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The Relevance of Inequality Research in Sociology for Inequality Reduction

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Abstract

Social inequality is a central topic of research in the social sciences. Decades of research have deepened our understanding of the characteristics and causes of social inequality. At the same time, social inequality has markedly increased during the past 40 years, and progress on reducing poverty and improving the life chances of Americans in the bottom half of the distribution has been frustratingly slow. How useful has sociological research been to the task of reducing inequality? The authors analyze the stance taken by sociological research on the subject of reducing inequality. They identify an imbalance in the literature between the discipline's continual efforts to motivate the plausibility of large-scale change and its lesser efforts to identify feasible strategies of change either through social policy or by enhancing individual and local agency with the potential to cumulate into meaningful progress on inequality reduction.

Keywords

inequality, education, stratification, neighborhoods, parenting

Historically, Americans have been ambivalent about inequality of income, wealth, and social status and even of opportunity for educational and labor market success (McCall 2013). Liberals see inequality as a cause of poverty and power imbalance in society. Conservatives see inequality as a reflection of natural differences among people and the source of market-based incentives for productive effort that benefits society in general and the successful in particular. Americans have been ambivalent about the truth of these claims, and majorities side either with the conservative or the liberal perspective depending on the framing (Lindh and McCall 2017). However, inequality has clearly become more controversial as the level of inequality has grown. The growth of inequality and its potentially negative impact on opportunity for the poor, the near poor, and the middle class has raised two big questions for social science scholarship. First, why has inequality been rising? Second, what can be done to reduce inequality and improve opportunities for the poor and the working and middle class?

The issue of "doing something" about inequality has become an important question in policy and academic realms. In the past few years the Ford Foundation, the Russell Sage Foundation, and the William T. Grant Foundation (Gamoran 2013) have all launched initiatives on "reducing inequality" and call for new research that might lead to concrete knowledge-based strategies for achieving this goal. Not long ago, the MacArthur Foundation announced an every-three-year competition to award $100 million "to a single proposal designed to help solve a critical problem affecting people, places, or the planet." These recent institutional efforts to focus social science attention on the solving of major social problems raise the question of whether social science research provides usable guidance about how to reduce inequality. Such questions became even more pressing with the 2016 electoral victory of Donald Trump and the strengthening of conservative control over government at both the federal and state levels.

Sociological research in particular and social science research more generally have an ambivalent stance on this question. Scholarly writing across many decades has attributed this ambivalence to a preference by academics for "basic" as opposed to "applied" research that is rooted partly in perceived higher prestige for basic research and its stronger claims to scientific legitimacy. Partly, the preference is thought to arise from a professional penchant for the remoteness and feigned objectivity of the ivory tower. Partly, it stems from doubts about the utility of social science

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knowledge to address practical and political issues or social science caution about unintended consequences. In some scholars, it has triggered anxiety over whether a society structured around the views of social science experts might be more troubled than one in which tradition and established institutions play the central role.

This tension between the merits of pure versus applied research and between social science's mission as the achieving of understanding versus the improvement of society has existed since the founding of the social science disciplines. It can be seen, for example, in the work of Lynd (1939), Merton (1949), Shils (1980), and Barber (1988). Progressives such as Robert Lynd saw a 1930s world facing depression and fascism and an "inability to make its institutions work" and saw social science in a position to improve society if the knowledge of the multiple social science disciplines was integrated and deployed to solve social problems. Lynd argued that

> the responsibility [of social science] is to keep everlastingly challenging the present with the question: But what is it that we human beings want, and what things would have to be done, in what ways and in what sequence, in order to change the present so as to achieve it? (p. 240)

Another significant example of progressive sociology is exemplified by the work of W.E.B. Du Bois, who advocated for inequality scholarship that spurred social change and racial emancipation (Du Bois 1899, [1903] 2008). Conservatives such as Edward Shils, in contrast, saw the human condition as not perfectible and saw reform-oriented social scientists as uninformed about the true nature of society and as unaware or unable to acknowledge the limitations of their models for effecting social improvement. In his essay "The Calling of Sociology," Shils argued that "the true calling of sociology is to contribute to the self-understanding of society rather than to its manipulated improvement."[1]

Although the tug of war between liberal and conservative is intense in American society, it is not now a dominant aspect of the social science professions, with the partial exception of economics. For the most part the progressives have won in the academy, and the strong conservative voices that still emanate from parts of economics are more likely to be market-oriented libertarians than British Tories in the mold of Edward Shils. And yet the ambivalence toward reform-oriented research persists.

[1]"Much is being demanded of science which it cannot give. . . . It cannot guarantee that the poor shall become rich . . . it cannot resolve the ecological and social problems of our societies. . . . Those scientists who persist in placing a utilitarian interpretation of the value of science . . . place a burden on science which it cannot bear—and which it should not have to bear. . . . Scientists and scholars must learn the valid arguments for their often wonderful accomplishments in making the world and its inhabitants more deeply intelligible to us" (Shils 1976). See also Orlans (1996).

The historical tension between basic research and social policy research is not (or no longer) the fundamental reason for sociology's ambivalent stance toward inequality reduction. We instead make a different argument. The disconnection between much sociological research on inequality and the development of a sturdier body of knowledge on inequality reduction may stem at least in part from an implicit consensus that inequality reduction is too big a problem for social science research to address absent major changes in the social policy climate of the United States. Therefore, much sociological research on inequality implicitly or explicitly focuses its attention on providing a narrative frame that logically implies the social benefits of the political realignment needed to support an activist and progressive social policy climate. At the same time, the body of work provides an evidence-based critique of explanations for inequality as stemming from individual and group-based differences in ability, motivation, or culture or from the pernicious consequences of progressive social policies intended to reduce inequality as the major causes of inequality. We use the term *frame shifting* to refer to this strategy for research and exposition.

The implicit assumption of frame-shifting research is that social scientists can best achieve their policy goals by either demonstrating the validity of assumptions consistent with a favored policy or demonstrating that the foundational assumptions of competing narratives and policies are inconsistent with the empirical evidence. The intended audience for frame-shifting research—sometimes implicit and sometimes explicit—is partly policy makers but partly the general public. In this respect, frame-shifting research is (or at least would seem to have the intention of being) a form of "public" scholarship, but we use the term *frame shifting* in the narrower sense just defined. Large-scale frame-shifting research competes with descriptive and analytical research on inequality and its causes as well as with more focused policy-oriented research for the attention of the discipline. Perhaps surprisingly, relatively little sociological research makes the identification of *feasible* strategies for inequality reduction or that assesses the *feasibility* of inequality reduction by various plausible mechanisms a direct object of research, even though a strong argument can be made that feasibility research is necessary to bridge the gap between frame-shifting research and focused policy-oriented research.

We consider feasibility research to be an important bridge between research on the causes or extent of inequality and policy research. Feasibility research in our sense of the term identifies and illuminates social realities that either imply strategies for reducing inequality or that provide insight into behavioral or institutional reactions that might impede inequality reduction. Much contemporary policy research focuses on rigorous evaluation of treatments intended to achieve a policy objective and on assessing the cost of generalizing such a treatment. It is analogous to drug trials that use experimental methods to estimate the efficacy and safety

of newly designed drugs for which there are solid grounds to believe that they could be beneficial. These tasks are important. However, research that leads to evidence-based designs of treatments that are likely to be effective, or that assesses social, institutional, and financial challenges to the implementation of potential policies, is also important. Research that provides guidance for empowering individual actors to reduce inequality, whether through self-help, through collective activity in communities or voluntary associations, or through political engagement, is similarly important. These qualities of a research project constitute what we mean by feasibility research. It is a quality that research can possess to a greater or less extent, just as a research product can focus more or less strongly on description, explanation, frame shifting, or the evaluation of proposed policies.

Research on inequality is especially strong in description, explanation, and frame shifting. It makes a strong case for the desirability of inequality reduction, and it points to large-scale social transformations that might accomplish this objective, but it often neglects or insufficiently engages in the task of elucidating how this social transformation might occur. This work is necessary but does not exhaust the potential of scholarship to identify and specify strategies that could be refined through focused policy research. Rigorous feasibility research would not only point the way toward potentially valuable social policies but also would strengthen the legitimacy of sociological arguments for frame shifting. However, neither feasibility research nor more focused social policy research on inequality reduction receive as much attention in the major books and journals of the discipline as one might imagine given the strong consensus on the importance of inequality reduction.

This imbalance in favor of descriptive, explanatory, and frame-shifting research has multiple causes. Partly it derives from a sociological desire to "think big," which is accomplished by the best frame-shifting research, which provides new or at least clearer understandings of why the world is as it is. Partly it derives from a relatively greater detachment of sociology from the social policy community than is true for political science or economics. Partly it derives from a critical and historical suspicion of social policy that may make it easier for sociological papers or books to be published that debunk existing policy initiatives than for those that identify and study policies that work. Much of the imbalance, we would submit, stems from an almost implicit assumption that sociological research can have a more powerful influence on society by frame shifting than it can by researching alternative strategies for reducing inequality.

The underlying assumption in favor of frame-shifting research calls to mind the observation of Daniel Patrick Moynihan (2010) that "the central conservative truth is that it is culture, not politics, that determines the success of a society. The central liberal truth is that politics can change a culture and save it from itself." Expressed in the style of Moynihan's observation, a central sociological truth is that social science can change politics and thereby change culture as well as social structure. If big problems require big solutions that require political support (and often also political realignment), then frame shifting becomes a more powerful strategy than research that directly addresses how inequality and the problems it causes can be reduced.

The sociological instinct in favor of frame shifting about big issues at the expense of more modest strategies for reducing inequality may in fact be correct. Indeed, we affirm the value of frame shifting, which at its best is a careful and convincing interpretation of research findings. Our point is that whether this instinct is right or wrong, it is not subjected to rigorous scrutiny. Sociology articles (sometimes) and books (frequently) conclude with calls for large-scale policy changes that are consistent with a progressive vision of how society should be structured. The proposed solutions are generally consistent with the research findings, but the solutions themselves are not typically the object of the research. Partly this is because the solutions may seem premature in the absence of a political consensus that would be receptive to the types of large-scale solutions that are proposed. Partly it stems from the lack of data to conduct such studies. Partly it may stem from a belief that efforts at frame shifting are more consequential than are efforts at identifying feasible strategies to reduce inequality.

This article is an effort to examine the validity of this conjecture and its implications. The evidence we discuss focuses largely on the American context, and therefore we emphasize American sociological research. Similar questions could be raised about inequality research by sociologists in other national contexts, and indeed, these questions should be raised. However, they go beyond the scope of the present essay. We begin by examining the differences between frame-shifting research and research focused on societal reconstruction in the next section. We then assess how scientific ideas can be used to inform social policy by juxtaposing the social science and natural science disciplines. In the following section, we describe the long-standing dissensus among sociologists regarding how research should be used to address social problems. We then examine the recent research literature on the inequality of life chances to assess the extent to which this research points to concrete strategies for inequality reduction. We conclude by discussing the nature of feasibility research and call for sociologists to undertake more feasibility research to enhance the potential for frame-shifting research to produce useful strategies for reducing inequality.

Frame Shifting versus Rational Reconstruction

In the American intellectual community, perspectives on reducing inequality are deeply split. Those with left-leaning political perspectives see inequality as economically inefficient because of large amounts of rent capture by

high-earning individuals. These scholars think inequality reduction is just, because people in a rich society have a right to a minimum standard of living and because low-earning individuals were generally deprived of opportunity to increase their skills and incomes. They also see inequality reduction as efficient, because an increase in opportunity for the less well off would have important economic as well as social benefits for the country as a whole. Those with right-leaning political perspectives see inequality as largely efficient, because inequality motivates the most talented to work hard, and fair, because hard-working, talented people deserve their "just deserts" (Mankiw 2013). According to this conservative perspective, inequality reduction is both morally wrong and, beyond limited transfers for poor relief, inefficient.

There are important empirical questions that underly these opposing positions. To what extent are opportunities to obtain valuable skills unequal? To what extent does inequality motivate efficient human and physical capital investment? To what extent are inequalities in human capital investment a consequence of individual differences in motivation, choice, and effort as opposed to structural constraints? To what extent are high incomes a result of productivity and effort compared with inefficient rent seeking? Big questions such as these are relatively easy to state but not as easy to answer.

Answering questions such as these is one of the fundamental goals of sociological research, as it is for large parts of the other social sciences. To develop an understanding of whether and how inequality might be reduced, it is of course necessary to accurately describe inequality and, crucially, to understand its causes. We would argue that most research on inequality takes these objectives as the primary goals. As we discuss later, understanding the causes of inequality does not automatically imply the most effective and achievable paths forward to reduce inequality. At the same time, some of the theories noted above imply to some the infeasibility (e.g., because people have unequal talents and motivations) or undesirability (inefficient and unjust) of efforts to reduce inequality in a serious way. Descriptive and analytical research probes the empirical bases for these theories. When research directly debates and—when the evidence supports—rebuts these frames, and when scholars propose alternative frames for making sense of the body of empirical research, we call this analytical research "frame shifting."

