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Inequality and Society: Mechanisms and Methods for Understanding the Consequences of Rising Income Inequality

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Inequality and Society: Mechanisms and Methods for Understanding the Consequences of Rising Income Inequality

by

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Abstract

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Professor Neil Fligstein, Chair

Income inequality has risen sharply in the United States over the past forty years, yet there remains substantial uncertainty about the consequences of income inequality on social life. This dissertation advances research on these consequences by focusing on mechanisms through which inequality may matter and on the methods by which the effects of income inequality are determined.

I primarily draw on data from the 1973–2014 General Social Surveys linked to administrative data of the demographic and economic characteristics of each respondent's state. This includes state-level income inequality, for which I utilize a new annual series based on household income tax returns. I also conduct an online survey experiment that manipulates perceptions of state-level income inequality.

First, numerous scholarly accounts posit that as income inequality rises, individuals will be less satisfied with their own finances as they feel increasingly deprived relative to others—driving individuals to try to spend more as they engage in positional competition and increasing their anxieties as position in the income distribution becomes ever more crucial. I find that higher state-level income inequality decreases financial satisfaction overall, and that this effect is especially pronounced for those in the middle of the income distribution. Counterfactual simulations suggest rising inequality explains a substantial portion of the over-time decline in financial satisfaction.

Second, concerns about rising income inequality are frequently linked to discussions about opportunity and mobility, yet little research explores if and how this inequality affects people's economic optimism, something with far reaching implications for life satisfaction, public opinion, and real economic mobility. Both the survey analysis and the experiment show that higher income inequality decreases economic optimism. The survey shows that the rate of change in inequality moderates the effect of the level of inequality, and that household income further moderates the effects of the level and change in income inequality on economic optimism. There was

no evidence of this moderation in the experiment. Key differences between the two methodological approaches are discussed.

Third, although both popular and scholarly accounts have argued that income inequality reduces trust, some recent research has been more skeptical, noting these claims are more robust cross-sectionally than longitudinally. Furthermore, although multiple mechanisms have been proposed for why inequality could affect trust, these have rarely been tested explicitly. I find little evidence that states that have been more unequal over time have less trusting people. There is some evidence that the growth in income inequality is linked with a decrease in trust, but these effects are sensitive to how time is accounted for. While much of the inequality and trust research has focused on status anxiety and feelings of relative deprivation, this mechanism receives the weakest support, and mechanisms based on societal fractionalization and exploitation receive stronger support.

Finally, social comparisons of income have far-reaching consequences for individual decision-making and public policy, yet there persists a significant gap between "true" relative income and what Americans perceive. Although one compelling explanation is that reference groups affect what people perceive as "average," there is little consensus about who people compare themselves with. Previous research has proposed reference groups based on both geographic proximity and on sociodemographic similarity, but few studies have considered multiple reference groups systematically or simultaneously. I find that the effect of reference group income depends on both egoist and fraternal comparisons: higher median incomes of large reference groups and those with weak status hierarchies increases perceived relative income, while higher median incomes of small reference groups and those with strong status hierarchies decreases perceived relative income. These results have important implications for how reference groups are used in research on neighborhood effects, residential segregation, and income inequality.

To Michelle

In the same week, we got engaged and I was accepted to graduate school.
Together we've made the seven years since good ones.

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These words do not do justice. But thank you all.

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Chapter 1

Introduction

1.1 More Inequality; So What?

Incomes in the United States are far less equal now than at any point since the Great Depression. This transformation is not only economic, but also social. Yet, at present, there is substantial uncertainty about the consequences of rising income inequality on social life. This dissertation advances research on the consequences of income inequality by focusing on mechanisms through which inequality may matter and on the methods by which the effects of inequality are determined.

The rise in income inequality in the United States has been documented in data from a variety of sources including the Census, the Current Population Survey, and the Internal Revenue Service's Statistics of Income reports (Piketty and Saez 2003 [2015]; Stone et al. 2016). Although the relative importance of the factors that led to this rise are debated, they at least include declining unionization (Western and Rosenfeld 2011), skill-biased technical change (Acemoglu 2002; Kristal 2013), financialization (Tomaskovic-Devey and Lin 2011; Lin and Tomaskovic-Devey 2013), declines in the real value of the minimum wage (Card and DiNardo 2002), and reductions in top marginal tax rates (Piketty and Saez 2003 [2015]; Piketty 2014).

However, explaining the consequences of income inequality (or even documenting them) has proven to be more elusive. That is, net of one's own income, does the income of others also matter? If so, on what outcomes? By how much? And through what mechanisms? These questions have recently spurred research among social scientists across an array of disciplines including sociology, economics, public health, public policy, and psychology. To be clear, current research consistently shows effects of *income itself* on important outcomes ranging from health and well-being to marriage and childbearing to political and social engagement. Thus, as income inequality grows, this mechanically means that inequalities in these outcomes (e.g., widening health disparities) also grow (Evans et al. 2004).

The effects this dissertation focus on are contextual. Net of income itself, does the *distribution of income* makes an additional difference on these outcomes? A recent body of work popularized by epidemiologists Wilkinson and Pickett has argued strongly that the level of income inequality (in addition to income) has negative effects on nearly every outcome of importance to social scientists (e.g., Pickett and Wilkinson 2015; Wilkinson and Pickett 2006; 2009; 2010). However, the empirical evidence, has been much less definitive (Deaton and Lubotsky 2003; Neckerman and Torche 2007; Truesdale and Jencks 2016; Kenworthy 2016).

This uncertainty is likely due to a number of factors. I specifically focus in this dissertation on two significant theoretical problems that have important implications for empirical research. The first problem is the underdevelopment and lack of testing of the specific *mechanisms* by which income inequality might matter. As I will show in an example below, the lack of mechanism development is particularly troubling given that different theorized mechanisms yield opposite effects. If income inequality affects important outcomes through multiple pathways, examining only the net effect limits our ability to fully understand, evaluate, and address the true consequences of income inequality. In the first three empirical chapters of the dissertation, I focus on explaining and testing three psychosocial effects of income inequality—factors that have been proposed as pathways through which income inequality has significant effects on individuals.

A second and related theoretical problem is the underdevelopment of theories about the level at which to measure inequality. By definition, inequality is a population-level characteristic, but is not clear what is the appropriate unit of aggregation at which to measure income inequality. To date, the data available make it difficult to study inequality at smaller levels, which may explain the tendency to measure inequality at fairly large units such as nations or U.S. states (which I do as well). But in my last chapter, I advance our understanding of which reference groups shape how people perceive their relative income. To the extent that inequality effects are driven by social comparisons to those in some reference group, these findings may help clarify which groups are most relevant.

Below, I describe both issues in greater detail and provide an overview of the remaining chapters of the dissertation.

1.2 What Mechanisms?

As scholars attempt to more carefully assess the effects of income inequality, relatively little attention has sought to evaluate the *mechanisms* by which inequality might matter. Though numerous mechanisms have been proposed, few are understood. This lack of understanding is the result of several factors.

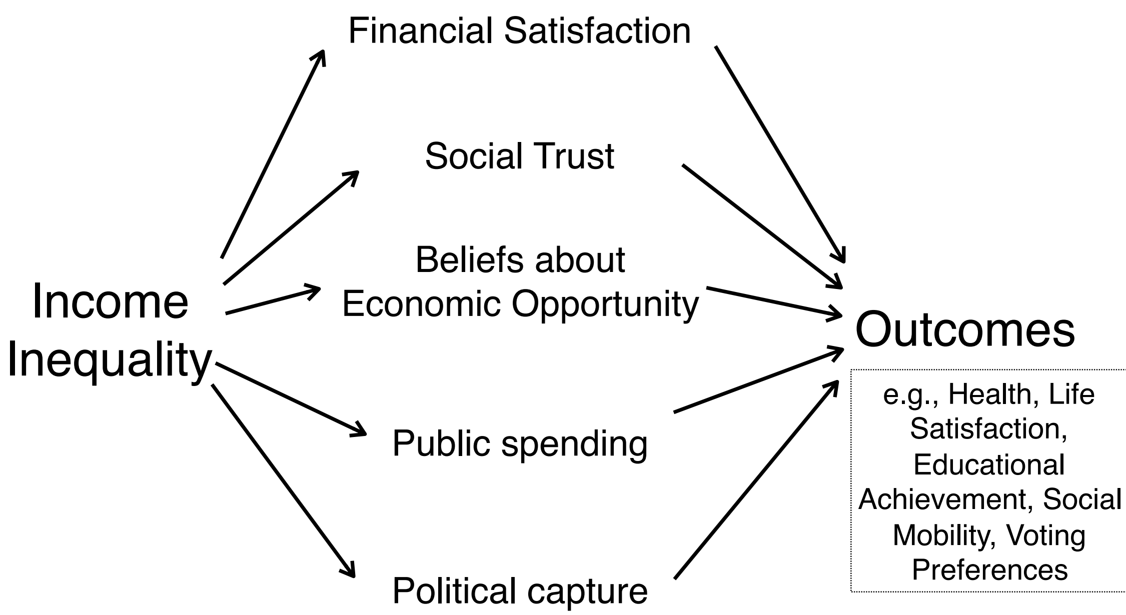
First, most research projects have typically focused on a single mechanism, rather than considering how multiple mechanisms might explain the same findings. In their extensive review of research on inequality, Neckerman and Torche (2007) identify this problem, writing "This research [on the consequences of inequality] is characterized by premature theoretical closure, with an overemphasis on a few pathways... more generally, research on contextual effects of inequality is characterized by insufficient attention to the mechanisms through which inequality might matter." (p. 349).

Second, research has often treated the unit of analysis as the area (e.g., examining the average difference on some outcome between equal and unequal counties, states, societies, communities, etc) instead of the individual, which means important differences between individuals at different places in the income distribution cannot be ascertained. Such differences are not only in and of themselves substantively interesting, but could also be helpful to evaluate multiple mechanisms.

Third, research has often not focused on the outcomes that would actually be useful to evaluate the assumed mechanisms. For example, mortality—though critically important—could be associated with inequality for a number of reasons, while outcomes that could be directly connected with some mechanisms but not others (and thus allow one to find evidence for some over others) are less frequently considered.

The lack of mechanism development is particularly troubling given that many theorized mechanisms could possibly yield opposite effects. Consider the examples illustrated below in Figure 1.1. Five possible mechanisms are given as to how income inequality could causally affect important outcomes such as health, life satisfaction, and mobility. First, inequality may increase feelings of relative deprivation that increases anxiety and stress which in turn generate other negative outcomes (e.g., Wilkinson and Pickett 2009). Second, inequality could decrease social trust, which would reduce social cooperation and coordination that would otherwise have benefits for individuals and society (e.g., Kawachi et al. 1997). Third, inequality might make people pessimistic about their future and less motivated to invest in themselves (e.g., Kearney and Levine 2016). Fourth, higher inequality might also mean that, because the rich are richer and they pay high taxes and give to philanthropic causes, they pay for better medical facilities, educational resources, and amenities. These "public goods" could, in fact, actually improve the lives of everyone (e.g., Boustan et al. 2013). On the other hand, fifth, as inequality increases, the rich might obtain more political power and try to limit public spending on things that benefit everyone and direct more of the spending towards what benefits themselves (e.g., Bartels 2008).

Figure 1.1: (Non-Exhaustive) Examples of Mechanisms Through Which Inequality Could Matter



Though this list is by no means exhaustive, variants of all of these arguments have been attached to research on a broad number of outcomes. Clearly, then, considering multiple pathways is important to understand the effects of income inequality. Why are attempts to adjudicate between mechanisms rare? Part of it may be a problem of disciplinary boundaries. For example, public health scholars have done much to advance these mechanisms, but they have typically focused directly on various aspects of health. This is clearly an important outcome worth the tremendous attention paid to it. But only researching inequality effects and pathways though health may not be the best approach to understand mechanisms. Health could quite reasonably be affected by every single mechanism described above and the mechanisms themselves could mediate each other before affecting health. Furthermore, since not every pathway between inequality and health can be measured, identification is challenging (and in some cases, perhaps impossible).

Given the limitations of this "social indicators" approach (Neckerman and Torche 2007), it may be helpful to study inequality mechanisms by analyzing outcomes that we expect to be affected by only one (or a limited number) of mechanisms. In the first three empirical chapters, I examine the effect of income inequality on three outcomes corresponding to the first three pathways in Figure 1.1. Namely, I examine the effect of state-level income inequality on financial satisfaction, economic optimism, and trust. All three outcomes are frequently proposed as part of a causal pathway between income inequality and critical social problems, but the relationship between inequality and each factor is rarely examined directly. Consistent with the attention to mechanisms, I not only estimate these associations, but also consider and test the multiple explanations for these relationships as well.

1.3 What Level of Inequality?

The theories and research described so far motivate looking at how individual-level outcomes are shaped by area-level inequality. But, it is not clear what the appropriate unit of aggregation is to measure income inequality. It is unlikely, for example, that there should be the same effects of inequality measured at the national level, state level, city level, county level, or neighborhood level. Furthermore, inequality need not only be defined in geographic terms. The degree of inequality within (or between) different racial groups, occupations, educational degrees, or life stages could also have consequences.

For example, Wilkinson and Pickett (2006; 2009) found stronger negative effects on health using cross-national studies, with less consistently strong effects for subnational studies (e.g., states), and even weaker effects for small areas, such as neighborhoods. This prompts them to suggest that larger areas are better, because the relevant area for health outcomes is how hierarchical a society is, whereas small areas are affected by the degree of residential segregation of rich and poor—that is, the amount of inequality between areas, rather than within them.

Other work on relative deprivation suggests that comparisons at different levels can actually work in opposite directions. Firebaugh and Schroeder (2009) found that Americans tended to be happier when they reside in richer neighborhoods (consistent with neighborhood studies literature) but in poorer counties (consistent with the relative income hypothesis). The appropriate reference group may not even be the same across the income distribution itself. At the extreme high end of the earning distribution, one may expect that comparisons are made on a global level (e.g., billionaires competing over the biggest luxury yacht [Knecht 2013]), while at the other end, the neighborhood block or census tract may suffice.

Some material mechanisms focus on the local economy or labor market, suggesting units such as MSAs or Commuting Zones should be the preferred unit, although data at the state and county level is more consistently available. Mechanisms that focus on political power and redistribution might be best studied at the levels where taxation is determined–which is primarily the national and state level.

In short, there rarely, if ever, appears to be a clear way to determine the appropriate population unit to measure inequality. This suggests it is important to reflect on this decision and recognize that finding an effect at one level does not mean one should find support at every other level. Likewise, lack of empirical support for a mechanism at one level does not mean the lack of support at every level.

In my last empirical chapter, I analyze how reference groups affect one's perception of their income relative to others. To the extent that the effects of inequality depend on relative comparisons between households, this analysis helps understand what levels for measuring inequality may be particularly useful to study.

1.4 Overview of Chapters

The dissertation chapters proceed as follows. Chapter 2 examines how inequality shapes individuals' satisfaction with their own financial situations. I analyze the 1973–2012 General Social Surveys linked to state-level administrative data from IRS tax reports, the Census, and the American Community Survey. I show that higher state-level income inequality decreases financial satisfaction overall. I further show that this effect is especially pronounced for those in the middle of the income distribution. These findings are consistent with accounts that there is an escalating "positional arms race" driven by inequality in which middle-class households experience a greater gap between their "American dream" aspirations and economic reality (Frank 2007; Schor 1998; Fligstein and Goldstein 2015). I then use counterfactual simulations to show that rising inequality explains a substantial portion of the over-time decline in financial satisfaction.

Concerns about rising income inequality are frequently linked to discussions about opportunity and mobility. In Chapter 3, I examine if and how inequality affects people's economic optimism—that is, individuals' beliefs about opportunities and expectations for future social mobility. I describe how existing theories imply that economic optimism should be shaped by both the level and rate of change of income inequality. The research design has two components. First, I analyze the 1987–2012 General Social Surveys linked to state-level administrative data. Then, I conduct an online survey experiment that manipulates perceptions of state-level income inequality. Both studies show that higher income inequality decreases economic optimism. The survey analysis shows that the rate of change in inequality moderates the effect of the level of inequality, and that income further moderates the effects of the level and change in income inequality on economic optimism. There was no evidence of this moderation in the experiment. Key differences between the two methodological approaches are discussed.

In Chapter 4, I ask if income inequality affects social trust. Although both popular and scholarly accounts have argued that income inequality reduces trust, some recent research has been more skeptical, noting these claims are more robust cross-sectionally (comparing inequality and trust in different places) than longitudinally (comparing inequality and trust over time in the same place). I examine the effect of income inequality on trust using the 1973–2012 General Social Surveys linked to state-level administrative data. This analysis improves on previous estimates of the effect of state-level income inequality on trust by using far more available observations, accounting for more potential individual- and state-level confounders, and using higher-quality income inequality data based on annual IRS tax returns. In contrast to previous studies, I find little evidence that states that have been more unequal over time have less trusting people, but I do find some evidence that the growth in income inequality is linked to a decrease in trust. However, these longitudinal effects are sensitive to how time is accounted for. I also consider the mechanisms that could explain the effect. While one popular and highly cited component of the inequality/trust literature has focused on status anxiety and feelings of relative deprivation, this mechanism receives the weakest support. In contrast, there is support for mechanisms based on societal fractionalization and on perceived exploitation by the rich.

In Chapter 5, the final empirical chapter, I examine the gap between households' "true" income relative to others and what they perceive. Although one compelling explanation for this gap is that reference groups affect what people perceive as "average," there is little consensus about who people compare themselves with. Previous research has proposed reference groups based on both geographic proximity and on sociodemographic similarity, but few studies have considered multiple reference groups systematically or simultaneously. Using the 1998–2014 General Social Surveys linked to administrative data, I find that the effect of reference group income depends on both egoist and fraternal comparisons. Higher median incomes of large reference groups and those with weak status hierarchies increases perceived relative income, while higher median incomes of small reference groups and those with strong status hierarchies decrease perceived relative income. These results have implications for how reference groups are used in research on income inequality, as well as for studies of neighborhood effects and residential segregation. Finally, I conclude in Chapter 6 by summarizing the main findings and presenting some possible future directions for this work.

Chapter 2

Who Feels It? Income Inequality and Financial Satisfaction in U.S. States, 1973–2012

Numerous scholarly accounts posit that as income inequality rises, individuals will be less satisfied with their own finances as they feel increasingly deprived relative to others—driving individuals to try to spend more as they engage in positional competition and increasing their anxieties as position in the income distribution becomes ever more crucial. To examine if and how income inequality reduces financial satisfaction, I conduct an individual-level analysis of the 1973–2012 General Social Surveys linked to state-level administrative data based on IRS tax reports, the census, and the American Community Survey. I find that higher state-level income inequality decreases financial satisfaction overall, and that this effect is especially pronounced for those in the middle of the income distribution. Counterfactual simulations suggest rising inequality explains a substantial portion of the over-time decline in financial satisfaction.

2.1 Introduction

Income inequality in the United States has risen sharply over the past forty years. In 1973, the top 10% earned one-third of the national income, but by 2012 their income share was just over one-half (Piketty and Saez 2003 [2015]). Over the same time period, the national Gini index in the United States rose from .47 to .64 (Frank 2014).[1] Scholars, pundits, and public figures alike have raised alarm about this trend, suggesting high or rising income inequality leads to a variety of negative outcomes.[2]

Many of the mechanisms to explain why inequality could or should matter utilize the concept of relative deprivation (Merton and Rossi 1950; Davis 1959; Runciman 1966). That is, in modern, developed economies inequality does not deprive people of seemingly life-sustaining necessaries (i.e., food, shelter, basic health care), but rather, because it makes them less well-off relative to others above them, inequality restructures people's evaluations of their own financial well-being. More negative evaluations then affect decisions about work, leisure, rest, debt, housing, policy preferences, family, and more. However, to date, definitive empirical evidence of these links remains elusive (Neckerman and Torche 2007; Moss et al. 2013).

Early scholarship defined relative deprivation as an objective measure of where one stood relative to others in terms of income (Yitzhaki 1979; Adjaye-Gbewonyo and Kawachi 2012). Yet, the theories of why inequality matters depend not just on objective position, but the extent to which one *feels* deprived. Income inequality may matter when it widens the gap between one's economic position and where one feels they "ought" to be based on what they see in others.

To see if income inequality affects these feelings, I study whether the level of income inequality—net of own's own income—reduces financial satisfaction. Financial satisfaction has indeed declined within the U.S. for at least the past 40 years, during the same time period that income inequality has increased. But are these causally linked? To better answer this question, this paper examines the empirical relationship between state-level income inequality and financial satisfaction.

The paper proceeds as follows. First, I elaborate on theories of relative deprivation and their link to financial satisfaction. Then, I explain why these may be affected by income inequality. I describe how the effects of income inequality may vary widely by one's position on the income distribution, but show that there are competing theories about which part of the distribution should be most affected by income inequality. Based on these theories, I develop a set of competing hypotheses that I test by analyzing the effect of the state-level Gini index on financial satisfaction using the

[1] These inequality calculations are from IRS tax data. Alternative calculations from the CPS and Census Bureau show a similar rise, though the values differ because of how that income data is collected.

[2] They may be talking about economic inequality, more generally. But in the U.S. income inequality has been a driving factor of other forms of economic inequality (e.g., wealth or consumption inequality) (Piketty 2014; Saez and Zucman 2016).

1973–2012 General Social Surveys linked to state-level data based on IRS tax returns, the decennial Census, and the American Community Survey.

Using models that account for time-invariant differences between states and that include a wide range of individual- and state-level controls, I find evidence that income inequality reduces financial satisfaction. Furthermore, I find that this effect of income inequality is most pronounced for those in the middle of the income distribution. I assess the robustness of these results to multiple measures of income inequality, various modeling approaches, and different contextual and individual covariates. Finally, I use a counterfactual simulation to show how income inequality has affected financial satisfaction since 1973.

This paper contributes to the sociological study of income inequality by focusing on feelings of relative deprivation and financial satisfaction—factors scholars have suggested should be a key mediator between income inequality and critical social problems, but that have rarely been examined directly. It makes an empirical contribution through a rigorous analysis of the effects across the income distribution of over four decades of growing income inequality in U.S. states.

2.2 Relative Deprivation and Financial Satisfaction

Scholars have studied financial satisfaction—that is, an individual's subjective evaluation of their financial situation—as both a "sub-construct" or domain of subjective well-being and life satisfaction (e.g., Van Praag et al. 2003; DePianto 2011; Diener and Biswas-Diener 2002) and as an important component of perceived economic standing (e.g., Kalleberg and Marsden 2012; Joo and Grable 2004). Unsurprisingly, there is a robust association across a number of contexts between the income of an individual or household and their level of financial satisfaction (e.g., DePianto 2011; Diener and Oishi 2000; Easterlin 2006). Additional work has shown financial satisfaction to be a mediator between income and overall or "global" life satisfaction (see review by Diener and Biswas-Diener 2002).

Yet research also suggests that the ability to derive satisfaction from income is highly context dependent—not only does it matter what one makes, but also how that stands in relation to others. One of the most concise accounts of this is articulated in the Easterlin Paradox, which notes that although individuals with more income report being happier, an increase in average income over time for a country does not translate into greater happiness overall in that country (Easterlin 1973). At present, some scholars debate whether average income really lacks *any* explanatory power on happiness (see Stevenson and Wolfers 2008; 2013, and responses by Easterlin et al. 2010). Also others suggest that absolute income may indeed have some positive effects, but that in the U.S. the efforts to obtain that income—namely, working more hours and increasing the number of dual-earning households—have had counteracting negative effects (Fischer 2008; Schnittker 2008). Still, there remains ample evidence that relative income is an important determinant of financial—and ultimately—overall satisfaction.

In fact, social comparisons of income and other material goods have been foundational to the social sciences. In *The Theory of the Leisure Class*, Veblen ([1899] 1963) described how social status is demonstrated and reified through visible forms of consumption that others can see and compare themselves against. Duesenberry's (1949) relative income hypothesis stated that people's consumption and savings behaviors depend heavily on income in relation to others. And Marx (1847 [1972]) illustrated the idea when writing that, "A house may be large or small; as long as the neighboring houses are likewise small, it satisfies all social requirement for a residence. But let there arise next to the little house a palace, and the little house shrinks to a hut." (p. 33)

These early ideas were formalized in the theory of "relative deprivation" which assumes that the comparisons individuals make with others who are more advantaged can cause them to feel deprived (Merton and Rossi 1950; Davis 1959; Runciman 1966). Rather than lacking in absolute terms—such as being without adequate shelter, clothing, or food—factors that are more relevant to conceptions of poverty (Iceland 2012), relative deprivation focuses on lacking the goods or resources that one desires or feels necessary to fully participate in society (Runciman 1966; Adjaye-Gbewonyo and Kawachi 2012).

Building on this understanding, financial satisfaction is clearly related to this theoretical conception of relative deprivation. In *Falling Behind*, Frank (2007) emphasizes that such comparisons that would lower financial satisfaction need not be made out of envy. Rather, it is "fundamentally about the link between context and evaluation" (p. *xx*). Wants and felt needs are shaped by one's context—the greater the standards, the more income necessary to be satisfied, and the easier it becomes to be dissatisfied with the income one has. Note it is not necessary to know exactly what others make, rather this is ascertained in the visible forms of consumption that people participate in. And these particular things that can become sources of evaluation are aptly described as "positional goods" (Hirsch 1977). Yet the resulting potential drawbacks from this competition are two-fold: not only does it require increasingly more income to acquire them, but, in turn households may also be prompted to reduce their consumption of so-called "non-positional goods" such as vacations, leisure time, and sleep in order to focus on income accumulation.

2.3 The Effect of Income Inequality

Does rising inequality increase these feelings of relative deprivation and decrease financial satisfaction? As income inequality grows, so does the distance between those at different parts of the income distribution, resulting in greater contrasts and more opportunities for negative comparisons.

Previously, most attempts to operationalize "relative deprivation" define it as a function of one's position in the income distribution, regardless of one's perception of that position. This is exemplified in Yitzhaki's (1979) definition of relative deprivation as the average of the income differences between a person and everyone else in the population who has a higher income than them. Yitzhaki's measure and variants of it remain the standard (see Adjaye-Gbewonyo and Kawachi 2012), but this measure mechanically links relative deprivation and income inequality. For any population, the average level of Yitzhaki's relative deprivation is the mean income multiplied by the Gini index, meaning that—net of mean income—any society with more income inequality will have more relative deprivation (Evans et al. 2004). But while rising inequality means people are objectively more deprived relative to others, existing work has not demonstrated to what extent people may or may not feel that deprivation and be less financially satisfied as a result.

Scholars have described two explanations of how income inequality could reduce financial satisfaction. One view focuses on how relative deprivation drives others to overspend to "keep up with the Joneses" or to be dissatisfied when they do not keep up. For example, Frank (2007) argues inequality arises when the growth of top incomes sets off a chain of local comparisons that cascade all the way down to low income earners. He describes this as the increasing "cost of adequate" which leads to dissatisfaction, stress, and other adverse outcomes.

Another view focuses on how relative deprivation may result in anxiety over one's place in the social hierarchy of status and prestige. As inequality increases, one's position becomes ever more crucial. Scholars have argued that this leads to status anxiety and increases levels of stress, which then results in a number of negative consequences that are psychological, physical, and social. Among the leading proponents of this position are Wilkinson and Pickett, who popularized this argument in *The Spirit Level* (2010). They use both cross-national and between-state comparisons to argue that the additional status anxiety generated by income inequality can explain associations between income inequality and loss of community life; poor mental and physical health; higher drug use, obesity, mortality, teen births, and crime; and lower educational performance (see also Wilkinson and Pickett 2009).

But Wilkinson and Pickett do not measure feelings of relative deprivation or financial satisfaction. Rather, they argue that the evidence supports their argument because the outcomes associated with income inequality have a "status gradient." That is, health and social problems that are typically more common lower down the income distribution (i.e., they have a status gradient) are also more common in unequal contexts, while problems without a status gradient are less associated with inequality. From this they infer that income inequality has effects through the relative differences between individuals. While "status" is presented broadly, implicit in these arguments is an increasing concern with one's income and where it stands in relation together.

Both arguments suggest that rising income inequality should be a cause of less financial satisfaction (to be clear, neither articulation supposes that people know the level of inequality—only that they experience it and navigate life through it), something this paper tests empirically. Although no study purports to do so directly, two studies use similar arguments to test related outcomes. One study examined the effect of income inequality on status anxiety in a cross-sectional study of European countries (Layte and Whelan 2014). Respondents were asked the extent to which they agreed with the statement "Some people look down on me because of my job situation or income." On average, individuals in more unequal countries were more likely to agree. In contrast, scholars in another study of European countries analyzed questions about status seeking and found that people reported being *less* concerned about status seeking in more unequal contexts (Paskov et al. 2017). They suggest as inequality rises, people may "give up" on their efforts to keep up. Comparable studies in the U.S. context do not yet exist.

The above discussion suggests there may be, on average, a negative effect of income inequality on financial satisfaction, leading to the following *ceritus paribus* hypothesis:

> *Hypothesis 1: Income inequality is associated with lower financial satisfaction.*

2.4 Heterogenous Effects

Many studies—both theoretically and empirically—have focused on the overall (i.e., population-average) effect of income inequality. Practically speaking, some of this is because much of the work on the effect of income inequality has been conducted using outcomes at the aggregate level, rather than at the individual level. Although sometimes adequate data exist only at the aggregate level, the problem of an ecological fallacy can occur when inferring individual-level processes from aggregate data (Evans et al. 2004). Beyond the ecological fallacy concern, aggregate data can, at best, only establish population-level effects. This makes it impossible to distinguish how income inequality may affect people differently who are at different places on the income distribution.

As noted above, prior scholarship has clearly established that income is positively associated with financial satisfaction. There are also compelling arguments that the effect of inequality itself on financial satisfaction should vary across the income distribution, but from the existing theories—all of which suggested an *average* decline in financial satisfaction—one can infer differing expectations about how this effect would (or would not) vary across the income distribution.

Effects smallest at top; largest at bottom

Relative deprivation theory emphasizes the negative comparisons that people make with those who are more advantaged. The further down one is on the income distribution, the more opportunities one has to make negative comparisons. Conversely, the higher up one is, the fewer negative comparisons there are to make. By definition, greater income inequality means a stretching of the income distribution, and thus, the relative deprivation of everyone not at the top increases.

We know there is an association between relative income and financial satisfaction, so we expect that, *ceritus paribus*, the effect of inequality should be smallest at the top of the income distribution and largest at the bottom of the income distribution. Layte and Whelan (2014) similarly proposed that the negative effect of individual income rank on status anxiety would be exacerbated (positively moderated) by increasing income inequality, but they did not find support for this hypothesis. However, financial dissatisfaction from feelings of relative deprivation is a different concept than status anxiety. For example, while it is plausible that feelings of relative deprivation may induce status anxiety, feeling deprived may also lead to overspending and financial dissatisfaction even among those who do not feel negatively judged because of their income (or lack thereof).

> *Hypothesis 2a: The association between income inequality and financial satisfaction is least negative for those at the top of the income distribution and increases for those lower on the income distribution (i.e., there is a steeper income gradient between individual income and financial satisfaction when inequality is higher).*

Relative deprivation theory typically assumes individuals mostly ignore those who are less advantaged than them. But an extension of this is "Relative Gratification Theory" which proposes that comparisons with those who earn less will make individuals feel better (Davis 1959; Evans et al. 2004). If people compare themselves to others both above and below them, then not only would we expect a steeper gradient between income and financial satisfaction, but we may find a positive effect of income inequality for those near the top of the income distribution.

> *Hypothesis 2b: The association of income inequality and financial satisfaction is positive for those near the top of the income distribution.*

Effects largest in the middle

In contrast, because income inequality in the U.S. has been primarily driven by the growth of top incomes (Piketty and Saez 2003 [2015]), those in the middle may be more sensitive to these gains, while those at the bottom (correctly) may not perceive those in the middle pulling away so much. This makes sense if what occurs is not comparisons to those throughout the entire income distribution, but local comparisons to those just above. This is precisely Frank's (2007) account: for the middle class, the losses from the "positional arms race" have been made worse by rising inequality, and through a chain of local comparisons, they spend more while diverting resources away from non-positional goods, causing large welfare losses.[3]

A variant of this explanation has a more cultural emphasis. Those near the middle may experience the greatest gap between their aspirations and economic reality. Studies suggest that a middle class lifestyle in the U.S. includes owning a house and car and having a good job, some modest savings, and enough money to pay for one's children's college education(s)—yet those in the middle often do not have the means to pay for these things (Schor 1998). As such, they may be the most discontent when they fail to achieve these goals or they may take on considerable debt to meet these aspirations (Fligstein and Goldstein 2015). Higher earning households may be able to meet more of these "middle-class" goals, while those at the bottom may recognize that these things are out of reach.

> *Hypothesis 3: The association of income inequality and financial satisfaction is most negative in the middle.*

Same effects across the income distribution

Finally, the work of Wilkinson and Pickett emphasizes similar effects across the income distribution. If inequality increases status anxiety by making everyone feel insecure about their position (regardless of whether they are high or low), then these effects should be felt everywhere.[4] This is also consistent with the findings mentioned above that income inequality increased self-reported status anxiety, but this was not moderated by income (Layte and Whelan 2014).

> *Hypothesis 4: The association of income inequality and financial satisfaction does not differ by one's place in the income distribution.*

2.5 Data and Methods

General Social Survey

The individual-level data come from the nationally-representative General Social Survey (GSS) which has been conducted annually or biannually since 1972 with response rates greater than 70% (Smith et al. 2013). Unlike other long-running nationally representative surveys, the GSS asks a large number of questions about attitudes, including financial satisfaction. I obtained the restricted-access GSS geographic identification file, which contains state identifiers for every survey year beginning with

[3] Hence Frank's (2007) book's unsubtle subtitle: *How Rising Inequality Harms the Middle Class.*

[4] The only caveat is that Wilkinson and Pickett cannot observe the very top of the income distribution, such as the super rich (e.g., the top 5% or 1%). Unfortunately I cannot either, but at least for everyone else, the hypothesis should apply.

