination and practical application of research results" (www.aera.net). It further claims to represent a "broad range of disciplines" in order to support its mission to "advance knowledge about education, to encourage scholarly inquiry related to education, and to promote the use of research to improve education and serve the public good" (www.aera.net).

Founded in 1916, AERA is considered the flagship organization for research in education. It produces a variety of academic journals that publish papers on educational research, theory, and practice (e.g., *Educational Researcher*, *American Educational Research Journal*, and *Educational Evaluation and Policy Analysis*). It also holds an annual meeting during which national and international educational researchers gather to present research and topics of interest in education. It could easily be said that AERA is a hub of knowledge, and therefore power, in educational research.

The diversity of methodology and theoretical frameworks represented by AERA members has been considered by some to be a liability and further evidence that educational research is lacking in rigor and quality. For example, each year following the annual meeting, Frederick M. Hess, currently Director of Education Policy Studies at the American Enterprise Institute for Public Policy Research, publishes a tongue-in-cheek review of session titles he deems unworthy of the time or attention of the educational research community. Using the titles of sessions, he writes a blanket dismissal of the work being done by those researchers, not allowing that there might be some value to students, teachers, and learning in their scholarship. He sets those sessions, which he describes as "promot[ing] narrow values" and "spout[ing] incomprehensible nonsense," in opposition to the serious and scientific work that is reported in sessions "analyzing public policy, improving teaching and learning, and addressing the practical concerns of parents and teachers" (Hess & LoGerfo, 2006, para. 16), as if the two categories were discrete and mutually exclusive.

His yearly indictment of AERA suggests that flashy titles and incomprehensible edujargon are valued while useful and important "scientific" work is relegated to hallway whispers and embarrassed presenters. On the contrary, AERA privileges research that is "scientific" and adheres to the standards set out by federal legislation and the NRC reports not only in its selection but also in its emphasis at the yearly meeting of AERA. However, the perception that AERA's annual meeting lacks substance reflects on the entire organization.

Social Science Standards: A Response to Perception

In response to this kind of critique and the critiques that research reports published in AERA journals "are methodologically weak," AERA attempted to "set some things out that if followed, would raise the general quality of our publications" (Sroufe, personal communication, 25 February 2011). In our interview, Sroufe explained the process of creating committees to explore various issues within the governing body of AERA:

Almost all of our activities follow the same general pattern, which is that the council has a discussion about an issue that's raised with them either by the central office here or by a council member, frequently by the president who in our governance structure is a very important person for one year at a time. ... Once a problem is identified that seems to merit some attention, the usual model is very similar to the national academy, and that is a task force or committee is formed to explore it and report back to the council, which ultimately must be the ones that endorse the document. So in all of our committees, there are very few exceptions to this, the purpose of the task force or the committee is to prepare a report for the approval of the council, but none of these task forces can approve their own report, and that's pretty much how we operate. So what happened was that the problem arose [that AERA journals are methodologically weak], and this [creating standards for publication] was deemed to be the right strategy for resolving or addressing

it, and then a committee was appointed by the president. (personal communication) Consequently, in 2006, *Educational Researcher* published the "Standards for Reporting on Empirical Social Science Research in AERA Publications," which were directed at editors, authors, reviewers, and readers of AERA publications and which emphasized that research reports must be warranted and transparent, demonstrating both adequate evidence, a clear line of inquiry, and a description of practices used in the conduct of research.

Although the requirements do not seem problematic, the concern is that imposing standards on research limits the possibilities for knowledge production. Standards are not the issue per se; rather, standards become worrisome when they are created without regard to methodology or epistemology. In fact, the standards are remarkably free of epistemological concerns. Additionally, in the first paragraph, the authors set up a binary between empirical social science research and other scholarship, which implicitly marginalizes that other scholarship.

Further, the social science standards devote time to the discussion of classification of data, or coding. The standards define classification as, "processes of segmenting data into units of analysis and categorizing or coding them" and set this technique apart from measurement, which is "the process by which behavior or observation is converted into quantities" (AERA, 2006, p. 36). However, not all qualitative research uses data collection and analysis methods that

can or should be converted into quantities and/or coded. Moreover, thematic analysis and coding generally only occur in research done within certain epistemologies. By omission, AERA proposes that research using writing as a method of inquiry, for example, would not meet standards for publication as social science. Indeed, any method of data collection or analysis that does not follow an already-determined and described procedure would not meet the standards. Consequently, similar to the NRC reports, the social science standards present a de facto narrowing and limitation on the possibilities for knowledge production.