Frame-shifting research points to the possibility and desirability of inequality reduction, but it does this largely through the quality of its investigation into description and causes and its debate with others who propose different descriptions and different causes. It is a necessary step on the path toward inequality reduction, in part through its efforts to reorient the political debate away from the position that inequality is the fault of individuals or the logical consequence of individual differences. However, we are surprised that social science research does not pay greater attention to how inequality in its multiple dimensions might be reduced, aside from strategies implementable via the tax system or as separate cash transfers connected with existing social welfare programs. Indeed, we would argue that frame-shifting research does not by itself typically provide sufficient research-based guidance as to the shape of these policies even were a decisive political realignment to be achieved. It often, at least implicitly, treats such questions as premature. In its focus on the possibility of change, frame-shifting research too often neglects to examine and therefore address potentially feasible pathways of change.

One does not have to agree in its details with the research program outlined in James S. Coleman's 1992 presidential address to the American Sociological Association (ASA) (Coleman 1993) to see the contrast it offers to an orientation based mainly on description, explanation, and frame shifting. Coleman's address focused on what he called the "rational reconstruction of society." Coleman argued that the societies of highly industrialized nations had been fundamentally transformed by political, social, economic, and technological forces and that these forces had weakened "primordial" social organization based on kinship. Sociology, he argued, needed to recognize that societies of the future will be "constructed," that the dominant actors of constructed societies ("corporate actors") are themselves constructed, and that "constructed" social organization is based on "rules, laws, supervision, formal incentives, and sanctions" instead of on the "social norms, status, reputation, and moral force" that controlled primordial social organization (p. 9). The change in the institutional foundations of society dictated that sociology (and by extension, other social sciences) recognize that its task "is not merely to describe and analyze the functioning of society" (p. 10) but also to engage in "the design of organizations, institutions, and social environments . . . intended to optimize relevant outcomes" (p. 14).

Coleman's presidential address occurred more than 25 years ago. If "optimizing relevant outcomes" in the context of inequality meant "reducing inequality" by one method or another, it begs three important questions. The first question is the extent to which 25 years of additional research on inequality—a core topic of sociological research if ever there was one—has further advanced the field beyond describing and analyzing to fully engage in the task of researching effective ways of reducing inequality that might actually be implemented in real societies. If the answer to the first question is "not as much as might be expected," then the second question is why has sociological research not moved in the direction called for more than a quarter century ago by Coleman, specifically whether lack of movement stems simply from the enormity of the problem and the strength of opposing political forces. The third question is whether the textbook distinction between "basic" and "applied" research has much to do with the extent of progress toward designing "organizations, institutions, and social environments"

(Coleman, 1993, p. 14) in a direction that would read to an achievable reduction in inequality in American society.

Concerning the second question, an obvious response is that the political environment of the United States has not been conducive to inequality reduction, and this response is largely correct as far as it goes. But we suspect that it does not go far enough. One might imagine the thought experiment of an election that realigns the country in a progressive direction. One might imagine that the new president assembles a council of policy advisers. She then asks them how best to accelerate the task of inequality reduction in a way that improves the country as a whole and that maintains political support for the policies that accomplish this goal. Imagine further that the new president stressed the importance of doing things right rather than just doing them quickly, and she cautioned about the dangers—socially, economically, and politically—of doing things wrong. We suspect that her scientific advisers would tell her that they can identify some policies with confidence that would help and that should be implemented quickly and that there are other potential policies that would benefit from further research—both at the level of evaluation research and at the level of what we have termed "feasibility research"—to establish how to (using Coleman's words) "optimize relevant outcomes." Over the past 25 years, we have made some progress toward these goals. Where progress has been slow, the lack of sufficient resources for research may play a role. With or without expansive resources, however, we suspect that research goals would be achieved more rapidly when scholars focused attention on what we do not know and would benefit from knowing before more specific policy research is brought to bear to sharpen the optimization.

The argument of this article applies to sociological research on inequality across multiple areas of the discipline. The breadth of inequality research in sociology—even American-focused research—is too great to be covered in a single article. Therefore, we focus on an important segment of inequality research, namely, the segment that focuses on the impact of parents, schools, and neighborhoods on the opportunity for mobility. There are other important topics in inequality research that could be the focus of scrutiny, for example, labor markets, or health or criminal justice, or immigration. However, we also have to set realistic scope conditions for this article. Our purpose is not to do a literature review but instead to analyze what we see as general tendencies in sociological research. We discern the tendencies we discuss below in these other areas of inequality research as well, but we leave the testing of this conjecture to others. Also, although our focus is primarily on sociology, it is important to contextualize sociological research within broader patterns of social science research that addresses these same issues, because these broader patterns provide important information for getting answers to the three questions we pose above.

Social and Natural Science versus Political Consensus

One might begin to answer the questions posed by Coleman's address in terms of the societal influence, or lack thereof, of social science in general and of sociology in particular. Scholars have studied the questions of whether and how much the formation of social policy is influenced by social science research, and the extent to which the influence of social science varies by national context (Campbell 2002; Campbell and Pedersen 2014; Prewitt, Schwandt, and Straf 2012; Weiss and Bucuvalas 1980).[2] To be very clear, these questions are not the topic for our article. Our interest is not in the consequences of social science research, including the reasons why it does not have more influence on the formation of social policy than some might expect. Our concern is with the type of sociological research that is conducted and ultimately published.[3] We focus on inequality research within sociology because of its importance as a social, intellectual, and moral issue, as well as the centrality of this question to the sociological discipline. We are interested in the type of inequality research that gets done, the conclusions it reaches, the implications authors draw from these conclusions, and the frame that they bring to this research concerning the nature of inequality, its causes, and possible solutions.

[2]Weiss and Bucuvalas (1980, p. 5) argued that the reason for what they called the "discontinuity" between social science research and the general neglect of social science in the formation and implementation of social policy was a question both complex and compelling enough to be the basis for a new specialty in sociology. They proposed "the sociology of knowledge application" as the name for this proposed specialty. Their call has, in a sense, been realized as the current ASA section on "sociological practice and public sociology," whose purpose (according to the section bylaws) is "to provide a forum for sociologists working in diverse applied settings . . . and to increase understanding of the relationship between sociological practice and sociological knowledge." What "sociological practice" means can be further discerned in the description of the William Foote Whyte Career Award, which is for individuals "who have made notable contributions to sociological practice which can include several of the following elements: outstanding clinical, applied or public sociological work, exceptional service to the section, publications that advance both the theory and methods of sociological practice or public sociology, or mentoring and training of students for careers in sociological practice or public sociology." "Sociological practice," "public sociology," and "the sociology of knowledge application" are related though certainly not identical, either as concepts or as institutionalized practices.

[3]In this respect, our research is closer in spirit to Marion Fourcade's (2009) efforts to understand how the national environment shapes a social science discipline—in her case, the discipline of economics—and, with specific reference to economics, how the combination of the national environment and the characteristics of economics shape moral perspectives on the market (Fourcade 2011; Fourcade and Healy 2007).

It is of course important to understand the extent to which ideas, including scientific ideas, affect public policy and evidence-influenced politics (Prewitt et al. 2012). Campbell (2002) argued that social policy is formulated as a function of particular paradigms for how the world works and for what the policies are intended to accomplish. Policy also reflects the self-interests of powerful actors engaged in policy formulation, legislative approval, and policy implementation. The frames supporting these policies may be relatively impermeable to even important facts that are discovered through social science research.[4] Frames may be controlled more substantially by normative beliefs than by scholarly consensus about facts and causal processes. Perceptions about the extent to which social science influences policy formulation are, as we will argue, a primary force that shapes the type of research that gets done. It is simplistic, however, to argue that a lack of policy influence discourages sociology from conducting policy-relevant research. Instead, we argue that paradigms and frames play a central role in the structure of research, and a primary purpose of this article is to make this connection explicit as a way of making the field more self-aware of the forces that shape its research output on inequality.

Although far from a perfect analogy, the example of climate research is instructive about the influence of political controversy on the structure of research. Climate change is in some respects an easier problem to address than social inequality, because an understanding of the main causes of global warming (a principal one being the release of carbon dioxide into the atmosphere from fossil fuel burning) provides a powerful strategy for a slowing of the trend (reduce the burning of fossil fuels). There is also a—perhaps naive—belief that strong scientific evidence about the causal effect of fossil fuel burning on global warming, sea level rise, extreme weather, and so forth, will either now or eventually strengthen the political will to confront the challenge of slowing and/or adapting to the warming trend.

One might see a further analogy between the global warming controversy and the controversy over the hypothesis that cigarette smoking increases the risk for lung cancer. This hypothesis similarly generated huge controversy in large part because both smokers and the tobacco industry had reasons to contest the claim. The resistance of the tobacco industry was greatly weakened by the mounting and ultimately irrefutable scientific evidence that smoking indeed causes lung cancer. It did not then take a complete understanding of the causal mechanisms linking smoking and cancer to generate cancer reducing policies and behavior. The lung cancer example is similar to climate change in that an identification of the major cause implied an obvious preventive strategy: modulate the cause to modulate the effect.

Inequality reduction is arguably a much harder problem. It is less obvious that understanding the causes of inequality in educational or occupational attainment, earnings, or risk for unemployment or poverty produces a straightforward strategy for modulating the consequence of these causes even as this understanding is a necessary condition for identifying strategies that would be effective (Brady 2019). Scholarly research on inequality could have an inequality-reducing effect through the discovery of social facts that point to feasible strategies implementable by individual, collective, corporate, or governmental actors (in the latter case, a social policy implementation) along the lines suggested by Coleman. However, the global warming and smoking analogies point to another strategy by which inequality research might produce inequality reduction, namely, by creating a sufficiently strong consensus around the ideas that inequality (or too much inequality) is bad in itself and produces undesirable social consequences. In other words, research that shifts the framing of the "inequality problem" to a scientific and popular consensus that current levels of inequality are "very bad for society" might have a bigger impact on inequality reduction than would research that probes more deeply into the mechanisms that produce inequality in order to identify new or better strategies to reduce inequality.

There are reasons to be cautious or even skeptical about the potential efficacy of the frame shifting strategy as a sufficient strategy for change. First, the case of global warming shows that an enormous body of scientific evidence all pointing in the same direction might fail to be persuasive. This is especially the case when politically powerful actors perceive a threat to their interests (tobacco companies in the lung cancer debate, the oil and gas industry in the case of global warming, the rich in the case of the debate about inequality).

Inequality is a harder problem than smoking or global warming because much of the political controversy about inequality shades into the question of whether poverty is caused by personal failings as opposed to structural conditions. Inequality is also a harder problem because the evidence for its negative effects on society is not as comprehensive and definitive as is the case for global warming or smoking. There is an at least ideologically powerful argument that inequality does not harm the material circumstances of the nonrich because of an hypothesized connection between inequality and economic growth. As with the controversy about global warming, many working- and middle-class people in the United States see their interests at risk from efforts at inequality reduction via tax policy in an effort

[4]Though not always: consider the connection between Ellwood's arguments about Aid to Families with Dependent Children (AFDC) and President Clinton's implementation of welfare reform. Clinton drew on Ellwood's research in his proposed reform of AFDC and appointed him an assistant secretary in the U.S. Department of Health and Human Services to play a role in developing what became Temporary Assistance for Needy Families, but the ultimate bill differed considerably from his proposal (Edin and Shaefer 2015; Ellwood 1989, 1996).

(that many of them would see as futile) to benefit low-income Americans.[5] These controversies fuel a widely held opinion that social scientists are partisans who underemphasize the role of personal responsibility, morality, and innate individual difference in their criticism of rising inequality. Skepticism about the possibility that social policies can reduce inequality is further fueled by the somewhat distorted collective memory of Lyndon Johnson's "War on Poverty" in the 1960s as a failure (Bailey and Danziger 2013).

Taken together, these considerations call into question the potential of frame-shifting research by itself to affect the political discourse about inequality in comparison—to take a notable example to the extent that Harrington's *The Other America* shifted the American conversation about poverty in the 1960s in a productive as well as progressive direction (Isserman 2001). To put it another way, frame-shifting research may (like an understanding of the causes of inequality) be another necessary condition for inequality reduction, but its potential for inequality reduction may itself depend on the existence of sufficient feasibility research to show that acceptable effective strategies for reducing inequality can be found and implemented. Feasibility research is the next logical step beyond research which describes inequality, that provides accurate and comprehensive explanations for its causes, or that critically rebuts narrative frames that would logically discourage efforts at inequality reduction rooted in misunderstandings or misinformation about inequality's causes.

Feasibility research in our sense of the term identifies and illuminates social realities that either imply strategies for reducing inequality or that provide insight into behavioral or institutional reactions that might impede inequality reduction. Although contemporary policy research focuses on evaluating the causal effects and the costs of implementing specific "treatments," feasibility research points the way toward designing treatments and other strategies that are likely to be effective. These treatments and strategies are then ripe for pilot implementation and rigorous evaluation as a prelude to implementation via government policy or adaptation by communities and other nongovernmental social groups. The successful design of treatments and strategies requires attention to causal mechanisms, because an understanding of feasible ways of reducing inequality requires an understanding of mechanisms that link existing social realities to inequality or that link possible interventions to a reduction in inequality. However, the focus is less on rigorous estimation of the causal effects of specific treatments than on the design of treatments and strategies that show promise and on the assessment of social, institutional, and financial challenges to the implementation of these treatments and strategies.