1973, and I matched each respondent to a variety of state-level measures described below.[5]

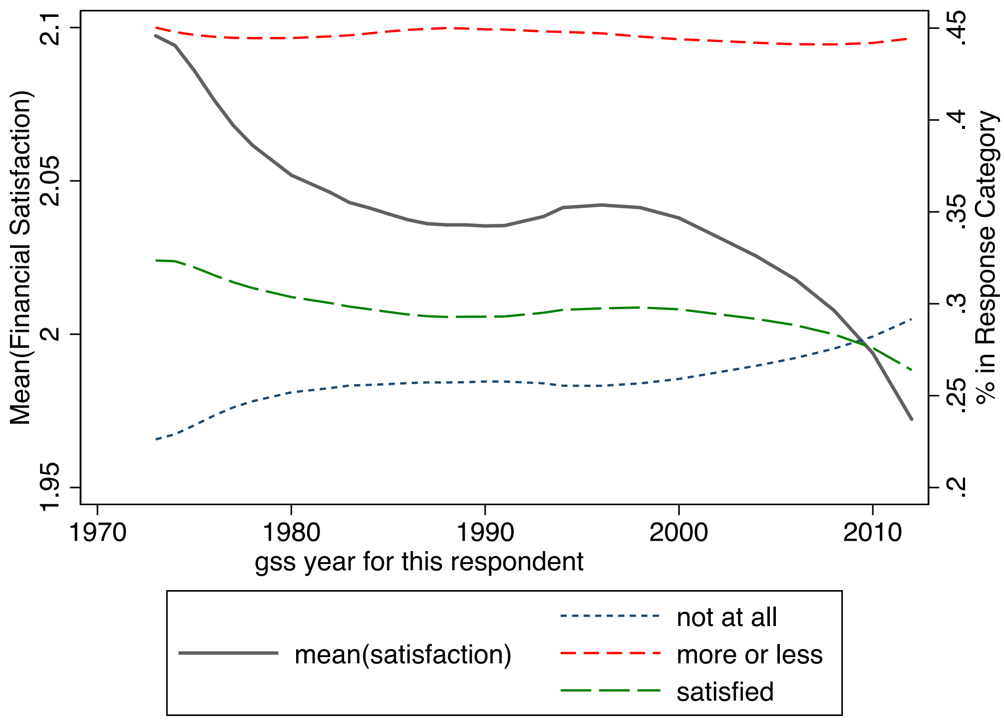
Outcomes

Financial satisfaction is measured with the question "So far as you and your family are concerned, would you say that you are pretty well satisfied with your present financial situation, more or less satisfied, or not satisfied at all?" (codebook item: satfin). In the main analysis I present this as a continuous variable, but I also found substantively identical results when using ordered logistic regression and multinomial logistic regression.

Figure 2.1 shows how this variable has changed over time. There has been an overall decline of about .15 on the 3 point scale (≈ 0.2 standard deviation decline). By looking at the change in each response category, it is evident that this change is driven by a decline in the proportion of respondents who report being "satisfied" (from 32% to 26%) and an increase in "not at all satisfied" (from 24% to 29%), while the proportion who are "more or less satisfied" has remained about the same (around 45%).

[5] Data from the GSS, Census, ACS, and BLS are available from the respective websites of each. The inequality data based on IRS tax returns are available at http://www.shsu.edu/eco_mwf/inequality.html. However, the GSS state-level identifiers can only be obtained through special contractual arrangements with NORC: http://gss.norc.org/documents/other/ObtainingGSSSensitiveDataFiles.pdf

Figure 2.1: Lowess Plots of Financial Satisfaction from 1973 to 2012 for each Response Category and the Overall Mean (Source: General Social Survey)



Individual-level Independent Variables

Income was measured by asking respondents to place their household's income into an income bin. The bins have changed over time, but the GSS provides a harmonized "best estimate" by assigning each respondent the midpoint of their income bin, except for the open-ended top income bin where incomes were assigned using a Pareto curve (Ligon [1989] 1994; Hout 2004). I divide the respondents into income quintiles by year. I found similar results when using 7 and 10 groups rather than 5, and when dividing the total sample into quintiles, instead of for each year. I also found similar results by dividing each household's income by their state-year median and then separating this state-year adjusted income into quintiles by year.

To control for characteristics that might bias the relationship between income inequality and the dependent variables, I include measures for the respondent's sex, age, age squared, race/ethnicity (non-Hispanic white, non-Hispanic black, other non-Hispanic, and Hispanic), years of education (using highest degree earned made no change to the main findings), marital status (married, widowed, divorced, separated, or never married), number of children in the household, number of adults in the household, work status (full-time, part-time, temporarily not working, unemployed, retired, in school, keeping house, other), political party affiliation (Republican, Democrat, independent/other), religious service attendance (from 0 = never attend to 8 = attend more than once a week), and type of area of residence (urban, suburban, or rural). The GSS did not measure Hispanic or Latino ethnicity until 2000, so for observations before that year I use country of ancestral origin. This correlates extremely well with self-identified Hispanic from 2000 onward (the tetrachoric correlation is .99) and has been used in previous research on race and ethnicity (Hout and Goldstein 1994).

Area Level Data

Income Inequality

While the outcomes are at the individual level, inequality itself is a population-level characteristic. This paper argues that financial satisfaction may be shaped by area-level inequality, but it is not readily apparent what the appropriate unit of aggregation is to measure income inequality. I focus on U.S. states for both theoretical and pragmatic reasons. First, although some studies measure inequality at the local level, scholars have argued that relative deprivation should operate at a larger level such as the state (Wilkinson and Pickett 2009). This may seem counterintuitive if one expects the psychosocial effects to result directly from face-to-face encounters with others, but the processes described above also depend heavily on the extent of the separation between those on different parts of the income distribution which may not be captured when looking at the inequality of smaller areas.

Second, states have been used extensively in prior studies of income inequality effects and evidence suggests there are net state-level income inequality associations for some of the outcomes that could result via financial dissatisfaction. Third, policy interventions are frequently applied at the state level, so understanding whether psychosocial effects appear at the same level may be especially useful for those interested in the potential policy implications of these analyses.

Fourth, unlike for sub-state areas, excellent annual state-level income inequality data are available (described below).

Fifth, unlike counties, MSAs, and other sub-state areas, states have maintained consistent boundaries over time.

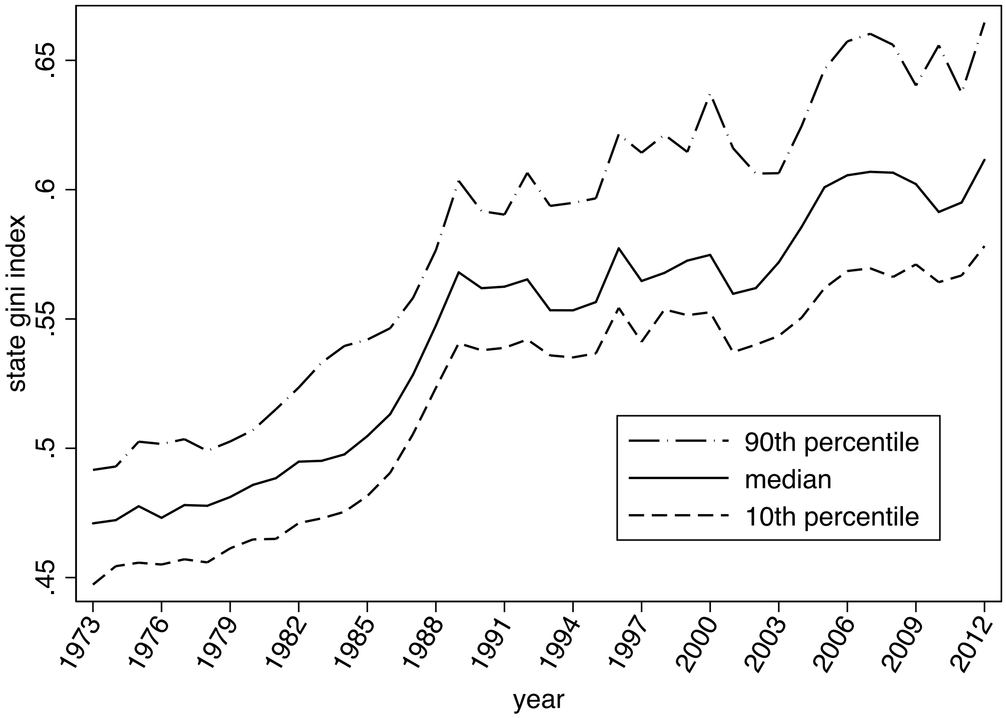
Finally, sixth, the GSS geo-coded data only has location identifiers below the state beginning in 1993, making it impossible to examine the effect of inequality during the first 20 years of the survey for smaller levels. Excluding these respondents would dramatically reduce the sample and omit about half the income inequality growth between 1973–2012.

My inequality measure is the state-level Gini index of income inequality. The Gini is the average distance between all pairs of proportional income in the population, ranging from 0 (if everyone had the same income) to 1 (if one household had all the income). The Gini assesses inequality across the entire income distribution and is the most common inequality measure in previous research.

The Gini is constructed from data published in the IRS's *Statistics of Income* and made available by Frank (2014). A limitation of IRS data is that it risks censoring households whose income is below the threshold for mandatory tax filing. However, these households may still file taxes to receive a refund or to benefit from tax credits. Alternative state-level data sources of income inequality come from the Current Population Survey (CPS) and the Census Bureau (decennial Census until 2000 and American Community Survey (ACS) afterwards) (Stone et al. 2016), but I prefer the IRS-based measures because (1) the Census was only collected each decade whereas I am using annual measures, (2) the ACS and CPS are based on much smaller sample sizes than the IRS data, (3) the other data sources rely on self-reported data where lower-income respondents are more likely to over-report income and higher-income respondents are more likely to under-report income than in the IRS which penalizes respondents for their income reporting errors (Akhand and Liu 2002), and (4) the other data sources are subjected to more top-coding of income, and as a result, the Gini indices based on the IRS data are substantially larger (i.e., more inequality) than those reported by the Census or CPS because they better account for high-income households (Richard et al. 2009).

Some recent research has focused on top income shares (Frank 2014; Piketty and Saez 2003 [2015]), but the hypotheses tested here would seem to be sensitive to changes in income inequality across the entire distribution, so I focus on a measure that is similarly sensitive to these changes. Figure 2.2 shows the Gini index across states during the analysis period. Two trends are notable. First, consistent with other accounts of income inequality, inequality grew considerably over the period of study. In fact, the Gini of the least equal state in 1973 (.50 in the District of Columbia) was lower than in the most equal state in 2012 (.55 in West Virginia). Second, the gap between the 10^{th} and 90^{th} percentiles is nearly twice as large in 2012 as 1973, meaning that inequality grew substantially more in some states than others.

Figure 2.2: 90th, 50th, and 10th Percentiles of the State Gini Index of Income Inequality, 1973–2012 (Source: Frank 2014)



Other Area Level Data

I control for state-level median household income (logged) which is often used as a measure of standard of living and is less sensitive than mean income to measurement errors of high income. I control for changing racial composition with the percent black in each state-year. Deaton and Lubotsky (2003) argue that race is a confounder of income inequality effects, showing in one analysis that the inclusion of the proportion black explains away a relationship between state-level income inequality and mortality. This study has since been challenged in several respects (Ash and Robinson 2009), but Pickett and Wilkinson (2015) question whether such explanations are even theoretically appropriate, suggesting that "ethnic differences attract more attention and seem more important not only when they become markers of social status differences but also when greater inequality makes social status differentiation more powerful, increasing the important of 'downward' social prejudices whether by class or ethnicity." (p. 7). Thus, controlling for race presents a more conservative estimate. Hispanic or Latino ethnicity was not consistently recorded in the census until 1980 and is not included, but I control for the percent foreign born in each state-year. Finally, I control for state population density (logged) to account for population shifts and growth. These measures come from the decennial Census for 1970-2000 (except that households reported the previous year's income, so median income is measured for 1969, 1979, 1989, and 1999) and the annual ACS for 2005-2012. I linearly interpolated between data points to create annual measures, except for percent black, for which model-based annual estimates are provided by the Census Bureau. Descriptives of all individual and state-level measures are presented in the Appendix.

Analytical Strategy

The analytic approach is to conduct an individual-level analysis of financial satisfaction. Though much prior work on the effects of income inequality has been conducted at the aggregate level, this research is vulnerable to an ecological fallacy. Moreover, aggregate outcomes can, at best, only establish population-level effects, making it impossible to distinguish how income inequality may affect people differently across the income distribution, something this paper also proposes.

I estimate a linear regression model with state fixed effects and time-varying controls. Many studies of income inequality employ cross-sectional comparisons across countries or states at a single point in time, but this design risks confounding the effects of income inequality with unobserved characteristics of areas that shape both income inequality and the outcomes being studied. Panel data can exploit within-area temporal variation in income inequality and net out unobserved time-invariant characteristics, as I do with the inclusion of state fixed effects. In all, I analyze 51,699 observations in 1,063 state-years.

Formally, consider observation i in state s collected in year t. The regression equation (with bolded terms indicating vectors) is,

$$\begin{aligned} Satisfaction_{ist} = {} & \beta_0 + \beta_1 Gini_{st} + \boldsymbol{\beta_2 Income_{ist}} \\ & + [\boldsymbol{\beta_3}(Gini_{st} \times \boldsymbol{Income_{ist}})] \\ & + \boldsymbol{\beta_4 Controls} + \boldsymbol{\mu_s} + \epsilon_{ist} \end{aligned}$$

where $Satisfaction_{ist}$ is the respondent's reported financial satisfaction, $Gini_{st}$ is the state-level inequality measure, $\boldsymbol{Income_{ist}}$ is a vector of the five indicator variables for each income quintile, $\boldsymbol{Controls}$ is a vector of the individual and state-level controls, $\boldsymbol{\mu_s}$ specifies a full set of state indicators (i.e., fixed effects), and ϵ_{ist} is the idiosyncratic error. I place the interaction of the Gini index and the income quintiles in brackets to show that I estimate this model both with and without these terms. Without these terms, the coefficient of β_1 represent the overall effect of the Gini on financial satisfaction. By including the interaction terms, I can assess how the effects vary across the income distribution. In all models I employ the sampling weights provided by the GSS and adjust the standard errors for clustering within states.[6]

[6]This clustering accounts for individuals appearing more than once because of the panel struc-

I do not include year fixed effects in the main models, as adding them nets out the average level of inequality for each year, forcing the estimate to be based on changes in income inequality within states net of the overall growth of income inequality across states. Inequality grew in all states, as shown in Figure 2.2. As a result, this approach may be overly conservative, netting out much of the variation in income inequality that we are interested in understanding. However, I present models with year fixed effects as a robustness check and show these produce results with a similar magnitude and direction but have much larger standard errors.

Many studies of income inequality prefer to specify a multilevel model (i.e., mixed or hierarchical linear models) to understand the effects of inequality. In the multilevel approach, β_1 is estimated from variation both between and within states and years and "borrows strength" across groups (i.e., state-years) when they are small (Gelman and Hill 2007). As a result, this model is more efficient (i.e., yields smaller standard errors) but requires the untestable "random effects" assumption—in this case that any unobserved characteristics of the states that influence the outcomes are not correlated with any of the individual- or state-level measures that are included with the model. These models also account in their standard errors for the clustering of observations within shared contexts without needing post-hoc clustering adjustments that are done in the fixed effects regression models (Rabe-Hesketh and Skrondal 2012). Despite the differences, I find substantively identical results with the random effects approach as compared to the fixed effects approach above.

Because the outcomes measure the attitude of the respondent at the moment of completing the survey, I use income inequality for the same year. It is possible that respondents take some time to "update" their mental understanding of how they compare to those around them, so for robustness I also consider the models with a lagged Gini.

I consider several other robustness specifications. I reestimate the models using alternative sets of control variables (no controls, individual controls only, state controls only). I consider the effect of outliers by removing the most and least unequal state observed in each year. And I reestimate the preferred models without survey weights. The results of the robustness checks are discussed in a section following the results from the preferred model.

After estimating the effect of inequality, I then examine the overall time trend in financial satisfaction and I present a counterfactual simulation of how financial satisfaction might have changed over time had inequality not risen. As noted in

ture of the GSS beginning in 2006. I cluster at the highest level (states). The clustered variance estimator accommodates an arbitrary variance-covariance matrix within each cluster, so it is robust to the presence of lower levels of clustering (Cameron and Miller 2011). This approach treats individuals that move between states as independent observations, but excluding repeat observations from movers does not alter the main results. As would be expected, removing clustering entirely generated smaller standard errors and smaller p-values for the coefficients of interest. Results available upon request.

Figure 2.1, financial satisfaction has declined over time. How much of this can be attributed to changes in inequality? We know inequality rose over the same time period, as shown in Figure 2.2, but, of course, many things changed in the U.S. besides inequality, which is precisely the motivation for doing the multivariate analysis in the first place.

The appropriate counterfactual is not "no change" in financial satisfaction over time, but rather the estimation of how financial satisfaction would have changed had inequality remained constant. To estimate this, I take the coefficients from the final model and make two predictions for each observation. First, I predict each respondent's financial satisfaction using all of the observed values for all individual- and state-level variables (this is the traditional $\hat{y}$ of a regression model). Second, I predict each respondent's financial satisfaction using the observed values *except* replacing the state Gini with its mean level in 1973. I compare these results visually using locally weighted regression (lowess) plots (bandwidth = .8) employing the sampling weights provided by the GSS, both for the full sample and by income quintile, in order to evaluate inequality's long-term effect on financial satisfaction.

2.6 Results

Main Effect

Table 2.1 presents the main results for the effect of income inequality on financial satisfaction (the full results are in the Appendix). The baseline income group is the middle-income quintile, which includes the median earner. Model 1 shows the main effect of inequality (Hypothesis 1) while Model 2 adds interactions between inequality and the respondent's income, in order to see how the effect varies across the income distribution (Hypotheses 2–4).

Table 2.1: Coefficients from Models of Financial Satisfaction

	(1)		(2)	
State Gini	-0.30^{*}	(0.13)	-0.54^{**}	(0.20)
Q1 (bottom)	-0.30^{***}	(0.014)	-0.30^{***}	(0.014)
Q2	-0.16^{***}	(0.0091)	-0.16^{***}	(0.0091)
Q3	0	(.)	0	(.)
Q4	0.16^{***}	(0.012)	0.16^{***}	(0.011)
Q5 (top)	0.41^{***}	(0.016)	0.41^{***}	(0.015)
Q1 (bottom) × Gini			0.38^{*}	(0.17)
Q2 × Gini			0.0032	(0.13)
Q3 × Gini			0	(.)
Q4 × Gini			0.27	(0.21)
Q5 (top) × Gini			0.55^{*}	(0.25)
State poverty rate	-0.011^{***}	(0.0031)	-0.011^{***}	(0.0031)
State median income (logged)	0.14	(0.092)	0.14	(0.092)
Individual Controls	*Yes*		*Yes*	
State Time-varying Controls	*Yes*		*Yes*	
State Fixed Effects	*Yes*		*Yes*	
Observations	51699		51699	

Standard errors in parentheses
* $p < .05$, ** $p < .01$, *** $p < .001$
Notes: The middle income quintile (Q3) is the baseline category. Each model uses sampling weights and the standard errors are adjusted for clustering within states. Full models with all coefficients are in the Appendix.

The overall effect of income inequality is negative and significant. In Model 1, a 0.1 increase in the Gini (about 1.5 standard deviations) is associated with a 0.03 ($\approx$ 0.04 standard deviation) decrease in financial satisfaction. To help understand these effects, I also report the coefficients for the state poverty rate and the logged state median income. Poverty also has a significant negative effect. For example, in model 1 a 0.1 increase in the Gini has about the same negative effect on financial satisfaction as a (-0.30*0.1/-0.011 =) 2.7 percentage point increase in the state poverty rate. State median income has a non-significant effect on financial satisfaction, but taking the point estimates at face value, in Model 1 a 0.1 increase in the Gini has approximately the same effect size as a 20% decrease in state median income.[7]

These back-of-the-envelope coefficient comparisons should probably be interpreted cautiously, as changes to the poverty rate or median income are likely to change

[7] State median income is logged, so the percent change will have the same effect on the outcome, regardless of the baseline income. Finding the comparable percent change to the effect of a 0.1

inequality as well, but they provide some evidence that these effects of inequality are indeed quite meaningful. Yet another way of sizing these effects is to consider the substantive implications from an over time perspective. Further below I also consider what portion of the overall change over time in the outcome could be explained by rising income inequality, given these models.

These models also show that the coefficients for the income groups are negative for quintiles below the middle quintile and positive for quintiles greater than the middle quintile. This is not surprising since this captures the effects of absolute income and the dimension of financial satisfaction that is about income rank, whereas the Gini captures the effect of a greater distance between ranks. Next, I consider how the Gini may have different effects depending on one's rank.

Heterogenous Effects by Income

Model 2 of Table 2.1 shows how the effect of income inequality varies across the income distribution. Here, the coefficient for the Gini represents the effect of income inequality for the middle income quintile. It is still negative, and it is greater in magnitude than in Model 1 (β = -.54, p <.01). The coefficients for the interaction term between the Gini and the 2nd and 4th income quintiles are not significant, but interaction coefficients for the highest and lowest income groups are both positive and significant. From this model, it appears the effect of income inequality is least negative for those at the top and bottom of the income distribution and is most negative for those in the middle (especially the second and third income quintiles).

To visualize these results, Figure 2.3 shows the predicted values of financial satisfaction for each income group in contexts of high and low income inequality while setting all of the remaining covariates to their mean values. The high inequality line shows the prediction when income inequality is at the level of the 90th percentile in the sample (Gini = .64), while the low inequality line shows the prediction for inequality at the level of the 10th percentile (Gini = .47). Another way to think of these levels is that the "low inequality" value is very nearly the median level of state inequality in 1973, while the "high inequality" value is very nearly the median level of state inequality in 2012.

increase in the Gini requires solving

$$\beta_{log(median\ income)} * log(1 + x) = 0.1 * \beta_{\text{gini}}$$

for x.

Figure 2.3: Predicted Values of Financial Satisfaction by Income Group in Contexts of High and Low Income Inequality (from Model 2 of Table 2.1). Error bars show 95% confidence intervals.

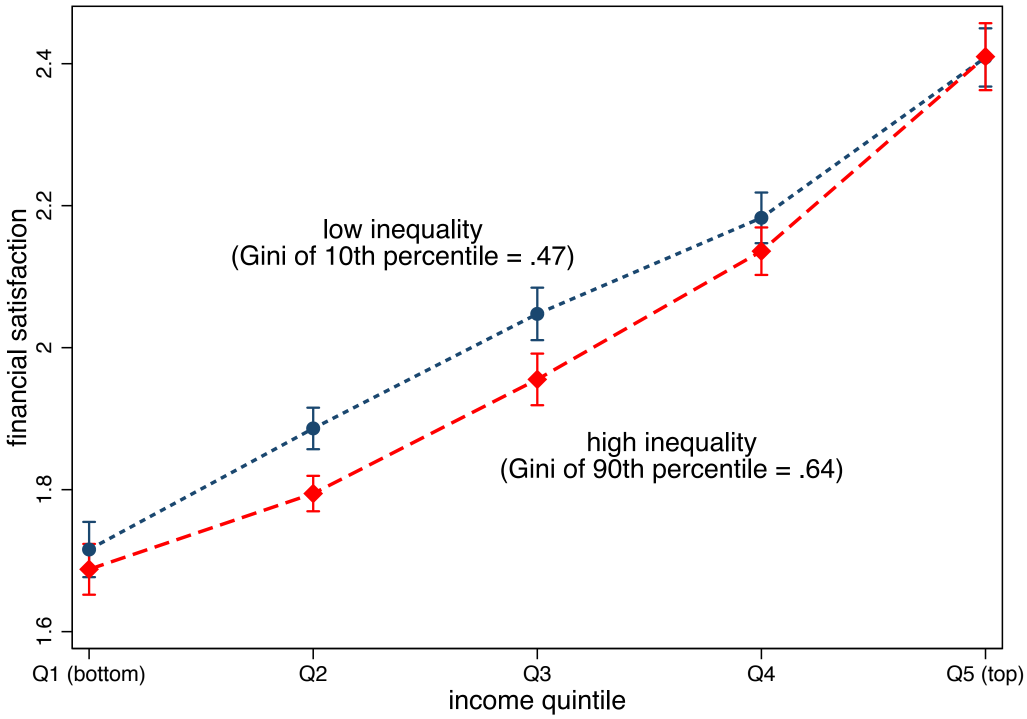


Figure 2.3 clearly illustrates that income inequality is negatively associated with financial satisfaction because the low inequality line is above the high inequality line. The gap is largest for the second and third income quintiles, and smallest at the top and bottom of the income distribution. For some perspective, the gap between high and low income inequality for the middle income quintile is slightly more than half the effect of moving from the middle quintile to one quintile above or below. Even if this is (debatably) not a large effect, it is important to note that changes in income inequality affect the entire population, whereas income changes for a single household affect only that household.

These findings show that there is substantial variation across the income distribution on the effect of inequality (rejecting Hypothesis 4). The findings are in line with the argument that those in the middle should be most affected (Hypothesis 3). They do not support the expectation that those in the bottom are most affected (rejecting Hypothesis 2a).

Hypothesis 2b further predicted that the effect of income inequality would be positive for the top income group. Returning to Model 2 of Table 2.1, the sum of coefficients for the Gini and the top income quintile × Gini interaction term is barely positive (-0.543 + 0.549 = 0.006) and a Wald test of the sum of these two coefficients shows there is not a statistically significant difference from zero (p = .98). Thus, I do not find any support for the idea that the effect of income inequality is positive for the top income group. One possibility is that the top income quintile is too large a group, but in an additional model I found no positive effect for the top 10% (available upon request). The data do not permit me to examine those at the extreme high end of the income distribution because the incomes of more than 5% of GSS respondents are top-coded each year.

Robustness Models

I also examined several alternative models and assessed the sensitivity of the results to a number of robustness tests. First, I estimated multilevel models with random incepts, both as a three-level model with random intercepts at the state, state-year, and individual level, and as a two-level model with crossed non-nested random intercepts at the state and the year level. These models produce results with similar coefficients and smaller standard errors ("more" statistical significance) than the main models. This is not surprising because the random effects model is a more efficient estimator and uses variation both within and between groups.

Turning to the other extreme, I analyzed models that added year fixed effects to the main models. In these models with two-way fixed effects, the magnitude and direction the coefficients are nearly the same as in the main results, but the standard errors of the estimates increase substantially and the results are not statistically significant at the conventional $p<.05$ level. Recall, however, that year fixed effects models net out most of the increase in inequality within states over the time period being examined. Although it is possible that some other trending characteristics are driving the main results, these unidentified characteristics would have to operate independently of the other time-varying state-level controls (median income, population density, percent black, percent foreign born, and poverty rate).

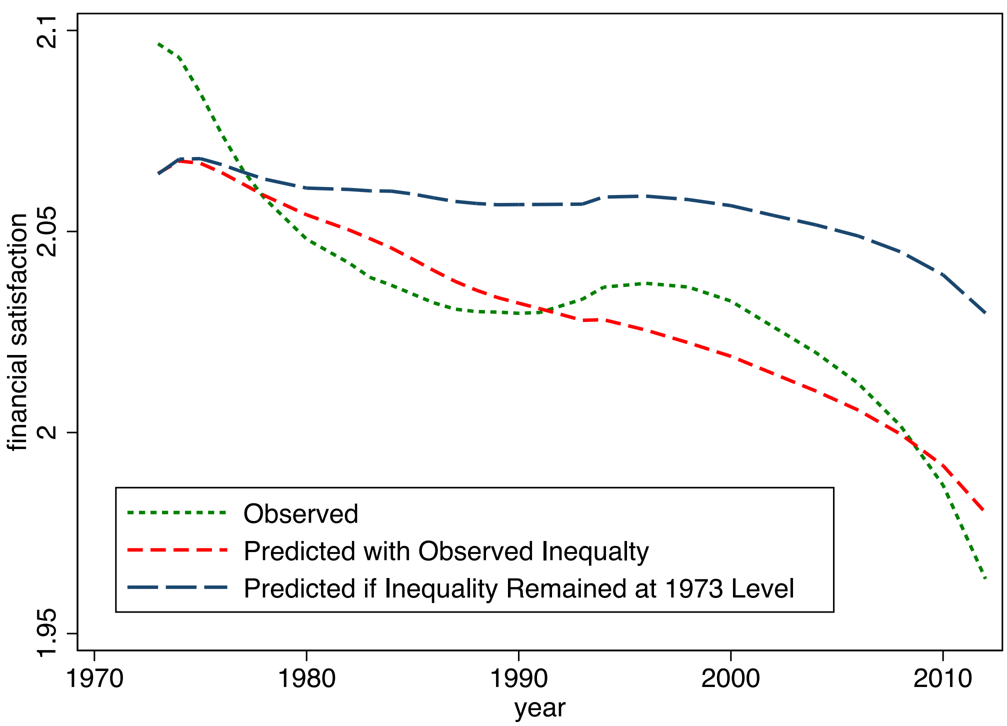
I also considered models that treat the three-value measure of financial satisfaction as categorical. Using both ordered logistic and multinomial logistic regression produces substantively identical results. The results were also robust to models using the income quintiles based on the ratio of household income to state-year median income, models reestimated without survey weights, models that removed observations from the most and least unequal state in each year, and models with a one-year lagged Gini index.

Counterfactual Simulation of Change Over Time

The analysis presented so far assesses the effect of income inequality by using the state-level variation in inequality over time. Given, as shown in Figure 2.2, that state-level inequality rose over the period of analysis, what implications did that have for financial satisfaction? I examine this by comparing the observed trend in financial satisfaction to a counterfactual trend—based on the estimated model—of how financial satisfaction might have changed had income inequality not risen.

Figure 2.4 presents these results. First, the dotted green line shows the true level over time for financial satisfaction—the same as in Figure 2.1. Next, the short-dashed red lines show the outcome's level for each year based on the predicted values obtained by calculating the predicted values for all of the observations in the sample, using the observed values for all of the covariates. If the model is effective, this should be close to the observed values, which it is. Finally, the long-dashed blue line shows the counterfactual simulation that predicts what financial satisfaction would have been in every year had income inequality remained constant since 1973, but everything had changed as it actually did. The figure demonstrates that had state-level inequality remained constant, the overall decline would have been by about half of what it actually was. For example, the predicted gap between the counterfactual and predicted lines for 2012 is about .06 (about .08 standard deviations).

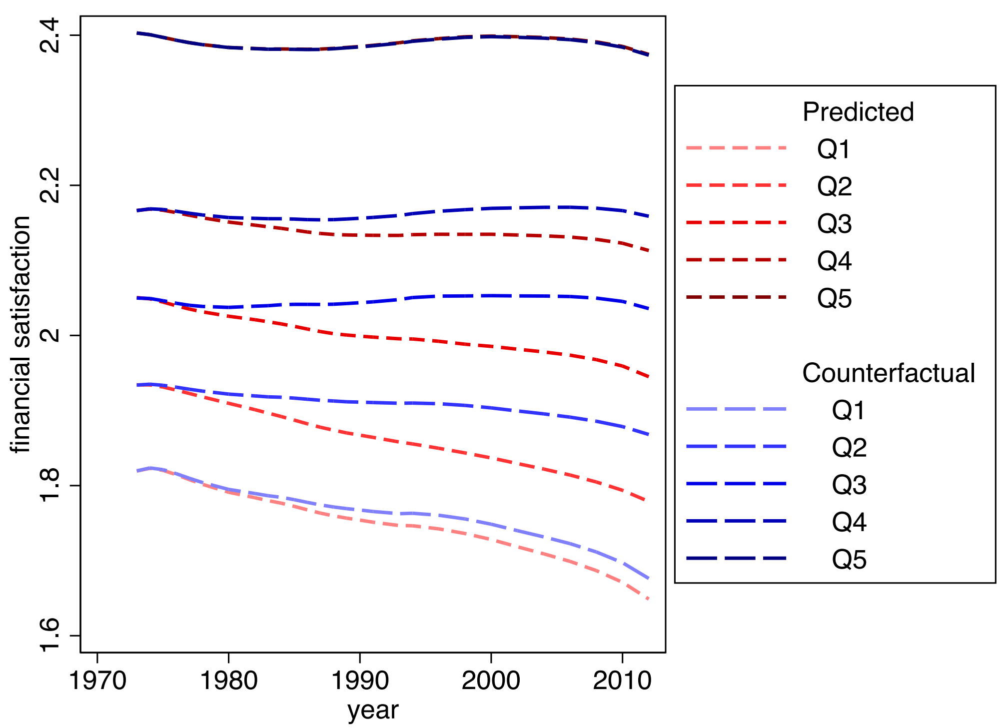
Figure 2.4: Counterfactual Simulation of Change Over Time in Financial Satisfaction



Note: Lines are lowess plots of data points aggregated by year.

Figure 2.5 presents the same results by income group. To avoid over-cluttering the figure, the true observed values are not shown, but the predicted line based on the actual inequality and the counterfactual line based on constant inequality are presented. From looking at the predicted values (red short dashes) it is clear that between 1972 and 2012 it was those in the bottom income quintile that experienced the greatest overall decline in their financial satisfaction (from 1.80 to 1.65). However, the largest gap attributable to income inequality is for those near the middle of the income distribution. The gap in 2012 between the predicted and counterfactual line for the middle quintile is 0.1—or almost double that of the overall effect from Figure 2.4. For the top quintile, there is no predicted change in financial satisfaction for the observed or counterfactual models over time, as seen by the nearly-flat, completely-overlapping lines at the top of the figure.

Figure 2.5: Counterfactual Simulation of Change Over Time in Financial Satisfaction by Income Quintile



Note: Lines are lowess plots of data points aggregated by year and income group. Note for Q5 the predicted and counterfactual lines overlap almost entirely.

2.7 Discussion and Conclusion

This paper examined the effect of rising state-level income inequality on financial satisfaction. Although the overall effect is negative, I find the middle to be the most negatively effected. This is consistent with the account that the middle class may be the most affected by income inequality. In the escalating "positional arms race," it is these households who likely experience a greater gap between their aspirations (the "American dream") and the economic reality they face (Frank 2007; Schor 1998; Fligstein and Goldstein 2015).

I employed a counterfactual simulation to predict what the consequences of rising inequality have been for financial satisfaction between 1973 and 2012. Overall, they suggest financial satisfaction would have declined only about half as much as it did. When breaking it down by income quintiles, I show that although those at the bottom experienced the largest decline in financial satisfaction, it is those in the middle whose decline is most attributable to rising income inequality.

Whether these effect sizes and over time declines are "large" depends on how much one thinks financial satisfaction is associated with other outcomes of interest. But small values can be substantively, as well as statistically, significant, especially when they apply to an entire population. If financial satisfaction (or lack thereof) is a factor into the decisions people and families make about aspects of life ranging from work and leisure to indebtedness and voting preferences to marriage and fertility, then rising income inequality may be an important part of the story to focus on.

This paper embraces causal language throughout, referring to the "effects" of income inequality. The analyses can, of course, only show associations between measures. As such, reverse causality cannot be entirely dismissed. It is possible that states where people are less satisfied with their finances are more likely to become unequal (though this would likely be a long-term process). But, I find the more straightforward direction of causality—income inequality causing the outcomes—far more plausible. A more likely problem is confounders. There are many differences between states and the people in them besides their Gini indices, and these differences could be driving some of the results (this reasoning applied to cross-national work on inequality is even more concerning). This paper tries to deal with confounders by including a large number of additional covariates at the individual and state level, including state fixed effects.