In addition, the social science standards claim that "[a]n important aspect of reporting is to provide evidence that the outcomes and conclusions are warranted and that disconfirming evidence, counter-examples, or viable alternative interpretations have been appropriately considered" (*Educational Researcher*, 2006, p. 36). Poststructural research, in particular, is not interested in providing an alternative description of the world, so the exploration of alternative interpretations is not a part of the research process. Data collection in this framework is always already an interpretation. That is to say, each researcher will see something different when investigating a topic, and the act of observation changes what is being observed. As a result, there is no end to the interpretations that can be made or the conclusions that can be reached.

The social science standards also require, "[c]ritical examination of the preexisting perspective, point of view, or standpoint of the researcher(s), of how these might have influenced the collection and analysis of evidence, and of how they were challenged during the course of data collection and analysis" (*Educational Researcher*, 2006, p. 38). This language appears to assume that the researcher might be biased, which is a problem in positivist social science. However, post-positivist work acknowledges that science cannot be value-free. The idea that one can leave her values, attachments, experiences, and so on at the door before conducting research

is a concept that has been critiqued heavily in qualitative research for some time (e.g., Peshkin, 1988: Scheurich, 1995). Furthermore, examining what allegiances a researcher has and which she resists are already part of the methodological process of conducting poststructural research, which is largely excluded in other areas of the standards.

Finally, the standards require that researchers be able to provide audit trails so that another researcher could confirm their procedures of data collection and analysis, and their findings. The problems with replicability and generalizability have already been discussed at length above, but notably, AERA chose to maintain those concepts in the standards for social science research rather than acknowledge that much social science research does not and cannot be replicated and generalized. If there is no place for novel processes, then there is limited potential for new knowledge or for methodological variation. In partial response to these concerns, AERA attempted to account for "other" scholarship in an additional set of standards.

Humanities Standards: A Matter of Convenience

AERA published the Standards for Reporting on Humanities-based Research in 2009. As Howe (2009) observed, the very separation of social science research and humanities research "serves to reinforce the dichotomy between empirical social science and the humanities because the two sets of standards map onto the dichotomy" (p. 432). Indeed, the publication of two separate sets of standards for research, one for empirical research and another for "other scholarship," directly mirrors the positivist statements made in the *SRE* report of 2002. Howe went on to explain that this kind of distinction sets up experimental science as value-free and politically neutral, which is impossible. The AERA standards maintain that "humanities-oriented research in education has a long history and continues to play a unique and indispensable role," although it is not science and is assumed not to use empirical methods. Instead, humanities-oriented research undertakes investigations into the relationships among reason and emotion, the ethical life, the good life, the just society, the characteristics of the good citizen, and concepts of self, knowledge and its grounds, and the arts and their appreciation. Humanities-oriented research in education explores these issues within the specific domain of education, as in how reason and emotion are represented in school practices or what role education plays and ought to play in the formation of the citizenry.

(Educational Researcher, 2009, p. 482)

Statements like this and others, such as, "humanities-oriented research in education is often intended to foster dissonance and discomfort with conventional practice and, in some cases, to suggest alternatives" (*Educational Researcher*, 2009, p. 482), position this research differently than social science research described in the first set of standards. If humanities research is the domain of values and emotion, it implies that empirical social science research is not only value-free, but it is also atheoretical in that the relevant philosophical framework that produced the questions driving the research is not acknowledged or discussed in detail. Similar to the atheoretical discussion of science in the NRC reports, AERA maintains and perpetuates the idea that it is possible for empirical research to be conducted in a theory vacuum.

Despite all the talk of humanities-oriented research being the realm of values and even bias, the standards claim that "clarity is especially important in preparing manuscripts in the humanities-oriented tradition because words and ideas themselves—their political meanings, other contextual connotations, and their historical usage—are often central to the exploration of educational phenomena and often are used in specialized ways" (*Educational Researcher*, 2009, p. 485). Lather's (1996) warning about the "non-innocence" of clarity is relevant here. As Lather notes, the kind of clear writing that attempts to be accessible to the common man is "part of a discursive system, a network of power that has material effects" (1996, p. 528).

Interestingly, the focus on the effects of the standards and the separation of social science research and humanities-oriented research is quite different than what was originally intended by AERA. Sroufe (25 February 2011) made clear that AERA had intended to create one comprehensive set of standards that would cover all of the work done by educational researchers wishing to publish in AERA journals. He explained, "it was thought by the people on the first document, who worked on the first one, the social sciences, if you will, that in fact it did address, it would encompass most educational research" (personal communication). However, as Sroufe said,

[t]he moment of truth came when the Council was prepared to adopt the standards that had been written by this task force, and several members on the council said, you know this is a really good publication, but it doesn't relate at all to the kind of work that I do. (personal communication)

Those researchers whose work did not fall under the purview of the Social Science Standards came from a variety of backgrounds and theoretical frameworks, including those who did historical work, curriculum theory, and arts education. Consequently, the Council had to make a decision because the document "for all of its strengths, really wasn't broad enough to cover the field that is represented by AERA members or their work" (Sroufe, 25 February 2011, personal communication).