[5]Hochschild (2016) demonstrated the intensity with which many working- and middle-class Americans think that policies to reduce global warming will harm their economic interests.

One might see an analogy between feasibility research and policy research on the one hand and the design and testing of new drugs on the other. Feasibility research focuses more on addressing questions whose answers would identify or further substantiate the identification of promising interventions and strategies that can later be more rigorously evaluated with follow-up policy research. Feasibility research is analogous to the design of potentially valuable drugs that show enough promise to be taken into phase 1, phase 2, and phase 3 trials. Policy research that uses experiments or high-quality natural experiments to assess the causal effects of a given treatment is analogous to the drug trials that assess the causal effects of a candidate drug. Taken together, these two steps build on high-quality research about particular forms of inequality and their causes to identify promising strategies that may take the form of government policies or of strategies implemented by community and other organizations to reduce inequality in society in its multiple forms.

The distinctions we make between descriptive, explanatory, frame-shifting, feasibility, and policy research do not constitute a system of classification for research studies on inequality. We do not argue that every study belongs in one and only one of these conceptual bins. Rather, we see these concepts as a set of attributes that apply to a greater or lesser extent to the articles and books that report the outcomes of research or that explain to relevant publics what these outcomes mean. Individual studies can and often do have more than one of these attributes. Our argument is that the goal of reducing inequality would be advanced if research projects devote greater attention to bridging the gap between descriptive, explanatory, or frame-shifting research on one hand and policy research on the other. Feasibility research bridges knowledge gaps to facilitate the design of more effective interventions. These designs can then be subject to further research to evaluate their efficacy and cost.

Sociological Dissensus on How to Solve Social Problems

Sociology has had a complex relationship with the question of how social science research can and should be used to address social problems. This complexity arises in part from the history of ambivalence by many sociologists toward state-based solutions to social problems. The conflict over policy research was arguably more intense in the 1960s and 1970s, but it still influences the discipline and the stance toward the idea of "reducing inequality," even though (it is fair to say) the overwhelming majority of sociologists favor inequality reduction.

Sociology's ambivalence toward social policy has been one of the major forces that has produced a distinct institutional evolution for sociology compared with economics, which, like sociology, pays a great deal of attention to the issue of inequality (see House 2019 for a recent discussion of this point). Aside from intellectual differences, the

organizational structure of the two disciplines is at present very different. Sociology as a discipline is predominantly located in the setting of the university. Within the university, sociology is largely carried out within arts and sciences (A&S) departments of sociology. The most prestigious location for scholarship in economics is also in the university and arguably within A&S departments of economics. But a great deal of economics research also takes place within business schools, public policy schools, and other schools and departments. Indeed, in the typical research university, the number of economists outside the A&S economics department (in schools of business, law, public policy, and elsewhere) probably exceeds the number of economists with A&S appointments. Additionally, economists greatly outnumber sociologists in private policy organizations such as the Brookings Institution, the American Enterprise Institute for Public Policy Research, and the Urban Institute. This much greater anchoring of economics research within academic settings that focus on policy by itself gives a different orientation to the research of the two disciplines.

Even academic economics research gets criticized by researchers in policy institutes as being too out of touch to be useful (Campbell and Pedersen 2014). Nonetheless, we think it obvious that the institutional setting within which research is conducted has an impact on the type of research that gets done. In the case of these two disciplines, one could cogently argue that the differences in their institutional arrangement is a consequence as well as a cause of different orientations toward the relationship between social science and social policy and the relationship between social policy and social change.

This difference is readily illuminated via the illustration of opposing sociological perspectives from the 1970s and 1980s. During the 1980s, Bernard Barber (1988) edited a volume for the Russell Sage Foundation that provided a series of case studies on "effective social science." In Barber's view, the tension between social science and effective social science was expressible in the frame that we have already criticized as simplistic, namely, the distinction between "science as pure versus science as useful." According to this argument, the ultimate justification for science and the highest prestige was reserved for the production of scientific knowledge that led to greater understanding. "Applied" social science was seen by Barber as less prestigious, though he noted that Merton had recommended that sociologists should take applied social science seriously and study the ways it can be used effectively.[6] Barber saw the roadblock to be located more in the implementation process, in the priorities of decision makers in terms of their orientations toward policy goals, and the relevance of scientific knowledge in implementing these goals. Nonetheless, Barber's perspective in 1988 was that social science indeed does affect policy.

Other social scientists have noted the source of tension that arises from the question as to whether the state's interests are aligned with those of social scientists. Weiss and Bucuvalas (1980) wrote in 1980 that

> many social scientists consciously reject the role of "technician for the state" and choose to be social critics. . . . They do not look upon government as their natural audience but choose to stand outside and raise fundamental questions about the assumptions and operations of the current order. (p. 2)

A more critical perspective on social policy is illustrated in the writings of Robert Bellah (1982). In a 1982 essay, Bellah commented on the 1979–1980 annual report of the Social Science Research Council (SSRC) by Kenneth Prewitt, the former director of the National Opinion Research Center at the University of Chicago and future director of the U.S. Census Bureau, who was then in his first year as SSRC director. Bellah began his essay by quoting from Prewitt's opening of his SSRC annual report: "As the social sciences enter the 1980s, they move from a decade in which purpose or relevance became the dominant metaphor into a new decade characterized by the themes of performance, productivity, and usefulness." Bellah characterized these words "as expressive of the views of the majority of social scientists."

Bellah (1982) criticized this "majority" emphasis on "usefulness" for what he considered to be its implicit acceptance of the possibility of being manipulated by "the hands of those with political and economic power" (p. 32). He called instead for social science to be guided by what he called "practical reason" rather than "technological reason." In his view, the most powerful product of sociology is not the production of social technology but rather its ability "to discover through mutual discussion and reflection between free citizens the most appropriate ways, under present conditions, of living the ethically good life" (p. 36) with an emphasis on human interaction structured by norms and a shared conception of what a good society looks like. He contrasted this with what he called the "strategic" paradigm that explains human action "primarily in terms of the behavior of individual persons motivated by needs and drives, chief of which are self-interest and will to power" (p. 37) and that sees this paradigm as an essential element in the development of "technological" social science. In Bellah's view, many sociologists have been optimistic that "technological social science" could be implemented "on the basis of the normative paradigm," but he concluded that all efforts to do so have ended in "self-contradiction" (p. 37).

Bellah (1982) argued that the distinction between "practical" and "technological" social science does not correspond to a methodological division between qualitative and quantitative social science but rather to a tension over who the audience is for the products of social science. In his mind,

[6] "The social sciences have been preoccupied with the nature of science in general and with the particular ways in which they can call themselves scientific" (Barber 1988:2).

technological social science provides "useful applications" for government and management, whereas practical social science has the public as its chief audience because its goal is to "influence the climate of opinion" (p. 38).[7] Bellah concluded that "practical social scientists" "do not claim the degree of scientific precision to which technological social science aspires, partly because they do not think it is possible in the study of human affairs" (p. 39). They do not make claims of expertise but instead see themselves as the carriers of "prudence and judgment" even as they do not see themselves as "strikingly different from or superior to those they address, whether ordinary citizens or decision makers" (p. 39). In many ways, Bellah's essay anticipates the attention to "public sociology" epitomized by Michael Burawoy's 2004 ASA presidential address. Burawoy's (2005) address develops some of the same themes that were expressed in Bellah's essay, though Burawoy saw what he called "professional sociology" (which overlaps with technological social science in Bellah's formulation) as an integral part of the discipline that should live in "respect and synergy" with public sociology.[8]

Two more recent and somewhat parallel perspectives on the relationship between sociological research and the solving of social problems come from William J. Wilson and Herbert Gans. Their perspectives, although different from that of Bellah or Shils, also differ from that of Coleman in their emphasis on frame shifting as opposed to concrete guidance for social policy that flows directly from research findings. Gans (1997), in an essay on sociological "best sellers," argued that sociology's support from the public arises from "how informative that public finds sociology, and what uses it can make of the discipline's work." He operationalized his measure of support as book sales, even as he noted that many of these sales were to undergraduates for books required in their courses. Gans further argued that "books that try to understand and explain American society as a whole are also among the leaders" and that

> most of the books on the list are not empirical research reports, but of those that fall into this category, ethnographies outnumber depth-interview studies and surveys by a considerable margin. This is not surprising, since they are apt to be most readable, to emphasize narrative over abstractions, and to minimize quantitative analyses. Probably the book on the list with the most numbers is William J. Wilson's *Truly Disadvantaged*,[9] but more completely quantitative studies have no chance in this competition; also, most are published as articles, not books.

Gans ended his essay with the contention that if sociology could produce "more relevant findings and influential ideas about society," it might be able to attract more popular writers who could report sociology's work to the general public even as he cautions that the "informational" role of sociology is increasingly "being taken over by talented journalists."

Wilson's (2011a) arguments in a recent essay align with those of Gans in many respects. Wilson argued that a scholar's impact in the national public policy arena "depends on the attention his or her work receives outside of academia, especially in the media. And it goes without saying that a scholar's insights have to be compelling enough to interest the media and policy makers." Drawing explicitly from Gans (1997), Wilson (2011a) went on to note the impact of other books in terms of sales numbers. He agreed with Gans that work that is "too technical and impenetrable to a lay audience, no matter how creative . . . is unlikely to be discussed in the media" and therefore, in his eyes, is unlikely to be influential.

The irony of both Wilson's and Gans's view about the need for sociology to become less technical in order to communicate with the general public is that it belies the arguably ever growing influence of economics in American social science despite its highly technical content. U.S. policy analysts and advisers themselves frequently have a technical background, most commonly in economics, and do not find the work of economists to be impenetrable. The communication of technical social science findings in the national media is arguably becoming increasingly common, as perhaps best illustrated by the *New York Times* venture The Upshot. The Upshot was launched on April 22, 2014, to replace the popular statistical analyses of Nate Silver (who wrote the column FiveThirtyEight and now manages the Web site of the same name) under the editorship of David Leonhardt, who has a BS in applied mathematics and became a specialist in

[7]Supporting Bellah's argument that the orientation toward the proper use of social science was not defined by type of methodology is the observation by Herman van de Werfhorst (personal communication) that many of the leading quantitative researchers in the Netherlands were critical of the concept of *maakbarrheid* or "makeability," that is, of the idea that society is "makeable" and that its "makeability" should be a goal of social science. In this respect their position is the opposite of that expressed by Coleman.

[8]The two alternative frames of "why don't they use our science" and "why it is more important to be social critics than technicians in the service of the state" do not encompass all perspectives on the tension between social science and policy. Merton (1949:164) argued that what he referred to as "interpersonal and organizational" problems between researchers and their potential "clients" were only part of the problem, the other major issue being what he called "scientific problems," meaning specifically the "difficulty of developing scientific research adequate to the practical demands of the situation." But his focus was mainly on the difficulties of policy makers' successfully originating applied research that would illuminate the best course of action. He acknowledged that applied research can also be originated by social scientists, either to "sensitize" policy makers to "new types of achievable goals" or "to more effective means of reaching established goals" (p. 170), but he paid more attention to the issue of policy makers' initiation of applied research.

[9]Wilson's book has a few tables and graphs, but the tables and graphs are entirely descriptive, showing percentages, rates, ratios, and numbers (e.g., of jobs created).

economics reporting (in the column Economic Scene) for the *Times* in 2000. The Upshot is now edited by Amanda Cox, who has a BA in mathematics and economics and an MA in statistics. The Upshot is very data driven and gives considerable publicity to quantitatively driven research, albeit more detached from any single "public intellectual" author than is the case from the book-driven research of the authors on Gans's list. The Upshot and similar venues, such as FiveThirtyEight, Freakonomics, National Public Radio's series Hidden Brain and Planet Money, or Vox.com, offer extensive discussion of technical material in the media. One hears criticism from sociologists (e.g., in Author Meets Critics sessions at the ASA meetings) about sociology books as "inaccessible" because they contain technical material, even though their technical content is often much less than the technical material discussed in the *New York Times*, the business media, and the Web sites and podcasts mentioned earlier. The reason for this disconnect has to do with the way impact is being measured, whether in terms of book sales and readability by a nontechnical audience, as for Gans and Wilson, or in terms of the level of visibility from translators of technical material by *Times* reporters such as Leonhardt or Eduardo Porter or Gretchen Morgenson, or Planet Money reporters such as Adam Davidson or David Kestenbaum, or Radiolab reporters such as Robert Krulwich (now retired) and Jad Abumrad, or the policy analysts and publicists at the many American think tanks who have become so prominent in recent years.

We do not argue that Gans and Wilson are wrong to emphasize frame shifting. The question we raise concerns the extent to which "relevant findings" go beyond description, explanation and frame shifting to encompass what Weiss (1977) referred to as "enlightenment" research in a comparison she made between the "problem-solving or social-engineering model of research utilization" and the "enlightenment" model. In Weiss's view, the "enlightenment model" "sees a role for research as social criticism," "does not consider value consensus a prerequisite for useful research," but rather "thinks big," searches for new concepts and new generalizations, and thereby broadens the range of policy options. Both present-day frame-shifting research and the "enlightenment" research described by Weiss see social criticism as important. However, it is less clear to us how much effort is currently expended by frame-shifting research to develop a broader range of policy options that are tested by and emerge from the research itself. The emphasis is more on disrupting competing frames than on researching feasible solutions—in the sense of making the research on feasible solutions the central focus of the research—that could be implemented if the frame shifting succeeded in substantially shifting the direction of the public and political discourse.