Despite the limitations, this paper makes a significant contribution to our understanding of the effects of income inequality. Existing literature has frequently implied that psychosocial factors such as feelings of relative deprivation are important mediators for income inequality's effects. But some critics have questioned this explanation, instead arguing for the supremacy of neo-material explanations (e.g., differences in healthcare or employment opportunities) for associations found between income inequality and various social problems (e.g. Deaton 2003; Lynch et al. 2000). Still others question whether there is really a relationship at all (e.g., Winship 2013)

Furthermore, classical sociological accounts of status have consistently noted that income (and economic standing, more generally) is only one dimension of social status. Weber (1946) described how "social honor" was typically expressed by members of "status groups" through what they consumed and their style of life. In *The Civilizing Process*, Elias ([1939] 1994) showed how forms of manner, dress, and politeness evolved so that nobles could distinguish themselves in an ongoing status competition within the kings' courts. As capitalism took hold, a new class of bourgeoisie with similar economic resources but non-noble birth began to emulate these behaviors, further escalating the creation of new ways to behave. Similarly Bourdieu (1984), in his study of lifestyles in *Distinction*, used survey data to map out a social field of France wherein status depended on at least two dimensions of resources: cultural capital and economic capital. In more recent work, Chan and Goldthorpe (2007) presented a significant difference between class and social status in British social life, and Goldthorpe (2010) pointed out—in a direct critique of Wilkinson and Pickett (2010)—that Japan has relatively low income inequality but a strong status hierarchy.

All this might suggest that income inequality would have little to no influence on individuals in normal life. Yet these results show that income inequality does make people feel more deprived and less satisfied with their financial situation. This in turn will undoubted shape their behaviors and attitudes, from everyday routines to major life decisions. To my knowledge, this is the first study to examine this link between income inequality and financial satisfaction, and it is an important step towards effectively evaluating and addressing the broader consequences of income inequality.

Chapter 3

Income Inequality and Economic Optimism: Findings from a Nationally Representative Survey and Online Experiment

Concerns about rising income inequality are frequently linked to discussions about opportunity and mobility, yet little research explores if and how this inequality affects people's economic optimism, something with far reaching implications for life satisfaction, public opinion, and real economic mobility. Using two data sources: the 1987–2012 General Social Surveys linked to state-level administrative data and an online survey experiment that manipulates perceptions of income inequality, I examine how economic optimism is shaped by both the level and rate of change of income inequality. Both the survey and experiment show that higher income inequality decreases economic optimism. The survey shows that the rate of change in inequality moderates the effect of the level of inequality, and that household income further moderates the effects of the level and change in income inequality on economic optimism. There was no evidence of this moderation in the experiment. Key differences between the two methodological approaches are discussed.

3.1 Introduction

How does income inequality shape people's beliefs about their own opportunities and their expectations for future social mobility? Inequality is linked by scholars, policy-makers, pundits, and publics to discussions of opportunity and mobility. In particular, debates about whether income inequality—something that has risen substantially in the U.S. over the past forty years (Piketty and Saez 2003 [2015]) — is "good" or "bad" frequently revolve around whether people think this inequality has consequences for inequality of opportunity and the possibilities of upward mobility.

Research on the true relationship between inequality and mobility is ongoing (Bloome 2014; Chetty et al. 2014; 2017), but little research has explored if and how the levels of income inequality that people experience affects what they believe about their potential to improve their own economic position (hereafter, "economic optimism"). Yet, what people believe about their own opportunity has important implications for their feelings (e.g., Alesina et al. 2004), their behaviors (e.g., Tenney, Logg, and Moore 2015), and the policies and politics they are likely to support (e.g., McCall 2013; 2016).

Not only has this link been largely unexamined, but existing research designs that explore how inequality matters entirely overlook an important aspect to the theoretical explanations. Prior research has focused exclusively on the level of inequality (whether it is high or low), while the articulated explanations also depend on how that inequality is changing (whether it is growing or stable). Based on existing theories, I propose that one's economic optimism is shaped by both the *level* and the *rate of change* of their state's income inequality. Further, by definition greater inequality advantages some and disadvantages others, so I consider whether the effects of inequality on economic optimism vary by one's place on the income distribution.

The paper proceeds as follows. First, I explain why economic optimism is a critical link in understanding the effects of income inequality. Then, I outline a set of hypotheses for how inequality could matter. I test these hypotheses using two research designs. I analyze nationally representative data from the General Social Survey linked to state-level administrative data, and I conduct an online survey experiment that manipulates the respondent's perception about the level and rate of change of inequality in their state. I find that high income inequality decreased economic optimism. The survey analysis, but not the experiment, found that the rate of change in inequality moderated the effect of the level of inequality and that inequality's effect is moderated by income. I discuss the implications and limitations of this analysis, including substantive differences between the survey and experimental design and their respective results.

This work makes three contributions to the study of income inequality. First, it analyzes economic optimism, a potentially important but understudied factor for understanding the consequences of income inequality. Second, it shows the importance of moving beyond analyzing the effect of the level of inequality and refocuses some of the attention to the effects of the rate of change in inequality. Finally, it highlights the differences between using experimental and survey research designs—both popular in current research—to study the consequences of inequality.

3.2 Economic Optimism: A Critical Link in Understanding The Effects Of Inequality?

The rise in income inequality has been well-documented (Piketty and Saez 2003 [2015]), and a number of scholarly accounts have described it as being driven by a variety of factors (for extensive reviews, see Kenworthy [(2017)] and McCall and Percheski [(2010)]). But understanding the consequences has been far more elusive (Neckerman and Torche 2007; Moss et al. 2013). There is little consensus on the effect of income inequality—that is, an effect of the overall distribution of income net of household or personal income—on unequivocally important outcomes such as health, happiness, educational achievement, intergenerational mobility, and policy preferences.

In part, this gap is likely due to the underdevelopment and lack of testing of the specific pathways through which income inequality might matter.[1] In their extensive review of inequality research, Neckerman and Torche (2007) articulated this problem, writing "This research [on the consequences of inequality] is characterized by premature theoretical closure, with an overemphasis on a few pathways..." (p. 349). A decade later, economic optimism has not yet become one of those pathways under significant consideration.

Yet, economic optimism is a compelling candidate to be such a link. First, public support for policies that affect income inequality may depend on what people *believe* about the relationship between inequality and economic opportunity (even as the actual relationship is a site of much ongoing scholarly attention and debate, e.g., Bloome 2014; Chetty et al. 2014; 2017). The "median-voter hypothesis" suggests that those on the underside of unequal income distributions should, in democratic societies such as the U.S., desire policies that increase redistribution and reduce inequality (Kenworthy and McCall 2008). In contrast, the "prospect of upward mobility hypothesis" suggests that although the poor would seemingly benefit from reductions in inequality, they may not oppose high inequality if they hope to become the rich of the future (Bénabou and Ok 2001).

In *The Undeserving Rich*, McCall (2013) demonstrates that public concerns and calls to action about income inequality may be best understood as fears of narrowing opportunities. In an updated analysis, McCall notes "rising concerns about upward mobility help to 'explain' rising concerns about inequality" (2016:426) and concludes

[1] In this paper I use "pathway," "mechanism," and "mediator" interchangeably.

that it is precisely strategies that seek to equalize outcomes (e.g., income) for the purpose of equalizing opportunity that have the most potential to gain traction.

Second, income inequality and economic optimism may shape happiness, an outcome considered intrinsically valuable by many. Previous work has focused on how inequality affects happiness by reducing feelings of trust and fairness (e.g., Oishi et al. 2008; Uslaner and Brown 2005). The implications are grim. Case and Deaton (2015) specially pointed to rising economic inequality as a part of the cause for mid-life mortality caused by overdose, suicide and alcohol. Although some aspects of this study have come into question the trend above has remained robust. A new update by Case and Deaton (2017) also noted a widening divergence in these "deaths of despair" between those with a high-school degree (who saw a mortality rate increase over time) and those with a college degree (who did not).

In contrast, some other recent work has suggested inequality's effect on happiness could be positive if inequality is interpreted as a sign of future upward mobility (Verme 2011; Alesina et al. 2004; Cheung 2016). Regardless of the direction of the effect, the implication is that economic optimism may be an unexamined link between a population's income inequality and their aggregate happiness.

Third, economic optimism itself could affect real economic mobility, although the relationship between optimism and success remains under investigation (e.g., Tenney et al. 2015). For example, those who believe their opportunities are vast may act on those opportunities (e.g., through work and career decisions) and become more upwardly mobile than those who are more pessimistic (Scheier and Carver 1993). Ironically, these beliefs may play a role in reproducing existing inequalities by becoming a self-fulfilling prophecy (Merton 1995).

For example, Kearney and Levine (2016) document that youth from low socioeconomic status (SES) backgrounds are more likely to drop out of school if they live in a state with a greater gap between the bottom and middle of the income distribution. They argue greater levels of income inequality could lead low-income youth to perceive a lower rate of return on investment in their own human capital. In another paper, they show low SES women were more likely to give birth at a young age and outside of marriage when they live in more unequal states (Kearney and Levine 2014) . They argue that poor women in unequal places give more births while still young and single because they perceive an inability to improve their situation through work or marriage, and thus have little incentive to delay the immediate gratification of parenthood. In both papers, the proposed mechanism is that inequality matters because it elevates a culture of "economic despair." Once individuals no longer believe socioeconomic success is achievable, they rationally behave in ways that ultimately ensure that outcome.

Despite its apparent centrality to the above work, to date the link between income inequality and economic optimism itself has not been empirically examined directly.

3.3 Empirical Predictions

Given the evidence that Americans who are concerned about income inequality are concerned because of its potential to reduce opportunities (McCall 2013; 2016), I expect to find that there will be less economic optimism when inequality is higher.

> *Hypothesis 1: Higher income inequality is negatively associated with economic optimism.*

But the trajectory of income inequality may also matter. This is illustrated aptly in the "tunnel analogy" first presented by Hirschman and Rothschild (1973):

> "Suppose that I drive through a two-lane tunnel, both lanes going in the same direction, and run into a serious traffic jam. No car moves in either lane as far as I can see... I am dejected. After a while the cars in the [other] lane begin to move. Naturally, my spirits lift considerably, for I know that the jam has been broken and that my lane's turn to move will surely come.... I feel much better off than before because of the expectation that I shall soon be on the move... But suppose that the expectation is disappointed and only the [other] lane keeps moving: in that case I... will at some point become quite furious and ready to correct manifest injustice by taking direct action." (p. 545)

In other words, when inequality is high but rising, it may inspire more economic optimism. But when high and stagnant, it will erode that optimism. This analogy has been used to explain cases where income inequality appears to be correlated positively with life satisfaction in some developing economies (e.g., Cheung 2016) and correlated less negatively with life satisfaction in America than in Europe (Alesina et al. 2004).

Yet, in fact, all of these studies explore only the effect of the level of inequality. Whereas the prediction is that the effect may be positive when people view rising inequality as a sign of a more promising future, but then turn negative if aspirations remain unfulfilled. This suggests the relationship between the *level* of income inequality and beliefs about future income is moderated by the *stability* of the level of inequality.

> *Hypothesis 2: The rate of change in inequality positively moderates the effect of higher income inequality on economic optimism.*

Note however that this analogy provides no predictions about those in the "fast lane" (who would be highest earners), because the theory is about how others are affected by seeing the relative success of those near the top of the income distribution, nor does it specify who is in the "moving" and "stuck" lanes. Should we expect something different for high income earners compared to low ones?

In the U.S., people are generally optimistic about their opportunities for social mobility (Bénabou and Ok 2001; Keister 2005), but higher income people may be even more optimistic, given their tendency to compare themselves more favorably to the general population (Newman et al. 2015; Andersen and Curtis 2012). But will this optimism vary by the level and stability of inequality?

There is good reason to expect that it will. First, literature on relative economic comparisons suggests that people generally focus on upward comparisons (Runciman 1966; Evans et al. 2004). The growth in income inequality in the U.S. has been driven by ever increasing returns at the highest levels (i.e., each income percentile experiencing greater economic growth than the one below it [Piketty and Saez 2003 [2015]]). As a result, everyone may perceive of themselves being in the "stuck" lane.

But more to the point, by definition rapidly rising inequality greatly favors those closer to the top of the income distribution. Because high earners benefit the most from rising income inequality, they may expect even higher relative incomes in the future if inequality continues to grow. Thus, even though I expect for everyone that rapidly increasing inequality will positively moderate the effect of the level of inequality, I expect this to be even more pronounced among those with higher incomes.[2]

> *Hypothesis 3: Income will positively moderate the moderating effect of the rate of change on the effect of higher income inequality on economic optimism*

I test all three hypotheses using nationally representative survey data and an online survey experiment that manipulates perceptions of inequality.

3.4 Nationally Representative Survey Study

Data

I examine individual-level data from the 1987–2012 General Social Survey (GSS), a nationally-representative survey with response rates greater than 70% (Smith et al. 2013) in all survey years. Using the restricted-access geographic identification file, I matched each respondent to the macro-level demographic and economic characteristics of their state, including both the level of and rate of change in income inequality.[3] Beginning in 2006, the GSS began re-interviewing respondents in up to two future waves (e.g., 2006-2008-2010). I include re-interviews, but the results are robust to

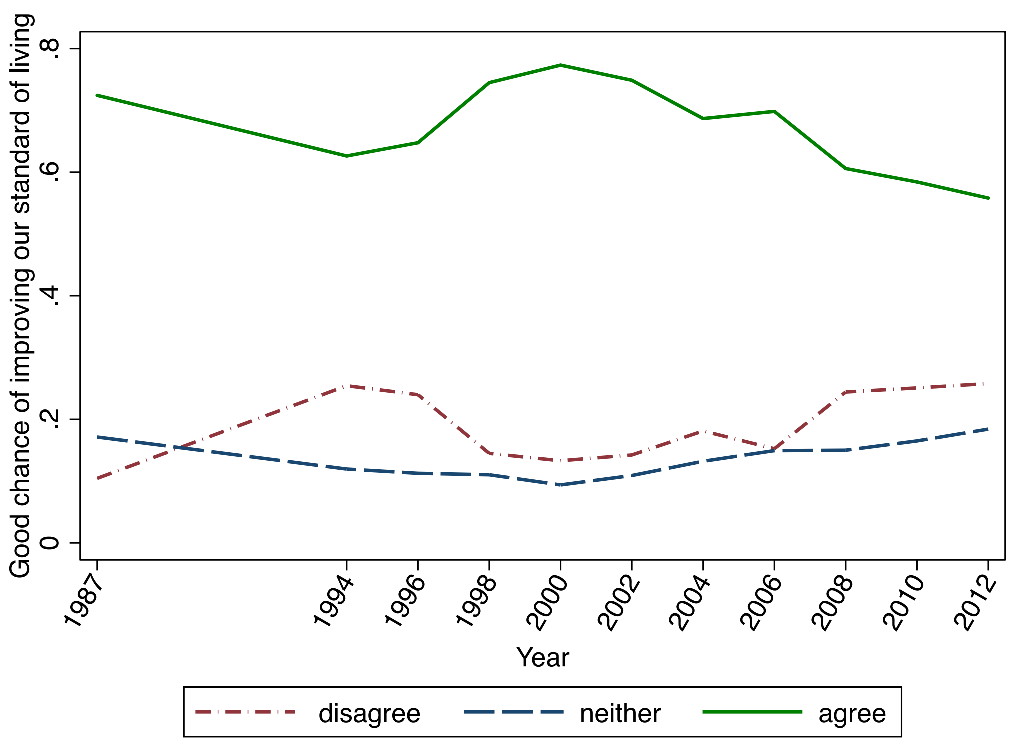
[2] Although if high earners actually saw themselves in the fast lane and if they truly believed in the tunnel effect, they might expect the opposite, believing that the current poor will catch up and join the rich.

[3] For information on obtaining the restricted access GSS data, see http://publicdata.norc.org:41000/gss/documents/OTHR/ObtainingGSSSensitiveDataFiles.pdf

dropping them. In all, I analyze 18,707 observations from 14,756 respondents in 444 state-years.

To test how income inequality is associated with optimism for future economic mobility, I use the response to the question, "The way things are in America, people like me and my family have a good chance of improving our standard of living" which was asked in 1987 and every even year from 1992 to 2012. Respondent's feelings were recorded on a 5-unit Likert scale. Figure 3.1 shows the general patterns. Americans are generally optimistic. Respondents were most likely to "strongly agree" and "agree" in the early 2000's, followed by a decline during the Great Recession that endured through 2012, but even then nearly 60% of responses were positive. "strongly disagree" and "disagree" had a nearly inverse pattern, just at a much lower level (topping out under 30%).

Figure 3.1: Economic Optimism from 1987 to 2012 (Source: General Social Survey)

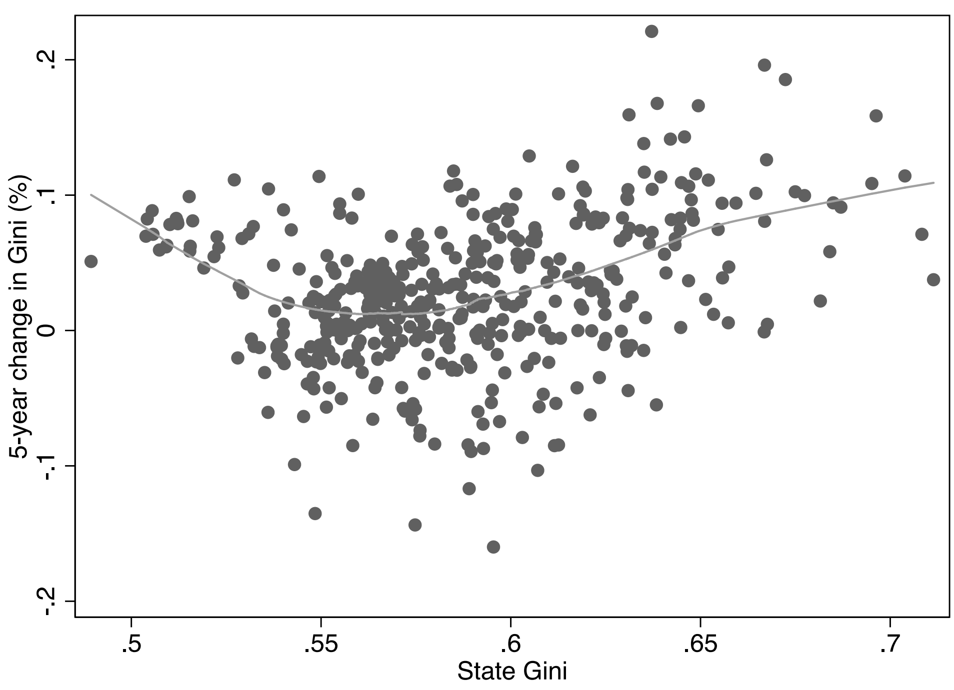


Note: For illustrative purposes "strongly disagree" and "disagree" are combined and "strongly agree" and "agree" are combined.

My key inequality measure is the Gini index of income inequality. This measure is the average distance between all pairs of proportional income in the population, ranging from 0 (if everyone had the same income) to 1 (if one household had all the income). The Gini assesses inequality across the entire income distribution and is the most common inequality measure in previous research. Recently some scholars have focused on the top 10% and top 1% income shares (Frank 2014; Piketty and Saez 2003 [2015]), but the hypotheses tested here would seem to be sensitive to changes in income inequality across the entire distribution, so I focus on a measure that is similarly sensitive to these changes.[4] The Gini is constructed at the state-year level from data published in the IRS's *Statistics of Income* and made available by Frank (2014).

I measure the change in income inequality as the percent change from five years prior (theory does not specify the length of the lag, but exploratory analysis found the results were strongest when looking at the change in the previous 3–7 years). Using the difference, rather than the percent change, in income inequality produces substantively identical results. Figure 3.2 shows the relationship between state-level inequality and its change from five years prior. Although there is a mostly positive trend—because states where inequality has grown rapidly are likely to now have high inequality—there is still substantial variation in both dimensions.

Figure 3.2: State Gini Level vs 5-year Change in State Gini



Note: Each point represents a state-year. Line shows lowess plot (non-parametric best-fitting relationship) (Source: General Social Survey, 1987–2012)

[4] A number of alternative inequality measures exist such as the Theil entropy index, Atkinson index, Robin Hood Index, and Coefficient of Variation. For further details and comparisons between various measures, see Evans et al. (2004) and Frank (2014).

Household income was measured by asking respondents to place their household's income into an income bin. The bin ranges have changed over time, but the GSS includes a harmonized inflation-adjusted "best estimate" of the household income, where respondents were assigned the midpoint of each income bin, except for the open-ended top income bin where incomes were assigned using a Pareto curve (Ligon [1989] 1994; Hout 2004).

For each observation I control at the individual level for the respondent's sex, age, age squared, race/ethnicity (non-Hispanic white, non-Hispanic black, non-Hispanic other, Hispanic), marital status (married, widowed, divorced, separated, never married), number of children in the household, number of adults in the household, religious service attendance (from 0 = never attend to 8 = attend more than once a week), years of education, and the type of area the respondent lived in (urban, suburban, or rural).[5] At the state level I control for median income (logged), percent black, percent foreign born, and population density (logged). All economic measures at the individual and state-level are adjusted to 2012 dollars (the last year of the analysis) using the Consumer Price Index (CPI-U-RS).

Methods

I am interested in the *causal* effect of income inequality on economic optimism, so I estimate a more conservative OLS regression that includes state and year fixed effects (i.e., two-way fixed effects). This approach is stronger for causal inference because it nets out all time-invariant differences between states (e.g., how New York is different from Texas) and between years (e.g., how the years during the Great Recession were different from the boom years preceding it), but comes at the "cost" of netting out most of the variation in inequality itself. The variation that matters for this model are changes within states over time net of the overall level in each year.[6] To test the hypotheses, I focus on the coefficients for the level of the Gini, the change in the Gini, the respondent's household income, and the interactions between these three variables.

Formally, for response i in state s in year t the full model is:

[5] The GSS did not measure Hispanic or Latino ethnicity until 2000, so for observations before that year I use the respondent's reported country of ancestral origin. This correlates extremely well with self-identified Hispanic from 2000 onward.

[6] I also estimated a random effects model (i.e., mixed or hierarchical linear models) with random intercepts at the state, year, and person level. This uses variation both between and within states, years, and individuals, and it accounts for the clustering of observations within shared contexts without post-hoc corrections (Raudenbush and Bryk 2002; Rabe-Hesketh and Skrondal 2012). This produced substantively identical results.

$$
\begin{aligned}
Optimism_{ist} = \; & \beta_0 + \beta_1 Gini_{st} + \beta_2 Gini\_Change_{st} + \beta_3 Income_{ist} \\
& + \beta_4 Gini_{st} \times Gini\_Change_{st} \\
& + \beta_5 Gini_{st} \times Income_{ist} \\
& + \beta_6 Gini\_Change_{st} \times Income_{ist} \\
& + \beta_7 Gini_{st} \times \beta Gini\_Change_{st} \times Income_{ist} \\
& + \beta_8 Individual\_covariates_{ist} + \beta_9 State\_covariates_{st} \\
& + \mu_s + \mu_t + \epsilon_{ist}.
\end{aligned}
$$

All models implement the survey weights provided by the GSS and adjust the standard errors for clustering at the state level.

Results of Survey Analysis

The key coefficients from the analysis are presented in Table 3.1 and full results are in the Appendix. In models without state and year fixed effects, there is a negative effect of inequality on trust. When and where income inequality was higher, people were indeed less optimistic about their chances of improving their standard of living. But is this causal? After the addition of the two-way fixed effects in Model 2, the coefficient is still negative but much smaller in magnitude and no longer significant. In other words there is not conclusive evidence that, *on average*, in states where inequality grew faster optimism decreased more, something predicted by Hypothesis 1.

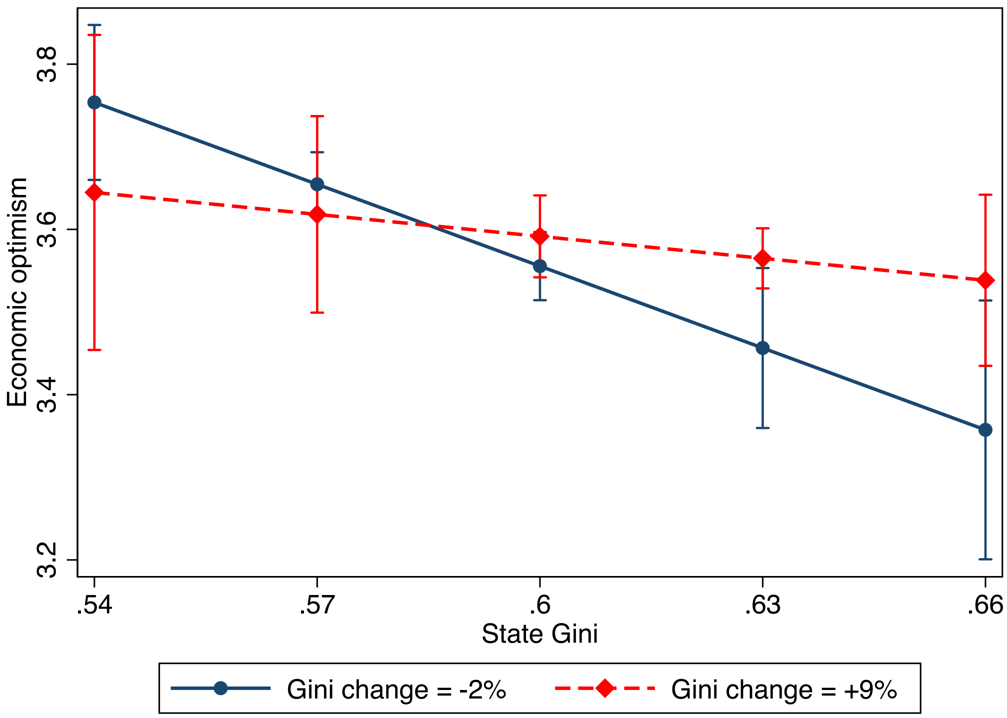
Table 3.1: Coefficients from Model of Economic Optimism from General Social Survey (1987–2012)

	(1)	(2)	(3)	(4)
State Gini	-2.24^{***}	-0.60	-2.86^{**}	7.30^{*}
	(0.40)	(0.64)	(1.03)	(3.25)
5-year Gini change			-12.9^{***}	15.1
			(3.47)	(29.1)
State Gini × 5-year Gini change			22.0^{***}	-34.0
			(5.38)	(47.3)
State Gini × Log(income)				-0.95^{**}
				(0.29)
5-year Gini change × Log(income)				-2.65
				(2.75)
State Gini × 5-year Gini change × Log(income)				5.30
				(4.47)
Log(income)	0.097^{***}	0.092^{***}	0.091^{***}	0.64^{***}
	(0.012)	(0.012)	(0.012)	(0.18)
Individual controls	*Yes*	*Yes*	*Yes*	*Yes*
State time-varying controls	*Yes*	*Yes*	*Yes*	*Yes*
State fixed effects	*No*	*Yes*	*Yes*	*Yes*
Year fixed effects	*No*	*Yes*	*Yes*	*Yes*
Observations	18710	18710	18710	18710

Standard errors in parentheses
* $p < .05$, ** $p < .01$, *** $p < .001$
Notes: Models are weighted to account for sampling and standard errors are clustered at the state level. Full models are in the Appendix.

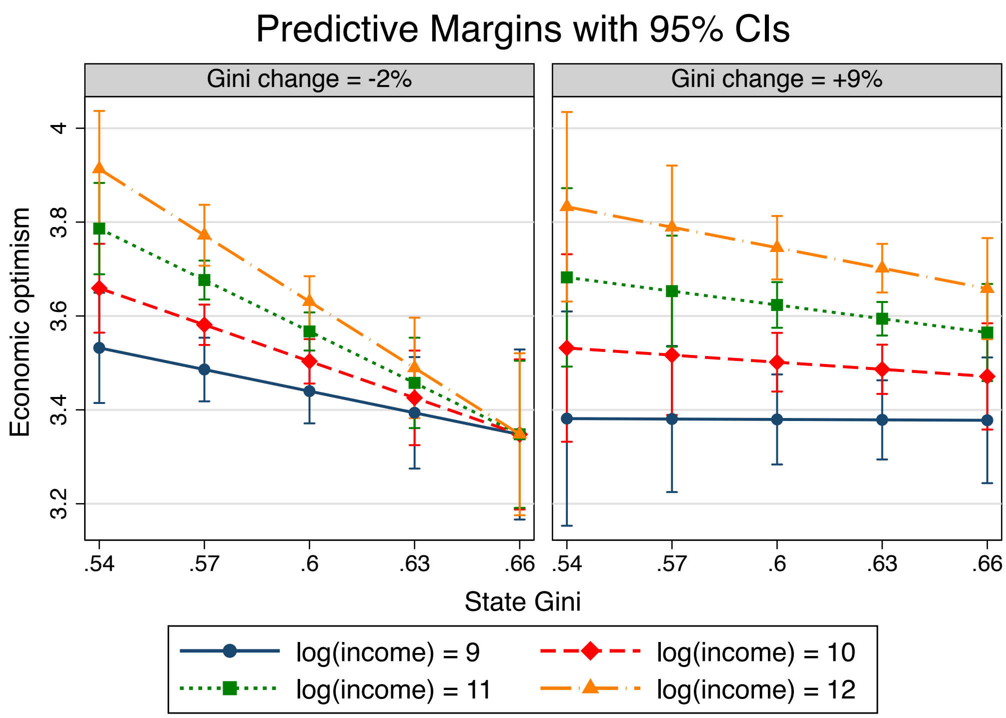
Could this be moderated by how inequality is changing within states, as predicted by Hypothesis 2? Model 3 shows that it is. The moderating effect is positive, as shown by the Gini level × Gini change interaction term. This is shown graphically in Figure 3.3 by plotting predicted optimism by the Gini level using the results from Model 3 for two levels of Gini change— -2% being the 10th percentile and +9% being the 90th percentile—while holding all other variables at their means. When inequality is not growing, respondents are far more optimistic in low inequality areas. When inequality is growing, there is much less effect of the actual level of inequality. Put another way, when inequality is high, respondents are more optimistic if the percent change in the Gini is also large.

Figure 3.3: Predicted Levels of Economic Optimism (from Model 3)



How then is this further moderated by income? These results are shown in Model 4. One of the new interaction terms (Gini level × Income) is significant, but all of the terms are substantial in magnitude and of different signs. It is far more clear to examine the results graphically. Figure 3.4 shows the predicted levels of economic optimism for various levels of income in separate plots for the two different values of the Gini change from the previous figure.

Figure 3.4: Predicted Levels of Economic Optimism by Income Level (from Model 4)



The models predict that when inequality is not growing, higher income respondents are more positive than low income respondents where inequality is low; but where inequality is high, respondents of all income levels are more equally pessimistic. When inequality is rapidly rising, the slopes flatten and gaps between income groups widen. In contexts of high, rapidly-rising inequality, the rich are far more optimistic than the poor. That is, high income respondents appear much more positively affected by rising inequality than low income respondents. In other words, as predicted in Hypothesis 3, income positively moderates the moderating effect of the Gini change on the effect of the level of inequality. For robustness, I also estimated models in which I interacted the Gini level and Gini change with a non-linear income measure (each income quintile) and found similar results, so I show only the findings with the much more parsimonious continuous income measure.

3.5 Experimental Study

Procedure

Next, I conduct an experiment with participants recruited from Amazon Mechanical Turk. Despite their convenience and relatively low cost, experiments with MTurk samples have been shown to consistently produce results similar to those based on population-based probability samples (Berinsky, Huber, and Lenz 2012; Weinberg, Freese, and McElhattan 2014). The participants completed a short survey (<10 minutes) that gathered demographic data, performed a manipulation of income inequality in the participants home state, and collected measures of economic optimism. Adapting the research designs by Côté et al. (2015) and Norton and Ariely (2011), I asked participants which state they lived in and then presented two pie charts that purported to show the distribution of income in their state in the years 2000 and 2015. In fact, each respondent was randomly presented with one of three conditions: (1) low inequality in 2000 and high inequality in 2015, (2) high inequality in both 2000 and 2015, or (3) low inequality in both 2000 and 2015. The figures and full prompt are shown in the Appendix.

In fact, the high inequality figures (Gini $\approx$ .55) are close to the actual income distribution in the U.S. The low inequality figures (Gini $\approx$ .25) are close to the actual income distribution in Sweden. Like Côté et al. (2015), I reason that the manipulation is more likely to work at the state level than national level because people will be less knowledgeable about state-level inequality, whereas national-level income inequality has been frequently discussed in the media. Similarly, I do not include a fourth condition of high inequality in 2000 and low inequality in 2015 because this is least compatible with media reports and more likely to make participants suspicious that the data are simulated.

I asked three comprehension questions about the chart to test whether respondents could interpret the pie charts, and I excluded responses that failed these questions. After the prime, I measured economic optimism is several ways. Respondents reported:

1. which "slice" of the pie chart they thought they belonged to now, and which one they expected to belong to in five years. A binary variable was created (1 = move to higher slice), but this variable only applies to respondents who did not choose the highest slice for current status, because then they cannot move up.

2. their level of agreement with the same statement in the GSS about economic optimism: "The way things are in America, people like me and my family have a good chance of improving our standard of living."

3. their level of agreement with three additional statements about economic mobility, which I combined with the question above into a four question scale (Cronbach's alpha = .72)

    - "Most people who work hard end up better off than their parents in terms of their financial or economic situation."

- "The social class children are born into is usually the one they end up in as adults."
- "If I continue to work hard, my income is likely to increase in the future."

In order to test whether the differing conditions manipulated feelings about income inequality at all, I also asked about one's level of agreement that, "Differences in income in America are too large."

At the end of the survey, participants were told that they had been given a false impression of the level of inequality in their home states in order to examine the effects of income inequality on optimism, and were offered a url to a report about state-level income inequality. An additional sample of participants received no prime at all. There were no major differences in the outcomes between those who did and did not receive an inequality condition, and this latter group is not discussed further.