According to Sroufe, no one knew how best to resolve this quandary, but the Council felt the most pragmatic decision was to set up a committee to focus on the humanities traditions. What they found was that "the humanities turned out to not be a generic term that encompassed postmodernism, feminism, literary criticism" and so on (personal communication). He posited that if "the task had been limited to talking about standards for reporting in history and philosophy, it would have been kind of a piece of cake," but they attempted to include a variety of "other" forms of scholarship as well, such as arts-based research and postmodernism, and the committee "had a difficult time finding a common vocabulary that [they] could make progress with" (personal communication). Consequently, the new task force decided that their work could not parallel the work of the first task force, so once again, rather than appearing as equal documents, the two sets of standards established a clear separation between empirical scientific work and "other" scholarship.

Sroufe (25 February 2011) described the work of the humanities standards committee as both interesting and informative to the committee members, as they worked "perfectly valid postures towards educational research into a common framework called humanities" (personal communication). The conversations in their deliberations ranged from discussions about whether the first set of standards "was actually about social sciences and why they lay a claim to empiricism" to what form the new humanities standards should take. The humanities standards committee "thought ... that almost any observation is empirically based and should be considered empiricism" so there was a problem in separating the two standards, with one laying claim to empirical work (personal communication). However, they persisted in their task despite the concerns "and didn't spend a lot of time looking back at that from that point on" (personal communication).

The impetus for the AERA standards was not examined critically. Rather, it was accepted at face value that standards were necessary. When problems arose with the first set of standards, rather than attempting to reconcile those issues, the easy solution was to create another set of standards. Sroufe (25 February 2011) agreed, saying

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when the first set of standards was being developed, it was sort of the implicit assumption that everything could be put into that one basket, and I think that perhaps suggests the dominance of one point of view about educational research that was not critically assessed in setting up the charge to that committee. (Personal communication).

The development of two separate sets of standards creates a discontinuity in the history of SBR because practical concerns about time and resources, as well as the concern that creating one set of standards was impossible, took precedence over concerns about the effects of separating the two sets of standards.

Two Standards: Science and Not-Science

It does seem that the intent was to create two sets of publication standards that separated science and not-science for educational scholarship or to echo the federal government and the NRC in AERA's production of the "scientifically based research" and "interesting, but not science" binary. But this analysis is about effects of power, not intentions. AERA presidents, members of task forces and committees, and the AERA Council all "circulate" in the threads of power and "are always in the position of simultaneously undergoing and exercising this power" (Foucault, 1980a, p. 98). In this situation, as Foucault explained, "[p]ower ... is never localised here or there, never in anybody's hands, never appropriated as a commodity or piece of wealth. Power is employed and exercised through a net-like organization" (p. 98). Although the AERA president has a lot of control over the direction of activities for the AERA Council in a given year, there is little opportunity for a single-minded agenda to take over because the presidency changes every year. That is, the intentions, purposes, theoretical biases and so forth, of one individual do not govern the overall trajectory of AERA activities, particularly given that the

work of a task force or committee often lasts for more than a year before results or reports are presented to the Council.

In the case of the social science standards, AERA was pressured to do its part in improving the quality of educational research. Rather than resisting the idea that educational research was weak, AERA acquiesced by forming two task forces to create standards that would ostensibly solve the problem. Those task forces, much like the committees created by the NRC, were appointed, and each member of the committee was tasked with writing a portion of the report or standards. Committee members then meet to discuss and collaborate on writing the finished product.

Sroufe (25 February 2011) was hesitant to call the products consensus reports and made the following distinction:

I was trying to select the word collaborative rather than consensus because it wasn't so much that people talked about an issue until the rough edges got worn off and they found a way they could all go home as it was actually dealing with the issues and trying to look at them from different perspectives, and in that particular task force, at the conclusion, there was certainly no inclination on anyone I can tell that they hadn't come to the right conclusion from their various perspectives. (personal communication)

Sroufe further explained how language played into the development of the standards, even the decision to call them standards. He, and many AERA members thought that "they ought to be called guidelines," but there were two reasons that the term standard was used instead. First, AERA had already created a variety of standards—ethical standards, standards for educational psychological testing, and so forth—so there was agreement that using the term standards would create consistency with prior AERA activities and publications. Additionally, "the argument was,

and the Council agreed with this, that guidelines are just too wimpy to get anyone's attention" (personal communication). He conceded that because they are professional standards and are not enforced in any real way, the use of this language is intended to create the *idea* of power. Standards have teeth, while guidelines can be disregarded. However, as Foucault (1980b) wrote, "'Truth' is to be understood as a system of ordered procedures for the production, regulation, distribution, circulation, and operation of statements" (p. 133). Consequently, the standards governed what could be published in AERA journals, and the result is the constitution of truth about educational research.