A careful consideration of currently prominent proposals for reducing poverty and reducing inequality shows both that there are many good ideas under debate and that there is plenty of room for feasibility research on additional potentially effective strategies. The recent two-volume issue of *RSF: The Russell Sage Foundation Journal of the Social Sciences* edited by Berger, Cancian, and Magnuson (2018), "Anti-poverty Policy Initiatives for the United States," contains many good ideas, but the emphasis is almost entirely on tax- and cash-based welfare policies, including a universal child allowance, a minimum Social Security benefit for older Americans, a public guarantee of a minimum level of child support for children in one-parent households, an increase in Supplemental Nutrition Assistance Program benefits, a renter's tax credit, a savings component to the earned income tax credit, an increase in the minimum wage, a federal job guarantee via subsidized employment, and additional "race to the top" competitive grants for states to improve their community colleges via accountability based on the earnings of community college graduates. A related recent effort is a 2019 report by the National Academies of Sciences, Engineering, and Medicine, which proposed a roadmap for reducing child poverty (National Academies of Sciences, Engineering, and Medicine 2019). Eight of the 10 proposed policies involve cash transfers or vouchers. The 9th is a training and employment program. The 10th is the proposal that immigrants be given greater access to safety net programs. This emphasis on cash transfers is certainly not an implicit claim that social interventions will not work. Instead, it suggests a lack of clarity about what these social interventions might look like.

When social interventions are proposed, they often lack precision. To take a prominent recent example, the American Enterprise Institute and the Brookings Institution's Working Group on Poverty and Opportunity proposed a comprehensive strategy for reducing poverty in America (Aber et al., 2015). They proposed to strengthen families to better prepare children for success in school and at work via strategies such as promoting new cultural norms around marriage and parenthood and increasing investment in preschool and postsecondary education. Although these are important goals, they also raise questions that are potentially answerable with evidence-based research. How much is known about promoting and strengthening norms? Can we devise effective strategies for doing so? Are there practical ways of implementing these strategies? The Brookings working group does not answer these questions. These are important examples in which social policies would be more effective if the underlying social science foundation for the presuppositions and proposals were better understood. Much of this foundation would naturally come from feasibility research in sociology.

How Focused Is the Inequality of Life Chances Literature on Reducing Inequality?

As we noted above, inequality research in sociology covers a huge terrain. We focus here on the inequality of life chances, that is, the processes that cause children to have different

likelihoods to achieve success as adults. Decades of research confirm five law-like generalizations. First, although family background does not wholly determine adult position, the correlation between position of parents and position of their adult children is considerable, especially near the bottom and near the top of the income distribution. Second, education is the most important determinant of adult status, earnings, or chances of avoiding poverty. Third, family background has a strong effect on educational attainment. Fourth, inequalities by race, ethnicity, and gender exist in educational attainment, occupational attainment, earnings, and wealth net of family background. Fifth, geography matters. A growing body of research indicates that local or regional context has a large effect on life chances.

These fundamental causes of inequality in life chances differ from the fundamental causes of lung cancer or climate change in that knowledge of the causes provides insufficient guidance about how to reduce inequality. Making progress on this goal requires an understanding of the various environmental factors that affect the development of inequality from childhood into adolescence and then adulthood. In the following, we discuss recent research literature on neighborhoods, families, and schools in order to identify the role "inequality reduction" plays in sociological research on inequality in these domains. At the same time, other themes, such as race and ethnicity, are treated in their aspects that overlap with these three themes. We then address the distinction between distributional and relational inequality, how each type contributes to the other, the extent to which research has identified feasible strategies to reduce relational inequality, and the extent to which the reduction of relational inequality is a mechanism for reducing distributional inequality. To state our conclusions at the outset, we find that description, explanation, and frame shifting dominate the research landscape. The counter-examples provide important models for an alternative emphasis, but research on feasible strategies for reducing inequality is underrepresented.

Housing, Neighborhoods, and Neighborhood Effects Research

The literature on neighborhood effects has engaged intensively with the task of establishing the size of neighborhood effects and identifying the mechanisms behind neighborhood effects. It has also struggled with the implications of these findings for reducing inequality. We start with William Julius Wilson's (1987, 2011a, 2011b) research on concentrated poverty both because of its historical significance and because it is illustrative of the importance of both frame shifting and policy-relevant sociological research on this topic. Wilson's (1987) findings in *The Truly Disadvantaged* are an argument against the "culture of poverty" thesis, which was often read to imply that the chief problems for those in poverty are their moral shortcomings, and effective solutions to their poverty could come only from the disadvantaged individuals themselves. From the moral-shortcomings perspective, the most useful policy response would be to teach them to overcome these shortcomings (e.g., sexual abstinence to avoid teenage pregnancy). Wilson's research is also a counter-argument to the work of Murray (1984), who saw the "moral failings" of the poor as a rational response to the disincentives to work and the incentives to have children outside marriage that were built into the Aid to Families with Dependent Children program. Murray concluded that reducing social welfare benefits would weaken their "culture of poverty" and improve their economic situation.[10]

Wilson, in contrast, offered a "structural" explanation for the conditions of the urban poor, which centered the cause on labor market changes and on spatial concentration of impoverished communities and their distance from job opportunities. In Wilson's view and that of other sociologists such as Sampson (2009) and Edin and Kefalas (2005), the family instability that Murray blamed on social welfare programs emerged from these economic and spatial conditions. Wilson and others argued further that structural remedies were needed to solve the structurally induced problems of the urban poor. For Wilson, the core problems of poor neighborhoods stemmed from their lack of financial resources and their concentration of poverty, which produced spatial isolation. Solutions required macroeconomic programs, job creation, and policies to provide housing outside of concentrated poverty areas as well as universal and targeted social welfare benefits that provide child care as well as income support. Wilson saw the lack of income and the family instability produced by an inadequate job market as producing other negative social conditions in the neighborhood and the lives of the children who grew up there.

In reviewing the literature in the years since Wilson's book, Sampson, Morenoff, and Gannon-Rowley (2002) identified a set of four "social-interactional and institutional" mechanisms that determined the extent to which neighborhood context produced positive or negative effects on residents: (1) "the density of social ties"; (2) the "conditions of mutual trust and shared expectations" among residents; (3) the "quality, quantity, and diversity of institutions in the community" (e.g., libraries, schools, medical facilities, employment opportunities) (p. 457); and (4) the pattern of residential, commercial, road, and public transportation land use in the neighborhood. Sharkey and Faber (2014) identified the air, water, and noise quality in a neighborhood as another important mechanism. The health effects of pollution (including their impact on cognitive growth) have been extensively studied in the health literature. Harding et al. (2011) offered their own (overlapping) list of mechanisms, which adds violence and highlights the cultural challenges

[10]In later work, however, Murray argued that their potential for improvement was limited by what he asserted was their poor average genetic endowment (Herrnstein and Murray 1994; Murray 2009).

produced by the above conditions. Harding et al. also noted the mechanism of what they termed "local incentives," which

> focuses on individuals as agents responding to incentives provided by the local environment and shaped by their time and monetary resources. For example, living in a neighborhood in which many students drop out of high school might reduce the stigma of dropping out and thus increase one's preference for doing so. (p. 8)

This list of possible mechanisms is a major achievement of a large research effort by many scholars and reflects a growing number of methodologically rigorous studies that show negative consequences of living in neighborhoods with considerable poverty or violence on academic and behavioral outcomes of adolescents. Indeed, an identification of the mechanisms that may account for the major portion of neighborhood effects as in is Harding et al. is clearly an important step toward identifying feasible strategies for reducing the inequality-generating effects of spatial clustering by income, race, or ethnicity.

What do these results imply for the prospect of reducing inequality? One approach, epitomized by the Moving to Opportunity (MTO) study, addressed the consequences of moving low-income people out of poor neighborhoods. Early evaluations found surprisingly small positive and sometimes even negative effects of the MTO vouchers on a range of outcomes (Kling, Liebman, and Katz 2007; Ludwig et al. 2008). More recent evaluations found that the impact of residential moves on children depends on their age when the family moved, with those who moved before they were 13 years old earning more than 30 percent more than a control group by their mid-20s, while older children experienced zero or negative effects of the move (Chetty, Hendren, and Katz 2015). These results imply both that neighborhoods matter and that residential change is disruptive for children (see also DeLuca and Dayton 2009 for more evidence of positive effects of housing voucher programs).

An alternative approach to moving families out of poor neighborhoods is to improve the neighborhoods themselves, which is the approach Rob Sampson favors. Sampson's "structuralist" approach to neighborhoods has its roots in the work of earlier generations of urban sociologists (Park, Burgess, and McKenzie 1925; Shaw and McKay 1942). This approach differs from the emphasis of the MTO approach both in its interest in understanding community-level outcomes (such as rates of social behavior) rather than individual outcomes and in its focus on neighborhood-level interventions rather than on individual behavior.

Structural intervention, however, is challenging for at least two fundamental reasons. First, full structural revitalization of poor neighborhoods in the United States would require trillions of dollars in a "Marshall plan-like intervention which . . . is simply not politically or fiscally foreseeable" (Goering 2013:17). Second, the list of causes and mechanisms noted above that link neighborhoods to individual outcomes do not by themselves provide sufficient guidance to know exactly what structural investments to make, even if funding is available. Sampson wrote about "considerable investments by the city and institutions" (e.g., the University of Chicago) as having the potential to improve neighborhoods and offered examples of planned interventions "such as mixed-income housing redevelopment, tax-financed economic development zones, and community policing" (Sampson 2012). There is controversy over the benefits of such investments, however. DeLuca, Clampet-Lundquist, and Edin (2016) recently concluded that "unfortunately, we have little empirical evidence that place-based interventions generate significant gains for residents" (p. 188). In particular, Rosenblatt and DeLuca (2015) examined the consequences of the $100 million urban revitalization project during the 1990s in the Sandtown-Winchester community of Baltimore, which involved housing construction, education reform, and employment services. They concluded that the program raised rates of home ownership and reduced unemployment but had very limited impact on poverty rates or educational performance. Or consider the study by Wodtke and Parbst (2017), who attempted to determine how much of the neighborhood effect is indirect through the schools neighborhood children attend. Their study was not definitive given the inadequacies of data and the modeling challenges, but it is notable that they found that very little of the neighborhood effect is mediated by schools, which begs the question of what it is about the neighborhood that matters.

Sampson's research also paid attention to neighborhood interventions that do not necessarily require large-scale government financial investments, including network-based AIDS interventions, community policing, and, most especially, the mobilizing of collective efficacy. Sampson, Raudenbush, and Earls (1997) introduced the theory of collective efficacy in their study of cross-sectional data on Chicago neighborhoods. In that study, they developed a measure of collective efficacy on the basis of perceptions of shared values and trust in the neighborhood and the perceived likelihood of social control interventions by neighbors under a variety of scenarios. They found their measure of collective efficacy to be highly correlated with structural neighborhood characteristics (concentrated disadvantage, immigrant concentration, and neighborhood stability) and also with crime and victimization rates. They also conjectured that collective efficacy mediated the relationship between structural disadvantage and neighborhood violence. Sampson (2012:402–403) identified select low-income and black communities in Chicago (i.e., Oakland and Riverdale) as well as high-income and white communities that increased their level of collective efficacy over his research period without major public investment.

The appeal of collective efficacy theory is readily visible in the symposium on Sampson's book that appeared in the journal *City & Community* in March 2013. Every article in

the symposium (Denton 2013; Fischer 2013; Goering 2013; Hagan and Rymond-Richmond 2013) discussed it. Hagan and Rymond-Richmond (2013) called it "the major theoretical and empirical innovation of Sampson's Chicago Project" (p. 22). The hypothesized importance of collective efficacy has strong theoretical appeal in sociology for reasons that are parallel to the theoretical appeal of rational expectations, utility maximization, and efficiency in economics; these theories fit well with core axioms of their disciplines about how the social world works. So it is instructive to examine the subsequent issues of *City & Community* to assess the influence of the theory of collective efficacy on research.

Across the next 29 issues of this journal published since the symposium (nearly 200 published articles), one finds many articles documenting aspects of urban inequality. However, relatively few articles come closer to a focus on mechanisms that might improve neighborhoods where the disadvantaged live (i.e., what we call feasibility research). Positive examples include Olvera (2015), who addressed the structural conditions that favor the growth of racial minority organizations, such as the rise in neighborhood home ownership, though her research did not link this process to the neighborhood factors conjectured by Sampson to improve neighborhoods and thereby affect community outcomes. In another example, Hein (2014) used the collective efficacy framework to study the frequency of civic engagement actions and their connection to local organizations by Hmong Americans living in the Minneapolis–St. Paul metropolitan area. A third contribution showed that neighborhood-based social ties predict the level of organizational involvement in an ethnographic comparison of two neighborhoods in Boston (Tran et al. 2013). A fourth example provides evidence of human agency and resistance to state authority in the shaping of communities via unauthorized alteration in the form of street art, graffiti, gardening, illegal bike lanes, peddling, and street festivals (Douglas 2014). It is hard, however, to find a great many such examples in this journal, even though the quality of research it contains is generally quite high.