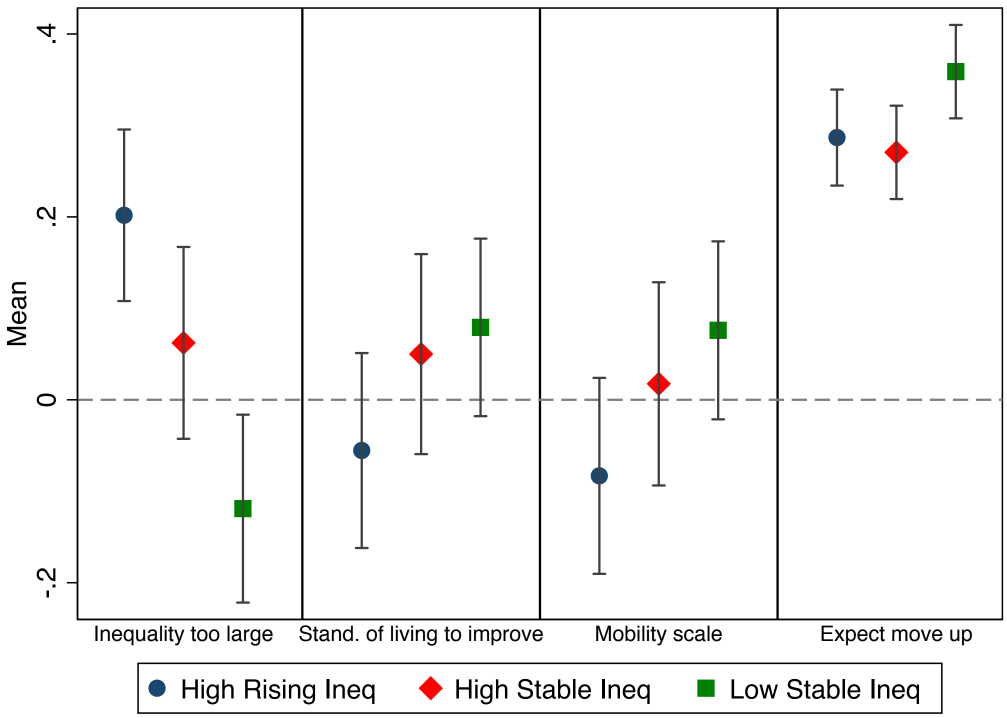
Plan of Analysis

The outcomes are standardized (mean = 0, SD = 1), except for the binary "expect to move up," and differences between those in each condition are examined. Hypothesis 1 is supported if those in the low inequality condition are more optimistic than those in the other two conditions. Hypothesis 2 is supported if those in the high-rising inequality condition are more optimistic than those in the high-stable inequality condition. Hypothesis 3 is supported if the gap between those in the high-rising and high-stable conditions is larger for higher income respondents. Note, however, that although inequality is randomized, income itself is not. Nevertheless, I did observe substantial variation in self-reported income in my MTurk sample.

Results of Experiment

Figure 3.5 and Table 3.2 present the main experimental results. In Model 1, the outcome is the response to the question "income differences are too large." This is a check to see if show if respondents internalized the manipulation at all, and there is some evidence of this. As would be expected, respondents who saw low inequality (condition 3) were the least in agreement that income differences are too large.

Figure 3.5: Experimental Results



Notes: The first three outcomes have each been standardized (mean = 0, SD = 1). The fourth outcome is binary. Error bars represent standard errors for 95% confidence intervals.

Hypothesis 1 predicted those who saw low inequality (condition 3) would be most optimistic. In support of this, for each economic optimism outcome, those in the low inequality condition reported the highest optimism. The differences between it and the high-rising inequality condition were statistically significant at least at a $p<.1$ level in Models 2–4, but only significantly different from the high-stable inequality condition in Model 4.

Table 3.2: Experimental results

	(1) income differences are too large	(2) standard of living will improve	(3) mobility scale	(4) expect to move up in 5 years
(1) Baseline: high rising inequality				
(2) high stable inequality	-0.14^{+}	0.11	0.10	-0.016
	(0.076)	(0.078)	(0.079)	(0.038)
(3) low stable inequality	-0.32***	0.13^{+}	0.16*	0.072^{+}
	(0.072)	(0.074)	(0.074)	(0.037)
constant	0.20***	-0.055	-0.083	0.29***
	(0.054)	(0.055)	(0.055)	(0.027)
Wald test of (2) = (3)	*	not sig.	not sig.	*
Observations	1030	1035	1035	918

Standard errors in parentheses
$^{+}$ $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$

Notes: Baseline condition is high rising inequality (condition 1 in the Appendix). The outcomes for Models (1), (2), and (3) are standardized (mean = 0, SD = 1). Symbols by coefficients reflect the p-value of difference to the baseline condition. Comparisons between condition 2 and condition 3 are shown in the "Wald test of (2) = (3)"

Hypothesis 2 predicted those in the high-rising inequality condition would be more optimistic than those in the high-stable inequality condition. This is not supported at all. The difference in all three optimism models between these two conditions is not statistically significant, nor is there a consistent direction of the difference.

Finally, Hypothesis 3 predicted that income would increase the size of the gap between those in the high-rising and high-stable inequality condition. Because there was no support for Hypothesis 2, there is no "gap" to increase. Accordingly, in models that interacted income with the experimental conditions, the interaction terms were neither substantively large nor statistically significant.

3.6 Discussion and Conclusion

In both the survey analysis and experiment I found that high income inequality decreased economic optimism once accounting for the stability in inequality. However, I found in the survey analysis that the rate of change in inequality also moderated the effect of the level of inequality and that this moderation was further moderated by income, but these findings were not supported by the results of the experimental study.

What explains the differences? Perhaps the simplest explanation is that these differences reflect the true differences between experienced vs perceived inequality.

The survey analysis examines the effect of respondent's actual level and change in income inequality (perceived or not), while the experiment manipulates the respondent's perception about their state's inequality. Previous research has found that inequality matters far more when it is visible (e.g., Nishi et al. 2015), suggesting perceived inequality is important, but it may fail to fully capture why inequality matters.

Alternatively, the differences may be the result of the study's methodology. In particular, although MTurk studies can generally be very useful (Berinsky et al. 2012; Weinberg et al. 2014), this experiment asks questions on economic opportunity among a sample of respondents who choose to earn money by working on MTurk. As such, these results may not be very representative of how most Americans' economic optimism would be affected by the experimental manipulations. Future survey experiments on nationally-representative populations may be critical for these types of questions.

Finally, it is also possible that the manipulations were not strong enough. For example, Côté et al. (2015) manipulated wealth inequality, for which there are more dramatic differences. Or it is possible MTurk participants had trouble perceiving the level of inequality in the pie graphs while speedily completing the survey, even though manipulation checks confirmed they correctly interpreted the charts. In future work it may be helpful to explore other ways to present both the level and change in inequality that might be more effective (e.g., bar charts or figures) and also find ways to make respondents spend more time internally processing the survey (perhaps through short response answers).

Despite the uncertainties, this work informs how we think about and seek to evaluate the consequences of inequality. First, this research moves beyond just considering the effect of the level of inequality. Existing theories already incorporate these ideas. In this case, I demonstrate the usefulness of considering the temporal track of inequality's growth, rather than merely its current level, in understanding the consequences of high and rising income inequality. A related notion is to focus on the "type" of inequality. Different processes generate the same levels of inequality, but may have different results. For example, research has shown that labor markets affected by increased industrial robot usage were affected differently than those affected by offshoring (Acemoglu and Restrepo 2017).

Second, this work highlights some of the differences between experimental and survey research designs. Scholars are implementing both approaches and they take more interest in understanding how inequality matters. The differences between the research designs often reflect disciplinary strengths. But, it may be (in fact, seems quite reasonable to think) that we should not expect at all to find the same effects when manipulating perceptions of inequality and when measuring the consequences of living and experiencing inequality in the course of one's life. More work using both methods is also needed to explore the relationship between true income inequality and perceptions of income inequality, and how the two may, together, shape one's economic optimism.

Finally, this paper focuses some attention on economic optimism, something with far-reaching consequences for both individuals and society as a whole. Understanding how income inequality affects economic optimism has important policy implications. It may help explain why people may or may not support economic policies (and by extension, the political leaders and parties that support and oppose such policies). Moreover, many policies—particularly those most likely to find some bipartisan support—seek to address inequality through opportunity (McCall 2013; 2016). Understanding people's perceptions of their opportunity may be important for understanding who takes advantages of new opportunities and ultimately who will benefit from proposed policies. While "optimism" connotes a positive perspective (in general, more is better), the other side of the same coin is despair (Kearney and Levine 2014; 2016). Efforts to counteract how income inequality—an inequality of outcomes—may tend to translate into an inequality of opportunity will need to address these mutually reinforcing processes.

Chapter 4

Less Equal, Less Trusting? Longitudinal and Cross-sectional Effects of Income Inequality Trust in U.S. States, 1973–2012

Does income inequality reduce social trust? Although both popular and scholarly accounts have argued that income inequality reduces trust, some recent research has been more skeptical, noting these claims are more robust cross-sectionally than longitudinally. Furthermore, although multiple mechanisms have been proposed for why inequality could affect trust, these have rarely been tested explicitly. I examine the effect of state-level income inequality on trust using the 1973–2012 General Social Surveys. I find little evidence that states that have been more unequal over time have less trusting people. There is some evidence that the growth in income inequality is linked with a decrease in trust, but these effects are sensitive to how time is accounted for. While much of the inequality and trust research has focused on status anxiety and feelings of relative deprivation, this mechanism receives the weakest support, and mechanisms based on societal fractionalization and exploitation receive stronger support. This analysis improves on previous estimates of the effect of state-level inequality on trust by using far more available observations, accounting for more potential individual and state level confounders, and using higher-quality income inequality data based on annual IRS tax returns. It also contributes to our understanding of the mechanism(s) through which inequality may affect trust.

4.1 Introduction

Income inequality in the United States has risen to its highest levels since the Great Depression (Piketty and Saez 2003 [2015]). Many have raised alarm about this trend, suggesting high or rising income inequality has a variety of negative consequences.[1] A substantial part of the concern has been placed on trust, or the potential lack thereof. Scholars have long proposed that trust is critical to a functional and flourishing society (e.g., Simmel 1950; Lewis and Weigert 1985), and thus the possibility that greater inequality reduces trust is cause for concern.

A substantial number of analyses conducted at the cross-national level report that, on average, more equal countries also have populations that are more trusting of each other (e.g. Bjørnskov 2008; Larsen 2013; Rothstein and Uslaner 2005; Uslaner 2002; Layte 2012). A smaller set of studies have argued for a similar relationship at the level of U.S. States (e.g., Kawachi et al. 1997; Uslaner 2002; Wilkinson and Pickett 2009; Fairbrother and Martin 2013). The stylized "fact" of a negative trust-inequality relationship has been disseminated and asserted widely—even then-sitting President Obama cautioning that:

> *"...rising inequality... [is] bad for our families and social cohesion—not just because we tend to trust our institutions less, but studies show we actually tend to trust each other less when there's greater inequality."*[2]

However, despite the appearance of a consensus, these existing analyses are limited in a number of respects. Most of the cross-national studies on trust come from the same sources (primarily, the World Values Survey), and substantial concerns have been raised about the quality of country-level data on income inequality (Ferreira et al. 2015). Similarly, data on state-level inequality have relied on Census-based measures that largely understate top-end inequality and require interpolation in the years between the once-per-decade census (Frank 2014; Galbraith and Hale 2008). Furthermore, most of the existing studies (but with the notable exceptions described in this paper) show only a cross-sectional relationship that makes it difficult to rule out other characteristics of countries or states that might explain why they are both less equal and their populations less trusting. Finally, although every study posits at least one explanation for why inequality would reduce trust, few studies take multiple explanations into consideration. Here, I advance our understanding of the U.S. state-level relationship between income inequality and trust by (1) presenting analyses that include new waves of individual-level trust data and new high-quality state-level

[1] They may also be talking about economic inequality, more broadly. But the research has primarily focused on inequality of income. And in the U.S., income inequality has been a driving factor of other forms of economic inequality (e.g., wealth or consumption inequality) (Piketty 2014; Saez and Zucman 2016).

[2] "Remarks by the President on Economic Mobility" made on December 4, 2013 (https://www.whitehouse.gov/the-press-office/2013/12/04/remarks-president-economic-mobility).

income inequality time-series based on IRS tax returns, and then (2) conducting additional analysis to better understand the plausible mechanisms through which inequality could shape trust.

The paper proceeds as follows: first, I outline three explanations for why inequality might decrease trust. Increases in income inequality may lead to greater social distance between those different places on the income distribution. Because of the human tendency toward homophily, as inequality increases people may interact less often and less effectively with people at different status levels, leading to decreased trust in others (e.g., Kawachi et al. 1997). Alternatively, the driving factor could be the rising incomes of those at the very top of the income distribution. As those at the top pull away, the rest (e.g., "the 99%") may feel exploited, generating distrust between social classes (e.g., Rothstein and Uslaner 2005). Yet a third explanation, rooted in theories of evolutionary development, is that people have different social strategies for social relations in a dominance hierarchy versus an egalitarian society. As inequality increases, people's status anxiety and feelings of relative deprivation increase, prompting behaviors more appropriate for a dominance hierarchy where cooperation and trust are disincentivized (Wilkinson and Pickett 2017).

Next, the I briefly review previous research on inequality and trust, both at the cross-national and state level. In particular, I detail the most rigorous analysis, by far, of inequality and state-level trust presented in Fairbrother and Martin (2013). In contrast to previous studies, they report that while less equal states are indeed less trusting, trust did not decrease more in states that had above-average increases in income inequality. Thus, they discourage interpreting the results as a causal effect of income inequality on trust.

I explain in detail how my research both differs from and advances this research, and I describe the data and methods used to test the inequality-trust relationship. Drawing from the 1973–2012 General Social Survey's linked to state-level administrative data based on IRS tax returns, the decennial Census, and the American Community Survey, I estimate the cross-sectional and longitudinal effects of the state-level Gini index on two measures: a binary measure of trust and six-value trust scale (a reverse-coding of the "misanthropy scale" [Smith 1997]). In these models, I show that a critical component of the model is how one addresses the year variable (or the "secular trend").

In the main models, I find weak evidence that more unequal states have less trusting residents. There is more evidence of a negative longitudinal relationship—a growth in income inequality is associated with a decrease in trust—but these results are sensitive to how time is controlled for. In the analysis of mechanisms, I find some evidence that the results are driven by changes in both very high incomes at the top (supporting the exploitation by the rich mechanism) and the inequality throughout the rest income distribution (supporting the social fractionalization mechanism). I do not find that the inequality-trust relationship is attenuated by accounting for status anxiety. I conclude by discussing why my results differ substantially from those of Fairbrother and Martin (2013) and outlining how future research may better understand the link between income inequality and trust.

4.2 Motivations and Mechanisms

Trusting others is a fundamental part of society, taking a prominent place in early works on the foundations for social exchange (Simmel 1950) and the collective (Durkheim 1933). Social scientists have reiterated the importance of trust in work on economic markets (e.g., Granovetter 1985; Adler 2001), networks (e.g., Coleman 1988), democracy (e.g., Fukuyama 1995; Warren 1999), neighborhood effects (e.g., Sampson et al. 2002), and social cohesion (e.g., Putnam 2000).[3] Research shows that trust itself has declined in the U.S (e.g., Robinson and Jackson 2001; Clark and Eisenstein 2013; Twenge et al. 2014; Putnam 2000) over roughly the same period inequality has increased (Piketty and Saez 2003 [2015]). Could these be causally linked? Below, I present three distinct (though quite possibly overlapping) explanations of why inequality may reduce trust:[4]

Social ties; societal fractionalization

Income inequality may affect trust through the restructuring of social life. By definition, higher income inequality means greater differentials across the income distribution. Because personal networks tend to be homogeneous (McPherson, Smith-Lovin, and Cook 2001), social mixing may be reduced by higher income inequality. This would reduce the number of ties that individuals make with others unlike themselves, a process described by Bjørnskov (2008) as "social fractionalization." Social ties are critical for trust itself (e.g., Coleman 1988), and thus more fractionalization could reduce trust in strangers, as individuals have less incentive to behave in trustworthy ways with those they are less likely to have future interactions with (Coffé and Geys 2006). Furthermore, as the distance grows between people on different parts of the income distribution, the psychological distinction between those in "my group" and outside it becomes increasingly salient, making it harder to identify with and thus trust others (Uslaner 2002).

To be clear, this argument does not necessarily mean people are less disposed towards making connections with others as inequality grows (although this is possible). People tend to trust people similar to themselves, something Fukuyama (1995) describes as one's "trust radius." As inequality increases and the income distribution becomes more spread out, fractionalization occurs and individuals find fewer people

[3] Many of these studies also consider "social capital" more broadly, a term that is often more confusing than helpful (see Fischer 2005). But all the studies cited discuss trust explicitly.

[4] For other reviews of inequality and trust, see Jordahl (2007) and Buttrick and Oishi (2017).

within their "trust radius" who they find easy to trust. As a result, social cohesion and trust decline (Bjørnskov 2008; Kawachi et al. 1997).[5]

Finally, increased separation between income groups means that even when individuals interact across income groups, they may more frequently lack the implicit but distinctive assumptions, values, and taken-for-granted knowledge of others. As a result, the interactions may not be as productive as they otherwise could have been (Ridgeway 2014). Lareau (2002) offers an example of this in how working-class parents and children had much less productive visits to the middle-class doctor than did other middle-class parents and children. Reduced social mixing and greater fractionalization not only means fewer cross-group social interactions, but less fruitful ones, which may further reduce trust.

Exploitation by the rich; resentment by the poor

An alternative explanation focuses specially on the gaps between the rich and poor. Recent concerns about inequality have emphasized the tensions between the rich and poor. The scholarly account of inequality is punctuated with theories of exploitation—most famously elucidated by Marx (Marx and Engels 1972)—and episodes of conflict. Even if revolution is not just around the corner, such themes motivated the Occupy and Occupy Wall Street movements, as exemplified in their slogan, "We are the 99%" (Chomsky 2012). In this account, class conflict produces the distrust.

It is possible that the feelings might not go both ways. For example, the poor might fail to recognize the exploitation of the rich or the rich might be oblivious to the resentment from those less well off. Still, recognition of the feelings in either direction would likely erode trust. If people believe those who are relatively (and increasingly) better off are exploiting them, then they are fundamentally less likely to trust them. Furthermore, if those at the top of the income distribution recognize the resentment, they may also be wary that the poor will act dishonestly towards them, again reducing trust (Fairbrother and Martin 2013).

Status anxiety

Another explanation is that decreasing social trust is a product of the status anxiety that arises from greater income differences between the rich and poor. One version of this is articulated by Wilkinson and Pickett (2017). In short, they argue that human beings have evolved with two psychological strategies for social relations. One is based on a dominance hierarchy and the other a more egalitarian society based on reciprocity, cooperation, and trust. Though individuals use both, growing income

[5]Some have referred to this cohesion as "social capital," though the term has taken on a wide variety of meanings (Putnam 2000; Fischer 2005). Whatever it is called, it has frequently been operationalized as interpersonal trust.

inequality may shift the balance away from latter and toward the former. This is because income inequality separates people by their position in the income distribution, thereby clarifying and accentuating status differences in income and creating a more hierarchal society.

Veblen ([1899] 1963) noted that status is, in part, reaffirmed through conspicuous consumption. In more unequal contexts, there is a greater tendency to focus on positional goods such as housing (Frank 2007; Fligstein et al. 2017), cars (Bricker et al. 2014), and luxury goods (Walasek and Brown 2015; 2016). As status competition picks up, people's status anxiety and feelings of relative deprivation increase. From this, Wilkinson and Pickett (2009; 2017) argue that this growing dominance hierarchy results in an environment that rewards those who pursue naked self-interest, rather than one that promotes reciprocity, cooperation, and trust.

4.3 Previous Findings

These theoretical reasons why inequality might reduce trust have spurred a number of empirical examinations of the subject, primarily across countries, but also across states. In an in-depth comparative project, Larsen (2013) showed how inequality has grown and trust declined in the U.S. and U.K., while the opposite trends for both trust and inequality are observed in Sweden and Denmark. Using only a cross-sectional comparison, Bjørnskov (2008) finds the same relationship in over 100 countries in the World Values Survey (WVS), but notes it is driven by relatively rich countries (specifically, the negative relationship is still observed using only the top half of countries in median income, but is not observed using the bottom half). Other cross-national studies have shown a similar negative relationship (e.g., Rothstein and Uslaner 2005; Uslaner 2002; Layte 2012). Finally, Fairbrother (2013), using the WVS, shows a longitudinal relationship across countries—in countries where inequality has increased between 1981 and 2008, the populations in those countries also became less trusting.

A few analyses have focused on comparisons of U.S. states.[6] In a cross-sectional analysis of five years of the GSS, Kawachi et al. (1997) found inverse relationships between state-level income inequality and trust and between trust and mortality rates, and they concluded that a substantial portion of the effect of income inequality on mortality is mediated by trust. Other studies also found a negative association between income inequality and trust using an aggregate time series of the United States (Uslaner 2002) and in another cross-sectional analysis of U.S. states (e.g., Wilkinson and Pickett 2009).

However, the most thorough analysis of inequality and trust in the U.S. context is provided by Fairbrother and Martin (2013). They separate out the average level of

[6] For examples of non-U.S. within-country inequality-trust studies, see work by Coffé and Geys (2006) and Leigh (2006).

inequality in each state from the over-time variation within states by subtracting each state-year Gini from that state's over-time average (a technique described in greater detail below). Fairbrother and Martin (2013) find a significant negative effect of the cross-sectional measure of state-level inequality, but no effect from the longitudinal measure. In other words, they find less equal states are indeed less trusting, but trust did not decrease more in states that had above-average increases in income inequality. In further checks, they also found no evidence of a longitudinal effect using a 10-year lagged measure of income inequality on trust or with the level of inequality for the respondent's state at age 16, nor did they find any effects of county-level inequality (cross-sectional or longitudinal). From this, they suggest caution in accepting a causal trust and inequality relationship.

With the exception of Fairbrother and Martin (2013), the studies reviewed here all conclude that inequality negatively effects trust. But these studies differ in their explanation, drawing from the theories of social fractionalization, exploitation and resentment, and relative deprivation that were presented above. In the next section, I describe how I use Fairbrother and Martin's analysis as a starting point and comparison for my own analysis, and then further explore how to disentangle the mechanisms.

4.4 The Present Study

In the first part of my analysis, I advance the study conducted by Fairbrother and Martin (2013) with three important improvements. First, their analysis ended with 2004 data, so my analysis (up to 2012) includes 4 more survey waves with over 6,900 additional observations (a 27% increase), as well as a greater variation in income inequality because inequality grew to even higher levels within states between 2004 and 2012.[7] Second, my analysis includes a much larger number of individual and state level controls that could confound the relationship between income inequality and trust. Finally, my analysis uses income inequality data from a new annual series based on IRS tax returns (Frank 2014). Previous state-level Gini measures, including those of Fairbrother and Martin (2013), are based on the decennial Census with, at best, model-based interpolation (using payroll and employment for a set of sectors within states) to fill in the data between years (Galbraith and Hale 2008).

I show that by implementing these three improvements I obtain substantially different results from the same multilevel models that follow Fairbrother and Martin's approach of separating out the cross-sectional and longitudinal differences in inequality.

In addition to the single measure of trust, I use a misanthropy scale (Smith 1997) which has also been used as a measure of general trust (e.g., Twenge et al. 2014; Simpson 2006). Fairbrother and Martin reported that models using this outcome

[7] At present, the IRS-based inequality data is not yet available beyond 2013.

had similar results. I examine this. Using both outcomes, I also show the sensitivity of the results to how time is (or is not) controlled for, and I discuss the costs and promises of each approach. No previous study of inequality and trust examines this crucial specification, which I show can lead to widely differing conclusions.

In the second section, I present analysis using alternative measures of inequality that provide suggestive evidence of the mechanisms behind the findings. Research shows that while inequality has grown across the income distribution, it has been particularly accentuated at the very top (Piketty and Saez 2003 [2015]; Kenworthy 2017). If societal fractionalizing is key to the explaining the trend, we should see stronger results using measures that capture inequality across the income distribution. On the other hand, if exploitation by the very rich is key, then the effect should largely be driven by the growth in income near the very top (i.e., top income shares). In their analysis of how income inequality affects mobility, Chetty et al. (2014) provide a way of separating these effects: they decompose the Gini index into inequality coming from the upper tail and the rest of the income distribution by defining the "Bottom 99% Gini" as the overall Gini index minus the top 1% income share.

Finally, if the negative effect of inequality on trust is about increased feeling of relative deprivation, then we would expect to be able to attenuate the results by controlling for proxies of this. To examine this, I add into the main model two additional variables: financial satisfaction and perceived relative income. If these variables mediate the relationship between income inequality and trust, the effect sizes should be attenuated.

It is possible, of course, that multiple explanations are valid. For example, people could suffer from status anxiety while also resenting the rich while also living in a more fractionalized society. The aim of this part of the analysis is not to pick one dominant explanation, but to determine the extent to which each explanation is supported.

4.5 Data and Methods

General Social Survey

The individual-level data come from the nationally-representative General Social Survey (GSS) which has been conducted annually or biannually since 1972 with response rates greater than 70% (Smith et al. 2013). In addition to its high quality, it is the only long-running nationally representative survey to measure trust. The restricted-access GSS geographic identification file contains state identifiers for every survey year beginning with 1973, with which I match each respondent to a variety of state-level measures.[8] Beginning in 2006, the GSS moved to a panel format, where respondents

[8]Data from the GSS, Census, and ACS are available from the respective websites of each. The inequality data based on IRS tax returns are available at http://www.shsu.edu/eco_mwf/inequality.html. However, the GSS state-level identifiers can only be obtained through special contractual

were reinterviewed in up to two additional waves. In the analyses presented here, I do not include re-interviews, but including them yields similar results. In all, I analyze 31,857 observations in 909 state-years from 1973–2012.[9]

Outcomes

Social trust is measured by asking:

- "Generally speaking, would you say that most people can be trusted or that you can't be too careful in life?" [codebook item: trust]

Four percent of respondents said, "it depends," which I included with "you can't be too careful," but excluding those yielded similar results. This question has been used extensively in studies of trust, but some have raised concerns that this single item can be interpreted as the related concept of "caution" and that the addition of two other measures better captures one's general trust toward others (Simpson 2006). These measures are collected by asking:

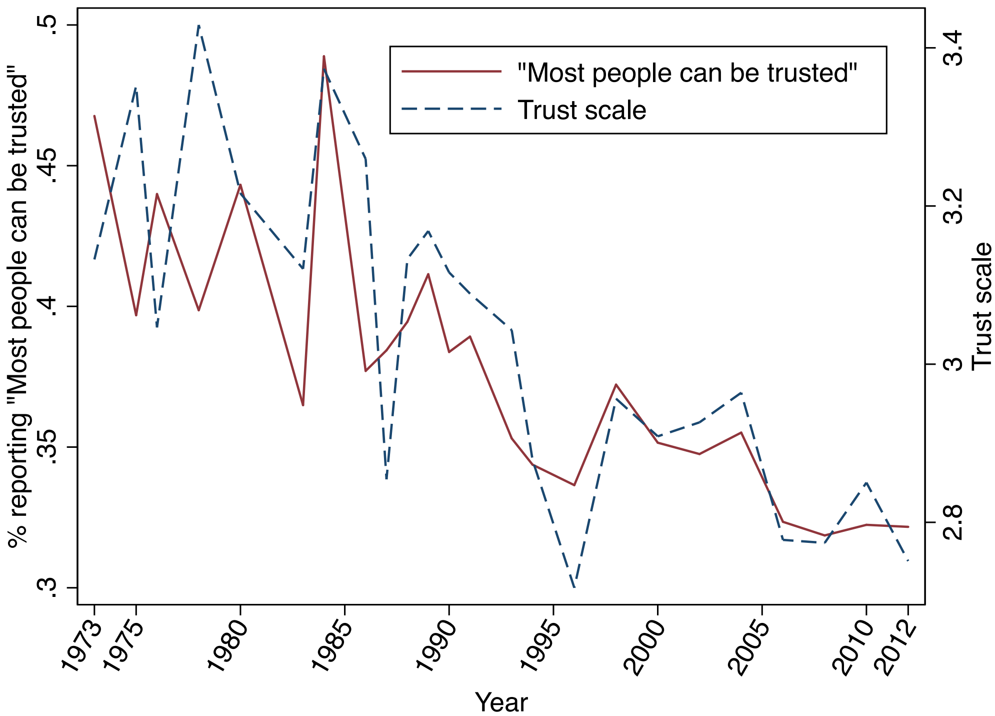
- "Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair?" [fair]
- "Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?" [helpful]

Together, these three questions make up the "misanthropy scale" (Smith 1997). I follow the standard coding that places "it depends" at the midpoint of the other two possible responses to each question, creating a single scale from 0–6 (Crobach's alpha = .67) which has the advantage of providing additional variation in one's level of trust. An analysis of recent panel data show that both the trust item itself and the misanthropy scale have a high degree of reliability (Hout and Hastings 2016). In order to be consistent with the direction of my effects, I reverse code the scale and refer to it as the "trust scale." Figure 4.1 shows that trust—as measured by either outcome variable—has declined substantially between 1973 and 2012.

arrangements with NORC: http://gss.norc.org/documents/other/ObtainingGSSSensitiveDataFiles.pdf

[9] Fairbrother and Martin (2013) report results that exclude District of Columbia. I include DC, which is not so much of an outlier in the IRS-based data. Regardless, I find the results do not substantively differ by excluding DC respondents from the analysis.

Figure 4.1: Trust in Others from 1973–2012 (Source: General Social Survey)



Individual-level Independent Variables

In the models that test for whether the inequality-trust relationship is mediated by feelings of relative deprivation, I add continuous measures of two GSS questions:

- "We are interested in how people are getting along financially these days. So far as you and your family are concerned, would you say that you are pretty well satisfied with your present financial situation, more or less satisfied, or not satisfied at all?" [satfin] (coded 1-3)
- "Compared with American families in general, would you say your family income is far below average, below average, average, above average, or far above average?" [finrela] (coded 1-5)

In all of my analyses, I control for a number of individual level variables that could bias the relationship between income inequality and trust. I control for income, which is measured by asking respondents to place their family's income into an income bin. The bins have changed over time, but the GSS includes a harmonized "best estimate" by assigning each respondent the midpoint of their income bin, except for the open-ended top income bin where incomes were assigned using a Pareto curve (Ligon [1989]

1994; Hout 2004). I control for a logged income measure, although additional analysis using multiple income bins revealed similar results.

I control for race/ethnicity using a four-item coding: non-Hispanic white, non-Hispanic black, other non-Hispanic, and Hispanic. The GSS did not measure Hispanic or Latino ethnicity until 2000, so for observations before that year I use country of ancestral origin. This correlates extremely well with self-identified Hispanic from 2000 onward and has been used in previous research on race and ethnicity (Hout and Goldstein 1994). Previous research finds that racial and ethic groups show different average levels of trust (Smith 2010; Abascal and Baldassarri 2015) and that this trust question works better for non-Hispanic whites than non-Hispanic blacks (Simpson et al. 2007). However, in additional analysis I found that the effect of income inequality on trust did not vary significantly by race or ethnicity.

Trust itself follows a lifecycle, so I control for age and age-squared (Robinson and Jackson 2001; Clark and Eisenstein 2013). Still other controls included sex, years of education (using highest degree earned made no change to the main findings), marital status (married, widowed, divorced, separated, or never married), number of children in the household, number of adults in the household, and religious service attendance (from 0 = never attend to 8 = attend more than once a week).

Area Level Data

Income inequality

The paper focuses on state-level inequality. In theory, it is not readily apparent what is the appropriate unit of aggregation to measure income inequality in order to expect to observe an effect on trust. Unfortunately, there is no equivalent IRS-based annual inequality data at the county level. Furthermore, the GSS geo-coded data only has location identifiers below the state beginning in 1993, making it impossible to examine the effect of inequality during the first 20 years of the survey for smaller levels.[10]

Following all previous research on state-level inequality and trust, my primary inequality measure is the state-level Gini index. The Gini is the average distance between all pairs of proportional income in the population, ranging from 0 (if everyone had the same income) to 1 (if one household had all the income), and assesses inequality across the entire income distribution. The data series is constructed from data published in the IRS's *Statistics of Income* and made available by Frank (2014).

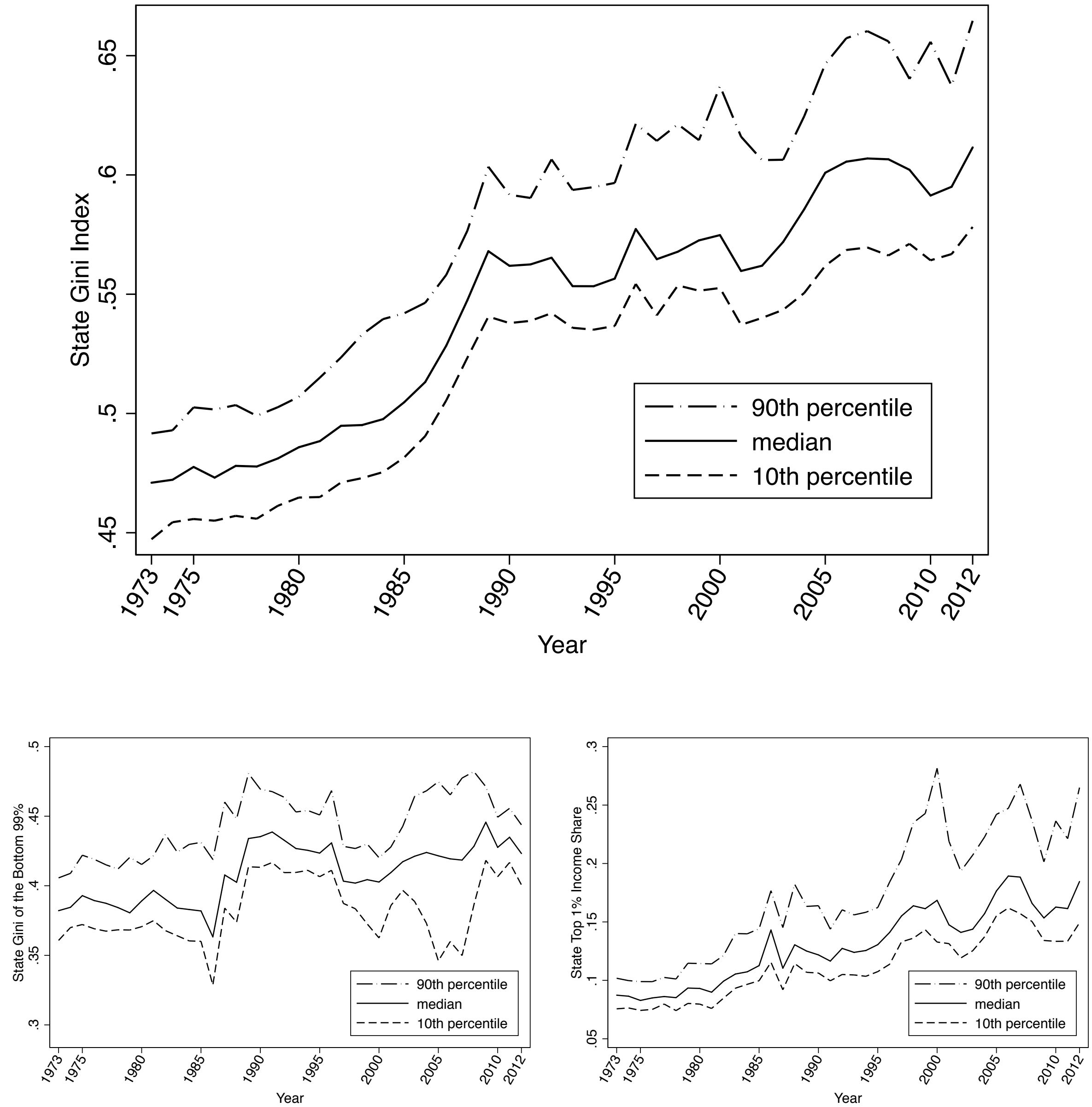
This is the first study I am aware of to use these IRS-based data to examine the relationship between inequality and trust. Previous studies have primarily used state-level inequality measures provided by the decennial Census, although state-level inequality measures can also be obtained from the Current Population Survey (CPS) and the American Community Survey (ACS) (after 2005) (Stone et al. 2016).