Conclusion

The federal government, the NRC, and AERA, both overtly and tacitly, accepted the discourse of SBR and allowed it to function as true. Foucault (1980b) argued that "'Truth' is centred on the form of scientific discourse and the institutions which produce it" (pp. 131-132), so taking up the question of science in education is an inquiry into truth itself. Because "'Truth' is linked in a circular relation with systems of power which produce and sustain it, and to the effects of power which it induces and which extend it" (p. 133), I explored the effects of those systems of power through examples of discontinuities, and I argued that SBR is simply one possible description of truth for educational research. In Chapter Eight, I explore the implications of this research, and I propose "the possibility of constituting a new politics of truth" (p. 133) in educational research.

CHAPTER SEVEN

Afterthoughts

As to those for whom to work hard, to begin and begin again, to attempt and be mistaken, to go back and rework everything from top to bottom, and still find reason to hesitate from one step to the next—as to those, in short, for whom to work in the midst of uncertainty and apprehension is tantamount to failure, all I can say is that clearly we are not from the same planet.

~Foucault, 1984/1985, p. 7

Introduction

Scientifically based research (SBR), an effect of positivist social science, has, over the last ten years, insinuated itself into many aspects of educational research even though, as noted earlier, many proponents of SBR seem not to understand its philosophical grounding. What makes SBR worthy of study is that it is a regime of truth that has had real, material effects on educational practice, policy, and research and on people—students, classroom teachers, educational researchers, policy makers, and so on. These effects produce a particular kind of history of SBR, one that erases itself as it is written, making SBR seem self-evident, natural, and normal.

This dissertation has provided an analysis of SBR and some of its effects to demonstrate that the "truth" of SBR is simply *one alternative, one description* of educational research. The analysis undertaken here demonstrates how one description of SBR came to be seen as The Truth and also describes some of the discontinuities within the discourse. In other words, the dissertation uses the past to understand "some of the incoherence in present ideas. It cannot aim at exhausting the historical material, but rather at producing a hypothesis about the relationship between concepts in their historical sites" (Hacking, 1991, p. 184). To that end, I addressed the following research questions:

- 1. What were the enabling conditions that made SBR possible?
- 2. How was SBR maintained, regulated, and resisted?
- 3. How did SBR function as a regime of truth?
- 4. What discontinuities exist within the discourse of SBR that make it subject to critique?

This chapter works to summarize, restate, and extend the work of the dissertation, as well as to demonstrate how I have answered my research questions. However, my theoretical commitments suggest that there are no tidy answers to these questions, so the findings of the study can be found in every chapter. Further, because my object of knowledge is SBR, which implicates methodology and epistemology, *how* I have conducted my research (and through what epistemological frame) matters.

I begin with a summary of the impact of SBR and an explanation of the need for this kind of study—how this work fills a gap in the field. Then, I revisit the reasons I used Foucault's genealogy and governmentality to analyze SBR. Next, I summarize the findings of the dissertation across chapters and then discuss the implications of this work for educational research. I conclude by reiterating the usefulness of Foucauldian theories for analyzing scientific discourse.

Research on SBR

Although SBR as a discourse has been extremely effective and efficient in permeating education, research about SBR has been somewhat limited. A search in ProQuest Dissertations & Theses for "scientifically based research" within doctoral dissertations and masters theses published in the last ten years, or since the passage of the No Child Left Behind Act of 2002 (NCLB) and the publication of Scientific Research in Education (NRC, 2002), yielded 30 hits. Many of the search results mention SBR in a cursory fashion by explaining that NCLB requires SBR (e.g., Carter, 2011), touting the importance of SBR in various field (e.g., Camarena-Cano, 2010) or bemoaning the absence of SBR in a particular field (e.g., Marshall, 2006). In those studies, SBR has become normalized as the truth about high quality educational research.