More evidence of the puzzle is expressed in a recent publication by Hipp and Wo (2015). They counted nearly 20 studies that have followed up the original Sampson et al. (1997) study to reveal a negative relationship between collective efficacy and either crime rate or some other undesirable or destructive behavior. But all these studies are cross-sectional, and, as Hipp and Wo noted, these studies cannot rule out the possibility that the neighborhood conditions (e.g., crime rate) are affecting the perception of collective efficacy rather than the other way around. Apparently, the visibility of collective efficacy theory over an 18-year period of time had not (or at least not yet) motivated scholars either to conduct rigorous tests of its importance net of the large structural factors known to be associated with it or to establish how it could be enhanced other than through the large structural factors that Sampson et al. (1997) found to be associated with it.

More recently, however, feasibility research concerning neighborhood interventions including, but not limited to, collective efficacy have begun to appear with greater frequency in *City & Community*. A recent article by Vargas (2019) addresses how nonprofit organizations might facilitate the enhancement of collective efficacy and reduction of gang violence in neighborhoods.[11] Carbone and McMillin (2019) found that collective efficacy is associated with a variety of neighborhood constructs, including neighborhood perceptions, strong social ties, and civic engagement. Shrider and Ramey (2018) found that public investment in neighborhoods—specifically through Seattle's Neighborhood Matching Fund—appears to reduce crime rates both directly and indirectly through its positive impact on private mortgage lending. Finally, Telep and Hibdon (2018) found evidence that "hot-spot interventions" by community-based nonprofit organizations produce a reduction in crime. Taken together, these recent articles suggest that feasibility research on strategies to reduce neighborhood inequality and specifically crime is increasingly seen as an important scholarly mission alongside long-standing research on neighborhood inequality that focuses on its documentation, its causes, and its negative consequences.

An important body of scholarship that is worth discussing in this context includes a set of recent books that address cultural aspects of neighborhoods and their impact on the adolescent and early adult development of impoverished youth (DeLuca et al. 2016; Patterson 2015). DeLuca et al. (2016) argued that the "identity project" for their youth respondents in Baltimore was a more powerful predictor of whether they would resist the street than "childhood trauma in the family of origin, direct exposure to violence, the presence of adult role models, and whether a parent or primary caregiver was employed, struggled with a substance abuse problem, or was in jail" (p. 66). Identity projects tend to take the form of a passion or a hobby that youth often adopt in middle or high school. The authors reported that roughly half of their sample had adopted identity projects. Why do some youth adopt identity projects while others do not? DeLuca et al.'s work offers important insight into the answer, namely, the ability to find a community of like-minded peers or mentors, both relatives and nonrelatives, who provided guidance or the presence of institutions offering opportunities for civic engagement and talent development such as church activities, summer employment, clubs, after-school programs in arts, music, and sports. The message that comes across in their book is that prosocial residents residing in impoverished communities can provide the scaffolding to allow some youth to develop mainstream identity projects that increase their chances for upward mobility.

[11]The author also argued that the intervention of nonprofits can involve a contest for neighborhood control that may erode democratic participation in the neighborhood by the most disadvantaged and may heighten the risk for more intense unrest even if it lowers crime rates in the short term (Vargas 2019).

This new research on culture and identity is clearly relevant for inequality reduction and at the same time creates a parallel with the research on collective efficacy concerning what the feasibility research that these studies call for. Some individuals in disadvantaged neighborhoods develop mainstream identity projects that increase their chances for mobility. Some poor communities have higher collective efficacy than one would predict on the basis of their structural characteristics. Yet we know relatively little about whether either identity projects at the individual level or collective efficacy at the community level have strong effects on their own or are instead consequences of other characteristics that may lead to better than average individual performance or community crime rates. We also know relatively little about how to produce a working identity project or stronger community collective efficacy.[12] These observations are not criticisms of this important work, but rather note how the knowledge gained from these studies generates new questions whose answers would clarify the potential for turning this knowledge into effective policies.

Matt Desmond's (2016) work on evictions simultaneously takes a frame-shifting and a policy-relevant approach to sociological research on inequality. Desmond reported that the difference between the average rate of mobility of low-income compared with average households in the United States can be almost fully accounted for by the rate of evictions from private rental housing. He also noted that evictions and relocation frequently move low-income households from better to worse neighborhoods, or from better to worse parts of neighborhoods. He persuasively argued that social welfare policies to increase residential stability would benefit low-income families, and he further argued that housing vouchers work better than public housing, tax credits, homeownership initiatives, or developer incentives.[13] Large-scale housing voucher programs, of course, are major government interventions that have large costs, and there are alternative ways to provide support to low-income families, such as increased cash payments (e.g., in the form of a child allowance) (Shaefer et al. 2018). Building a consensus for such a program would require a considerable shift in the dominant policy frame in the U.S. population and government.[14]

[12]This is not to say that we do not have clues about potentially productive avenues of investigation, such as low-cost strategies for building social capital in the neighborhood (Small, Jacobs, and Massengill 2008). Whether such strategies build collective efficacy and their full impact on neighborhoods remain open questions.

[13]Desmond also argued that legal reforms would be needed to make it illegal for landlords to discriminate against voucher holders.

[14]Additionally, DeLuca et al. (2016) argued for better counseling to help the two million households holding Department of Housing and Urban Development vouchers identify good neighborhoods with good schools, as the actual neighborhoods many voucher holders move to are generally similar to the neighborhoods they would live in had they lacked vouchers.

If the disappointing results of the MTO experiment question whether it is feasible to accomplish large-scale inequality reduction with housing vouchers, more recent research by DeLuca and Rosenblatt (2017), along with innovation by community and regional organizations, shows the potential of social science to identify feasible treatments that could lead to more effective policies. The Baltimore Housing Mobility Program improved on the MTO treatment with its intensive and sustained counseling to improve skills at housing search, landlord negotiation, and keeping a budget. The program also included landlord engagement, short-term financial assistance, vouchers that could be used in multiple counties in the Baltimore region, and a higher payment standard. DeLuca and Rosenblatt demonstrated in their analysis of this program how a deeper understanding of the housing mobility process produces feasible interventions that obtained a large initial effect (through a move to a much higher quality neighborhood) and a way to maintain these gains over time. Follow-up evaluation research by Bergman et al. (2020) found a large impact of a treatment similar to that proposed by DeLuca and Rosenblatt (2017) on the percentage of housing voucher recipients in the Seattle area who used their vouchers to move to high-opportunity areas. Aside from confirming the value of this intervention, Bergman et al.'s study also provides a basis for building political support. At the same time, the key social science was not in the experimental evaluation in Bergman et al. but rather the research done from the late 1990s to the present on the intervention itself.

If DeLuca et al. (2016), Desmond (2016), Bergman et al. (2020) and Chetty et al. (2015) are correct that housing vouchers for affluent neighborhoods would have a significant impact on mobility opportunities, their results further beg the question of the political feasibility of large-scale residential voucher programs that allow residents of poor neighborhoods to move into affluent communities and the likelihood that existing residents would not respond to these programs with counter-moves that undermine their impact (Owens 2015, 2016). Sociologists in particular have done tremendous work in describing the process of residential segregation, trends in segregation, and the consequences of segregation. But what about processes that reduce segregation or that limit the counter-mobility that maintains segregation at high levels? How much do we know about this? Charles (2003) argued that there are two major forces producing residential racial segregation: socioeconomic differences that produce racial segregation through the mechanism of socioeconomic segregation and what she called "place-based" segregation that emphasized forces of prejudice and discrimination that existed independently of socioeconomic differences. Her work suggests two distinct strategies for reducing segregation. Regarding the feasibility of reducing prejudice, Quillian published an article in the *Annual Review of Sociology* in 2006 titled "New Approaches to Understanding Racial Prejudice and Discrimination" (Quillian 2006). He argued that new (at the time) research on implicit prejudice (perhaps

now more commonly thought of as implicit bias) had developed in sociology and could be combined with sociological research to "understand how the macro context influences the content of micro beliefs." What research has been done in the past 13 years that delivers on this promise? We do not know of a major article that has attempted to systematize what social science has learned about interventions with the potential to reduce implicit bias, social distance, and segregation. Given the importance of this topic, this gap suggests the need for a stronger focus in this voluminous literature on interventions with the potential of making a difference.

In summary, tremendous progress has been made in the "neighborhood effects" literature. Recent efforts to better understand collective efficacy and its causes and to understand the potential positive impact of nonprofit organizations on neighborhood conditions constitute important progress. However, there is much we do not know about how to translate this knowledge into feasible strategies for reducing the impact of geography on social inequality.

Family Structure and Parenting Strategies Research

Family Structure. The role of unstable family structure as a cause of inequality has been controversial since the days of the Moynihan report (Moynihan 1965), largely because family instability was seen by conservatives as a matter of personal responsibility, determined by culture and moral orientation rather than social structure. One of the reasons Wilson's (1987) research was so influential was because it was a compelling effort to shift the frame for explaining the instability of the black family from a broad-based failure of personal responsibility to the combined roles of residential segregation for the black urban poor, out-migration from inner-city disadvantaged neighborhoods by the growing black middle class, and a shift in the spatial distribution of employment opportunities that undermined the ability of less educated black men to obtain employment. This led Wilson to emphasize policies intended to increase men's employment and earnings rather than policies focused on family structure as solutions to racial inequality. It also triggered research on the "family values" of the poor. Edin and Kefalas (2005) in particular found a strong positive valuation of marriage among unmarried mothers in the abstract but a perception that their male partners were not yet stable enough to make marriage a sensible option. Like Edin, McLanahan (2009) found positive valuation of marriage among the unmarried parents (though not as positive as for married parents). She found that unmarried cohabiting fathers are highly involved with children at birth but that involvement declines dramatically over time for many fathers.

Childhood family structure clearly affects a child's life chances through mechanisms other than family income (Bloome 2017). McLanahan and her collaborators (e.g., Lee and McLanahan 2015; McLanahan and Percheski 2008) explore the causal pathways linking unstable family structure with child outcomes, including socioemotional outcomes. In the process, she broadens the concept of family instability to include father's and mother's economic status, father's and mother's stress and broader mental health, multipartner fertility, the quality of the parental relationship including issues of trust and cooperation in parenting, the sharing of resources including the level of child support from nonresident fathers, social support from family and friends, and the quality of parenting. She also ponders strategies to reduce family instability and its effects on children. Four approaches in particular merit her attention. First, she developed Moynihan's early recommendations of government economic support for fragile families to make them less fragile. Second, McLanahan argued that support should be available for two-parent families in order to reduce monetary incentives for family instability. Third, she called for adequate opportunities for jobs. Fourth, she called for government programs to improve relationship skills and for programs that can effectively motivate young women to delay fertility until they find long-term partners.

Have these calls led to solution-oriented sociological research that informs strategies for welfare reform, job provision, improved development of relationship skills, or delay of fertility? In some ways, the answer is clearly yes, but progress is uneven. The principal pushback against McLanahan's call for support for fragile and two-parent families has been the conservative argument best associated with Murray (1984) that cash assistance undermines families, particularly when it flows to single mothers. Two decades of experience with the Temporary Assistance for Needy Families (TANF) program has not provided relationship stability to low-income families, and indeed the wholesale elimination of cash welfare benefits has left millions of poor families forced to survive on $2 a day or less (Edin and Shaefer 2015). Edin and Shaefer's (2015) conclusion from their exploration of such intense poverty is that it is the opportunity to work, not the will to work, that is lacking. This opportunity could be enhanced by a large-scale job creation program by the federal government, government-subsidized private sector jobs, and higher minimum wages. They also argue, like Desmond, that low-income families need a stable place to live, though they see much of the problem in the lack of income for the poor. Third, they argue for a large expansion of TANF, including federal regulation that incentivizes states to stop redirecting federal government-provided TANF funds to other state programs at the expense of cash assistance for low-income families.

Edin and Shaefer's call for these large-scale solutions, we would argue, does not derive its power from in-depth research on feasible strategies for reducing poverty other than the demonstration that their subjects are cash starved and suffer adversity because of it. Its power comes from criticizing frames that fail to fathom the extent of the predicament of those living on less than $2 a day despite their efforts to

alleviate it. They also pull readers away from the frame that sees TANF as solving poverty problems by providing strong incentives for the poor to work that can readily be satisfied by the market.[15] Instead, they show that work-based welfare programs make life worse for millions of American families that are not to blame for their often economically desperate circumstances. They do not primarily research the specifics of alternative social welfare strategies, which is inherently difficult, given the lack of social experimentation with alternative strategies. We say this not to criticize their important work—indeed, their conclusions powerfully rebut conservative frames—but rather to highlight the role it plays in broader debates over what to do about antipoverty policies. In this context, Edin and Shaefer's (2015) book can be contrasted with research by Halpern-Meekin et al. (2015), which demonstrates the positive social and psychological benefits of work-based safety nets such as provided by the earned income tax credit (EITC). This work is commendable for its contribution to social science knowledge about an important and feasible strategy to reduce inequality; because of their work, we know more about the nonfinancial benefits of the EITC and therefore about the overall magnitude and durability of EITC effects.