[10] Fairbrother and Martin (2013) conducted a county-level analysis on a subsample of the GSS data and found no effect with Census-based inequality data.

However, the IRS data has several advantages. First, the IRS data are based on the full population, whereas Census inequality is based on the long form census (which went to about one in six households). Similar measures from the CPS and ACS are based on even far smaller samples. Second, the Census was only collected each decade with linear or model-based interpolation between years (Galbraith and Hale 2008), while the IRS data are annual. Third, the other data sources rely on self-reported data where lower-income respondents are likely to over-report income and higher-income respondents are likely to under-report income (Akhand and Liu 2002). Of course, the IRS is the only data source that penalizes respondents for their income reporting errors. Fourth, the other data sources are subjected to more top-coding of income, and as a result, the Ginis based on the IRS data are substantially larger (i.e., show more inequality) than those reported by the Census or CPS because they better account for high-income households (Richard et al. 2009). A possible limitation of IRS-based data is that it risks censoring households whose income is below the threshold for mandatory tax filing. However, these households may still file taxes to receive a refund or to benefit from tax credits.

The same data series also includes a calculation of the top income shares, which some scholars have preferred (e.g., Piketty and Saez 2003 [2015]). In a recent paper, Chetty et al. (2014) subtracted the top 1% income share from the overall Gini index. The decomposition separates the inequality coming from the upper tail and the rest of the income distribution—the latter they define as the "Bottom 99% Gini." This decomposition allows me to separate the effect of top-end inequality from the inequality throughout the rest of the distribution.

Figure 4.2: 90th, 50th, and 10th Percentiles of Income Inequality Across States from 1973–2012 (Source: Frank 2014)



The top panel of Figure 4.2 shows the state-level Gini index across states during the analysis period. Two trends are notable. First, consistent with other accounts of income inequality, inequality grew considerably over the period of study. In fact, the Gini of the least equal state in 1973 (.50 in the District of Columbia) was lower than the most equal state in 2012 (.55 in West Virginia). Second, the gap between the 10th and 90th percentiles is nearly twice as large in 2012 as 1973, meaning that inequality grew faster in some states than others. It is this variation that will be used to assess the effect of inequality on trust. The two bottom panels of Figure 4.2 show the same trends for the Top 1% Income Share and the Bottom 99% Gini. Both show substantial variation over time. Although both grew, the increase of the Bottom 99% Gini was substantially more modest than the growth of the Top 1% Income Share, whose median nearly doubled between 1973 and 2012.

Other state level data

At the state level I control for state-level income per capita (but using logged median household income—which is less sensitive to high incomes—produced nearly identical results). Racial and ethic diversity may also affect trust (e.g., Putnam 2007; Stolle et al. 2008; but see Abascal and Baldassarri 2015), so I include measures of % black and % foreign born. Hispanic or Latino ethnicity was not consistently recorded in the census until 1980 and is not included, but the changing trends should be largely captured in % foreign born. I account for population shifts and growth by controlling for state population density (logged), and I include a southern state indicator variable, as previous research has found that Southerners are, on average, less trusting than non-Southerners (Simpson 2006). These state-level controls are the same as those used by Fairbrother and Martin (2013) except for the inclusion of % foreign born. Table 4.1 presents descriptives of all individual and state-level measures.

Table 4.1: Descriptives

	mean	*sd*	*min*	*max*	*count*
Trust (binary)	0.38	0.49	0	1	31857
Trust (scale)	3.05	2.23	0	6	29457
Between State Gini	0.55	0.03	.48	0.70	31857
Within State Gini	0.00	0.05	−.11	0.15	31857
Between State Top 1% share (SD)	0.00	0.50	−.88	2.95	31857
Within State Top 1% share (SD)	−0.00	0.87	−2.1	3.25	31857
Between State Gini 99% (SD)	0.00	0.56	−.97	3.68	31857
Within State Gini 99% (SD)	−0.00	0.83	−2.9	3.89	31857
Financial satisfaction	2.04	0.74	1	3	30586
Perceived relative income	2.91	0.84	1	5	30465
Log(income)	10.76	0.98	6.2	12.6	31857
Female	0.53	0.50	0	1	31857
Age (years)	43.87	16.54	18	89	31857
(Age x Age)/100	21.98	16.12	3.2	79.2	31857
Non-Hispanic white	0.78	0.42	0	1	31857
Non-Hispanic black	0.12	0.33	0	1	31857
Non-Hispanic other	0.03	0.17	0	1	31857
Hispanic	0.07	0.26	0	1	31857

continued on next page

Table 4.1 – continued from previous page

Married	0.62	0.49	0	1	31857
Widowed	0.06	0.24	0	1	31857
Divorced	0.10	0.30	0	1	31857
Separated	0.03	0.16	0	1	31857
Never married	0.20	0.40	0	1	31857
Num of adults	2.22	0.91	1	8	31857
Num of children	1.96	1.78	0	8	31857
Religious service attendance	3.81	2.69	0	8	31857
Years of education	12.87	3.07	0	20	31857
Urban	0.60	0.49	0	1	31857
Suburban	0.27	0.45	0	1	31857
Rural	0.13	0.34	0	1	31857
State income/capita	25.03	5.48	13	54.2	31857
State percent foreign born	0.08	0.07	.007	0.27	31857
State population density (logged)	4.97	0.99	.017	9.28	31857
State percent black	0.13	0.08	.0023	0.71	31857
Southern state	0.35	0.48	0	1	31857

Standard errors in parentheses
* $p < .05$, ** $p < .01$, *** $p < .001$
Notes: Descriptives are weighted to account for sampling. Individual level data come from the General Social Survey 1973-2012. State level data come from the Census (1970, 1980, 1990, 2000), American Community Survey (2006-2012), and IRS Statistics of Income (1973-2012).

Analytical Strategy

Main models

I employ a multilevel modeling strategy that allows for the simultaneous but separate estimation of cross-sectional and longitudinal relationships. The key feature of this model, used also by Fairbrother and Martin (2013), is to decompose and separately enter the mean of inequality for each state (the "between" or "cross-sectional" portion) and the deviations in each state-year from the state mean (the "within" or "longitudinal" portion). Denoting the states by s and years by t, I define:

$$Gini\_Between_s = \overline{Gini_{st}} \tag{4.1}$$

$$Gini\_Within_{st} = Gini_{st} - \overline{Gini_{st}} \tag{4.2}$$

and estimate the effect of both measures with a random effects model (also known as a multilevel, mixed, or hierarchical linear model). Comparisons with and advantages of this approach over fixed effects models are discussed extensively in Raudenbush and Bryk (2002), Bell and Jones (2015), and Fairbrother (2013). However, I also estimated supplemental models with state fixed effects which are presented in the Appendix. Models with state fixed effects can only show the within-state change (because the

$Gini\_between_s$ is controlled away), but the estimated effect of $Gini\_within_{st}$ is nearly identical between the random effects and fixed effects models.

For the binary trust outcome, I use a logit specification. Formally, the individual-level model is:

$$\begin{aligned} Ln\left(\frac{Trust_{ist}}{1-Trust_{ist}}\right) = \quad & \beta_0 + \beta_1 Gini\_Within_{st} + \beta_2 Gini\_Between_s \\ & + \beta_3 Individual\_covariates_{ist} + \beta_4 State\_covariates_{st} \\ & + \left[\beta_5 Time_t\right] + \zeta_s + \zeta_{st} + \epsilon_{ist} \end{aligned} \tag{4.3}$$

where β_3 and β_4 denote vectors of all individual and state-level controls, and ζ_s and ζ_{st} denote random state and state-year effects. This approach accounts in the standard errors for the non-independence of observations within shared contexts without needing post-hoc clustering adjustments (Schmidt-Catran and Fairbrother 2016). For analysis with the continuous trust scale as the outcome, the entire left hand side of the equation is simply replaced with $Trust(scale)_{ist}$.[11] Because the outcome is the trust of the respondent at the moment of completing the survey, I use level of income inequality from the survey year. It is possible, however, that respondents take some time to "update" their perceptions, so for robustness I also estimated models that lagged inequality by a year and I found substantively identical results.

β_5 is in brackets to denote that I consider and present three ways of accounting for time, which have substantial consequences for the estimate of the $Gini\_Within_{st}$ coefficient. First, I estimate a model without any time controls. This approach utilizes the entire growth of income inequality over time shown in Figure 4.2. Using the full variation in inequality allows for a more efficient estimate, but the approach may fail to account for other aspects that were changing in the U.S. between 1973 and 2012 besides the level of income inequality. Already, the most likely time-varying confounders are controlled for at the state level: changes in income per capita, population density, percent black, and percent foreign born. However, another omitted variable could still be factor.

Another approach that I estimate is to account for a possible "secular trend" by adding a linear year term as a control. Both inequality and trust follow roughly linear trends upward and downward, respectively, and the linear term nets out these trends. This parsimonious approach controls for the linear version of any unobserved confounding variables, and is the model implemented by Fairbrother and Martin (2013).

Finally, the most conservative approach that I estimate is to include year fixed effects (an indicator variable for every year). This approach controls for all aggregate

[11] Models were estimated in Stata 14.2 with the `melogit` and `mixed` commands using the sampling weights provided by the GSS.

trends. Any national level factors or shifts that could change state-level inequality and trust are accounted for. But this approach also nets out the average level of inequality for each year, forcing the estimate to be based exclusively on changes in income inequality within states *net* of the overall changes of income inequality across states. By presenting all three time specifications, I am able to compare how sensitive the results are to these different approaches.

Testing the mechanisms

In the second part of my analysis, I estimate three sets of additional models in order to better understand the mechanisms. First, I present models that examine the effects of inequality measured by the Top 1% Income Share and Bottom 99% Gini. This allows for the comparison of the effects of the right-tail inequality of the top 1% (which is key to the exploitation mechanism) and the inequality spread out through the rest of population (key to the social fractionalization mechanism). Then, I estimate models adding additional controls for financial satisfaction and perceived relative income. If relative deprivation (or feelings of it) are key to explaining the decrease in trust, then the effect of inequality should attenuate once these are accounted for.

4.6 Results

Main results

The full models that examine the effect of income inequality on the binary trust measure and the continuous trust scale are presented in Tables 4.2 and 4.3. To visualize and more easily compare the key results, the income inequality coefficients are also presented in a coefficient plot in Figure 4.3.

Table 4.2: Coefficients (in Log Odds) from Models of Trust in Others

	(1) Trust (binary)		(2) Trust (binary)		(3) Trust (binary)	
Between State Gini	−2.40	(1.55)	−1.09	(1.38)	−0.41	(1.50)
Within State Gini	−4.45***	(0.57)	−1.26*	(0.62)	0.017	(0.99)
Log(income)	0.21***	(0.019)	0.21***	(0.019)	0.21***	(0.019)
Female	−0.10**	(0.032)	−0.10**	(0.032)	−0.11***	(0.032)
Age (years)	0.043***	(0.0053)	0.043***	(0.0053)	0.043***	(0.0053)
(Age x Age)/100	−0.025***	(0.0053)	−0.025***	(0.0054)	−0.025***	(0.0054)
Non-Hispanic white	0	(.)	0	(.)	0	(.)
Non-Hispanic black	−1.06***	(0.083)	−1.05***	(0.083)	−1.07***	(0.083)
Non-Hispanic other	−0.51***	(0.066)	−0.50***	(0.067)	−0.51***	(0.068)
Hispanic	−0.51***	(0.061)	−0.50***	(0.060)	−0.51***	(0.060)
Married	0	(.)	0	(.)	0	(.)
Widowed	−0.10	(0.064)	−0.10	(0.063)	−0.10	(0.062)
Divorced	−0.14**	(0.047)	−0.13**	(0.047)	−0.13**	(0.047)
Separated	−0.24**	(0.077)	−0.24**	(0.078)	−0.24**	(0.079)
Never Married	0.052	(0.051)	0.058	(0.051)	0.055	(0.051)
Num of adults	−0.020	(0.017)	−0.021	(0.017)	−0.022	(0.017)
Num of children	−0.0053	(0.0075)	−0.0056	(0.0076)	−0.0053	(0.0075)
Religious service attendance	0.027***	(0.0071)	0.026***	(0.0071)	0.026***	(0.0071)
Years of education	0.17***	(0.0066)	0.17***	(0.0067)	0.17***	(0.0067)
Urban	0	(.)	0	(.)	0	(.)
Suburban	−0.042	(0.037)	−0.040	(0.037)	−0.045	(0.037)
Rural	0.021	(0.063)	0.032	(0.062)	0.032	(0.060)
State income/capita	−0.0069	(0.0059)	0.0080	(0.0065)	0.0074	(0.0086)
State percent foreign born	0.065	(0.67)	0.22	(0.60)	0.29	(0.62)
State pop. density (log)	−0.027	(0.042)	−0.056	(0.042)	−0.051	(0.042)
State percent black	−0.82	(0.60)	−0.76	(0.56)	−0.86	(0.55)
Southern state	−0.33**	(0.11)	−0.29**	(0.10)	−0.28**	(0.10)
Year (linear)			−0.022***	(0.0038)		
Year fixed effects	*No*		*No*		*Yes*	
Constant	−4.20***	(0.98)	38.4***	(7.41)	−4.94***	(0.89)
var(State)	0.031*	(0.012)	0.026*	(0.011)	0.029*	(0.012)
var(State-year)	0.041***	(0.0088)	0.033***	(0.0077)	0.010	(0.0073)
Observations	31857		31857		31857	

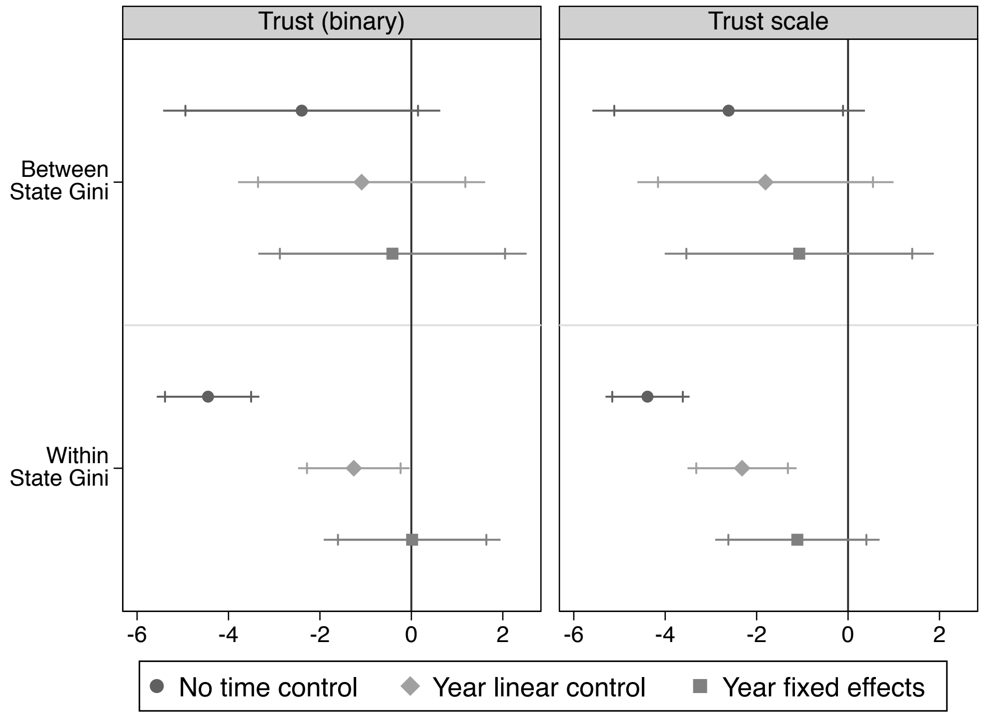
Standard errors in parentheses
* $p < .05$, ** $p < .01$, *** $p < .001$
Note: Models from multilevel mixed-effects logistic regression.

Table 4.3: Coefficients from Models of Trust Scale

	(1) Trust (scale)		(2) Trust (scale)		(3) Trust (scale)	
Between State Gini	−2.61	(1.52)	−1.81	(1.43)	−1.07	(1.50)
Within State Gini	−4.39***	(0.47)	−2.32***	(0.61)	−1.11	(0.92)
Log(income)	0.21***	(0.013)	0.21***	(0.013)	0.21***	(0.014)
Female	0.17***	(0.026)	0.17***	(0.026)	0.16***	(0.026)
Age (years)	0.042***	(0.0040)	0.042***	(0.0040)	0.043***	(0.0040)
(Age x Age)/100	−0.016***	(0.0041)	−0.016***	(0.0041)	−0.017***	(0.0040)
Non-Hispanic white	0	(.)	0	(.)	0	(.)
Non-Hispanic black	−1.05***	(0.079)	−1.05***	(0.079)	−1.06***	(0.077)
Non-Hispanic other	−0.37***	(0.085)	−0.37***	(0.087)	−0.38***	(0.090)
Hispanic	−0.49***	(0.069)	−0.48***	(0.068)	−0.48***	(0.067)
Married	0	(.)	0	(.)	0	(.)
Widowed	−0.13*	(0.055)	−0.14*	(0.055)	−0.13*	(0.054)
Divorced	−0.25***	(0.030)	−0.24***	(0.030)	−0.24***	(0.030)
Separated	−0.26***	(0.063)	−0.26***	(0.063)	−0.26***	(0.064)
Never Married	0.11*	(0.046)	0.11*	(0.046)	0.11*	(0.045)
Num of adults	−0.020	(0.015)	−0.020	(0.015)	−0.021	(0.015)
Num of children	0.0047	(0.0084)	0.0046	(0.0084)	0.0047	(0.0083)
Religious service attendance	0.056***	(0.0070)	0.055***	(0.0069)	0.055***	(0.0069)
Years of education	0.17***	(0.0053)	0.17***	(0.0054)	0.17***	(0.0055)
Urban	0	(.)	0	(.)	0	(.)
Suburban	−0.050	(0.042)	−0.048	(0.043)	−0.054	(0.043)
Rural	0.087	(0.061)	0.093	(0.061)	0.090	(0.059)
State income/capita	0.0027	(0.0052)	0.013*	(0.0064)	0.015	(0.0091)
State percent foreign born	−0.20	(0.73)	−0.029	(0.72)	−0.21	(0.74)
State pop. density (log)	−0.037	(0.042)	−0.060	(0.041)	−0.061	(0.039)
State percent black	−0.63	(0.64)	−0.59	(0.60)	−0.61	(0.58)
Southern state	−0.38***	(0.10)	−0.34***	(0.095)	−0.34***	(0.095)
Year (linear)			−0.014***	(0.0031)		
Year fixed effects	*No*		*No*		*Yes*	
Constant	−1.08	(0.95)	26.9***	(6.06)	−1.91*	(0.90)
var(State)	0.035***	(0.012)	0.030***	(0.011)	0.031***	(0.011)
var(State-year)	0.053***	(0.0091)	0.051***	(0.0090)	0.031***	(0.010)
var(Residual)	4.02***	(0.033)	4.02***	(0.033)	4.03***	(0.033)
Observations	29457		29457		29457	

Standard errors in parentheses
* $p < .05$, ** $p < .01$, *** $p < .001$
Note: Models from multilevel mixed-effects linear regression.

Figure 4.3: Coefficient Plots of Main Models with Different Year Controls. Error bars show 95% confidence intervals; ticks show 90% confidence intervals.



In all six models, the between-state coefficient is negative, but non-significant. Contrary to previous cross-sectional studies, as well as the findings of Fairbrother and Martin (2013), there is little evidence that states with higher levels of income inequality have lower levels of trust.[12]

In contrast to the null findings of the between effect, there is some evidence of a within effect. In the multilevel logit models, the effect is negative and large with no time control (Model 1). This is consistent with the descriptive pattern that—net of individual-level characteristics and other observed time-varying state measures—trust decreased where inequality increased. With a year trend (Model 2), the preferred model of Fairbrother and Martin (2013), the effect of within-state inequality is much smaller, but remains significant. For example, a 0.1 increase in the Gini within a state would predict a $[1 - e^{(-1.26*.1)} =]$ 11% decrease in the probability a respondent would

[12]One might be surprised that the cross-sectional between estimate would vary at all by the inclusion of time controls. This is a compositional issue. Although the between-state Gini is constant for each state, not every state appears in the GSS in every year, and states that appear more often in later years than early years have higher average levels of inequality. The time controls adjust for this too. Nevertheless, a comparison of the between-state estimates (Clogg, Petkova, and Haritou 1995) with and without year controls shows they are not statistically different from each other.

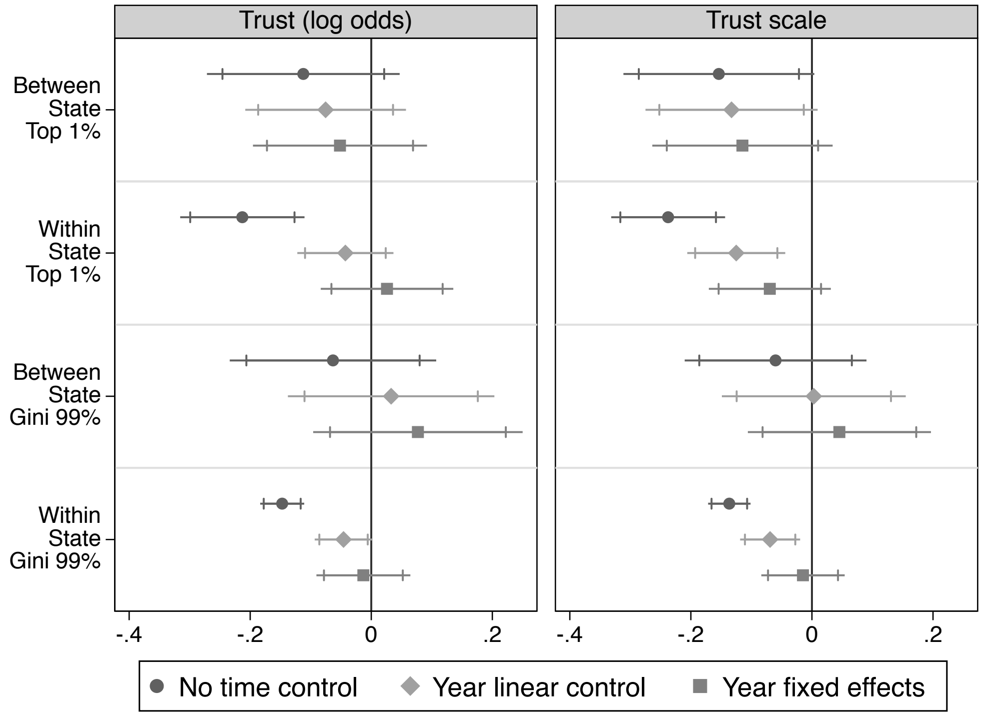
believe most people can be trusted. Once the more conservative year fixed effects are applied, this between-state coefficient is nearly zero and not significant.

The same trend is observed in the multilevel models of the trust scale. Again the results are significant in Models 1 and 2. In the latter model a 0.1 increase in the Gini within a state would predict a 0.23 decrease in trust on the 6-point scale. The within-state Gini is not significant once year fixed effects are included. Note, however, that for both sets of models the addition of year fixed effects does not only reduce the effect size, but also expands the standard errors (because there is less variation in inequality from which to derive the estimate). In Model 3 from the trust scale model, the magnitude of the within-state Gini is more than double the standard error from Model 1 (i.e., the Model 3 coefficient would be statistically significant if it had the certainty of Model 1).

Mechanisms

Next I present models that show the effects of the Top 1% Income Share and Bottom 99% Gini. As before, I separate out the cross-sectional and longitudinal effects. The inequality coefficients are plotted in Figure 4.4 (tables of the full results are in the Appendix). For clarity in comparing the coefficients of the two inequality measures, I standardized (mean = 0, standard deviation = 1) both the Top 1% Income Share and Bottom 99% Gini before decomposing them into the within and between state measures.

Figure 4.4: Coefficient Plots of Models with Alternative Inequality Measures. Error bars show 95% confidence intervals; ticks show 90% confidence intervals.



For both measures, the patterns are fairly similar, both to each other, and to the effect of the Gini in the main results. At the top of the figure are the coefficients for the Top 1% Income Share. The cross-sectional results of the Top 1% Income Share are consistently negative but, at most, marginally significant ($p<.1$) in the trust-scale models with no time control or the linear year measure. The longitudinal effect of Top 1% inequality is negative in both models without time controls and the trust-scale model with a year linear term. For example, in this latter model, a one-standard deviation increase in the Top 1% Income Share is associated with a 0.13 decrease on the trust scale.

The cross-sectional effect of the Bottom 99% Gini is close to zero and non-significant, but the longitudinal effect of the Bottom 99% Gini is negative and significant in the models without time or with a year linear control (but at $p<.1$ in the case of binary trust outcome). Using the same model above, a one-standard deviation increase in the Bottom 99% gini is associated with a 0.07 decrease in trust.

These results suggest that the negative (but non-statistically-significant) coefficient of the between-state Gini found in the main models is largely driven by states with higher top 1% income shares. However, our interest is particularly in the within-state effect. There is some evidence of a causal relationship between lower trust and both changes in the Top 1% Income Share and Bottom 99% Gini, but these effects almost entirely disappear in the most conservative models with year fixed effects. There is no clear evidence that either type of inequality—the extreme pulling away of the rich or the inequality in the rest of the population—is driving the results from the main models.

To test for the feelings of relative deprivation mechanisms, I reestimate the main models while controlling for financial satisfaction and perceived relative income. I present models with the key coefficients in Tables 4.4 and 4.5 (and the full models are in the Appendix). Because some cases had missing data on at least one of the two new controls, I also reran the main models using the same subsample. These are shown in Models 1, 3, and 5 (with no time control, year linear term, and year fixed effects, respectively), and the models with the new controls are in Models 2, 4, and 6 (with the same time controls).

Table 4.4: Coefficients (Log Odds) from Trust Models Controlling for Financial Satisfaction and Perceived Relative Income

	(1)	(2)	(3)	(4)	(5)	(6)
Trust (binary)						
Between State Gini	−2.47	−2.34	−1.15	−1.08	−0.35	−0.20
	(1.60)	(1.61)	(1.41)	(1.43)	(1.53)	(1.54)
Within State Gini	-4.41^{***}	-4.24^{***}	−1.22	−1.21	0.24	0.37
	(0.57)	(0.57)	(0.63)	(0.63)	(1.04)	(1.03)
Financial satisfaction		0.16^{***}		0.16^{***}		0.16^{***}
		(0.023)		(0.023)		(0.024)
Perceived relative income		0.12^{***}		0.12^{***}		0.12^{***}
		(0.018)		(0.018)		(0.019)
Year (linear)			-0.022^{***}	-0.021^{***}		
			(0.0038)	(0.0037)		
Year fixed effects	*No*	*No*	*No*	*No*	*Yes*	*Yes*
Individual controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	30427	30427	30427	30427	30427	30427

Standard errors in parentheses
* $p < .05$, ** $p < .01$, *** $p < .001$

Table 4.5: Coefficients from Trust-Scale Models Controlling for Financial Satisfaction and Perceived Relative Income

	(1)	(2)	(3)	(4)	(5)	(6)
Trust (scale)						
Between State Gini	−2.68	−2.48	−1.89	−1.76	−1.10	−0.88
	(1.52)	(1.55)	(1.43)	(1.47)	(1.51)	(1.55)
Within State Gini	-4.39^{***}	-4.13^{***}	-2.35^{***}	-2.29^{***}	−1.08	−0.90
	(0.47)	(0.46)	(0.61)	(0.60)	(0.93)	(0.95)
Financial satisfaction		0.25^{***}		0.25^{***}		0.25^{***}
		(0.024)		(0.025)		(0.025)
Perceived relative income		0.12^{***}		0.12^{***}		0.12^{***}
		(0.018)		(0.018)		(0.018)
Year (linear)			-0.014^{***}	-0.013^{***}		
			(0.0031)	(0.0030)		
Year fixed effects	*No*	*No*	*No*	*No*	*Yes*	*Yes*
Individual controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	29275	29275	29275	29275	29275	29275

Standard errors in parentheses
* $p < .05$, ** $p < .01$, *** $p < .001$

If feelings of relative deprivation explains the effect, the inequality coefficients should attenuate. This does not happen at all. Although financial satisfaction and perceived relative income are both positively associated with trust, they barely reduce the inequality coefficients. These models suggest that inequality does not reduce trust because inequality increases people's feelings of relative deprivation.[13]

4.7 Discussion and Conclusion

This paper finds little evidence that cross-sectional state-level inequality (the between effect) is associated with less trust. That is, states that have been more unequal over time do not necessarily have less trusting people. There is some evidence, however, that the growth in income inequality (the within effect) is associated with a decrease in trust. However, these longitudinal effects are much weaker when year fixed effects are included.

I do not take a strong position on which time control is the "best." The more conservative the approach, the less likely that there are unobserved confounders bi-

[13]This is effectively a test of mediation. To control for the possibility that the effect of inequality on trust is mediated by feelings of relative deprivation, it may be necessary to include an interaction term, or else the model assumes there is no interaction between the cause (income inequality) and the mediator (feelings of relative deprivation) (VanderWeele and Vansteelandt 2009). I find similar results for the other coefficients with and without interaction terms.

asing the results, but the more true variation in inequality that is also netted out. In the most conservative model, the year fixed effects net out most of the growth in inequality that we are precisely interested in understanding the effect of. If some other trending characteristics are driving the main longitudinal results, these unidentified characteristics would have to operate independently of the other time-varying state-level controls and independent of even a year time trend.

These findings also differ substantially from those reported by Fairbrother and Martin (2013), who—using the year time trend specification—found a strong negative cross-sectional inequality effect and absolutely no longitudinal effect. What explains the differences? To explore this, I conducted a supplemental analysis in which I first began with their model (I was able to obtain nearly, but not exactly, the same coefficients) and then iteratively changed parts to make it more like mine. I found two critical differences:

First, my inequality measure is based on annual IRS tax returns, instead of voluntarily self-reported income from a sample of Census respondents. Merely replacing their model with the IRS data series makes the cross-sectional effect go away entirely. For reasons specified in the data section above, I believe the tax return data are better, and tax-based data have become the staple of economic research (e.g., Chetty et al. 2014; 2017; Piketty 2014; Piketty and Saez 2003 [2015]; Saez and Zucman 2016). Unfortunately, to date, no one has provided a thorough consistent comparison of the two state-level inequality measures. Second, I find that the addition of state % foreign born substantially reduces the cross-sectional effect in the Fairbrother and Martin (2013) model. Given the income differences between racial and ethnic groups, it is unsurprising that % foreign born is correlated with inequality. As reviewed above, there is a contentious debate on the possible relationship between diversity and trust. Once this is accounted for, even the results based on Census inequality data are weakened to the point of being non-significant.[14]

This paper also presented two different but popular measures of trust—the binary trust question and the trust scale (the reverse-coded misanthropy scale). Both gave similar results, although the evidence in favor of a negative relationship is slightly stronger using the scale measure.

The results of the analysis of mechanisms are inconclusive, but provides a jumping off point for future research. While one popular and highly cited component of the inequality-trust literature has focused on status anxiety and feelings of relative deprivation, this mechanism received the weakest support, having almost no effect in my analysis. In contrast, the mechanisms of societal fractionalization and exploitation by the rich both receive more support.

More work is needed to understand these mechanisms. For example, future studies could investigate how income inequality is related to societal fractionalization. This

[14]I am very grateful to Malcolm Fairbrother for corresponding with me about the analysis in Fairbrother and Martin (2013) and my own analysis.

mechanism is also consistent with research suggesting rising economic inequality in the U.S. has increased economic residential segregation (Mayer 2002; Reardon and Bischoff 2011). But neighborhoods represent only one dimension of where widening stratification based on income may occur.

Similarly, to understand the exploitation and resentment mechanisms, researchers need better measures of these feelings. McCall (2013; 2016) provides the best research on what people think about the rich, especially as it relates to inequality, but unfortunately, these respondent attitudes were only measured in a few more recent waves of the GSS. Now that these concerns have moved more to the forefront, future studies will undoubtedly measure these important attitudes and provide more data to analyze.

Taking a step in the other direction, it is also important for research to study if and how trust itself is the pathway between income inequality and many important social problems. In fact, this work finds that the relationship between inequality and trust is not especially large. As such, if there is a "net effect" of income inequality on outcomes of interest, these results suggest that trust—despite being a prominently discussed factor—may not have a great deal of explanatory power. At minimum, researchers should not solely focus on trust. And more generally, this study underlies the importance of considering multiple mechanisms in seeking to understand the consequences of income inequality (Neckerman and Torche 2007; Moss et al. 2013). Doing so is an important step towards the greater effort to effectively evaluate and address the broader consequences of income inequality.

Chapter 5

Compared to Average? Geographic and Sociodemographic Reference Group Effects on Perceived Relative Income

Social comparisons of income have far-reaching consequences for individual decision-making and public policy, yet there persists a significant gap between "true" relative income and what Americans perceive. Although one compelling explanation is that reference groups affect what people perceive as "average," there is little consensus about who people compare themselves with. Previous research has proposed reference groups based on both geographic proximity and on sociodemographic similarity, but few studies have considered multiple reference groups systematically or simultaneously. Using the 1998-2014 General Social Surveys linked to administrative data, I examine how reference groups help explain perceived relative income. The effect of reference group income depends on both egoist and fraternal comparisons: higher median incomes of large reference groups and those with weak status hierarchies increases perceived relative income, while higher median incomes of small reference groups and those with strong status hierarchies decreases perceived relative income. These results have implications for how reference groups are used in research on income inequality, neighborhood effects, and residential segregation.

5.1 Introduction

Rising income inequality in the U.S. has amplified the longstanding concerns social scientists have expressed about the social inequality that accompanies economic differences between households.[1] While the consequences of these differences manifest themselves in multiple ways, many scholars believe how these differences are *perceived* matters for important outcomes such as happiness, health, and beliefs about inequality itself. In this study, I focus on how one's household income and the incomes of various reference groups—sub-national groups defined by geographic proximity or sociodemographic similarity—shapes the way people report how their household's income compares to "American families in general" (hereafter, perceived relative income).[2]

Perceived relative income is a measure of one's perception of an objective fact, because relative income is simply a function of one's income and the average income of Americans, and both quantities can be easily known. However, there is frequently a "gap" between one's real and perceived relative income. One explanation for this gap is that people look to reference groups to make evaluations about what is average. Yet despite this intuition that reference groups should matter, there is little theoretical or empirical consensus on how or with whom people compare themselves. Previous research has focused on various dimensions of two broad forms of reference groups: geographic proximity and sociodemographic similarity. However, irrespective of the outcomes being investigated, few studies have sought to understand how different types of reference groups within either category may matter differently (e.g., counties vs states; racial vs occupational reference groups), and no previous study has sought to adjudicate between geographically- and socidemographically-defined reference groups. The latter point is particularly important because those with similar characteristics are more likely to live closer together.