Some of the studies I found focused on schools or individual teachers trying to contend with the mandates of NCLB and to "speak out against the powers that be" (Foucault, 1976/1978, p. 7). Whether a discussion of the implications of NCLB for rural schools (e.g., Deighan, 2009), urban schools (e.g., Lee, 2009), special education classrooms (e.g., Gaughan, 2008), ESL classrooms (e.g., Martin, 2009), or new teachers (e.g., Nelson, 2010), those studies demonstrate specific effects of NCLB on specific groups. That is, they take SBR as given, real, and normalized, as the truth about educational research—the imposition of a sovereign power that must be contended with and resisted—rather than simply a description of educational research that can be re-written. Another set of studies explored both teacher effectiveness when using curricula that were considered scientifically based and also teachers' knowledge of SBR programs (e.g., Griffieth, 2006). Still others explored the challenges of conducting SBR within public schools as sites of its practice (e.g., Clair, 2006). Once again, the studies treat SBR as true and real. I found only one study that focused on the history of SBR and how it was implemented through the What Works Clearinghouse (WWC) (e.g., Zoellner, 2010). However, this study focused on whether practitioners would use the WWC and how it could become more userfriendly rather than questioning SBR itself. I did find a dissertation informed by poststructural theories that explored SBR and its consequences for rural citizens (Eppley, 2007). This study

provided a critique of SBR but also reinforced SBR as truth because it did not place SBR itself in question.

All of the studies mentioned above, regardless of how they use SBR, serve to normalize it by treating it as the truth about educational research rather than a description. The Foucauldian genealogy used in this dissertation, in contrast, begins "from a question posed in the present" (Foucault, 1988c, p. 262). That question was, in short, how does SBR function as a regime of truth? This analysis has attempted "to account for the fact that it [SBR] is spoken about, to discover who does the speaking, the positions and viewpoints from which they speak, the institutions which prompt people to speak about it and which store and distribute the things that are said" (Foucault, 1976/1978, p. 11). That is, this study examines how SBR was spoken into discourse and the power relations that maintained the truth of that discourse. That researchers perpetuate the discourse of SBR by treating it as normal and true in their work exemplifies Foucault's agentive self-discipline. As Burchell (1991) wrote, "to govern individuals is to get them to act and to align their particular wills with ends imposed on them through constraining and facilitating models of possible actions" (p. 119). In other words, citizens are given only a certain range of behaviors they can exercise, but they interpret the freedom to exercise their limited behaviors within that range as liberty. What has happened since it first entered the discourses and material practices of educational research, policy, and practice is that, in too many instances, SBR has become normalized as an uncontested truth. Thus, those who resist it for various reasons too often work within its limited structure, accepting the structure as true, rather than questioning the truth of that structure, the discourse, itself. Following Foucault, Rajchman (1985) noted that what is required to free oneself from such structures is, "a constant 'civil disobedience' within our constituted experience'' (p. 6).

Rather than taking SBR as a problem to be dealt with, this dissertation makes SBR the question, the object of knowledge. What that means is that I do not think SBR is self-evident, true, or good. To the contrary, I destabilize its claims of being the truth about high quality educational research and illustrate that it is simply one available discourse among others that took hold at a particular historical moment in a particular power-knowledge relation. In this way, I challenge its "institutional régime for the production of truth" (Foucault, 1980b, p. 133).

Using Foucault

As Foucault (1976/1978) explained, "where there is power, there is resistance" (p. 95), and I argue that it is to those moments of resistance that educational researchers should attend. Foucault argued that most often, resistance is "mobile and transitory," is "distributed in irregular fashion," and takes a variety of forms within a "strategic field of power relations" (p. 96). That is to say, resistance does not exist outside of power but is instead an effect of power. Consequently, resistance, in its many forms, makes power visible and necessitates an analysis of the exercise of that power. In order to accomplish that kind of analysis, I used Foucault's theory of governmentality, with its focus on various kinds of state power, and genealogy, with its focus on disruptions, because both the circulation of power as well as resistance to power is evident in the discourse of SBR.

Genealogy

The purpose of Foucauldian historical analyses is not to show that a mistake was made at a particular moment in history but to show how a particular discourse came to be accepted as true. Burchell, Gordon, and Miller (1991) noted that genealogical work is useful because it aids "criticism [that] can be a real power for change, depriving some practices of their self-evidence, extending the bounds of the thinkable to permit the invention of others" (p. x). In this study, I used Foucauldian genealogy to explain the enabling conditions and conditions of existence for SBR as a discourse. I also identified discontinuities in the discourse that belie its status as truth without making judgments about intentions. Instead of simply re-presenting the events in the history of SBR, genealogy allowed me to produce a counter-history or counter-memory. A counter-memory, rather than being a negation or replacement of a history already-told, is an "affirmation of the peculiarities that attend any practice, and perhaps the activity that permits new practices to emerge" (Bouchard, 1977, p. 9). That is, a counter-memory provides a different account of an event or practice so that it might be understood in different ways and permit new knowledge to be produced. Like Foucault (1976/1978), my purpose "is not merely to construct counterarguments that are symmetrical and contrary" (p. 10) to the current way of speaking science and education, or to "show it to be mistaken" (p. 11), but instead to demonstrate how SBR is a discourse among other possible discourses and to define the regime of truth that "sustains the discourse" (p. 11) in educational research.