Parenting and Parenting Strategies. Blau, Duncan, Sewell, Hauser, Featherman, and other early developers of the status attainment model saw parental skills as resources that were transmitted to children through the process of parenting. Educational inequality of children was at least partly explainable in terms of parental inequality in material resources and skills. Bourdieu produced an overlapping but also distinctive argument that parents transmit not only skills but also cultural characteristics that facilitate academic achievement, including the ability to transmit cultural signals as to the status group in which one belongs. Bourdieu's work itself can be seen as building on earlier work by Kohn (1963, 1977), who argued that middle-class parents are disposed to develop self-direction in their children, while working-class parents favor conformity. Lareau (2003) connected the work of Kohn and Bourdieu in her argument that class-based differences in parenting strategies contributed to class reproduction. Lareau argued that middle-class parents typically follow a strategy of "concerted cultivation" that teaches children how to use a language of reasoning and negotiation as a way of controlling their children but also teaching them how to interact effectively in the middle-class world.[16] In contrast, she argued, working-class parents use directives in "custodial matters" and allow autonomy ("natural growth") in their leisure time, which fails to prepare them to successfully convert their schooling experience into an engine for upward mobility. Many other studies also find class-based differences in cultural capital, which affect students in elementary and secondary school (Calarco 2014a, 2014b; Jaeger and Breen 2016) and also in higher education (Hamilton, Roksa, and Nielsen 2018), even as recent studies assert a continued "lack of clarity" in the literature about the concepts and mechanisms of cultural reproduction theory (Jaeger and Breen 2016).

Lareau saw parenting style as following more or less automatically from class position. Indeed, she put forward the theory that parenting style is the primary mechanism by which class differences are reproduced in the next generation. Moreover, she found little variation in parenting styles within the groups that she identified as working and middle class, and she found little evidence that lower or working-class parents might similarly modify their parenting strategies to further the life chances of their children. Other studies, in contrast, show that both working- and middle-class parents have modified their parenting behavior in response to changing advice from scientific experts, though diffusion is greater and more rapid for higher status and more educated parents (Apple 2006; Walzer 1998). Coleman (1988), meanwhile, introduced a distinct conceptual dimension—what he called "social capital" as distinct from "human capital"—to allow for within-class variation in the parent-child relationship, specifically the within-class variation in the extent to which parents of a given social class invest in their children's education (for a recent example, see Li and Fischer 2017). He argued that there is also variation in the extent to which communities have "intergenerational closure," measured as the extent to which the parents of a child knew the parents of the child's friends. Coleman further argued that intergenerational closure is a resource that could potentially affect school outcomes.[17]

A persisting difficulty with the sociology literature on parenting is its lack of integration with the large parallel literature on parenting styles in psychology. The psychology literature argues that there are various styles of parenting and that "authoritative" parenting (i.e., simultaneously "demanding" and "supportive") improves both cognitive and noncognitive skills of adolescents and improves their academic achievement (Steinberg 2001). Like Coleman (1988), and like Robinson and Harris (2014), the psychological literature on authoritative parenting finds that parenting style is not uniform within social class (Steinberg 2001). It also finds that authoritative parenting produces benefits for adolescents regardless of the social class of the parents (Steinberg 2001). When juxtaposed with Lareau's work, this literature creates a puzzling string of unanswered questions that persist at least

[15]We might contrast their analysis of TANF with the success of the earned income tax credit in providing the poor with higher income as a consequence of working (Halpern-Meekin et al. 2015).

[16]The extent to which this advantage is indeed a cultural style, as opposed to a higher level of verbal skill that itself is valued in school, has not been established.

[17]Gamoran et al. (2012) recently built on Coleman's work by examining the ability of experimentally enhanced social capital to improve educational outcomes for Hispanic families in two research sites; their study found relatively limited effects (see also Mouw 2006).

partly because of the lack of attention paid by the sociological and psychological literature to each other. To what extent are "concerted cultivation" effects really the effects of authoritative parenting? How strongly are either of these aspects of parenting actually connected to social class? To what extent can these parenting strategies be taught to parents—in particular low-income and working-class parents—in order to improve educational outcomes for their children?

Both the psychology and sociology literatures show some awareness of these knowledge gaps. For example, in Duckworth's (2016) recent book on the noncognitive skill that she called "grit," she argued that although "we need more research on the topic," her review of the evidence suggests that authoritative parenting (and, she noted, also authoritative mentoring by other significant adults) increases a child's grit. Steinberg (2001), meanwhile, noted in his review that psychologists knew more about the consequences of authoritative parenting than about how to teach it to parents and called on psychologists to "bridge the gap between research and practice" by providing information to parents about parenting styles and their consequences. Lareau argued for social policies (e.g., child allowances) to increase material resources for parents, to make recreational programs and academic enrichment activities cheaper for working-class families, and to convince working-class families to actively work to increase the size of their children's vocabularies and to play a more active role in their schooling.

Lareau's book on parenting styles was published 18 years ago. Even though Lareau's work is very well known in the discipline, it is hard to detect a large-scale sociological research effort on the potential to change working-class parenting behavior to improve the life chances of children, assuming that parenting styles have the powerful effect on educational attainment that Lareau saw in her data. Indeed (we think it is safe to say), the widespread, if not dominant, interpretation of Lareau's work within the sociological community is, first, that she established "concerted cultivation" as the main reason why the children of upper-middle-class parents have educational advantages over the children of working-class parents and, second, that she has firmly tied this mechanism to the macro-structure of class inequality, which is difficult to change.[18] In this respect, we see a strong parallel between the sociological response to Sampson's work on collective efficacy and to Lareau's work on concerted cultivation. Both claims are honored as important theoretical and empirical advances. They are widely cited and other work builds on these important contributions. But neither claim has yet stimulated a surge of rigorous studies to test their implications and to address the feasibility of reducing inequality via mechanisms connected either with neighborhood collective efficacy or with the parenting style of concerted cultivation.

Education and Schools Research

Whereas "big solution" studies of the poor focus on frame shifting as a strategy to build support for large-scale government interventions, research on schools is similar to research on neighborhoods in trying to establish the mechanisms by which contextual effects produce their outcome. Schools are prime candidates for intervention because of their importance in the process of generating inequality (Domina, Penner, and Penner 2017) and because there are continuing efforts at school reform that provide the necessary variation and opportunities for rigorous evaluation studies (Schneider and Keesler 2007). Moreover, schools are sites where one can readily imagine incremental change that propagates into meaningful reform without calling for the huge and politically infeasible policy interventions that would be necessary to provide housing vouchers for low-income families or to provide a major increase in job opportunities for the working poor. The outstanding question is whether these interventions can be uncovered by sociological research and whether they can be accomplished without impractically large overhauls of residential patterns, state funding mechanisms, or the U.S. income distribution. Another pertinent question concerns the extent to which useful research on these issues is coming from sociology as opposed to the other social science disciplines (notably economics) that also focus considerable research effort on education.

Equality of Educational Opportunity (generally now known as the Coleman report; Coleman et al., 1966) concluded that the school attribute that best explained variation in student outcomes was the socioeconomic level of other students in the school (Alexander and Morgan 2016). Court-ordered desegregation following the 1968 Supreme Court decision substantially reduced racial segregation in schools, primarily by reducing within-school district segregation especially in the South; more than half of black students attended schools that were more than 90 percent black in 1968, compared with only 33 percent in 1980 (Coleman, Kelly, and Moore 1975; Orfield 1983; Reardon and Owens 2014).[19] At the same time, however, between-district segregation increased, particularly in large cities where court-ordered desegregation produced white flight to the suburbs (Coleman

[18]Lareau also suggested that middle-class families be encouraged somehow to slow down the intensity of concerted cultivation for their children to alleviate stress on parents and children alike, and this message to upper-middle-class families is frequently picked up in the mainstream media (Bridges and Tober 2014; McKenna 2012).

[19]Since then, the level of segregation measured with the usual indices has remained flat. Levels of exposure of black students to white students have declined as the proportion of public school students who are white has declined (Reardon and Owens 2014).

[20]Inequality in achievement is also driven by income segregation, which has increased in recent decades (Owens 2018; Owens, Reardon, and Jencks 2016).

1975; Coleman et al. 1975).[20] Recent studies have demonstrated positive effects of racial desegregation on educational attainment for black students and no negative effects for white students (Quillian 2014; Reardon and Owens 2014). Other recent research (Hanushek and Rivkin 2006; Page, Murnane, and Willett 2008; Quinn 2015) shows that a considerable portion of the black-white achievement gap is associated with the school one attends. This finding parallels other sociological studies that find substantively important effects of high schools on the likelihood of college attendance (Jennings et al. 2015) or on the gender gap in the science, technology, engineering, and mathematics orientation of high school seniors (Legewie and DiPrete 2014). This research all supports the conclusion that schools play an important role in the maintenance, increase, and reduction of inequality.

Following the emphasis in the Coleman report on the negative educational consequences of racial segregation, education researchers identified magnet schools as a model for promoting integration. These schools were originally conceived as a remedy to racial segregation. In recent years, however, stagnant funding for magnet schools and legal challenges to race-conscious desegregation programs (Reardon, Yun, and Kurlaender 2006; Siegel-Hawley 2016) have diverted attention away from magnet schools and toward charter schools and has led some magnet schools to move away from their racial integrationist roots. Many school districts have now opted to implement race-neutral policies, with income as the most often used assignment criterion (Reardon et al. 2006). However, research suggests that income integration policies are unlikely to produce racially integrated schools given the extent of race-based residential segregation in the United States (Reardon et al. 2006). Reardon et al. (2006) suggested that socioeconomic status (SES)–based school assignment programs could enhance racial integration if they are accompanied by robust transportation policies and if SES is determined by multiple factors "to the extent that the multiple factors are collectively more highly correlated with race than income alone" (p. 68). As a policy, however, large-scale SES-based school assignment is infeasible in the current political climate of the United States.

Along with the development of accountability policies (Dee and Jacob 2011; Jennings and Sohn 2014; Ravitch 2010), the charter school movement has been the other major development emerging from school reform movements. Studies of the effectiveness of charter schools have taken place largely outside of sociology. These studies generally conclude that charter schools are highly variable, do not outperform public schools on average (Baude et al. 2014; Ladd, Clotfelter, and Holbein 2016) and, at least in North Carolina, may be contributing to increased racial segregation in the educational system. At the same time, several recent papers in economics find persuasive evidence that lottery-based "no-excuses" charter schools produce large achievement gains in SAT scores, on high stakes accountability tests, in AP calculus credit, and on the rate of attendance at four-year colleges (see Angrist et al. 2016 for the Boston case; see also Dobbie and Fryer 2011, 2013 and Whitehurst and Croft 2010 for comparable evidence from New York City). Their research (recently summarized by Leonhardt 2016) shows that two years of middle school charter school experience almost eliminates the large race gap in mathematics performance that existed for these students at the time of entry (the start of sixth grade).

These findings beg the question of how "no-excuses" charter schools produce their positive results. Although various factors may underlie their success (Berends 2015), a recent evaluation of their effectiveness across several cities (Chabrier, Cohodes, and Oreopoulos 2016) concluded that the characteristic of no-excuses schools that mattered most is the use of required intensive tutoring. Rigorous studies of tutoring effects have found results of tutoring that are as large or larger than the effects of having a good teacher (Ritter et al. 2009). The recently completed experimental intervention using the tutoring techniques of Match, Inc., on a sample of disadvantaged black and Hispanic 9th and 10th grade boys in Chicago achieved larger gains of between .2 and .3 standard deviations on math achievement tests, which is between two thirds and a full year of achievement in math for the typical student in 9th and 10th grade. This intervention also achieved an impressive .5 standard deviation increase improvement in math grades in school (Cook et al. 2015; see also Cook et al. 2014).

Corresponding to the literature on tutoring effects is a literature (again, mostly in economics) on teacher effects. Rigorous estimates based on value added models that control for nonrandom sorting of students and teachers generally find that a teacher who is 1 standard deviation above the mean (roughly the 84th percentile) improves student performance by between .1 and .2 standard deviations, with effects being somewhat larger for mathematics than for reading (Jackson, Rockoff, and Staiger 2014). Studies have found that about 50 percent of the gain from a good teacher fades after a year or two (Jacob, Lefgren, and Sims 2010), but some of the benefits appear to be durable (Chetty, Friedman, and Rockoff 2014).

The findings that teacher effects are rather strong and that teachers differ substantially in their effectiveness would seem to provide a strategy for improving education by either selecting for or training highly effective teachers. However, the literature to date has not yet succeeded in establishing the characteristics of good teachers or of good teaching, and the extent to which these characteristics can be taught (or learned through teaching experience) or must be selected for. This research is nonetheless of interest because of its focus on systematic data gathering via video-taping and rating by various observers, including students, mentors, and principals (Blazar 2015; Kane and Staiger 2012; Kane et al. 2011), or via analysis of the specific character of teacher subject knowledge and how it relates to student gains (Sadler et al. 2013), or via in-depth ethnographic study (e.g., Golann's

[2018] study of how teachers respond to school-based training in disciplinary practices in a "no-excuses" charter school). Meanwhile, comparative literature has suggested that countries with the highest Program for International Student Assessment scores have a teaching profession that is relatively highly paid, is high status, and draws teachers from the upper half of the performance distribution of four-year college students (Ripley 2013; Sahlberg 2014).