To understand how reference groups matter, I link respondents from the 1998-2014 General Social Surveys (N = 15,789) to reference group data assembled from the Census, Small Area Income and Poverty Estimates (SAIPE), Current Population Survey (CPS), and American Community Survey (ACS). I analyze the effect of the

[1] This study focuses on the United States. Income and reference groups clearly matter throughout the world, and thus the results may be generalizable to other contexts. However, the other countries may vary on which geographic entities (e.g., states, counties, and neighborhood) and sociodemographic characteristics (e.g., race, education, and age) are salient in people's lives.

[2] Technically, a household includes all people who occupy a housing unit regardless of relationship. My outcome measure is of the "family income" but for many of my reference groups I only know the median "household income." More than 97% of the respondents I examine (who are between ages 25–65) live in households comprised only of family members. Furthermore, "non-family" household members could include unmarried partners who respondents may also consider part of their family. I use "family" and "household" interchangeably in this paper. Excluding respondents with a non-related person in their household produces nearly identical results for my analyses. See the Methods section for more details.

median income of one's reference groups on perceived relative income. I find that including reference groups substantially improves the model of perceived relative income, and I find two distinct patterns to the direction and size of the reference group effects: for smaller geographic reference groups and for sociodemographic groups with a strong status hierarchy, the fraternal comparison between one's group and others dominates, yielding a positive effect of reference group income. For large geographic reference groups and sociodemographic groups without a strong status hierarchy, the egoist comparison dominates and the effect of reference group income is negative.

This paper demonstrates that reference group income is important for understanding perceived relative income, an outcome with far-reaching implications on both individual decision-making and public policy. Furthermore, this paper contributes broadly to our understanding of how and with whom people make economic comparisons. This is particularly applicable to the study of income inequality, where there is considerable disagreement on the level at which to measure income inequality. To the extent that the effects of inequality depend on relative comparisons between households, my results suggest that the effects should be stronger when inequality is measured at the level of states or commuting zones rather than smaller areas—a finding that that is consistent with empirical studies of inequality's consequences (e.g., Wilkinson and Pickett 2009; Pickett and Wilkinson 2015). This paper also shows that—consistent with the neighborhood effects literature—people do not receive a psychological boost from living in poorer neighborhoods even though it makes them richer relatively (e.g., Leventhal and Brooks-Gunn 2000; Sastry 2012). Finally, this paper shows that people construct economic reference groups based on both geographic and sociodemographic characteristics, which is particularly important for thinking about the consequences of increasing residential segregation within the U.S. (Taylor and Fry 2012; Reardon et al. 2015).

5.2 Perceived Relative Income

Understanding social comparisons has been a fundamental part of the sociological endeavor. In *The Theory of the Leisure Class*, Veblen ([1899] 1963) described how social status is demonstrated and reified through the "conspicuous consumption" that others see and compare themselves. Other classic discussions of social comparisons include Marx's (1847 [1972]) writings on class consciousness and Durkheim's ([1912] 1951) analysis of suicide. Income has long been a key marker of social stratification and is associated with a number of positive outcomes for individuals. While people can make social comparisons across a wide range of dimensions, income remains one of (if not, the) most salient dimensions studied by social scientists. Empirically, it is more difficult to systematically compare who has what (wealth) or spends what (consumption) than to compare who makes how much. Beyond this practical advantage, studying income is highly relevant because income is what enables the lifestyles that are competed over.

Perceptions of income are often implicitly crucial to many explanations of income's importance. This is illustrated in the study of happiness. The "Easterlin paradox" (Easterlin 1973) notes that higher earners are happier at any point in time, yet average happiness in a society does not increase as income rises. One explanation is that the satisfaction from greater income derives from having more than one's peers have, meaning that one's relative, not absolute, income is what matters. If income is important because of its marker of relative status, the direct implication is that how people *perceive* of their income differences with others is crucial to understanding how income affects happiness (Schnittker 2008).[3]

More generally, perceived relative income is thought to help explain levels of satisfaction with income, work, and life itself (Guven and Sorensen 2012; Senik 2009), and thus influences decisions about work, family, and finances. Furthermore, perceived relative income also helps capture what people believe is "average" in America, something that affects beliefs about fairness and equality and the extent to which people may support or oppose public spending and safety nets (Brown-Iannuzzi et al. 2015).

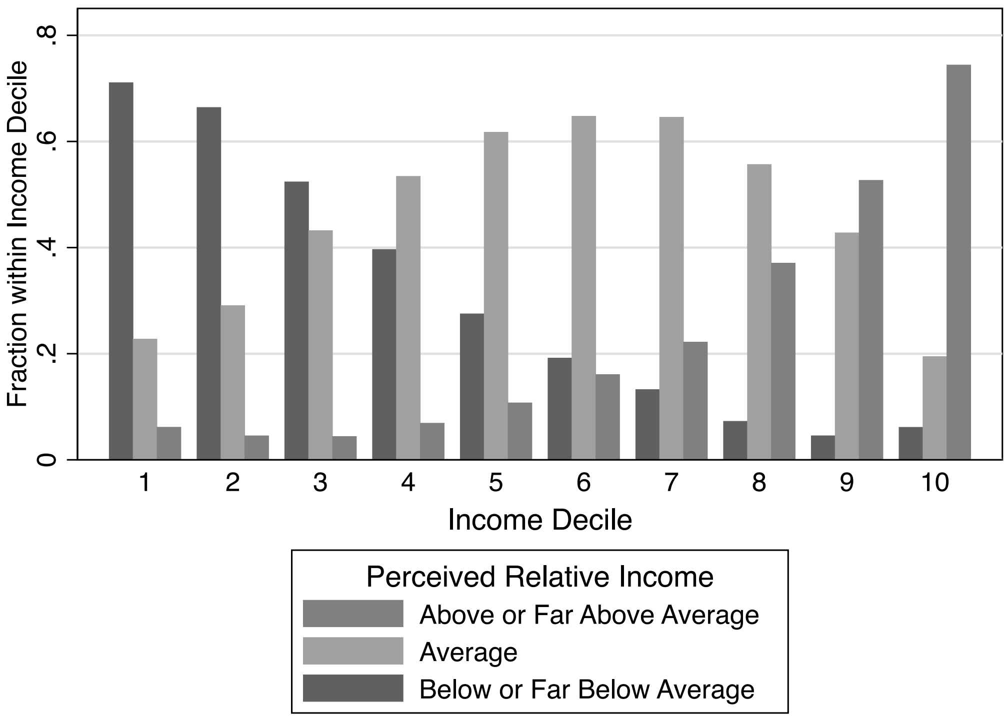
Yet despite these far-reaching implications, the influences on perceived relative income are not well understood. One measure of perceived relative income comes from the long-running General Social Survey. It asks: "Compared with American families in general, would you say your family income is far below average, below average, average, above average or far above average?" The question has been used as an explanatory variable, both by itself and in a scale of perceived economic standing (e.g., Kalleberg and Marsden 2012), but rarely as an outcome. Among the exceptions, Hout (2003) found that real income affected perceived relative income "30 to 40 percent more in the 1990s than it had in the early 1970s" and DePianto (2011) found that some of the relationship between real and perceived relative income was moderated by race and sex.

In Figure 5.1, I plot perceived relative income for each decile of the national income distribution among GSS survey respondents between 1998–2014 (data are described in the Data section below). Forty-five percent report having an average income. Unsurprisingly, this is highest for those in the middle of the income distribution (as high as 65% for those in the 6th income decile), but also includes 41% of those in the 9th income decile and 17% of those in the top income decile, as well as 21% and 29%

[3] One empirical test of this argument found little actual mediating effect of perceived income on the income-happiness relationship in explaining the Easterlin paradox in the case of the U.S. (Schnittker 2008). This study attributed more explanatory power to changes in family structure. Other counterarguments to this relative-income explanation of the Easterlin paradox are that income is not rising for all, but only (or at least primarily) for the rich (Morris and Western 1999; Piketty and Saez 2003 [2015]) and also that median household incomes rose only because Americans (especially women) worked more hours on average and not because hourly wages rose (Fischer 2008). These other factors might also account for (some of) the failure of happiness to rise in America that motivates the Easterlin paradox.

of those in the lowest and second-lowest income deciles, respectively.

Figure 5.1: Distribution of Perceived Relative Income by National Income Decile Rank



Notes: Far below average is combined with below average. Far above average is combined with above average. Source: General Social Survey 1998–2014.

This measure asks about income relative to "American families in general"—in other words, it asks people to compare how they believe they compare to the national average. However, other reference points are also possible for making comparisons, and this may help explain something puzzling: how, as seen in Figure 5.1, can nearly half of working age Americans, including many at both the top and bottom of the income distribution, believe they are average? How can nearly five percent of those in the top 30% believe they are below average? And similarly, how can nearly five percent of those in the bottom 30% believe they are above average? Next, I consider how various "reference groups" may shape people's understanding of average.

5.3 Reference groups: Theoretical Origins, Applications, and Conceptual Advances

In *The American Soldier*, Stouffer et al. (1949) described how both promoted and non-promoted soldiers in World War II were more critical of the promotion system in units with high promotion rates. This observation has become the quintessential example of comparative reference group behavior—in units with high promotion rates, non-promoted soldiers experienced more opportunities for "relative deprivation" by comparing themselves to promoted peers, and promoted soldiers experience fewer opportunities for the "relative gratification" that occurred when comparing themselves to non-promoted peers (Stouffer et al. 1949; Merton and Rossi 1950; Davis 1959). From *The American Soldier* to the present, the logic of reference group comparisons has been ubiquitous in explanations of social science phenomena—ranging from religious pluralism affecting the volunteering behavior of the non-religious (Borgonovi 2008) to "compensation networks of peer groups" elevating CEO pay (DiPrete et al. 2010).

Two prominent examples of income reference groups appear in studies of happiness and health. The Easterlin Paradox described above focused on how national-level comparisons affect happiness, but studies have demonstrated an effect of reference group incomes—for example, the average income of subnational geographic units (e.g., Luttmer 2005; Firebaugh and Schroeder 2009) and of subnational age-specific categories (e.g., Firebaugh and Tach 2012)—on happiness or life satisfaction. The health literature presents a second prominent example of how reference group logic is employed. Here the argument is that negative income comparisons are stressful, and they induce less healthy lifestyles and more risky behaviors. For example, Eibner and Evans (2004) found that being deprived relative to a reference group was positively associated with smoking and negatively associated with exercise and regular seat belt use. Other health examples come from Wilkinson and Pickett, who document an association between income inequality and poor mental health, poor physical health, drug use, obesity, mortality, and teen births (Wilkinson and Pickett 2006; 2009; Pickett and Wilkinson 2015). They conclude that the negative inequality-health association is explained by the status anxiety of income comparisons.[4]

Although understanding how reference groups themselves matter remains an empirical challenge—something this paper helps to address—scholars have made several conceptual advances about reference groups. First, a distinction needs to be made between reference groups and interaction groups (Merton [1949] 1968). The key insight is that who one interacts with is not necessarily the same as who one compares oneself with. In principle, someone can compare oneself to anyone else. But, second, people generally compare themselves to those who are somewhat similar to themselves (Festinger 1954). This insight supports the common-sense notion that reference groups will be comprised of those who share some similar characteristics, whether it be some

[4]Other scholars are more cautious about the causal claims (e.g., Deaton 2003; Lynch et al. 2000). The potential complicating factor, which also matters for the happiness studies reviewed above, is that variation in income may also be associated with differences in the material resources available that are important to maintaining good health—in other words, relative differences (which are the foundation of income comparisons) and absolute differences (e.g., who has access to superior health care or better opportunities) are highly correlated and it is difficult to separate the two.

aspect of geographic proximity or sociodemographic similarity. Lipset (1985:73) recognized both of these insights, noting that "[Reference groups] can, but need not be, groups to which an individual belongs," but at the same time "individuals... judge their own status by comparison with smaller, more closely visible groups."

Third, reference groups provide structure for two kinds of comparisons with *opposing* effects (Runciman 1966). The first kind of comparison—which is more prominent in the reference groups literature—is between the individual and their group. This "egoist comparison" leads to the expectation that, net of one's own income, higher income of one's reference group will be negatively associated with one's perceived position.

However, a second comparison is between one's own group and everyone else. This "fraternal comparison" leads to the expectation that, net of own income, higher income of one's reference group will be positively associated with income. The second comparison has also been described as "relative subordination" or "relative superiority" depending on whether one's group is better or worse off than others (Davis 1959).

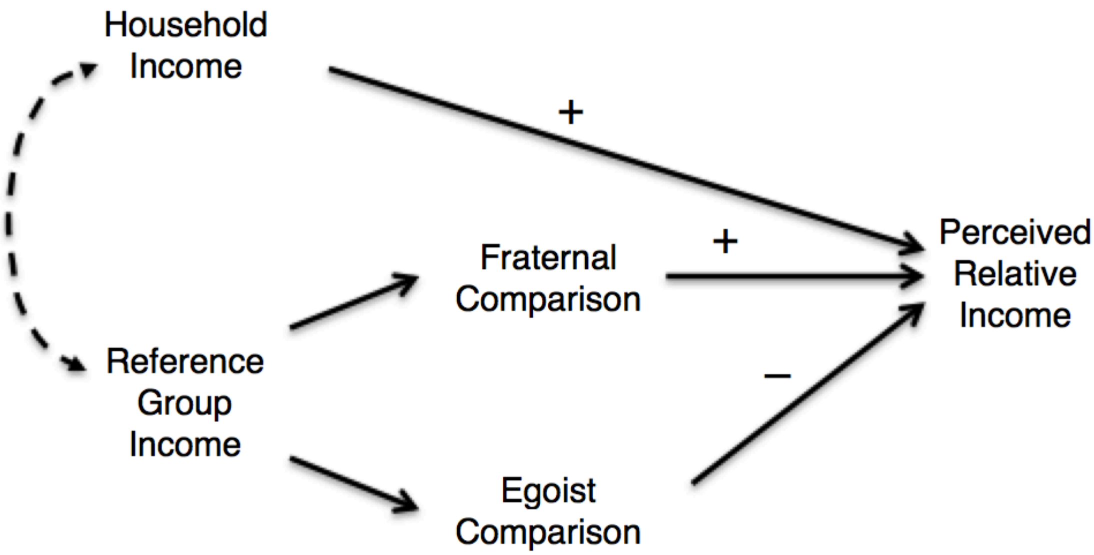
Despite being conceptually distinct, the two comparisons are empirically inseparable if we also seek to account for the independent effect of household income itself (which we expect to be very important). The reason is this: the fraternal comparison depends on the reference group's income, and the egoist comparison depends on the household's income relative to the reference group's income. Thus, we have only two parameters (household income and reference group income) to determine three effects (household income effect, egoist effect, and fraternal effect).[5]

Figure 5.2 illustrates the pathways by which household and reference group income affect perceived relative income. The curved dashed line denotes that household income and reference group income may be associated (e.g., living in a particular area or being part of a certain racial group may be associated with income-related advantages or disadvantages). The top pathway shows that household income is expected to be positively associated with perceived relative income, as described by Hypothesis 1. The bottom portion of the figure shows the expectation that reference group income will have an independent effect on perceived relative income. Thus, I propose the following hypothesis:

> *Hypothesis 1: Net of household income, reference group income will substantially help explain some of the perceived relative income.*

[5]The problem is similar to the well-known age-period-cohort problem, where it is impossible to simultaneously assess the effects of all three.

Figure 5.2: Pathways and Expected Directions of Effects on Perceived Relative Income



Although we cannot distinguish the positive and negative effects though the fraternal and egoist comparisons (the bottom two pathways of Figure 5.2), we can determine the *net* reference group effect. For some reference groups, the (negative) egoist effect may dominate the reference effect, while for others, the (positive) fraternal effect may be greater. Next, I describe some of these reference groups and under what conditions we may expect one effect to be larger than the other.

5.4 Reference Group Effects

Despite the seemingly intuitive fact that income comparisons to one's reference groups matter, there is little theoretical or empirical consensus as to *which* groups these are. Sometimes studies of reference groups explicitly acknowledge this problem. For example, Miller and Paxson (2006) noted that "Reference groups could in theory be defined by any level of geography (e.g., a neighborhood, a state, or even an entire country), or could instead be defined by membership in a common group (e.g., a religious or ethnic group) that cuts across regions." Frequently, however, scholars make an implicit choice of reference group without a theoretical or empirical prior, and without much attention to the consequences.

The lack of clarity about specific reference groups has been a source of some critique. Pettigrew pointed out in the late 1970s the "failure to explain adequately how the relevant comparisons are selected in the first place" (1978:36). The four decades since have produced only minimal progress. In practice, reference groups have generally been based either on geographic proximity or sociodemographic similarity, and within each of the two categories scholars have proposed and used a variety of criteria to select particular reference groups.

Geographic reference groups

Geographic areas are natural units for reference groups, since people are physically bound by space.[6] But what size? Administrative units provide income data for a range of well-defined geographic areas of difference sizes. Studies have employed reference groups at the level of U.S. states (e.g., Eibner and Evans 2004; Wilkinson and Pickett 2009), Public Use Microdata Areas (PUMAs) (e.g., Miller and Paxson 2006; Luttmer 2005), metropolitan statistical areas (MSAs) (e.g., Charles and Lundy 2013), counties (e.g., Firebaugh and Schroeder 2009), census tracts (e.g., Hipp 2007), and census block groups (e.g, Firebaugh and Schroeder 2009).

Unfortunately, few analyses have attempted to compare multiple levels of geographic reference groups across this wide range of possibilities. However, two notable exceptions provide helpful insights: first, in a meta-study of research on the association between income inequality and health, Wilkinson and Pickett (2006; 2009) found a consistent negative relationship for studies at the cross-national level (in 80% of the studies at this level) and at the level of states, regions, and cities (70%), but they found much weaker support at the level of smaller areas like counties and census tracts (45%). They found the opposite effect for the average income of the area—average income was predictive of better health in small areas, but at the national level it was not. They concluded that,

> *"If inequality within small areas is less important than inequality across the whole society, perhaps what we are seeing is a reflection less of social comparisons between neighbors than of the effects of the extent of social class differentiation in society as a whole... The reason a small, deprived neighborhood within a rich nation is likely to have poor health is not because of the inequality within that neighborhood, but because the neighborhood is deprived in relation to the rest of society."* (Wilkinson and Pickett 2009:503)

Second, Firebaugh and Schroeder (2009) used reference groups defined by county and census-block in the same model to explore happiness. They found that county income had positive effects while census-block income had negative effects (i.e., happiness was maximized by living in a rich neighborhood in a poor county). From

[6] Cross-national studies that explain findings with theories based on social comparisons implicitly assume that the nation is a reference group itself. Studies that examine the U.S. over time are also assuming that the nation is a reference group, but in this case the comparison is between time periods in the same country. This paper focuses on how sub-national reference groups affect how people actually think they rank in the U.S.

these results they proposed the "geographic scale principle of income effects" where residential income's effect on individual happiness reverses from positive to negative with increasing geographic scale. The conclusions of both studies are consistent with Runciman's (1966) distinction between egoistic (individual) and fraternal (group) comparisons, but they largely ignore the variation in the physical deprivation and material resources of smaller areas. The neighborhood effects literature has extensively documented how low-income neighborhoods are frequently disadvantaged in multiple ways (for reviews, see Leventhal and Brooks-Gunn 2000; Sastry 2012; Sharkey and Faber 2014). The material differences between neighborhoods may overwhelm any effect from social comparisons with those nearby.

Thus, this may help explain why there is little evidence of a negative reference group effect at the neighborhood level. For example, in their review of neighborhood effects, Leventhal and Brooks-Gunn (2000) note that one way neighborhood conditions could matter is by how individuals evaluate their own situation relative to neighbors or peers, but they do not review work suggesting this may be the case. Such types of effects have not been noted in later studies either (Sastry 2012; Sharkey and Faber 2014); however, these studies have typically focused on poor neighborhoods that lack critical material resources.

Altogether, this literature provides a fairly consistent picture of how social comparisons work. When it comes to perceived relative income, they suggest that the effects for small geographic reference groups should be dominated by the fraternal comparison. In contrast, for the large geographic reference groups, the effect should be dominated by the egoist comparison. Fraternal comparison effects of reference group income should have a positive effect. But for egoist comparisons, reference group income should have a negative effect.

> *Hypothesis 2a: The effect of geographic reference group incomes on perceived relative income is negative for larger areas.*
>
> *Hypothesis 2b: The effect of geographic reference group incomes on perceived relative income is positive for smaller areas.*

I test these hypotheses using four geographic reference groups: state, commuting zone, county, and census tract (which has been the default to represent neighborhoods [Sharkey and Faber 2014]).

Sociodemographic reference groups

Scholars have also proposed reference groups based on various sociodemographic characteristics. Reference groups based on race and ethnicity are the most prominent example. Social comparisons between racial and ethnic groups (i.e., fraternal comparisons) form the foundations for interrelated theories of group threat, intergroup prejudice, and group position theory (e.g., Quillian 1996; Pettigrew et al. 2008; Bobo

1999; Willer et al. 2016). At the same time, some evidence suggests that different racial groups may evaluate their income differently because their comparisons are with others in their same group (i.e., egoist comparisons) (DePianto 2011).

Scholars have also noted effects for reference groups defined by age (e.g., Firebaugh and Tach 2012) and birth cohorts (e.g., Deaton 2001). Still other scholars have focused on occupation. For example, in *Choosing the Right Pond*, Frank (1985) emphasizes this comparison:

> *"People in similar circumstances, even though located far away, can be even more important than people nearby whose circumstances are markedly different. For example, a 35-year-old vice president in a bank branch in San Francisco may take a much greater interest in the salary of her counterpart at the Los Angeles branch than in the salary of the 50-year-old dentist in her own neighborhood."* (pp. 33-34)

The literature above clearly suggests that sociodemographic reference groups can serve as sources for both fraternal and egoist comparisons—comparisons that would have opposite effects on perceived relative income. Under what conditions might we expect one effect to dominate the other? Returning to the example of the bank vice-president: it is possible that she more negatively perceives her income because of comparisons to higher earning vice-presidents, but it is also likely that she recognizes that those in her occupation generally make more than the security guard working at the bank's entrance. This latter fraternal comparison—comparing one's occupation's average income to that of other occupations—could result in more positively perceiving income, net of what one actually makes.

I propose that the stronger the status hierarchy of groups within a reference group category is, the stronger the fraternal comparison will be. For example, education has a clear status hierarchy, where those with higher degrees are always above those with less (within the education context). Thus, I expect those with high educational attainment would have a tendency to overestimate their relative income, while those with low education attainment would have a tendency to underestimate it. In contrast, there is no status hierarchy for birth cohort, so I expect little fraternal comparison. Net of one's own income, I expect that the higher the cohort's average income, the less well off people will perceive their positions, because being in a birth cohort with higher income means doing less well relative to the cohort reference group. More generally:

> *Hypothesis 3a: The effect of sociodemographic reference group incomes on perceived relative income is positive for groups that have a strong status hierarchy.*
>
> *Hypothesis 3b: The effect of sociodemographic reference group incomes on perceived relative income is negative for those that have a weak status hierarchy.*

I test these hypotheses using four reference groups defined by educational degree, occupational prestige, race/ethnicity, and birth cohort. Education has the strongest status hierarchy of the four. Occupations have a weaker status hierarchy; scholars rank occupations but recognize that prestige scores are subjective and that the corresponding occupational prestige rankings are not universally accepted (e.g., Treiman 1977; Nakao and Treas 1994). Racial and ethnic groups have a more complicated hierarchy—although such groups are often ostensibly defined by origin and ancestral lineage, scholars have shown how the formation of racial groups themselves occurs through a process of consolidating advantage and disadvantage (Omi and Winant 1994), and a racial income gap has been well documented (e.g., Grodsky and Pager 2001). Finally, birth cohort has little status hierarchy. Although income varies in predictable ways through the life course, we have little reason to think that particular birth periods are of higher status than others. I expect the strongest positive effects for reference groups defined by educational degree, and I expect the effect to decrease (shift towards negative) for occupational prestige, race-ethnic, and birth cohort reference groups.

5.5 Data and Methods

Data

The individual-level data come from the nationally-representative General Social Survey (GSS) (Smith et al. 2015). The restricted-access geocoded data are available at the census tract level beginning in 1998, so I use the eight waves of available data, collected in even numbered years from 1998 to 2014.

Perceived relative income

Perceived relative income [codebook item: finrela] is categorized into five responses: "far below average," "below average," "average," "above average," and "far above average." I analyze the dependent variable as a continuous, standardized (mean = 0; standard deviation = 1) measure. Few responses are far below average (6.9%) or far above average (2.6%). Collapsing to a continuous three-value measure (combining "far below" with "below" and "far above" with "above") yields nearly identical results, as does analyzing the five-value measure as a categorical variable with an ordered logistic regression model (Appendix Table 9). Using Tobit models to account for lower and upper limit censoring (e.g., because respondents cannot choose to say they are "very far below" or "very far above" average) also yielded substantively identical results (Appendix Table 10). During the period I study there has been little change in the percent of responses to each category of the dependent variable (Appendix Figure 1), so I analyze the cumulative version of the measure. Furthermore, as described below, whatever nationwide year-to-year changes exist are also accounted for by adding year fixed effects to all models.

The GSS question is well-suited for studying reference groups because it is a question about social comparison itself. In contrast, although satisfaction, happiness, and health may be affected by social comparisons, they are also more likely to be strongly shaped by actual material differences between those in different reference groups.

Household income

Income in the GSS is collected by asking respondents to place their family's income into income bins. The bin ranges varied over time, but the GSS has created a harmonized best estimate (Hout 2004). My reference group measures are at the household level, so I treat the report of the total family income measure as household income. Although a household includes people who occupy a housing unit regardless of relationship, less than 3% of my respondents (N = 383) include a non-related person in their household. Part of this is that the GSS excludes those living in institutional arrangements and because I focus on ages 25–65, whereas younger adults and the elderly are more likely to co-reside with unrelated others. Furthermore, non-related household members could still include unmarried partners and their children, depending on whether the respondents think of them as "family" or not. Excluding the households with non-related household members produces nearly identical results.

Work on income inequality suggests the consequences of income comparisons are driven primarily by comparisons based on income rank (Boyce et al. 2010), and in additional analyses I confirmed that income rank best captured the relationship between household income and perceived relative income. To create a national-level income rank measure, I placed respondents in their income decile relative to all respondents in their survey year.

Reference group income

I append to each respondent the best available data on the median household income of each type of reference group. I focus on median income because it captures what people think of as the "average" and follows previous work (e.g., Firebaugh and Schroeder 2009; Firebaugh and Tach 2012). State and county median incomes come from the Small Area Income and Poverty Estimates (SAIPE), which provide model-based annual estimates and are the official measures used for determining how federal programs are administered. Commuting zones (CZs) are aggregations of counties designed to capture local labor markets (Tolbert and Sizer 1996; Autor and Dorn 2013). They are similar to metro areas and MSAs but cover the entire U.S. CZ median income is estimated by averaging the SAIPE county-level incomes in each CZ, weighted by the total population in each county. This is not necessarily the true median, since a weighted average of the median in multiple subpopulations is not the same as the median of the total population. However, it provides a close approximation, and dropping the CZ-level reference groups produced similar results for the remaining reference groups.

Census tract income is estimated by using the 2000 census (which collects 1999 income) and 2005-2012 American Community Survey (ACS), where each year is based on the average of the available five-year ACS estimates that include that year. Linear interpolation is used to cover missing years (1998, 2002, 2004). Tract boundaries were updated by the Census Bureau in 2000 and 2010. 1998 GSS respondents are coded with 2000 Census tract boundaries, so 1990 census tracts do not appear in the data. But panel respondents who were first interviewed in 2006 or 2008 are coded with 2000 census tracts for all waves (including those in 2010 and 2012). For 2010 and 2012 observations coded with 2000 census tracts, I estimated what the median income of old tracts would have been by using the Longitudinal Tract Data Base to recombine new census tracts (Logan et al. 2014).

I use three race-ethnicity reference groups: non-Hispanic white, non-Hispanic black, and Hispanic. GSS began to measure Hispanic in 2000, so for 1998, I use the respondent's reported country of ancestral origin (Hispanic = originated from Mexico, Puerto Rico, or Spain). This correlates extremely well with self-identified Hispanic from 2000 onward and has been used in previous research on race and ethnicity (Hout and Goldstein 1994). I exclude the "other" race category among non-Hispanic respondents because it contains quite a bit of heterogeneity (e.g., both Asians and Native Americans) and is unlikely to capture a true reference group. Additional analyses including a non-Hispanic "race other" reference group found similar results.

I use five educational degree reference groups: less than high school, high school or GED, associates degree, bachelors degree, and professional/graduate degree. I obtained the median income of each race-ethnicity reference group and educational degree reference group from the Current Population Survey (CPS) public-use microdata.

For the birth cohort reference groups, I use the median income of those born within five years of the respondent (an 11-year window). Using the CPS microdata, I calculate year-cohort estimates, and then append those to each GSS respondent. For example, for a respondent born in 1976 and surveyed in 2002, the measure would be the median household income in 2002 of all CPS respondents born between 1971 and 1981. I found nearly identical results using smaller 9-year and 7-year windows.

I create measures of occupational prestige reference group income using the General Social Survey.[7] I divide the sample for each survey year in occupational prestige

[7]Instead of using occupational prestige, another approach would be to match respondents to the income of others in the same occupation. Unfortunately, the occupational coding schemes between the GSS and CPS are not the same in most years because the CPS updated its occupational classification more frequently than the GSS. Yet another approach would be to collapse the GSS respondents into a broader classification scheme such as the occupational class scheme of Weeden

quintiles. The GSS prestige scores were updated in 2010 (Hout et al. 2016), but this is not a problem because reference group comparisons are within year. The effects of the occupational prestige reference group income can only be calculated for respondents who have an occupation, so my analysis excludes respondents without an occupation (but this still includes the unemployed and those currently not working who report an occupation). Additional analyses that include respondents without an occupational prestige score found similar results for the other reference groups.

For each reference group, I analyze the effects of both real and logged median income. Measures based on one's reference group's rank or household-to-reference-group-median ratio are problematic because of the high collinearity of one's position in multiple reference groups. If someone is a high earner relative to their state, they are, on average, also quite high relative to their county, census tract, racial-ethnic group, etc. In theory, this variation could be useful, because one might not rank quite as high in one group as in another. However, the correlations are so large that they violate the standard regression assumptions. In preliminary analyses using these measures, the variance inflation factors for these reference group variables always surpassed 10 (a rule of thumb for high multicollinearity) and frequently surpassed 100. No such problem occurs using reference group income or logged income. Table 5.1 presents the means, standard deviations, and correlations for perceived relative income, household income rank, and the reference group incomes in dollars adjusted to 2014 dollars using the Consumer Price Index (CPI-U-RS). The same table available in the appendix using logged reference group income (Appendix Table 2), but the correlations are nearly the same.

Table 5.1: Descriptives and Correlations of Perceived Relative Income and Key Independent Variables using Reference Group Income (in units of $10,000) (N = 15,789)

	Mean	SD	Correlation									
			1	2	3	4	5	6	7	8	9	10
1. Perceived relative income	0	1.00	1									
2. Household income decile (national)	5.57	2.85	0.59	1								
3. State income	5.60	0.77	0.09	0.15	1							
4. Commuting zone income	5.69	1.17	0.14	0.22	0.70	1						
5. County income	5.73	1.47	0.17	0.26	0.54	0.78	1					
6. Tract income	6.14	2.73	0.32	0.47	0.34	0.48	0.55	1				
7. Degree income	6.78	2.41	0.35	0.43	0.12	0.19	0.19	0.32	1			
8. Prestige income	6.36	2.20	0.32	0.41	0.12	0.17	0.17	0.28	0.57	1		
9. Cohort income	6.48	0.91	0.08	0.16	0.07	0.07	0.07	0.11	0.06	0.10	1	
10. Race/ethnicity income	6.53	1.31	0.17	0.24	0.01	-0.01	0.09	0.26	0.22	0.22	0.12	1

and Grusky (2005). However, these are based on the 1970 Standard Occupational Classification (SOC codes). At present, a crosswalk exists to merge these into the 1980 SOC codes (which covers the 1998-2010 in the GSS) but not into the 2010 SOC codes (for 2012 and 2014).

Other variables

I include a number of individual and household level controls, adding each as categorical variables: sex, marital status (married, widowed, divorced, separated, never married), number of adults in the households (1, 2, 3+), number of children in the household (0, 1, 2, 3+), political views (conservative, moderate, liberal, and unknown/other) and work status (full time, part time, temporarily not working, unemployed, retired, student, keeping house, and other). Finally, I include year fixed effects to net out changing national level factors that may confound the relationship between the reference group income and perceived relative income. I do not control for any of the reference groups themselves (i.e., reference group fixed effects), because this would net out the variation between reference groups for which I am interested in estimating the effect. I limit my sample to respondents between ages 25-65 to capture reference group effects for those in the "working adult" life stage. Only 63 cases were dropped due to listwise deletion of the control variables, leaving a final analytical sample of 15,789. Descriptives of all the variables from the analyses are available in Table 3 of the Appendix.

Method

The analytic approach is to estimate a linear regression model of perceived relative income. Formally, let P denote perceived relative income, I denote household income rank, R denote a vector of each reference group category's median income, and Z denote a vector of the remaining control variables (including the year indicator variables). The linear regression model I estimate is

$$P_i = \beta_0 + \beta_1 I_i + \sum_r \beta_r R_{ir} + \sum_z \beta_z Z_{iz} + \epsilon_i$$

where subscript i denotes the respondent.

To test Hypothesis 1 (whether reference groups improve the model) I compare a model that excludes reference group incomes (i.e., no β_rs) with models that include them. I evaluate whether they improve the model by using two standard model-fit statistics: Akaike's Information Criterion (AIC) and the Bayesian Information Criterion (BIC) (Raftery 1986). The BIC rewards simpler models more than the AIC, but in both statistics the preferred model is the one with the lowest value.

To test the remaining Hypotheses, I focus on the β_rs, which show the net effects of reference group incomes. For each β_r, a negative coefficient is evidence that the egoist comparison is stronger than the fraternal comparison, while a positive coefficient suggests the opposite. For parsimony, the control variables (β_zs) are not shown or discussed in the paper, but all coefficients from the full model are in the Appendix, as are the β_1s and β_zs for every model.

In addition to estimating the full models with reference groups of all eight types, I also estimate separate models for each type of reference group individually, and I separately estimate models with the four geographic reference groups and then with the four sociodemographic reference groups. As shown in Table 5.1, nearly all reference group incomes are positively correlated with each other, so I pay particular attention to interpreting the results from the full model that shows the effect of each reference group's income while controlling for the other reference groups.