Governmentality

I used Foucault's theory of governmentality in this study to define SBR as a regime of truth. Governmentality provided a way to think of the institutional power of the federal government, the NRC, and AERA and how those institutions produced specific documents such as legislation and standards—and specific human subjects—such as proponents of SBR—that both perpetuated and administered the discourse of SBR. As Burchell, Gordon, and Miller (1991) explained:

government is not just a power needing to be tamed or an authority needing to be legitimized. It is an activity and an art which concerns all and which touches each. And it is an art that presupposes thought. The sense and object of governmental acts do not fall from the sky or emerge ready formed from social practice. They are things which have had to be—and which have been—invented. (p. x)

Governmentality also helped me identify SBR as an invention, a discourse that produced itself as reality instead of describing an existing reality. Governmentality and genealogy together provided a framework with which to examine SBR in terms of its power relations in order to trace how it became true. Below, I describe the knowledge I produced when I thought SBR with governmentality and genealogy.

Summary of "Findings"

Because the findings bled from one chapter to the next and were produced *in the writing* of this text, a simple summary is both difficult and unproductive. SBR, like any other discourse, is not the coherent story of an idea with a specific origin that proceeded in a rational, logical, and progressive way to its present status. Rather, it is a description that has become accepted as true. St. Pierre and Pillow (2000) pointed out that

Poststructuralism ... does not offer an alternative successor regime of truth, it does not claim to have 'gotten it right,' nor does it believe that such an emancipatory outcome is possible or even desirable. Rather it offers critiques and methods for examining the functions and effects of any structure or grid of regularity that we put into place, including those poststructuralism itself might create. (p. 6)

Consequently, the findings in this study are also contingent truths.

I return to the previous chapters to (re)present the findings in relation to the research questions that guided this study. In Chapter One, I introduced the dissertation study, as well as the broad theoretical framework that produced it—poststructuralism. In Chapter Two, I described Foucault's theories of genealogy and governmentality, which were the specific theories that framed the study. In Chapter Three, I described the documents and interviews that served as data sources for the study. I also explained key analytical concepts—power/knowledge and discontinuity—with which I analyzed data using writing as a method of inquiry.

Chapter Four answers the question: *What were the enabling conditions that made SBR possible*? I described the "origin" of SBR—the perception that educational research was of poor quality—and explained how that perception came about. I reviewed literature about the history of educational research, education as a discipline, and teaching as a profession. This literature demonstrates the perception that educational research is of poor quality, and, as a result, that education is failing society. I also reviewed literature on how positivist social science has been defined, including a discussion of scientism, the slavish devotion to the scientific method without regard to cultural and historical context. I then explained how the conversations about educational research in the academy, the ongoing debates about what constitutes science, and the deprofessionalization of education in general have all contributed to the perception that educational research is of poor quality. Those bodies of literature, when taken together, provide an explanation of the conditions that enabled the emergence of scientifically based research in education.

Chapter Five answers the question: *How was SBR maintained, regulated, and resisted?* By exploring how SBR was taken up, how it proliferated, and also how it was also resisted, I explained the conditions of existence for the discourse of SBR. I noted that in federal legislation the definition of scientifically based research included a gesture towards qualitative research that revealed a misunderstanding of what qualitative research is and does. Consequently, that definition of SBR was confused epistemologically, ontologicially, and methodologically and could not serve as foundation on which to ground high quality educational research. Chapter Five also answers the question: *How did SBR function as a regime of truth?* 1 argued that SBR has functioned as a regime of truth with the federal government, the National Research Council (NRC), and the American Educational Research Association (AERA) as important loci of activity. That is, the enabling conditions for SBR were present in that historical moment and consequently, the conduct of educational research was disciplined according to a positivist understanding of science. It goes without saying that the federal government, the National Research Council, and the American Educational Research Association all existed as institutions before the emergence of SBR. However, when each of those institutions joined the conversation, they began to legitimate the emerging description of SBR as the truth about high quality educational research. Each institution made decisions to speak science and education together to address a perceived deficit in educational research. The effects of this discourse—"a set of codes, practices, institutionalized arrangements and discursive processes that produce what comes to be taken for granted as knowledge, while, at the same time, providing the vehicles that render it true" (Silberstein, 2002, para 6)—came together to create a regime of truth.