Consider, as another example, recent calls for increased investment in pre-K education as a strategy for improving educational outcomes for disadvantaged children and thereby reducing inequality (e.g., by the American Enterprise Institute and Brookings Institution Working Group on Poverty and Opportunity), which beg the question of whether this would involve a scale-up of existing programs or a focus on understanding what works and using this knowledge to design feasible treatments that work better. Almond, Currie, and Duque (2018) discussed some existing high-quality evaluations of the effects of the Perry Preschool Program, the Abecedarian intervention, or Head Start on later educational and posteducation outcomes. These studies are important, but they focus on effects of existing treatments rather than on learning more and hopefully improving the treatment itself. One approach to this goal might be through a comparison of the multiple efforts to increase participation in, and the quality of, publicly funded universal pre-K programs.

Many sociologists pay close attention to this literature on school and teacher effects and how to use them to improve outcomes and reduce inequality (e.g., Berends 2015). Our review of recent articles in *Sociology of Education*, which is the discipline's leading journal in this field, finds an increasing number of articles that are engaging in feasibility research, including strategies to increase social capital (Cox 2017), the structure of school choice lotteries (Hanselman 2018), the potential of guidance counselors to affect high school choice (Sattin-Bajaj et al. 2018), parent-provided help with homework (Li and Hamlin 2019), and the provision of bond funds for capital investments (Rauscher 2020). At the same time, our review also reveals that the majority of the articles are more strongly in the direction of documenting dimensions of inequality in the American educational system. The relative lack of attention to the potential benefits of schools by sociologists may stem from what Downey and Condron (2016a) recently called an "unwarranted certainty among scholars that schools promote inequality" (p. 218). Their article illustrates the themes and tensions in the sociological literature on education. Downey and Condron feared that too much complacency about the value of educational policy reinforces the public's view of "public investment in schools as a legitimate action of the welfare state yet are skeptical of state involvement via other means" (p. 217). The fact that the characteristics of schools may be more manipulable than those of families or neighborhoods concerned them as potentially supporting the narrative that school policy is "the only lever available" for policy. Instead, they argued for a "more contextual understanding of how schools matter" (p. 216).

In our terms, Downey and Condron's call for contextual understanding shifts the frame away from schools as policy levers to the fact that "nonschool environments" (which affect the family and neighborhood) are more important causes of inequality than are schools. Although their article is partly an attempted corrective to the imbalance in sociological research that sees schools as part of the inequality problem rather than a potential part of the solution to inequality, Downey and Condron underemphasized the value of research that increases our understanding of how schools might reduce inequality, for example, by year-round school (Downey, von Hippel, and Broh 2004), preschool (Bailey et al. 2017), starting elementary school at an earlier age (Bernardi 2014), or deemphasizing school tracking (Gamoran and Mare 1989).[21] The authors seem concerned that sociology will focus too much attention on understanding how schools might reduce inequality at the expense of studying "how characteristics of the broader social context shape the pattern of inequality observed in schools" (Downey and Condron 2016b:235).

Downey and Condron argued for greater effort by sociologists of education to shift the frame toward the importance of nonschool context. They worried that continued research on school policies to reduce inequality will reinforce an incorrect frame that school reform is the route to an egalitarian society. We agree with their analysis but caution against the conclusion. Sociology already focuses tremendous attention on the broader social context and its impact on educational inequality. Relative to economics, the sociology of education is weak on examining feasible sources of inequality reduction via the schools. Our ability to contribute to the reduction of inequality through a combination of frame shifting and through educational policy might be strengthened if we devoted greater resources to the analysis of feasible ways to reduce inequality in the educational process as well as the family and community.

Relational Inequality Research

Inequality research tends to focus on distributional outcomes, while paying less attention to the micro-level social relations that create and reproduce inequality (Schwalbe et al. 2000; Tomaskovic-Devey and Avent-Holt 2019). Drawing on the interactionist perspective, Schwalbe et al. (2000) identified four "generic processes" that maintain inequality across various domains: othering, subordinate adaptation, boundary maintenance, and emotion management. In this section, we describe each generic process and highlight relational inequality studies pertaining to issues of housing and neighborhoods, family structure and parenting, and education and schools. Whereas most of the studies we discuss in this section are descriptive, explanatory, or frame shifting in their focus, we also bring attention to research that

[21]See Gamoran (2016) for a similar conclusion.

offers feasible strategies for reducing relational inequality, particularly in education.

Othering describes the process whereby groups construct a dominant identity by categorizing others as subordinate (Burton and Welsh 2015; Schwalbe et al. 2000). There are three main forms of othering that contribute to inequality: oppressive othering, implicit othering, and defensive othering (Schwalbe et al. 2000). Through the process of oppressive othering, a dominant group seeks to gain an advantage or maintain social superiority by identifying out-group members as morally or intellectually inferior. Oppressive othering also works to justify feelings of indifference or acts of cruelty directed toward disadvantaged groups. Racial classification systems represent a clear instance of oppressive othering (Omi and Winant 2014). In an analysis of how subordinate groups experience this relational process in everyday life, Shedd (2015) demonstrated that members of racially minoritized groups are more inclined to perceive oppressive othering when they are in diverse settings. The students of color in this study who attended diverse public high schools in Chicago had more acute feelings of marginalization relative to white students than their counterparts who attended racially segregated schools.

Implicit othering occurs when members of a dominant group are assumed to possess desirable traits that give them legitimate claim to their social advantage. For example, students attending the elite St. Paul's boarding school learn "to emphasize their hard work and talent when explaining their good fortune" (Khan 2011:15). As another example, consider the "preps"—the upper-middle-class, mostly white girls attending "Waretown High School" in the Central Valley of California. The "preps" get their sense of entitlement from the assumption that their positive status in the school was earned through their high achievement. Additionally, the "preps" assumed that lower achievement is a choice made by girls identifying with "nonprep" groups (i.e., "las chicas," the "skaters," etc.). "The preps are the school" is a routine description by the nonpreps to signify the monopoly that preps experience with regard to academic favor and extracurricular activities (Bettie 2014:102). Defensive othering, on the other hand, happens when members of a disadvantaged group attempt to distance themselves from in-group members as a way to avoid social stigma or seek honorary membership in the advantaged group. Smith's (2005) study provides evidence of defensive othering among African American low-wage workers residing in urban communities. Some of the participants in this study described actively distancing themselves from job seekers in their network in order to deflect "the stain of bad behavior and the stigma associated with common, negative stereotypes of the black urban poor" (p. 29). Although defensive othering does not create exploitable groups, this process still reproduces inequality by undermining solidarity among the marginalized and reinforcing negative stereotypes propagated by those in the dominant group (Schwalbe et al. 2000).

Subordinate adaptation describes the "behaviors and strategies that the disadvantaged employ to cope with their diminished status, which can inadvertently perpetuate the existing social hierarchy" (Burton and Welsh 2015:7). Forming alternative subcultures is one example of subordinate adaptation whereby disadvantaged groups create their own systems of prestige, power, and economic access (Schwalbe et al. 2000). In an ethnographic account of impoverished Philadelphia neighborhoods, Anderson (1999) depicted the code of the street as a subordinate adaptation strategy of some community members to their social deprivation and the indifference of the justice system. The code of the street is an informal, and often violent, set of rules regulating interpersonal public behavior that is based on a "desperate search for respect" (Anderson 1994). Similarly, Edin and Kefalas (2005) identified an adaptation strategy among low-income, unmarried women who sought out motherhood at early ages as a way to enhance their sense of self-worth and social standing in their communities. At the same time, single motherhood curtailed their already limited prospects for social mobility.

Boundary maintenance describes the activities dominant groups engage in to limit or control access to valuable resources available to members of an out-group (Burton and Welsh 2015). The boundaries erected by dominant groups can be "symbolic, interactional, spatial, or all of these" (Schwalbe et al. 2000:430). Redlining is a prime example of spatial boundary maintenance at the neighborhood level (Massey and Denton 1993; Rothstein 2017). This form of boundary maintenance looms over the lives of many African Americans who remain stuck in impoverished communities across multiple generations (Sharkey 2013). These communities are generally situated farther from desirable employment opportunities, high-quality schools, and social institutions that generate the capital necessary for social advancement (Bourdieu 1985; Burton and Welsh 2015; Wilson 1987).

In a study of a suburban school district, Lewis-McCoy (2014) observed the subtle and overt processes of boundary maintenance that made it challenging for black parents to access coveted educational resources that were often hoarded by white parents. In particular, more privileged families were the first to be notified about educational opportunities given their closer ties to school staff, while families with tenuous connections to schools were often left out of the loop. Hamilton et al. (2018) identified a similar boundary dynamic between affluent and less affluent families with children attending a selective public university in the Midwest. Opening communication channels about programming and resources for all families could be a feasible lever for reducing boundary maintenance in educational settings (Corcoran et al. 2018; Lewis-McCoy 2014). Research by Carter (2005) also offers the idea of "multicultural navigators" as a feasible strategy for reducing the social boundaries low-income black and Latinx youth face in schools. A multicultural navigator

can be an educator or other adult role model who understands students' lived experiences, respects their nondominant cultural capital, and teaches them how to draw on different "cultural know-hows" to successfully navigate mainstream, middle-class institutions (Carter 2005:150). Likewise, Ispa-Landa's (2015) research on a suburban busing program emphasizes the valuable role of adult mentors, particularly same-race mentors, for helping traditionally marginalized youth navigate relational inequality. The black guidance counselors and busing program coordinators at the suburban schools she studied actively taught black students the cultural skills and dispositions necessary for successfully interacting with educational gatekeepers.

Social inequality can generate "feelings of anger, resentment, sympathy, and despair" that threaten to destabilize the status quo (Schwalbe et al. 2000:434). Emotion management describes the process of controlling these destabilizing feelings at the individual or group level. Those in power have an obvious interest in managing the emotions of the less powerful (Schwalbe et al. 2000). One form of emotion management, conditioning emotional subjectivity, occurs when people learn to suppress their natural reaction "by habitually reinterpreting the meanings assigned to the circumstances that trigger the emotion" (Burton and Welsh 2015:8). Jones's (2014) research on proactive policing depicts this process at work in a high-surveillance neighborhood in San Francisco where stop-and-frisk is a fixture of everyday life. Direct and vicarious police encounters become so normalized for the young black men in this study that they learn to behave like "professional suspects" and suppress their feelings of anger, shame, and frustration.

Regulating discourse is another form of emotion management that describes how dominant groups control popular narratives that support the existing social order (Burton and Welsh 2015). Schwalbe et al. (2000) emphasized that regulating discourse is a powerful strategy for inspiring action that can lead to othering and boundary maintenance. In *Killing the Black Body*, Roberts (1999) detailed how centuries-old myths about black women's inferiority, deviance, and unfitness for motherhood emboldened policy makers and medical practitioners to violate their bodies and reproductive rights. Relatedly, Watkins-Hayes and Kovalsky (2016) examined how tropes about the deserving and undeserving poor are used by politicians, the media, and the larger public to justify who is worthy of government assistance. The authors noted that the "politics of deservingness" is a universal narrative that is used to "categorize, divide, distribute, and withhold among populations across the globe."

Taking an interactionist approach can help scholars better understand how micro-level interactions perpetuate inequality in neighborhoods, homes, and schools. At the same time, the gap between better understanding of micro-level interactions and inequality reduction is rather wide. The sometimes implicit and sometimes explicit message of this literature is that greater understanding will reduce status hierarchies and tribalism and create a more respectful and egalitarian culture built around the fundamental importance of human dignity. This approach assumes, perhaps not unreasonably, that making relational inequalities more visible makes them more unacceptable and triggers cultural shifts. However, we have all witnessed the power of the Trump-era counter-movement that arose in the United States on the basis of a denial of the continued existence of racism and a repudiation of "political correctness." These events suggest that something more than visibility, understanding, and frame shifting is needed to reduce relational inequality.

Useful insights into what we can see and not see concerning the shape of feasibility research can be found in the last chapter of Tomaskovic-Devey and Avent-Holt's (2019) recent book on relational inequalities within organizations. They argue that relational equality can be promoted via laws, regulations, and organizational policies but that "equality in interaction is much more difficult to achieve than changes in law or organizational policy, particularly when there are cultural messages that reinforce categorical status distinctions" (p. 231) They argued that class-based inequalities generate relational inequalities. They further argued that policies to reduce hierarchy in pay and authority and to reduce segregation-enhanced associations between hierarchy and categorical distinctions are necessary to reduce relational inequalities. In other words, reducing distributional inequalities for them seems to be a necessary if not sufficient step toward reducing relational inequality based on status distinctions and status boundaries. This view certainly does not diminish the importance of feasible strategies to modify the "cultural messages that reinforce categorical status distinctions" alongside strategies for reducing segregation and hierarchy. Instead, it calls for more intensive research to find feasible strategies for curtailing the mundane emotions and behaviors that contribute to the reproduction of inequality. At present, however, this research program is underdeveloped in our discipline.