All models include the individual-level controls and year fixed effects, and they employ the survey sampling weights provided by the GSS. Beginning in 2006, some respondents were reinterviewed in future waves (of all respondents in the main analysis, 9% appear three times, 13% appear twice, and 78% appear only once) so the standard errors are adjusted with clustered standard errors at the respondent level.

5.6 Results

The main results are presented in Table 5.2. Model 1 contains income rank, individual-level controls, and year fixed effects, but no reference group measures. In Models 2 and 3, I add the reference group median incomes both in real dollars (divided by 10,000 for clarity) and logged dollars, respectively (the full set of coefficients are presented in Table 4 of the Appendix).

Table 5.2: Coefficients of Models of Perceived Relative Income

	(1) No RG incomes	(2) RG income/10,000	(3) ln(RG income)
Household income decile (national)	0.20***	0.18***	0.19***
	(0.0034)	(0.0041)	(0.0041)
Reference group (RG) median incomes			
State income		-0.027*	-0.16*
		(0.014)	(0.077)
Commuting zone income		-0.023+	-0.13+
		(0.012)	(0.070)
County income		-0.0039	0.017
		(0.0086)	(0.053)
Tract income		0.024***	0.13***
		(0.0042)	(0.025)
Degree income		0.029***	0.17***
		(0.0043)	(0.028)
Prestige income		0.018***	0.11***
		(0.0043)	(0.026)
Race/ethnicity income		-0.0081	-0.054
		(0.0065)	(0.037)
Cohort income		-0.025**	-0.16**
		(0.0094)	(0.058)
Observations	15789	15789	15789
AIC	37838.2	37590.5	37637.2
BIC	38068.2	37881.9	37928.5

Standard errors in parentheses
$^{+}$ $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$

Notes: Models are weighted to account for sampling. In Model 2, the reference group coefficients represent the effect of income in dollars (divided by 10,000 for clarity) while in Model 3 they represent the effect of logged dollars. All models include the individual-level controls, year fixed effects and a constant term. All coefficients are available in Table 4 of the Appendix.

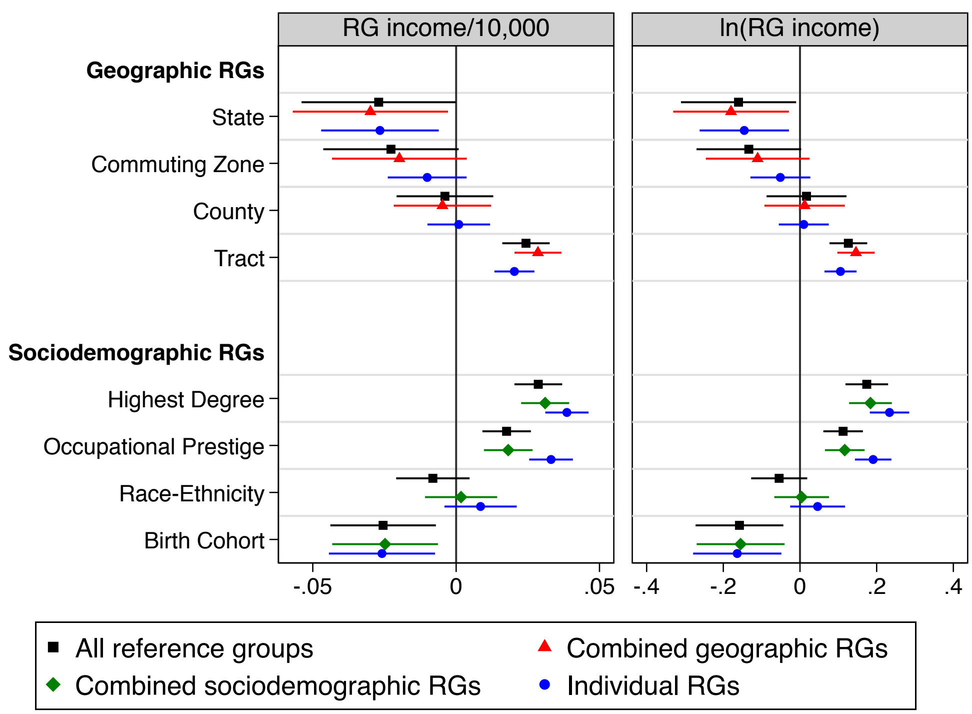
The best "fit" is assessed by examining the AIC and BIC values, and the addition of the reference group measures substantially lowers (i.e., improves) both criteria. The difference between using the reference group income as unlogged or logged is small, but the unlogged income measure performs slightly better.[8] These results

[8] The AIC and BIC are based on the log-likelihoods from the linear regression models. Using the log-likelihoods of the equivalent ordered logistic regression models yields the identical substantive conclusions.

support Hypothesis 1, that reference group income has an effect on perceived relative income. Furthermore, a Wald test confirms that adding the reference group incomes significantly (p <.001) improves the model, also supporting Hypothesis 1. Controlling for reference group income only slightly reduces the effect of household income rank itself (from 0.20 to 0.18-0.19).

To more easily compare the reference group effects, I plot the reference group coefficients in Figure 5.3. Each row corresponds to a different geographic or sociodemographic reference group. In the first column, the coefficients represent the effect of reference group income in real dollars (divided by 10,000 for clarity) while the second column shows the effect of logged dollars. As can be seen from the two columns, the results are substantively identical using either measure of reference group income. For parsimony, I focus on interpreting the results using unlogged reference group dollars on the left.

Figure 5.3: Coefficients Plot of Reference Group (RG) Effects on Perceived Relative Income



Notes: Error bars show 95% confidence intervals. Black squares show the coefficients from Models 2 and 3 of Table 5.2.

For each reference group, the first coefficient (the black squares) is the coefficient from the "full" models with all eight reference group measures (the coefficients from Table 5.2). The red triangles in each column show the reference group coefficients from a single model with all four geographic reference group income measures, and the green diamonds in each column show the reference group coefficients from a single model with all four sociodemographic reference group income measures. Finally, the blue circles show a model with only that reference group and the controls. Thus, each blue circle shows the reference group effect from a separate model. These models are available in Tables 5–8 of the Appendix.

Hypotheses 2a and 2b predicted that larger geographic reference groups would have negative effects while smaller groups would have positive effects. This is supported by the results. The geographic groups are ordered from largest to smallest, and the coefficients increase (shift to the right) as the reference group size decreases. In the full model, a $10,000 increase in state income is associated with -.027 standard deviation (SD) decrease in perceived relative income ($p<.05$ in both models). The effect of commuting zone is slightly less negative ($p<.1$), the effect of county is close to zero and not significant, and the effect of tract is positive and significant. In the full models, a $10,000 increase in census tract income is associated with a .024 SD increase in perceived relative income. Even though the incomes of different types of reference groups are positively correlated, the geographic reference group income effects are fairly stable regardless of which other reference groups are controlled for, suggesting these really are independent effects of comparisons within and between reference groups of different geographic scales.

Hypotheses 3a and 3b predicted that sociodemographic reference groups with a strong status hierarchy would have positive effects while those without would have negative effects. This is also supported by the results. The sociodemographic groups are ordered from those with the strongest status hierarchy to least, and the coefficients decrease (shift to the left) as the strength of the hierarchy decreases. For the reference groups based on educational degree, occupational prestige, and race-ethnicity, controlling for the other reference groups reduces the effect sizes. For example, in the model where degree is the only reference group, a $10,000 increase in the median income of those with one's educational degree is associated with about a 0.039 SD increase in perceived relative income. In the full model, the effect is reduced to 0.029.

Similar downward (leftward) shifts can be seen for occupational prestige and race-ethnicity. These results suggest that the correlation between reference groups explains away some of the fraternal (positive) effects of each. However, these shifts do not change the substantive conclusion that Hypotheses 3a and 3b are supported. The effects of degree and occupational prestige are positive and statistically significant ($p<.001$) in all models. The effect of race-ethnicity income is zero and not significant in any models. And the effect of cohort income is negative in all models ($p<.01$).

It is important to also consider the substantive size of the reference group effect, which I do by comparing it to the household income effect. A standard deviation of census tract income is about $27,000. Based on the full model, a standard deviation change in census tract income has about the same effect as a [$2.7\times.024/.18=$] .36 decile change—or approximately a change of 3-4 percentiles in household income. Similarly, a standard deviation of state income is about $8000, so a standard deviation change in state income has about the same effect as a [$.8\times$-.027/.18=] -.12 decile change—or a bit more than a change in the *opposite* direction of one income percentile. The sizes of the remaining coefficients in Figure 5.3 suggest similarly sized effects for the sociodemographic reference groups.

Even if these are (debatably) not large effects, it is important to note that changes in median income affect the entire reference group, whereas income changes for a single household affect only that household. However, these back of the envelope coefficient comparisons should be treated with caution. For example, if *many* households in a reference group increased their income by a decile in the national distribution, the median income itself of the reference groups would likely change as well. To put it another way, the median income in a reference group tract cannot change without affecting the income of at least some households in that group.

5.7 Discussion and Conclusion

The results support all the hypotheses. Reference groups affect perceived relative income, but the effects vary by the type of reference group, suggesting different reference groups are used for different types of comparisons. For smaller geographic reference groups and for sociodemographic groups with a strong status hierarchy, the fraternal comparison between one's group and others dominates, yielding a positive effect of reference group income. For large geographic reference groups and sociodemographic groups without a strong status hierarchy, the egoist comparison dominates and the effect of reference group income is negative. For reference groups with null effects (e.g., county and race), the results do not necessarily mean that social comparisons do not occur at these levels, as it could be the net effect is zero because the egoist and fraternal comparisons are creating equally sized effects in opposing directions.

This paper has some important limitations. First, these analyses cannot explain *how* information about references groups is acquired and absorbed by individuals. To be clear, these findings do not necessarily mean respondents make explicit simultaneous comparisons with so many different reference groups. Rather, I would suggest, it is more likely the case that the perceptions of one's context (both near and far) and status (including who one is similar and different to) implicitly bias individual's subjective evaluations of their "objective" financial position. Future work is needed to understand this process.

Second, these analyses can only determine the "average" effect of each reference group, but reference groups can be unique to each individual. In theory, each person's relevant reference group(s) could be an individual-specific weighted sum of multiple categories of groups. Future research should also explore heterogeneous effects of reference groups. For example, the appropriate reference group may vary by income. Perhaps at the very high end of the earning distribution, comparisons are made on a national level (e.g., millionaires competing over the biggest yacht [Knecht 2013]), while among the poorest, perhaps neighborhoods and census tracts are more relevant for comparisons than they appear in this analysis.

Third, subjective comparisons need a reference point. This paper focuses on people's comparisons to what they perceive as "average," but other comparisons within the reference group may also matter. For example, "relative deprivation" is often operationalized as the average income difference of everyone with an income greater than the respondent's (Yitzhaki 1979), a measure captured at the population level through the Gini index. People may also direct their comparisons towards the top or the bottom, in which case the relevant measure might be a different percentile than the median (e.g., 90th or 20th percentile) or an income share (e.g., the top 10% or top 1% income share). Exploring the effects of these alternative measures across a range of reference groups—as I have done here with median income—may be fruitful for future research.

Fourth, this paper only explores eight reference groups. The results can provide insights for those wishing to predict the effects of other geographic or sociodemographic reference groups, but some scholars have suggested reference groups based on specific members of small social networks (e.g., immediate co-workers, family members, friends, and people you went to school with) that are not easily categorized by the above criteria (Alderson and Katz-Gerro 2016). Still another reference "group" could include oneself in the past (both real or itself perceived). Future research should consider how these types of possible reference groups may affect perceived relative income and other outcomes of interest.

Despite the limitations, this study has important implications for how reference groups are used in several scholarly literatures. First, many of the proposed consequences of income inequality rely on the mechanism of comparisons with others, which is an egoist comparison. As such, my findings suggest the effects of income inequality should be greater for larger areas such as commuting zones and states. Indeed associations between social problems and income inequality have frequently been found for these larger areas and less so for smaller areas such as counties and neighborhoods (e.g., Wilkinson and Pickett 2009; Pickett and Wilkinson 2015). Also, given the strong effect of fraternal comparisons for small areas, future research may want to also pay more attention to the income inequality *between* small areas (e.g., neighborhoods), in addition to the attention to the distribution of household incomes.

Second, although hypothetically people could receive a psychological boost from living in poorer neighborhoods because it makes them richer relatively, these findings suggest the opposite and support the assumption in most neighborhood effects research that within-neighborhood comparisons are not essential for understanding how neighborhood contexts matter (e.g., Leventhal and Brooks-Gunn 2000; Sastry 2012; Sharkey and Faber 2014). On the whole, people benefit from perceiving themselves as better off, so this suggests yet another benefit to living in an affluent neighborhood (though, conceivably, overestimating one's relative income could lead to overspending—so it should not be assumed to have wholly positive benefits). It is possible the positive effect of neighborhood income is also driven by differences in the resources and amenities in rich and poor neighborhoods. This analysis cannot rule that out, but because my outcome is based on perceived social comparisons, it should be less affected by material differences than outcomes such as satisfaction, health, and happiness that have motivated much of the reference group literature. For example, a person in a rich area might benefit from better healthcare access (leading to well-known positive neighborhood income effects on health and happiness), whereas my analysis seeks to understand whether they also feel relatively richer or poorer as a result of living in a particular neighborhood.

Finally, this research has implications for understanding the consequences of the changing levels of residential segregation, something that scholars have recently noted—particularly segregation by race and by income (e.g., Taylor and Fry 2012; Reardon et al. 2015). My results suggest that changes in the levels of racial segregation will not significantly affect how people perceive their income position, but income segregation will. The implication of this study is that neighborhood level income segregation would increase perceived relative income for those in rich neighborhoods and decrease it for those in poor neighborhoods, but, conversely, state or commuting zone level income segregation would have the opposite effect—decreasing perceived relative income in rich areas and increasing it in poor areas. To date, most research has focused on neighborhood- (i.e., census tract-) level segregation *within* counties or states, rather than segregation *between* counties or states, but my results suggest that income segregation at larger levels will matter as well, and future research should probably also pay more attention to segregation between geographic units larger than the neighborhood.

Chapter 6

Conclusion

The first three empirical chapters of this dissertation examined the effect of state-level income inequality on financial satisfaction, economic optimism, and trust. Data for the analysis were constructed by linking individual-level General Social Surveys to state-level administrative data based on IRS tax returns, the Census, and the American Communities survey. For the economic optimism study, I also conducted an online experiment that manipulated perceptions of state-level inequality.

To my knowledge, this is the first study to empirically examine the links between income inequality and financial satisfaction and between income inequality and economic optimism. This study also presents the most rigorous analysis so far of the relationship between state-level income inequality and trust by using more available observations, accounting for more potential individual and state level confounders, and using higher-quality income inequality data based on annual IRS tax returns compared to existing studies.

All three outcomes have been theorized to be key links between income inequality and critical social problems. I find stronger support that income inequality reduces financial satisfaction and economic optimism, but only weak support that it lowers social trust. This suggests future work interested in how these outcomes are themselves mediators between income inequality and social problems might focus less on the latter outcome, even though it is the outcome of the three to have received the most scholarly attention so far.

Throughout the studies, I paid careful attention to how the effect of income inequality varied across the income distribution. Most notably, I find that the effect of income inequality of financial satisfaction is largest for those in the middle of the income distribution, consistent with the account that these households experience the greatest gap between their "American dream" aspirations and economic reality as inequality increases.

The final study focused on determining the economic reference groups people have. The results suggest that groups where the level of inequality *within* them matter (the type of inequality being considered in measurements of household income inequality) are geographically large and have weak status hierarchies. This supports the decision to look for the effects of state-level inequality. But, the findings also suggest that looking at the effects of inequality between geographically smaller groups and groups with strong status hierarchies may also be important. For example, considering the consequences of inequality between neighborhoods within a state. This is related to neighborhood segregation measures (Reardon et al. 2015), but has not yet been significantly explored in research on the effects of income inequality more generally.

In future work, it would also be useful to conduct an actual analysis of whether financial satisfaction, economic optimism, and trust mediate the relationship between income inequality and other outcomes of substantive interest such as health and happiness. This is challenging, however, because such outcomes could be affected by inequality through material pathways as well. As noted in the introduction, higher income inequality could be associated with lower levels of investment in public infrastructure such as healthcare, transportation, education, and social services, which in turn generate many negative outcomes (Lynch et al. 2000). This might happen because of politics—if the richest are capturing a larger share of the total income, they may have greater influence among policy makers and encourage laws and policies that unduly benefit themselves and subvert the democratic process. However, in more unequal places the wealthy may contribute disproportionally more to civic organizations and charities (e.g., parks, libraries, etc) or contribute more tax revenue that leads to increased spending on public goods (Boustan et al. 2013)—which could yield positive effects of income inequality.

This example shows that the possible pathways between income inequality and many outcomes of interest are numerous. Moreover, different pathways may actually have effects in opposite directions. Specifying all the various material and psychosocial pathways is challenging, and data to test many of them do not currently exist. As such, attempts to adjudicate between multiple mechanisms would require a number of assumptions about how inequality matters (e.g., Elgar 2010; Layte 2012). Nevertheless, this area presents tremendous possibilities for better understanding the effects of income inequality (Neckerman and Torche 2007; Moss et al. 2013).

This work focuses on income inequality and comparisons of income. But this represents only one dimension of economic inequality (albeit, the best measured one). Wealth inequality has recently gained notable attention (Keister and Moller 2000; Piketty 2014; Saez and Zucman 2016). Similarly scholars have taken interest in consumption inequality—that is, inequality of expenditures. We can think of income as the "flow in" and consumption as the "flow out" of household finances. One nascent area of research is examining if and how income inequality affects consumption inequality (e.g., Charles and Lundy 2013; Schneider and Hastings 2017; Schneider, Hastings, and LaBriola 2017)

More broadly, economic inequality is only one dimension of social inequality. Sociologists have traditionally recognized inequalities by race, gender, and class (while, at times, income is equated with "class", the latter is far more complex [Hout 2008]). Yet, these different types of inequality are often interrelated and self-reinforcing. Understanding how growing economic inequality intersects with these other dimensions—something McCall (2001) describes as "configurations of inequality"—is an important direction for future work.

To conclude, this dissertation advances research on the consequences of income inequality by contributing to work on the mechanisms through which inequality may matter and on the methods by which its effects are determined. Given the factors that have brought about the surge in income inequality since the 1970s, it is most likely the case that income inequality itself will continue to rise. Any efforts to curb this growth or to justify its continued trajectory will benefit from being based on the evidence of careful empirical analysis of what the consequences actually are. Digging deep into the precise mechanisms behind these effects not only brings clarity about why inequality matters, but also provides important insights about how any negative effects might be addressed and counteracted.

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Appendix A

Appendix to Chapter 2: Income Inequality and Financial Satisfaction

Table A.1: Descriptives

	mean	sd	min	max
Financial satisfaction	2.02	0.74	1	3
State gini	0.56	0.06	0.43	0.71
Income quintile 1 (bottom)	0.19	0.40	0	1
Income quintile 2	0.20	0.40	0	1
Income quintile 3	0.19	0.39	0	1
Income quintile 4	0.21	0.41	0	1
Income quintile 5 (top)	0.20	0.40	0	1
Female	0.53	0.50	0	1
Age (years)	44.14	16.67	18	89
(Age x Age)/100	22.26	16.26	3.24	79.2
Non-Hispanic white	0.77	0.42	0	1
Non-Hispanic black	0.13	0.33	0	1
Non-Hispanic other	0.03	0.17	0	1
Hispanic	0.08	0.27	0	1
Married	0.61	0.49	0	1
Widowed	0.06	0.23	0	1
Divorced	0.10	0.29	0	1
Separated	0.03	0.16	0	1
Never married	0.21	0.41	0	1

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Table A.1 – continued from previous page

Num of adults	2.23	0.92	1	8
Num of children	1.96	1.79	0	8
Religious service attendance	3.80	2.71	0	8
Years of education	12.93	3.08	0	20
Working fulltime	0.52	0.50	0	1
Working parttime	0.11	0.31	0	1
Temp not working	0.02	0.15	0	1
Unemployed	0.04	0.18	0	1
Retired	0.11	0.31	0	1
Student	0.04	0.18	0	1
Keeping house	0.15	0.36	0	1
Other work status	0.02	0.13	0	1
Democrat	0.49	0.50	0	1
Republican	0.35	0.48	0	1
Other party; No party	0.16	0.36	0	1
Urban	0.60	0.49	0	1
Suburban	0.27	0.45	0	1
Rural	0.13	0.34	0	1
State median income (logged)	10.87	0.14	10.4	11.3
State percent foreign born	0.09	0.07	0.0070	0.27
State population density (logged)	4.98	0.99	0.017	9.28
State percent black	0.13	0.08	0.0023	0.71
State poverty rate	13.48	3.35	2.90	27.2

Note: N = 51699. Individual measures are weighted to account for sampling. State Gini index variable is reported before mean centering. State median income is adjusted to 2012 dollars using the CPI-U-RS. Sources: General Social Survey: 1973-2012. Census: 1970, 1980, 1990, 2000. American Community Survey: 2006-2012. IRS Statistics of Income: 1973-2012.

Table A.2: Full Regression Coefficients from Models of Financial Satisfaction

	(1)		(2)	
State Gini	-0.30^{*}	(0.13)	-0.54^{**}	(0.20)
Q1 (bottom)	-0.30^{***}	(0.014)	-0.30^{***}	(0.014)
Q2	-0.16^{***}	(0.0091)	-0.16^{***}	(0.0091)
Q3	0	(.)	0	(.)
Q4	0.16^{***}	(0.012)	0.16^{***}	(0.011)
Q5 (top)	0.41^{***}	(0.016)	0.41^{***}	(0.015)
Q1 (bottom) × Gini			0.38^{*}	(0.17)
Q2 × Gini			0.0032	(0.13)
Q3 × Gini			0	(.)
Q4 × Gini			0.27	(0.21)
Q5 (top) × Gini			0.55^{*}	(0.25)
Female	0.0095	(0.0097)	0.0093	(0.0096)
Age (years)	-0.019^{***}	(0.0013)	-0.019^{***}	(0.0013)
(Age x Age)/100	0.028^{***}	(0.0015)	0.028^{***}	(0.0015)
Non-Hispanic white	0	(.)	0	(.)
Non-Hispanic black	-0.12^{***}	(0.013)	-0.12^{***}	(0.013)
Non-Hispanic other	0.085^{**}	(0.025)	0.084^{**}	(0.025)
Hispanic	0.013	(0.015)	0.015	(0.016)
Married	0	(.)	0	(.)
Widowed	-0.064^{***}	(0.013)	-0.062^{***}	(0.013)
Divorced	-0.11^{***}	(0.0095)	-0.11^{***}	(0.0091)
Separated	-0.11^{***}	(0.024)	-0.11^{***}	(0.024)
Never Married	0.066^{***}	(0.011)	0.068^{***}	(0.011)
Num of adults	-0.034^{***}	(0.0047)	-0.033^{***}	(0.0046)
Num of children	-0.019^{***}	(0.0024)	-0.018^{***}	(0.0024)
Religious service attendance	0.020^{***}	(0.0024)	0.020^{***}	(0.0024)
Years of education	0.0027	(0.0015)	0.0026	(0.0015)
Urban	0	(.)	0	(.)
Suburban	-0.024^{*}	(0.012)	-0.024^{*}	(0.012)
Rural	0.036^{**}	(0.013)	0.035^{**}	(0.013)
full time	0	(.)	0	(.)
part time	-0.0090	(0.010)	-0.0095	(0.010)
temp not wrk	-0.0097	(0.025)	-0.0097	(0.025)
unemployed	-0.31^{***}	(0.025)	-0.32^{***}	(0.026)
retired	0.090^{***}	(0.021)	0.089^{***}	(0.021)
in school	0.058^{*}	(0.023)	0.058^{*}	(0.023)
keeping house	0.043^{***}	(0.0088)	0.043^{***}	(0.0088)

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Table A.2 – continued from previous page

other	-0.10^{***}	(0.028)	-0.11^{***}	(0.028)
Democrat	0	(.)	0	(.)
Republican	0.063^{***}	(0.0087)	0.063^{***}	(0.0087)
Other party; no party	-0.0043	(0.012)	-0.0035	(0.012)
State median income (logged)	0.14	(0.092)	0.14	(0.092)
State percent foreign born	0.11	(0.36)	0.035	(0.35)
State population density (logged)	-0.16	(0.083)	-0.15	(0.083)
State percent black	-0.25	(0.39)	-0.23	(0.39)
State poverty rate	-0.011^{***}	(0.0031)	-0.011^{***}	(0.0031)
State Fixed Effects	Yes		Yes	
Observations	51699		51699	

Standard errors in parentheses
* $p < .05$, ** $p < .01$, *** $p < .001$
Note: The middle income quintile (Q3) is the baseline category. Each model uses sampling weights. and the standard errors are adjusted for clustering within states.

Appendix B

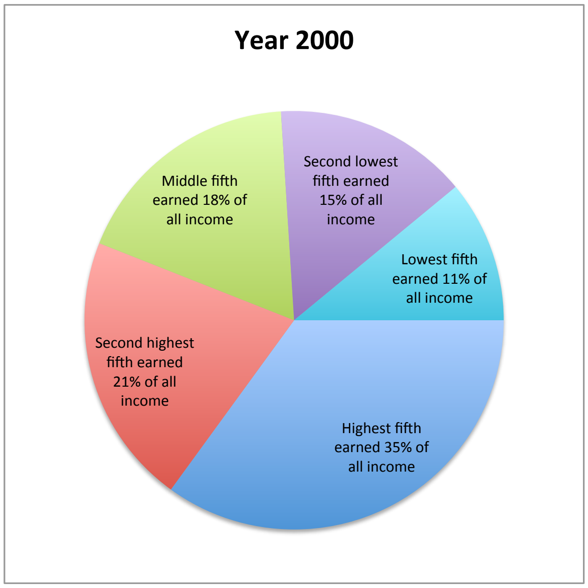
Appendix to Chapter 3: Income Inequality and Economic Optimism

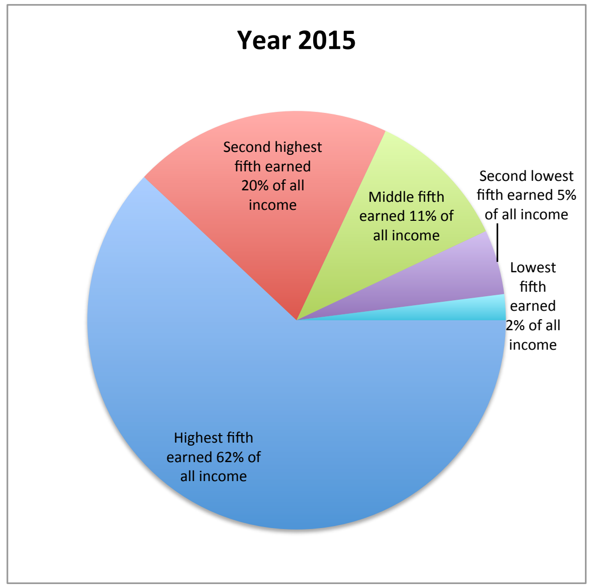
Figure B.1: Experimental Manipulation Conditions

After selecting their state of residence, respondents were shown the following prompt and one of three experimental conditions:

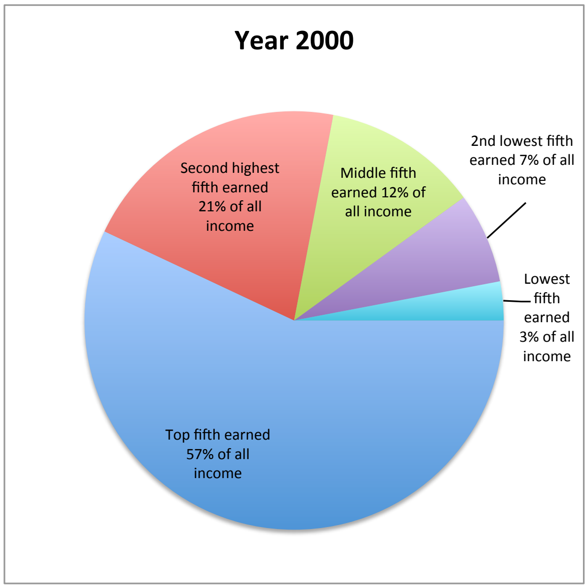
“The following information describes the level of income inequality (how evenly income is distributed) in [state] by showing the percent of total income earned by each fifth of households in the years 2000 and 2015. In other words, each slice of the pie represents 20% of all households in your state, and the size of the slice shows what portion of the total income earned by everyone in the state went to households belonging to that slice. Please read the description below and examine the two charts.”

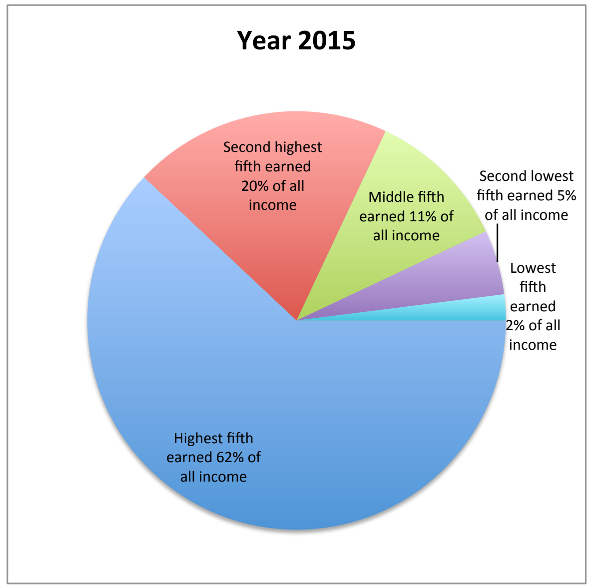
Condition 1: High rising inequality: "Income inequality has changed a lot in [state]. In 2000 the highest fifth of earners made 35% of all income (much less than the national average) and in 2015 the highest fifth of earners made 62% of all income (much more than the national average)."



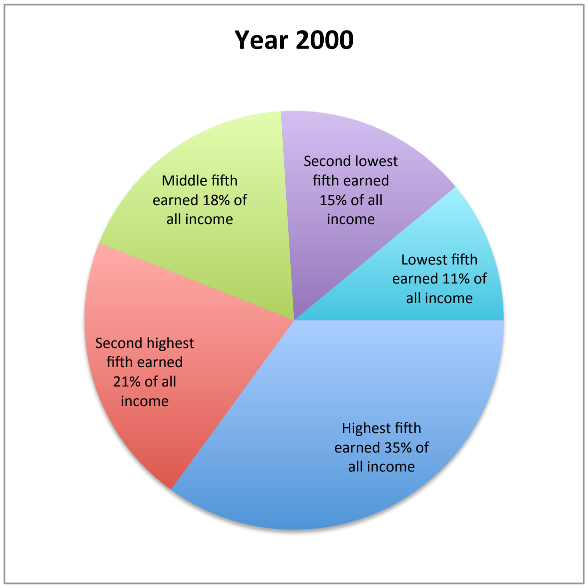


Condition 2: High stable inequality: "Income inequality has been high in [state]. In 2000 the highest fifth of earners made 57% of all income and in 2015 the highest fifth of earners made 62% of all income. For both years, this was much higher than the national average."





Condition 3: Low stable inequality: "Income inequality has been low in [state]. In 2000 the highest fifth of earners made 35% of all income and in 2015 the highest fifth of earners made 39% of all income. For both years, this was much lower than the national average."



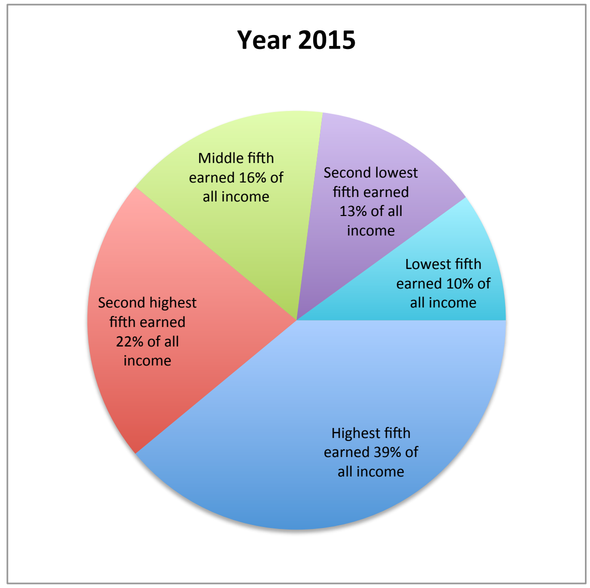


Table B.1: Descriptives (Survey Study)

	mean	*sd*	*min*	*max*
Economic Optimism	3.63	1.05	1	5
State Gini	0.59	0.04	0.49	0.71
5-year Gini percent change	0.04	0.05	−0.16	0.22
Log(income)	10.76	1.04	6.19	12.6
Female	0.53	0.50	0	1
Age (years)	44.34	16.32	18	89
(Age x Age)/100	22.33	16.07	3.24	79.2
Non-Hispanic white	0.73	0.44	0	1
Non-Hispanic black	0.14	0.34	0	1
Non-Hispanic other	0.04	0.20	0	1
Hispanic	0.09	0.29	0	1
Married	0.57	0.50	0	1
Widowed	0.06	0.23	0	1
Divorced	0.12	0.32	0	1
Separated	0.03	0.16	0	1
Never married	0.23	0.42	0	1
Num of adults	2.16	0.90	1	8
Num of children	1.85	1.67	0	8
Religious service attendance	3.65	2.70	0	8
Years of education	13.35	2.97	0	20
Urban	0.61	0.49	0	1
Suburban	0.28	0.45	0	1
Rural	0.11	0.31	0	1
State median income (logged)	10.91	0.13	10.4	11.3
State percent foreign born	0.10	0.08	0.0083	0.27
State population density (logged)	5.01	1.00	0.017	9.25
State percent black	0.13	0.08	0.0031	0.67

Standard errors in parentheses
* $p < .05$, ** $p < .01$, *** $p < .001$
Note: N = 14,358. Descriptives are weighted to account for sampling. Individual level data come from the General Social Survey 1987-2012. State level data come from the Census (1980, 1990, and 2000, with linear interpolation between census years), American Community Survey (2006-2012), and IRS Statistics of Income (1987-2012).