The practices of government within the federal government, the NRC, and AERA created discontinuities in the discourse of SBR that demonstrate the power relations at play and their effects on the exercise of educational research, which I address in Chapter Six in order to answer the question: *What discontinuities exist within the discourse of SBR that make it subject to critique?* Making SBR visible as one discourse among others and not the truth in this Foucauldian analysis has implications in a variety of areas. In the following section, I describe implications for educational research, which continues to feel the effects of SBR.

Implications for Educational Research

Discourse (any discourse) produces thought in power/knowledge relationships. Or as Foucault (1984a) stated, genealogical analysis that focuses on a problematization like SBR is "the work of thought" (p. 390). Further, any attempt to organize that thought produces a structure—e.g., the structure of an argument, the structured definition of science, structural institutions—that deconstructs itself as it is enacted. The analysis provided in this dissertation suggests that researchers attend to the "structurality of the structure" (Derrida, 1978, p. 278). That is to say, researchers should endeavor to "see" the discourses that structure their lives and work. Because SBR was repeated until it had become invisible, the center of that discourse had become fixed, both reinforcing it as a structure and limiting the play within that structure. However, the identification of deconstructive moments—discontinuities—opens up spaces for play.

The analysis in this dissertation, the acknowledgement and deconstruction of SBR as a structure, frees educational researchers to think research differently. Foucault (1988a) saw it as his responsibility "to show people that they are much freer than they feel, that people accept as truth, as evidence, some themes which have been built up at a certain moment during history, and that this so-called evidence can be criticized and destroyed" (p. 10). Consequently, it is against such discourses as SBR that educational researchers must *think* because "[t]hought is freedom in relation to what one does" (Foucault, 1984a, p. 388). The kind of freedom offered by a structure such as SBR is not liberty because there is only a certain range of options for thought and action within a discourse. On the contrary, Foucault (1984b) claimed, "liberty is a *practice*" and it "must be exercised … it can never be inherent in the structure of things to guarantee freedom. The guarantee of freedom is freedom" (p. 245). If SBR is neither real nor true, then a

practice of freedom for educational researchers would be not to respond to it at all, either through resistance or capitulation.

After recognizing SBR as one of many possible descriptions, the next step for social scientists is to enact social science as they describe it—not necessarily to take up the dominant, normalized social science as if it's real and true and good. In fact, what has become conventional should always be critiqued. As Butler explained, the "failure of certain kinds of ideals is itself mobilizing, vitalizing, expansive, inaugurative of the new, productive of possibilities" (Butler, 1993, p. 7). That is not to say that some new form of social science can guarantee high quality science. Any claims to science produce another structure, another grid of intelligibility through which to make sense of the world. That structure, too, will fail if it has no play, if it is not malleable, if we are so dogmatic about its enactment that "we've forgotten we made it up" (St. Pierre, 2011, p. 613).

Rather, I argue that we might view the unraveling of the discourse of SBR as an invitation to produce a *non-innocent science*. As mentioned throughout this dissertation, SBR is grounded in positivist social science, but it either does not acknowledge that epistemological foundation or is unaware of it. Work that does not claim its allegiances—epistemological, ontological, methodological, and otherwise—represents itself as innocent. That is to say that work that claims to be atheoretical, value-free, and objective assumes itself to be harmless, blameless, and above suspicion because it is self-evident. On the contrary, social science that does not claim its allegiances is dangerous, just as everything is dangerous. Epistemology is never self-evident. And, as Foucault (1983) said, "if everything is dangerous, then we always have something to do" (pp. 231-232).

The non-innocent science I suggest we enact is an ontological move to *be* with research differently. It is not just in multiplicity that high quality science is found—more science and more kinds of science are good, but they are not the point—rather, it is *in the enactment* of social science that looks different from itself, claims its allegiances, and actively seeks out what we cannot yet think. This is an argument for the ongoing rupture of science until it becomes unrecognizable. Every enactment of research is an opportunity to rethink science. Freedom from SBR gives us permission to do research in unconventional ways so that we can "produce different knowledge and produce knowledge differently" (St. Pierre, 1997, p. 175).

Rather than the traditional move to produce findings relevant to a specific field or content area, I wonder what might happen if we began to ask, and take seriously the question, what kind of science are we producing when we do research? Research *findings* about language education or dropout rates or teacher retention are useful, but research *becomings* about what kind of science was produced in the doing of research is the "work of thought" to which Foucault referred. I follow St. Pierre's (2011) entreaty, "*Do tell me what you think you are thinking with when you think—what are your data*?" (p. 622) with a similar appeal: Tell me what your science looks like, that I might *be* with your study in a different way as well.