Plausibility, Feasibility, and Practicality

The question of how sociological research—and, more generally, how social science research—is relevant to the question of reducing inequality is a complicated one. This question obviously relates to the broader question about the extent of influence (or lack of influence) that social science research has on social policy and on society at large. Some of this influence (or its lack) is related to the tension between "basic" and "applied" social science. However, as we have argued above, this perspective can be misleading, particularly with regard to sociological research and maybe also for the other social science disciplines. Sociological research is not typically about disinterested knowledge acquisition to improve our "self-understanding of society" (Shils 1980). It is generally reform oriented. But in large part, sociology has not directed itself toward Coleman's goal of rich research findings geared toward "optimizing relevant outcomes."

Instead, the emphasis on description and explanation, often from a critical perspective, remains the dominant focus, though the explanatory frames are generally not so well developed as to establish feasible strategies. In Michael Burawoy's (2005) presidential address on the topic of public sociology, he argued that the general "pathology" of professional sociological research was its tendency to focus on the "seemingly irrelevant." Our analysis of sociological research on inequality does not support this conclusion. Research on inequality focuses on highly relevant issues but is imbalanced toward description, explanation, and frame shifting at the expense of constructive findings that point toward viable strategies to reduce inequality.

The emphasis on frame shifting in the context of inequality research has the potential to undermine flawed, yet politically consequential arguments about the social arrangements that maintain inequality. In this sense, what we have called frame-shifting research is a necessary step toward developing social and political support for the idea that inequality reduction is feasible as well as desirable. But without feasibility research, frame shifting is not enough. Research showing that inner-city neighborhoods are poor because of a lack of opportunity in the local labor market is of scientific importance in countering the conservative argument that the poor are poor because of a self-produced deficient "culture" (as opposed to structural conditions). Research findings that neighborhood effects are large shift the frame away from individual explanations. Research showing that absolute mobility rates have fallen substantially in the United States contributes to the frame that opportunity could be greater. Research that residential segregation disadvantages minority families and is not a condition that they choose supports a frame that desegregation would enhance opportunity and reduce inequality. Sociogenomics research showing a relatively large shared environment effect on the educational attainment of twins (Branigan, McCallum, and Freese 2013; Conley and Fletcher 2017; Freese and Jao 2015) argues against conservative conjectures that, given genetic differences across people, American society may be already be close to providing equal opportunity (e.g., Mankiw 2013). Research on the structural sources of residential instability (Desmond 2016), the psychological consequences of poverty (Mullainathan and Shafir 2013), or the cognitive consequences of the environmental toxins, which are more common in low-income neighborhoods (Aizer et al. 2016), is all further evidence against the argument that the difficulties of the poor are attributed to personal characteristics that are (or should be) under their own control. But each such frame shift begs the question of whether these shifts imply feasible strategies for reducing inequality, what these feasible strategies might be, and what do we need to know that we do not yet know that would get us closer to implementation. The feasibility side of the research is underdeveloped compared with the descriptive, explanatory, and frame-shifting research effort that points the way toward a more just and equal society but does not yet provide sufficient guidance as to how to get there.

What does good feasibility research look like? Many of the studies discussed in this article already point to the feasibility of inequality reduction and thereby provide examples of a research focus on the practicality and durability of positive environmental interventions. One important strategy is the identification of specific causes of inequality that are subject to reversal via the identified causal mechanisms themselves. For example, Aizer et al.'s (2016) research about the toxicity of lead for cognitive development demonstrates the feasibility of closing at least part of the educational achievement gap by abating exposure to lead in the home and neighborhood. To take another example, the extent to which information deficits are a prime mechanism by which inequality in family resources creates inequality in educational attainment points to strategies to reduce the information deficit concerning both high school choice (Corcoran et al. 2018; Sattin-Bajaj et al. 2018) and application to college (Bettinger et al. 2012; Hoxby and Turner 2013; Oreopoulos and Dunn 2013) without taking on the gargantuan task of reducing the inequality in family resources. A third example might be the study of the factors that make for a good school teacher, to determine the feasible strategies for improving less than good teaching, or more generally, shifting the quality distribution of teaching and its correlation with the socioeconomic advantage of students (Kane et al. 2011, 2013).

Another strategy, which gets closer to the standard conception of policy research, concerns interventions that are intended to offset deficits stemming from disadvantage and thereby reduce inequality. This strategy requires sufficient understanding about the nature of disadvantage in order to reduce deficits without necessarily tackling underlying causes that are too politically difficult or expensive to change (e.g., socioeconomic inequality, or neighborhood inequality and racial segregation). The goal is partly to evaluate the effects of a treatment, but feasibility research needs to go further than that. Valuable interventions in the childhood life course must be durable as well as substantively important and practical in terms of cost. The Abecedarian program, for example, was notable for having durable effects on academic performance in contrast to the apparently short-term effects of Head Start and similar preschool programs (Bailey et al. 2017). Evidence about the size of teacher effects or "no-excuses" charter schools or classroom size or tutoring or vouchers for low-income families to move from disadvantaged to better neighborhoods needs to be coupled with evidence about the durability of these effects (e.g., Chetty et al. 2015), as well as the costs of interventions relative to the gains. Dynarski, Hyman, and Schanzenbach (2013), for example, compared experimental evidence on several educational interventions that appear to have positive effects and concluded that providing information about the Free Application for Federal Student Aid (FAFSA) (Bettinger

et al. 2012) cost $1,100 per additional disadvantaged student going to college, compared with $133,000 for Head Start, $171,000 for class size reduction, and $400,000 for the Abecedarian program. Their calculations imply that an Abecedarian-type treatment for all low-income children is not a feasible strategy. In contrast, the provision of greater information about high school or college choice may well be feasible. Whether the cost structure of the Harlem Children's Zone project is more feasible than that of an Abecedarian-type treatment depends on the size of its impact, which is still a matter of debate among academics (Dobbie and Fryer 2011, 2013; Whitehurst and Croft 2010).

Aside from effect size, durability, and cost, feasibility research must also pay attention to the mechanisms by which treatment effects work. Sociological research also emphasizes the relational character of inequality (Desmond and Western 2018; Schwalbe et al. 2000; Tilly 1998; Tomaskovic-Devey and Avent-Holt 2019). This work implies that feasible and effective strategies must also be relational. Consider, for example, the issue of improving the quality of schools. Socioeconomic composition of the student body is a prime determinant of school quality, but large-scale socioeconomic integration is infeasible. No Child Left Behind–type accountability systems had only small positive effects (Dee and Jacob 2011) and widespread undesirable consequences (Jennings and Sohn 2014). The possibility that high-intensity tutoring is behind the positive effects on achievement of no-excuses charter schools (Chabrier et al. 2016) is potentially of great value, but the feasibility of tutoring for inequality reduction depends on how much we know about tutoring and its effects, costs, and scalability. Is it feasible to improve the effectiveness of less than intensive tutoring, for example, through better tutor training, or through the combination of traditional in-person tutoring with online tutoring using (continually improving) educational software? Is one form of tutoring more scalable than another? Does scaling up mandatory intensive tutoring (as in the "no-excuses" charter schools) require the large-scale introduction of "no-excuses" charter schools in disadvantaged areas of the country? Could properly designed auxiliary tutoring services (e.g., including the use of volunteer or work-study college students combined with online tutoring) achieve similar results? Would the latter approach be more scalable? And finally, and assuming that tutoring is effective, why is it effective? The transmission of information is surely a major component but what about the relationship between the tutor and the student? Might these relationships build both interpersonal trust and generalized trust, and thereby help the student in other aspects of the schooling experience as well? More generally, are there feasible strategies of increasing trust between low-income and minority students and their regular school teachers that enhances educator effectiveness (Fox-Williams 2020)?

Knowing that an intervention is financially feasible still leaves important research questions about its feasibility aside from cost. Is it feasible to deliver information about the FAFSA at scale to reduce the information deficit faced by less advantaged children? Are guidance counselors the best delivery vehicle? Should teachers also be engaged in the process of spreading such information? Some disadvantaged parents manage to gain this information through existing institutional sources or through their own social networks (Brown 2017). Do we know enough about social capital processes to enable parents to increase the efficiency through which this information spreads? Are there ways to set up positive feedback loops in communities that increase the density of this information? Might this be connected somehow to collective efficacy?

The situation with respect to the valuable positive identity projects described by DeLuca et al. (2016) is in some ways similar to that of information about colleges, though probably more challenging. Like Duckworth's "grit," positive identity projects would seem to have beneficial mobility enhancing effects and could therefore be a source of inequality reduction. Is it feasible to increase their frequency through achievable investments in neighborhoods? Could such investments be enhanced through peer-to-peer transmission or through mentoring to create positive feedback loops? Feasibility research searches for positive feedback loops—the essential mechanism of cumulative advantage processes (DiPrete and Eirich 2006)—that might enhance the agency of lower and middle-income people and communities to improve their situation and thereby reduce inequality.

Collective efficacy offers another possibility for a positive feedback loop. Assuming that collective efficacy really does improve neighborhoods (which perhaps remains an open question), is it possible to improve the collective efficacy of neighborhoods? Is it possible to develop programs to improve relations between the residents of poor communities and the police that patrol these communities? Could these programs raise trust and lower the detrimental impacts of policing on these communities, including offsetting the potentially negative consequences of strained interactions with police on young people's educational attainment (Legewie and Fagan 2019)? More generally, can sociological research support the development of strategies to desegregate neighborhoods that are politically as well as economically feasible?

A similar issue comes up in the context of the coronavirus disease 2019 pandemic, which has already increased inequality in its differential economic and health impact on racial minority and working-class communities. To reduce the inequality-enhancing impact of the pandemic, the rate of infection and its consequences need to be mitigated. The intervention with the greatest potential is an effective vaccine. However, the impact of this vaccine will depend to a great extent on the proportion of the population who receive it, and current surveys suggest a substantial part of the population does not sufficiently trust the government and the medical establishment to seek vaccination (Gramlich and Funk 2020; O'Keefe 2020). Which social processes can reduce the impact

of misinformation? Which social processes can enhance institutional trust especially in light of historical and contemporary racism in the medical establishment? Recent events have demonstrated that there is a great deal that we do not know about how misinformation spreads and the conditions that are sufficient or necessary for it to be corrected.

These types of questions are part of or closely related to policy research. But they are also close to what we typically think of as academic social science research. They focus on description, causal effects, and mechanisms, but in a way that links up social science with the feasibility of change whether through policy intervention or through actions by individuals, organizations, and communities acting individually and collectively. In addition to researching the causes of inequality and its consequences, these types of questions involve research on the causes of conditions and actions that might reduce inequality in order to establish the feasibility of alternative strategies. Feasibility research searches for conditions under which particular actions have positive and durable outcomes, and it seeks to understand how policies or people could produce or enhance these actions in order to harness their potential to reduce inequality.

Are there strategies that could be undertaken that would push sociology in particular but also other social sciences toward the search for feasible strategies to reduce inequality as well as the types of descriptive, explanatory, and frame-shifting research that the field is good at, along with the rigorous evaluation of policies that some sociologists do and that are more common in other social science fields? One change might be to shift the culture of the field in a more optimistic direction so that the "what do we need to know to be able to design more effective policies" becomes a natural question to ask at the end of any study that has enhanced our understanding of a cause of policy. Another strategy is for the field to be more explicit in its reward structure to honor research that brings descriptive or explanatory social science and policy research closer together. Papers in the *Annual Review of Sociology* that involve inequality should routinely see as their mission a description of recent work that bridges between descriptive and explanatory research on the one hand and policy research on the other. They should also highlight questions without current answers for which additional research could construct additional bridges. We have given several examples in this article. Maybe in the near future the number of possible examples will have multiplied. At the same time, frame-shifting research is critical for building a foundation in the political culture that supports effectively designed inequality-reducing policies.[22]

Despite the centrality of frame-shifting research in sociology, there is not overwhelming evidence in recent decades that, by itself, it produces a large impact on society regardless of the political party that controls the presidency or Congress. Perhaps this is because of the increased polarization of our society and the increased disinclination of political leaders to engage in dialogue informed by social science findings. Given the current reality, it is possible that the goals of frame-shifting research would be enhanced if it were more frequently coupled with feasibility research to identify actions that are potentially effective at reducing inequality, even at a local level, enough to matter. If social science could enhance this potential, its research would be simultaneously frame shifting and problem solving, technological and practical, professional and public. This would strengthen social science claims to basic knowledge as well as to practical relevance. Such a state of affairs would not mirror the "optimizing relevant outcomes" mission that Coleman called on sociology to undertake, and it neither would nor should eliminate the healthy tension between problem solving and social criticism that must always remain a part of the social sciences. But it might bring the discipline to a more gratifying as well as socially beneficial place, and that would be a good thing.

[22]Frame-shifting research may, for example, have played an important role in the passing of President Biden's coronavirus disease 2019 relief bill, which contains a minimum income for families with children and, indeed, the fact that there was some bipartisan support for the concept even if the passage of the bill itself was accomplished exclusively with votes from Democrats.

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