Table B.2: Full Regression Results from Analysis of Economic Optimism (Survey Study)

	(1)	(2)	(3)	(4)
State Gini	−2.24***	−0.60	−2.86**	7.30*
	(0.40)	(0.64)	(1.03)	(3.25)
5-year Gini change			−12.9***	15.1
			(3.47)	(29.1)
State Gini × 5-year Gini change			22.0***	−34.0
			(5.38)	(47.3)
State Gini × Log(income)				−0.95**
				(0.29)
5-year Gini change × Log(income)				−2.65
				(2.75)
State Gini × 5-year Gini change × Log(income)				5.30
				(4.47)
Log(income)	0.097***	0.092***	0.091***	0.64***
	(0.012)	(0.012)	(0.012)	(0.18)
Female	−0.14***	−0.15***	−0.14***	−0.14***
	(0.020)	(0.020)	(0.020)	(0.020)
Age (years)	−0.025***	−0.024***	−0.024***	−0.024***
	(0.0030)	(0.0029)	(0.0029)	(0.0029)
(Age x Age)/100	0.017***	0.017***	0.017***	0.018***
	(0.0029)	(0.0028)	(0.0028)	(0.0028)
Non-Hispanic white	0	0	0	0
	(.)	(.)	(.)	(.)
Non-Hispanic black	0.11**	0.10**	0.10**	0.11**
	(0.035)	(0.034)	(0.034)	(0.033)
Non-Hispanic other	0.22***	0.23***	0.23***	0.23***
	(0.044)	(0.043)	(0.043)	(0.044)
Hispanic	0.30***	0.30***	0.30***	0.29***
	(0.040)	(0.039)	(0.039)	(0.039)
Married	0	0	0	0
	(.)	(.)	(.)	(.)
Widowed	0.11*	0.11*	0.11*	0.11*
	(0.052)	(0.050)	(0.050)	(0.050)
Divorced	−0.0076	−0.0076	−0.0094	−0.0078
	(0.026)	(0.024)	(0.024)	(0.024)
Separated	−0.0061	−0.0017	−0.0040	−0.0043
	(0.052)	(0.051)	(0.051)	(0.051)
Never Married	0.034	0.048*	0.048*	0.049*
	(0.024)	(0.023)	(0.022)	(0.022)
Num of adults	−0.026	−0.022	−0.022	−0.024
	(0.013)	(0.013)	(0.013)	(0.013)
Num of children	0.0071	0.0081	0.0082	0.0081
	(0.0059)	(0.0061)	(0.0061)	(0.0062)
Religious service attendance	0.028***	0.025***	0.025***	0.025***
	(0.0034)	(0.0034)	(0.0034)	(0.0034)
Years of education	0.0066	0.011**	0.011**	0.011**
	(0.0035)	(0.0037)	(0.0036)	(0.0036)

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Table B.2 – continued from previous page

Urban	0	0	0	0
	(.)	(.)	(.)	(.)
Suburban	0.0030	0.010	0.011	0.0097
	(0.025)	(0.022)	(0.023)	(0.023)
Rural	-0.11^{**}	-0.080^{*}	-0.079^{*}	-0.076^{*}
	(0.030)	(0.033)	(0.033)	(0.033)
State median income (logged)	0.43^{**}	-0.011	-0.042	-0.062
	(0.13)	(0.24)	(0.24)	(0.24)
State percent foreign born	0.92^{**}	2.48	2.33^{*}	2.62^{*}
	(0.30)	(1.33)	(1.15)	(1.13)
State population density (logged)	-0.082^{***}	0.23	0.34	0.30
	(0.021)	(0.25)	(0.25)	(0.25)
State percent black	0.91^{***}	-0.052	0.49	0.29
	(0.22)	(1.72)	(1.67)	(1.66)
Constant	-0.013	2.79	3.79	-1.66
	(1.42)	(2.88)	(3.23)	(3.54)
Year fixed effects	No	Yes	Yes	Yes
State fixed effects	No	Yes	Yes	Yes
Observations	18710	18710	18710	18710

Standard errors in parentheses

* $p < .05$, ** $p < .01$, *** $p < .001$

Note: Models are weighted to account for sampling and standard errors are clustered at the state level

Appendix C

Appendix to Chapter 4: Income Inequality and Trust

Table C.1: Coefficients (Log Odds) from Models of Binary Trust with Top 1% Income Share and Bottom 99% Gini Measures

	(1) Trust (binary)		(2) Trust (binary)		(3) Trust (binary)	
Between State Top 1% share (SD)	-0.11	(0.081)	-0.075	(0.068)	-0.052	(0.073)
Within State Top 1% share (SD)	-0.21^{***}	(0.052)	-0.043	(0.041)	0.026	(0.056)
Between State Gini 99% (SD)	-0.063	(0.087)	0.033	(0.087)	0.077	(0.088)
Within State Gini 99% (SD)	-0.15^{***}	(0.019)	-0.046^{+}	(0.024)	-0.013	(0.040)
Log(income)	0.21^{***}	(0.019)	0.21^{***}	(0.019)	0.21^{***}	(0.019)
Female	-0.10^{**}	(0.032)	-0.10^{**}	(0.032)	-0.11^{***}	(0.032)
Age (years)	0.043^{***}	(0.0053)	0.043^{***}	(0.0053)	0.043^{***}	(0.0053)
(Age x Age)/100	-0.025^{***}	(0.0053)	-0.025^{***}	(0.0054)	-0.025^{***}	(0.0054)
Non-Hispanic white	0	(.)	0	(.)	0	(.)
Non-Hispanic black	-1.05^{***}	(0.083)	-1.05^{***}	(0.083)	-1.07^{***}	(0.083)
Non-Hispanic other	-0.51^{***}	(0.065)	-0.51^{***}	(0.066)	-0.52^{***}	(0.067)
Hispanic	-0.51^{***}	(0.061)	-0.50^{***}	(0.061)	-0.51^{***}	(0.060)
Married	0	(.)	0	(.)	0	(.)
Widowed	-0.10	(0.064)	-0.10^{+}	(0.063)	-0.10	(0.062)
Divorced	-0.14^{**}	(0.047)	-0.14^{**}	(0.047)	-0.13^{**}	(0.047)
Separated	-0.24^{**}	(0.077)	-0.24^{**}	(0.078)	-0.24^{**}	(0.079)
Never Married	0.052	(0.051)	0.058	(0.051)	0.055	(0.051)
Num of adults	-0.020	(0.017)	-0.021	(0.017)	-0.022	(0.017)
Num of children	-0.0053	(0.0075)	-0.0056	(0.0076)	-0.0052	(0.0075)
Religious service attendance	0.027^{***}	(0.0071)	0.026^{***}	(0.0071)	0.025^{***}	(0.0071)
Years of education	0.17^{***}	(0.0066)	0.17^{***}	(0.0067)	0.17^{***}	(0.0067)
Urban	0	(.)	0	(.)	0	(.)
Suburban	-0.042	(0.037)	-0.039	(0.037)	-0.044	(0.037)
Rural	0.020	(0.063)	0.030	(0.061)	0.029	(0.060)
State income/capita	-0.011	(0.0089)	0.0052	(0.0096)	0.0056	(0.010)
State percent foreign born	-0.027	(0.66)	0.19	(0.61)	0.20	(0.61)

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Table C.1 – continued from previous page

State population density (logged)	−0.016	(0.047)	−0.023	(0.045)	−0.0073	(0.045)
State percent black	−0.85	(0.58)	-0.86^{+}	(0.51)	-1.00^{*}	(0.50)
Southern state	-0.35^{**}	(0.12)	-0.30^{**}	(0.099)	-0.29^{**}	(0.097)
Year (linear)			-0.023^{***}	(0.0040)		
Constant	-5.47^{***}	(0.29)	39.1^{***}	(7.83)	-5.30^{***}	(0.25)
var(State)	0.032^{*}	(0.013)	0.026^{*}	(0.011)	0.027^{*}	(0.011)
var(State-year)	0.040^{***}	(0.0093)	0.033^{***}	(0.0079)	0.0092	(0.0076)
Year fixed effects	*No*		*No*		*Yes*	
Observations	31857		31857		31857	

Standard errors in parentheses
* $p < .05$, ** $p < .01$, *** $p < .001$
Note: Results are from a multilevel logistic regression model.

Table C.2: Coefficients from Models of Trust Scale with Top 1% Income Share and Bottom 99% Gini Measures

	(1) Trust (scale)		(2) Trust (scale)		(3) Trust (scale)	
Between State Top 1% share (SD)	-0.15^{+}	(0.080)	-0.13^{+}	(0.073)	-0.11	(0.076)
Within State Top 1% share (SD)	-0.24^{***}	(0.048)	-0.13^{**}	(0.041)	-0.070	(0.051)
Between State Gini 99% (SD)	-0.060	(0.077)	0.0031	(0.077)	0.045	(0.077)
Within State Gini 99% (SD)	-0.14^{***}	(0.018)	-0.069^{**}	(0.025)	-0.015	(0.035)
Log(income)	0.21^{***}	(0.013)	0.21^{***}	(0.014)	0.21^{***}	(0.014)
Female	0.17^{***}	(0.026)	0.17^{***}	(0.026)	0.16^{***}	(0.026)
Age (years)	0.042^{***}	(0.0040)	0.042^{***}	(0.0040)	0.043^{***}	(0.0040)
(Age x Age)/100	-0.016^{***}	(0.0041)	-0.016^{***}	(0.0041)	-0.017^{***}	(0.0040)
Non-Hispanic white	0	(.)	0	(.)	0	(.)
Non-Hispanic black	-1.05^{***}	(0.078)	-1.05^{***}	(0.078)	-1.06^{***}	(0.077)
Non-Hispanic other	-0.38^{***}	(0.085)	-0.37^{***}	(0.086)	-0.38^{***}	(0.089)
Hispanic	-0.49^{***}	(0.069)	-0.48^{***}	(0.069)	-0.48^{***}	(0.068)
Married	0	(.)	0	(.)	0	(.)
Widowed	-0.13^{*}	(0.055)	-0.14^{*}	(0.055)	-0.13^{*}	(0.054)
Divorced	-0.25^{***}	(0.030)	-0.24^{***}	(0.030)	-0.24^{***}	(0.030)
Separated	-0.26^{***}	(0.063)	-0.26^{***}	(0.063)	-0.26^{***}	(0.064)
Never Married	0.11^{*}	(0.046)	0.11^{*}	(0.046)	0.11^{*}	(0.045)
Num of adults	-0.020	(0.015)	-0.020	(0.015)	-0.021	(0.015)
Num of children	0.0047	(0.0084)	0.0045	(0.0084)	0.0047	(0.0083)
Religious service attendance	0.056^{***}	(0.0070)	0.055^{***}	(0.0069)	0.055^{***}	(0.0069)
Years of education	0.17^{***}	(0.0053)	0.17^{***}	(0.0054)	0.17^{***}	(0.0055)
Urban	0	(.)	0	(.)	0	(.)
Suburban	-0.050	(0.042)	-0.048	(0.042)	-0.055	(0.043)
Rural	0.086	(0.061)	0.092	(0.061)	0.088	(0.059)
State income/capita	0.0028	(0.0082)	0.014	(0.0092)	0.019^{*}	(0.0097)
State percent foreign born	-0.18	(0.73)	0.035	(0.76)	-0.092	(0.79)
State population density (logged)	-0.028	(0.041)	-0.035	(0.038)	-0.033	(0.037)
State percent black	-0.67	(0.62)	-0.69	(0.56)	-0.73	(0.53)
Southern state	-0.38^{***}	(0.10)	-0.34^{***}	(0.090)	-0.32^{***}	(0.087)
Year (linear)			-0.015^{***}	(0.0033)		
Constant	-2.57^{***}	(0.26)	27.2^{***}	(6.55)	-2.70^{***}	(0.23)
var(State)	0.034^{***}	(0.013)	0.028^{***}	(0.010)	0.027^{***}	(0.0100)
var(State-year)	0.053^{***}	(0.0092)	0.051^{***}	(0.0091)	0.032^{***}	(0.010)
var(Residual)	4.02^{***}	(0.033)	4.02^{***}	(0.033)	4.03^{***}	(0.033)
Year fixed effects	*No*		*No*		*Yes*	
Observations	29457		29457		29457	

Standard errors in parentheses
* $p < .05$, ** $p < .01$, *** $p < .001$
Note: Results are from a multilevel regression model.

Table C.3: Coefficients from Logit Models of Trust with State Fixed Effects

	(1)		(2)		(3)	
Trust (binary)						
State Gini	-3.76^{***}	(0.53)	-1.14^{*}	(0.57)	0.32	(0.98)
Log(income)	0.21^{***}	(0.020)	0.20^{***}	(0.019)	0.21^{***}	(0.019)
Female	-0.099^{**}	(0.032)	-0.099^{**}	(0.032)	-0.11^{***}	(0.032)
Age (years)	0.043^{***}	(0.0052)	0.043^{***}	(0.0053)	0.043^{***}	(0.0053)
(Age x Age)/100	-0.025^{***}	(0.0053)	-0.025^{***}	(0.0054)	-0.025^{***}	(0.0054)
Non-Hispanic white	0	(.)	0	(.)	0	(.)
Non-Hispanic black	-1.05^{***}	(0.083)	-1.05^{***}	(0.083)	-1.06^{***}	(0.084)
Non-Hispanic other	-0.49^{***}	(0.066)	-0.49^{***}	(0.066)	-0.51^{***}	(0.068)
Hispanic	-0.50^{***}	(0.061)	-0.49^{***}	(0.062)	-0.50^{***}	(0.060)
Married	0	(.)	0	(.)	0	(.)
Widowed	−0.10	(0.063)	-0.10^{+}	(0.063)	−0.10	(0.063)
Divorced	-0.14^{**}	(0.046)	-0.13^{**}	(0.047)	-0.13^{**}	(0.047)
Separated	-0.24^{**}	(0.078)	-0.24^{**}	(0.078)	-0.25^{**}	(0.080)
Never Married	0.056	(0.050)	0.059	(0.051)	0.052	(0.051)
Num of adults	−0.019	(0.017)	−0.020	(0.017)	−0.022	(0.017)
Num of children	−0.0051	(0.0074)	−0.0055	(0.0075)	−0.0056	(0.0075)
Religious service attendance	0.027^{***}	(0.0072)	0.026^{***}	(0.0072)	0.025^{***}	(0.0072)
Years of education	0.16^{***}	(0.0065)	0.17^{***}	(0.0066)	0.17^{***}	(0.0067)
Urban	0	(.)	0	(.)	0	(.)
Suburban	−0.041	(0.038)	−0.037	(0.038)	−0.044	(0.038)
Rural	0.014	(0.063)	0.017	(0.061)	0.019	(0.060)
State income/capita	−0.0084	(0.0070)	0.0078	(0.0065)	0.0093	(0.010)
State percent foreign born	1.54^{+}	(0.81)	1.26^{+}	(0.67)	1.03	(0.70)
State population density (logged)	−0.26	(0.18)	0.21^{+}	(0.11)	0.26^{*}	(0.13)
State percent black	-5.11^{*}	(1.99)	−1.78	(1.76)	−1.80	(1.75)
Southern state	0.92	(0.81)	−1.09	(0.70)	−1.14	(0.74)
Year (linear)			-0.026^{***}	(0.0031)		
Constant	-2.83^{***}	(0.43)	47.1^{***}	(5.96)	-5.81^{***}	(0.60)
Year fixed effects	*No*		*No*		*Yes*	
State fixed effects	*Yes*		*Yes*		*Yes*	
Observations	31857		31857		31857	

Standard errors in parentheses
* $p < .05$, ** $p < .01$, *** $p < .001$
Note: State fixed effects models. Including clustering and sampling weights.

Table C.4: Coefficients from Models of Trust Scale with State Fixed Effects

	(1)		(2)		(3)	
State Gini	-3.63^{***}	(0.49)	-2.34^{***}	(0.56)	-0.66	(1.01)
Log(income)	0.21^{***}	(0.013)	0.20^{***}	(0.013)	0.21^{***}	(0.014)
Female	0.17^{***}	(0.027)	0.17^{***}	(0.026)	0.17^{***}	(0.026)
Age (years)	0.042^{***}	(0.0040)	0.042^{***}	(0.0040)	0.043^{***}	(0.0040)
(Age x Age)/100	-0.016^{***}	(0.0040)	-0.016^{***}	(0.0040)	-0.017^{***}	(0.0040)
Non-Hispanic white	0	(.)	0	(.)	0	(.)
Non-Hispanic black	-1.06^{***}	(0.079)	-1.06^{***}	(0.079)	-1.06^{***}	(0.077)
Non-Hispanic other	-0.38^{***}	(0.082)	-0.37^{***}	(0.082)	-0.38^{***}	(0.087)
Hispanic	-0.47^{***}	(0.073)	-0.46^{***}	(0.073)	-0.47^{***}	(0.069)
Married	0	(.)	0	(.)	0	(.)
Widowed	-0.14^{*}	(0.056)	-0.14^{*}	(0.056)	-0.13^{*}	(0.055)
Divorced	-0.25^{***}	(0.029)	-0.24^{***}	(0.029)	-0.24^{***}	(0.030)
Separated	-0.27^{***}	(0.065)	-0.27^{***}	(0.064)	-0.27^{***}	(0.065)
Never Married	0.12^{*}	(0.046)	0.12^{*}	(0.046)	0.11^{*}	(0.045)
Num of adults	-0.020	(0.015)	-0.020	(0.015)	-0.021	(0.015)
Num of children	0.0049	(0.0084)	0.0047	(0.0084)	0.0044	(0.0083)
Religious service attendance	0.056^{***}	(0.0069)	0.056^{***}	(0.0069)	0.055^{***}	(0.0070)
Years of education	0.17^{***}	(0.0055)	0.17^{***}	(0.0056)	0.17^{***}	(0.0056)
Urban	0	(.)	0	(.)	0	(.)
Suburban	-0.040	(0.044)	-0.039	(0.045)	-0.049	(0.045)
Rural	0.086	(0.063)	0.088	(0.063)	0.081	(0.060)
State income/capita	-0.0012	(0.0055)	0.0069	(0.0060)	0.0044	(0.0083)
State percent foreign born	0.96	(1.04)	0.81	(0.98)	0.40	(0.91)
State population density (logged)	-0.37^{*}	(0.18)	-0.12	(0.16)	-0.049	(0.15)
State percent black	-2.16	(1.70)	-0.54	(1.49)	-0.33	(1.53)
Southern state	0.72	(0.60)	-0.30	(0.55)	-0.47	(0.56)
Year (linear)			-0.013^{***}	(0.0030)		
Constant	0.11	(0.39)	24.8^{***}	(5.72)	-2.06^{**}	(0.64)
Year fixed effects	*No*		*No*		*Yes*	
State fixed effects	*Yes*		*Yes*		*Yes*	
Observations	29457		29457		29457	

Standard errors in parentheses
* $p < .05$, ** $p < .01$, *** $p < .001$
Note: State fixed effects models. Including clustering and sampling weights.

Appendix D

Appendix to Chapter 5: Reference Groups and Perceived Relative Income

Figure D.1: Perceived Relative Income from 1988 to 2014 (Source: General Social Survey)

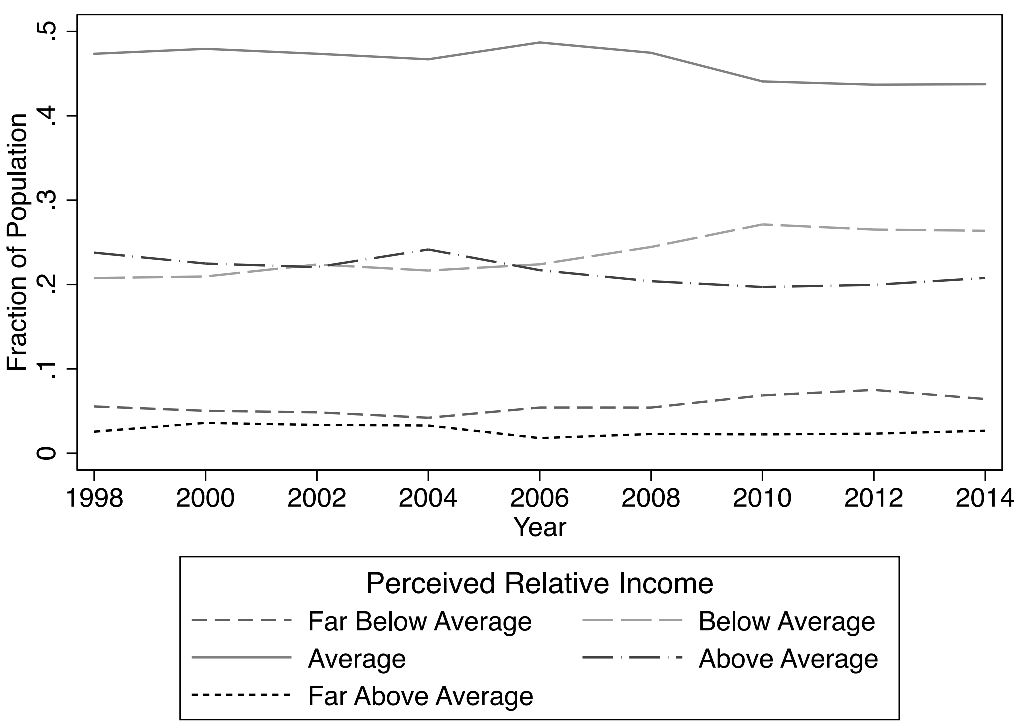

Table D.1: Reference Group Income Data Sources (all links live as of April 18, 2017):

Reference Group	Source	Link
State; County	SAIPE	https://www.census.gov/did/www/saipe/data/index.html
Commuting Zone	SAIPE county data aggregated	The county-to-commuting-zone crosswalk comes from http://www.ddorn.net/data.htm
Census tract	Census, ACS	http://www.socialexplorer.com/explore/tables. For tracts that changed in 2010, I estimated the median income of old and updated tracts using the Longitudinal Tract Data Base (Logan, Xu and Shults 2014) available at http://www.s4.brown.edu/us2010/Researcher/Bridging.htm.
Degree; Race/Ethnicity; Birth Cohort	CPS	https://cps.ipums.org/cps/
Occupational Prestige	GSS	http://gss.norc.org/

Table D.2: Descriptives and Correlations of Perceived Relative Income and Key Independent Variables using Logged Reference Group Income

	Mean	SD	Correlation									
			1	2	3	4	5	6	7	8	9	10
1. Perceived relative income	0	1.00	1									
2. Household income decile (national)	5.57	2.85	0.59	1								
3. State log(income)	10.9	0.14	0.09	0.15	1							
4. Commuting zone log(income)	10.9	0.20	0.14	0.23	0.69	1						
5. County log(income)	10.9	0.24	0.17	0.26	0.56	0.80	1					
6. Tract log(income)	10.9	0.43	0.31	0.47	0.34	0.48	0.55	1				
7. Degree log(income)	11.1	0.36	0.34	0.43	0.12	0.19	0.20	0.32	1			
8. Prestige log(income)	11.0	0.36	0.32	0.41	0.13	0.18	0.18	0.29	0.55	1		
9. Cohort log(income)	11.1	0.15	0.08	0.17	0.07	0.06	0.07	0.12	0.06	0.10	1	
10. Race/ethnicity log(income)	11.1	0.23	0.17	0.24	0.01	0.00	0.10	0.31	0.24	0.23	0.11	1

Table D.3: Descriptives of All Variables Appearing in Any of the Models in the Paper

	mean	sd	min	max
Perceived relative income	0	1.00	-2.16	2.35
Household income (2014 real dollars)	81435.9	72969.7	486	319207
Log(Income)	10.2	1.01	5.47	12.0
Income/(national median)	1.47	1.32	0.0091	5.76
Income decile (national)	5.57	2.85	1	10
State income	5.60	0.77	3.83	7.74
Commuting zone income	5.69	1.17	3.08	9.35
County income	5.73	1.47	2.81	12.9
Tract income	6.14	2.73	1.04	25.0
Race/ethnicity income	6.53	1.31	4.02	7.65
Degree income	6.78	2.41	2.78	12.3
Cohort income	6.48	0.91	4.39	8.11
Prestige income	6.36	2.20	2.39	11.0
State log(income)	10.9	0.14	10.6	11.3
Commuting zone log(income)	10.9	0.20	10.3	11.4
County log(income)	10.9	0.24	10.2	11.8
Tract log(income)	10.9	0.43	9.25	12.4
Race/ethnicity log(income)	11.1	0.23	10.6	11.2
Degree log(income)	11.1	0.36	10.2	11.7
Cohort log(income)	11.1	0.15	10.7	11.3
Prestige log(income)	11.0	0.36	10.1	11.6
Female	0.53		0	1
Liberal	0.26		0	1
Moderate	0.37		0	1
Conservative	0.34		0	1
Unknown Political Views	0.025		0	1
Married	0.60		0	1
Widowed	0.023		0	1
Divorced	0.14		0	1
Separated	0.031		0	1
Never married	0.20		0	1
No children at home	0.24		0	1
One child	0.17		0	1
Two children	0.29		0	1
Three+ children	0.30		0	1
One adult in household	0.18		0	1
Two adults in household	0.60		0	1
Three+ adults in household	0.22		0	1

Working fulltime	0.64	0	1
Working parttime	0.11	0	1
Temp not working	0.025	0	1
Unemployed	0.044	0	1
Retired	0.050	0	1
Student	0.014	0	1
Keeping house	0.096	0	1
Other work status	0.026	0	1
1998	0.12	0	1
2000	0.11	0	1
2002	0.054	0	1
2004	0.052	0	1
2006	0.11	0	1
2008	0.14	0	1
2010	0.19	0	1
2012	0.076	0	1
2014	0.15	0	1

Notes: Statistics are weighted to account for sampling

Table D.4: Full Regression Results for Models with All Reference Groups

	(1) No RG incomes		(2) RG income/10,000		(3) ln(RG income)	
Income decile (national)	0.20***	(60.42)	0.18***	(44.34)	0.19***	(45.28)
State income			-0.027*	(-1.97)	-0.16*	(-2.09)
Commuting zone income			-0.023+	(-1.89)	-0.13+	(-1.91)
County income			-0.0039	(-0.45)	0.017	(0.32)
Tract income			0.024***	(5.77)	0.13***	(5.03)
Degree income			0.029***	(6.74)	0.17***	(6.14)
Prestige income			0.018***	(4.08)	0.11***	(4.27)
Race/ethnicity income			-0.0081	(-1.24)	-0.054	(-1.45)
Cohort income			-0.025**	(-2.71)	-0.16**	(-2.70)
Female	-0.041*	(-2.56)	-0.059***	(-3.71)	-0.059***	(-3.68)
Liberal	0	(.)	0	(.)	0	(.)
Moderate	-0.068***	(-3.63)	-0.045*	(-2.39)	-0.052**	(-2.78)
Conservative	0.0066	(0.33)	0.0081	(0.41)	0.0049	(0.25)
Politival views unknown	-0.076	(-1.47)	-0.041	(-0.82)	-0.043	(-0.86)
Married	0	(.)	0	(.)	0	(.)
Widowed	-0.048	(-0.95)	-0.036	(-0.73)	-0.036	(-0.72)
Divorced	-0.013	(-0.50)	-0.0040	(-0.16)	-0.0060	(-0.23)
Separated	0.016	(0.32)	0.042	(0.88)	0.044	(0.90)
Never married	0.042	(1.63)	0.049+	(1.81)	0.050+	(1.86)
No children in household	0	(.)	0	(.)	0	(.)
One child	-0.055*	(-2.31)	-0.032	(-1.32)	-0.032	(-1.33)
Two children	-0.054*	(-2.34)	-0.023	(-0.99)	-0.023	(-0.99)
Three+ children	-0.068**	(-2.87)	-0.021	(-0.88)	-0.020	(-0.84)
One adult in household	0	(.)	0	(.)	0	(.)
Two Adults	-0.079***	(-3.61)	-0.058**	(-2.62)	-0.060**	(-2.72)
Three+ adults	-0.22***	(-7.87)	-0.17***	(-6.11)	-0.17***	(-6.28)
Full time	0	(.)	0	(.)	0	(.)
Park time	-0.041	(-1.58)	-0.041	(-1.57)	-0.036	(-1.39)
Temp. not working	-0.0041	(-0.10)	-0.015	(-0.36)	-0.011	(-0.26)
Unemployed	-0.31***	(-7.51)	-0.30***	(-7.22)	-0.30***	(-7.12)
Retired	0.051	(1.59)	0.030	(0.92)	0.034	(1.06)
School	-0.020	(-0.33)	-0.046	(-0.75)	-0.043	(-0.71)
Keeping House	-0.016	(-0.56)	0.00012	(0.00)	0.0046	(0.16)
Other	-0.23***	(-4.26)	-0.21***	(-3.93)	-0.20***	(-3.81)
1998	0	(.)	0	(.)	0	(.)
2000	-0.0046	(-0.15)	0.0073	(0.25)	0.0047	(0.16)
2002	0.0024	(0.07)	-0.0041	(-0.12)	-0.0024	(-0.07)
2004	0.015	(0.41)	-0.000052	(-0.00)	0.0034	(0.10)
2006	-0.071*	(-2.41)	-0.074*	(-2.53)	-0.072*	(-2.45)

2008	-0.040	(-1.42)	-0.036	(-1.31)	-0.031	(-1.10)
2010	-0.17***	(-6.33)	-0.17***	(-6.13)	-0.16***	(-5.81)
2012	-0.075*	(-2.06)	-0.078*	(-2.12)	-0.067+	(-1.81)
2014	-0.084**	(-2.97)	-0.080**	(-2.80)	-0.074**	(-2.59)
Observations	15789		15789		15789	

t statistics in parentheses

$^{+}$ $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$

Notes: Rows with a coefficient of zero denote baseline categories for categorical variables. Models are weighted to account for sampling. In Model 2, the reference group coefficients represent the effect of income in dollars (divided by 10,000 for clarity) while in Model 3 they represent the effect of logged dollars.

Table D.5: Effect of Geographic Reference Groups

	(1)	(2)	(3)	(4)	(5)
Income decile (national)	0.21***	0.20***	0.20***	0.19***	0.20***
	(60.12)	(59.11)	(57.94)	(51.37)	(51.69)
State income	-0.027*				-0.030*
	(-2.54)				(-2.16)
Commuting zone income		-0.010			-0.020+
		(-1.44)			(-1.65)
County income			0.00092		-0.0048
			(0.16)		(-0.55)
Tract income				0.020***	0.029***
				(5.71)	(6.84)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	15789	15789	15789	15789	15789

t statistics in parentheses
$^{+}$ $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$
Notes: All individual-level control variables are included in every model. Models are weighted to account for sampling.

Table D.6: Effect of Geographic Reference Groups (Logged Income)

	(1)	(2)	(3)	(4)	(5)
Income decile (national)	0.21***	0.20***	0.20***	0.20***	0.20***
	(60.10)	(58.97)	(57.84)	(52.49)	(52.75)
State log(income)	-0.15*				-0.18*
	(-2.44)				(-2.34)
Commuting zone log(income)		-0.051			-0.11
		(-1.29)			(-1.60)
County log(income)			0.0099		0.012
			(0.30)		(0.23)
Tract log(income)				0.11***	0.15***
				(4.96)	(5.90)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	15789	15789	15789	15789	15789

t statistics in parentheses
$^{+}$ $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$
Notes: All individual-level control variables are included in every model. Models are weighted to account for sampling.

Table D.7: Effect of Sociodemographic Reference Groups

	(1)	(2)	(3)	(4)	(5)
Income decile (national)	0.19***	0.19***	0.20***	0.20***	0.19***
	(51.30)	(53.87)	(59.22)	(59.91)	(49.07)
Degree income	0.039***				0.031***
	(10.04)				(7.27)
Prestige income		0.033***			0.018***
		(8.51)			(4.20)
Race/ethnicity income			0.0085		0.0017
			(1.33)		(0.27)
Cohort income				-0.026**	-0.025**
				(-2.74)	(-2.64)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	15789	15789	15789	15789	15789

t statistics in parentheses

$^{+}$ $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$

Notes: All control variables are included in every model. Models are weighted to account for sampling.

Table D.8: Effect of Sociodemographic Reference Groups (Logged Income)

	(1)	(2)	(3)	(4)	(5)
Income decile (national)	0.19***	0.19***	0.20***	0.21***	0.19***
	(51.72)	(54.12)	(59.23)	(59.90)	(49.27)
Degree log(income)	0.23***				0.18***
	(8.92)				(6.46)
Prestige log(income)		0.19***			0.12***
		(7.88)			(4.42)
Race/ethnicity log(income)			0.046		0.0044
			(1.26)		(0.12)
Cohort log(income)				-0.16**	-0.15**
				(-2.79)	(-2.65)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	15789	15789	15789	15789	15789

t statistics in parentheses

$^{+}$ $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$

Notes: All control variables are included in every model. Models are weighted to account for sampling.

Table D.9: Ordinal Logistic Regression Results for Models with All Reference Groups

	(1) RG income/10,000		(2) ln(RG income)	
Perceived relative income				
Income decile (national)	0.49***	(41.69)	0.50***	(42.30)
State income	-0.065+	(-1.92)	-0.38*	(-2.03)
Commuting zone income	-0.057+	(-1.90)	-0.32+	(-1.87)
County income	-0.018	(-0.86)	0.0018	(0.01)
Tract income	0.068***	(6.79)	0.32***	(5.40)
Degree income	0.078***	(7.34)	0.45***	(6.42)
Prestige income	0.045***	(4.19)	0.30***	(4.70)
Race/ethnicity income	-0.027+	(-1.67)	-0.16+	(-1.77)
Cohort income	-0.064**	(-2.77)	-0.40**	(-2.81)
cut1				
Constant	-1.74***	(-6.61)	-3.57	(-1.38)
cut2				
Constant	0.72**	(2.73)	-1.10	(-0.43)
cut3				
Constant	3.72***	(13.89)	1.89	(0.73)
cut4				
Constant	6.72***	(24.14)	4.86+	(1.88)
Observations	15789		15789	

t statistics in parentheses

$^{+}$ $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$

Notes: All control variables are included in every model. Models are weighted to account for sampling. In Model 1, the reference group coefficients represent the effect of income in dollars (divided by 10,000 for clarity) while in Model 2 they represent the effect of logged dollars.

Table D.10: Tobit Regression Results for Models with All Reference Groups

	(1) RG income/10,000		(2) ln(RG income)	
model				
Income decile (national)	0.19***	(42.38)	0.20***	(43.27)
State income	-0.031*	(-2.08)	-0.18*	(-2.21)
Commuting zone income	-0.024+	(-1.87)	-0.14+	(-1.89)
County income	-0.0047	(-0.51)	0.014	(0.25)
Tract income	0.027***	(5.76)	0.14***	(5.09)
Degree income	0.031***	(6.71)	0.19***	(6.11)
Prestige income	0.018***	(3.81)	0.11***	(3.96)
Race/ethnicity income	-0.0097	(-1.37)	-0.065	(-1.60)
Cohort income	-0.032**	(-3.10)	-0.19**	(-3.07)
Constant	-0.74***	(-6.36)	0.55	(0.48)
sigma				
Constant	0.86***	(97.05)	0.86***	(97.30)
Observations	15789		15789	

t statistics in parentheses

$^{+}$ $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$

Notes: The Tobit models account for both upper and lower limit censoring. All individual-level control variables are included in every model. Models are weighted to account for sampling. In Model 1, the reference group coefficients represent the effect of income in dollars (divided by 10,000 for clarity) while in Model 2 they represent the effect of logged dollars.