Conclusion

The question of what constitutes science has been a topic of debate for decades, and it continues today. Who gets to define science is one of the most significant questions in the SBR debates. However, in this dissertation, I am not interested in what science *is* because defining science in one way or another simply serves to produce it as a monolithic concept. The non-innocent science I describe above, conversely, proposes science as always already imbricated with philosophy, which means that science always looks different from itself because, as

Foucault noted, power and knowledge work together to produce the truth of something. This is not to say that the question of how science is defined and who defines it is not important. Lather (10 February 2010), for example, "spends a fair amount of [her] energy trying to take that on," and she claimed, "every generation needs to do that" (personal communication). However, because scholars like Lather are already taking up those questions, this dissertation focused on the effects of those definitions.

Epistemology became key in the SBR debates even though it was chiefly ignored in documents produced by the federal government, the NRC, and AERA. Howe (2003) wrote that "epistemological bias is more deep-seated, subtle, and unconscious than the kind of bias that characterizes, say, partisan political wrangling. These features complicate the task of discerning whether an epistemology is biased and so does the dialectical relationship between the normative and descriptive elements of epistemology" (p. 100). Those normative elements, implicated in the power/knowledge relations of the discourse of SBR, produced a regime of truth. In this dissertation, I investigated how the discourse of SBR became so pervasive that it functioned as truth—normal and natural to the extent that it was invisible and beyond question—and how its normalizing function constituted a regime of truth. I further explored the discontinuities in the discourse of SBR that made it visible as a construct.

Of course, in any epistemology, descriptions of concepts such as science become so ingrained in common parlance and practice that they are no longer questioned. They become impervious to critique because in their journey to becoming true, they became invisible. At that point, a discourse becomes dangerous. The purpose of genealogy is to track the history of how discourses become true. As Foucault noted, what is often found at the beginning of discourse is not intentional, rational deliberation but chance, accidents, petty politics, disagreements among factions, and so on. Thus, today's truthful knowledge is not based on a firm foundation but on contingency. Counter histories, counter discourses were and still are possible. We know that the truth of things changes over time as different power/knowledge formations emerge.

Discursive formations such as SBR appear to be but are not totalities. As Foucault (1980a) wrote, "the attempt to think in terms of a totality has in fact proved a hindrance to research" (p. 81), and his point holds true in the SBR debates. In this dissertation, I did not attempt to produce another truth of science—some would claim that the concept has been used so differently in so many different discourses that it has effectively been stripped of all meaning. Instead, I attempted to understand how SBR, a concept grounded in positivist social science, came to be the truth about high quality educational research. A central concern of those opposed to SBR is that it excludes any science that does not meet its definition—it exists only by exclusion. The project of poststructural research is not to produce an alternative, a successor science, but to "struggle against the coercion of a theoretical, unitary, formal and scientific discourse" (Foucault, p. 85) such as SBR.

Such struggle is assisted by an analysis of how power circulates through a variety of mechanisms. Foucault (1980a) explained that it is necessary to:

conduct an *ascending* analysis of power, starting, that is, from its infinitesimal mechanisms, which leave their own history, their own trajectory, their own techniques and tactics, and then see how these mechanisms of power have been—and continue to be—invested, colonised, utilised, involuted, transformed, displaced, extended, etc., by ever more general mechanisms and by forms of global domination. (p. 99)

The mechanisms analyzed in this study were documents produced by powerful institutions and subjects through which scientifically based research was cultivated and disseminated. Those

mechanisms burgeoned into still more institutions, into more subjects, and into additional practices that spread, making SBR truth, a truth that both normalized and disciplined a certain kind of science for the lofty purpose of improving education. But the imposition of a positivist social science on education has not produced the kind of revolution in quality that was promised.

A question always to be asked of such powerful discourses is how they came to be real and true—the question genealogy asks of whatever fictional truth we've created and put into play. On the one hand, SBR continues to hold sway among certain groups, particularly those deeply embedded in positivist social science. On the other, SBR never was accepted by counter discourses that recognized the problems always inherent in its epistemological allegiances. But we must be wary of our allegiances. As Butler reminds me, "[t]he key question of whether or not a position is right, coherent, or interesting is, in this case, less informative than why it is we come to occupy and defend the territory that we do, what it promises us, from what it promises to protect us" (Butler, 1995, pp. 127-128). In short, what Foucaltian theories have given me is a way to begin to think a non-innocent science, a research that "*produces* rather than *protects*" (Spivak, 1974, p. lxxv).

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