Politics and Economic Stratification: Power Resources and Income Inequality in the United States[1]

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What factors best explain the U.S. growth in economic stratification since the late 1970s? This research tests the power resource hypothesis with a pooled time-series analysis of income inequality. In a departure from prior research, the explanatory power of both national- and state-level political accounts is evaluated. By analyzing tax statistics, the authors isolate the factors that produce the largest gap in U.S. income distributions. Findings indicate that increases in the political strength of neoliberal national administrations and skill-biased technical change (SBTC) are the most influential determinants—although the SBTC account became much less influential after the 1980s. Other accounts have considerable explanatory power, as the results show that inequality grew after decreases in manufacturing and expansions in minority populations. But these findings show that national-level neoliberal political determinants best explain the extraordinary increase in U.S. income inequality.

After 1929, income inequality in the United States began a long but modest decline. In the last quarter of this century, however, this trajectory reversed, and income inequality, along with dispersion in earnings and wealth, began a rapid growth in place of the prior unhurried decrease. What factors best account for this underresearched departure? This issue is important partly

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because in comparison to the other wealthy democracies, the U.S. increase in economic stratification was most pronounced (Atkinson 2015). To provide additional insight about an issue that once was a dominant concern in sociology, we use Korpi's (1985) power resource approach as the conceptual basis for a pooled time-series analysis of income inequality in the U.S. states.

Korpi's perspective is valuable because it highlights the centrality of politics. In contrast to the technological story favored by almost all economists—which stresses the purported skills derived from higher education—sociologists (Lenski 1966; DiPrete 2007; Volscho and Kelly 2012; Jacobs and Myers 2014), political scientists (Bartels 2008; Hacker and Pierson 2010*b*), and a few iconoclasts in economics (Card and DiNardo 2002; Limieux 2008; Piketty 2014; Atkinson 2015) suggest that political and institutional accounts help explain the sharp U.S. growth in economic stratification. But despite their likely explanatory power, and except for two recent studies (Volscho and Kelly 2012; Jacobs and Myers 2014), political accounts have not been systematically researched.

The two studies that examined political factors (Volscho and Kelly 2012; Jacobs and Myers 2014) relied on time-series methods.[2] This meant that shifts in the size of minority populations—which are likely to have potent effects on inequality (Western, Bloome, and Percheski 2008; Bloome 2014)—could not be assessed. Because longitudinal data on income inequality in the United States are restricted to yearly frequencies, these political studies were based on 60 cases. The pooled cross-sectional time-series design we use will let us assess the determinants of this outcome with an analysis based on 1,615 state-years. Such an analysis is likely to capture the effects of less influential determinants better than pure time-series studies limited to about 60 years. The greater statistical power provided by a pooled time-series design allows the inclusion of more explanatory factors. This approach will let us use fixed effects to estimate these influences. These combined advances are likely to reduce omitted variable bias and produce superior estimates.

This study relies on another departure. Almost all studies of U.S. income inequality analyze census statistics, but these data are problematic (Atkinson, Piketty, and Saez 2011; Piketty 2014). Although other gaps in the income distribution grew, the post-1970s acceleration in inequality created the greatest void between citizens with the highest incomes and all others (Brady and Leicht 2008; Piketty 2014).[3] Except for the few researchers

[2] Other noneconomic statistical studies that examined U.S. income or earnings inequality or related matters with data that ended well before ours include Devine (1983), Jacobs (1985), Hibbs and Dennis (1988), Raffolovich, Leicht, and Wallace (1992), Raffolovich (1993), and Wallace, Leicht, and Raffolovich (1999).

[3] According to Hacker and Pierson in the journal *Politics and Society*, e.g., "American inequality is not mainly about the gap between the well educated and the rest. . . . It is

who used tax data to analyze shifts in the income share of the top 1% (Piketty and Saez 2003; Volscho and Kelly 2012; Piketty 2014; for a review, see Atkinson et al. 2011), the studies based on census data had to ignore the largest incomes and the most pronounced income gap because the census top income categories are too low. Yet studies that focused only on the income share of the top 1% by analyzing tax data could assess the disparity between the most substantial incomes and all other income recipients only indirectly. This is so because such top share percentage measures do not take lower incomes expressly into account.

We address these problems by analyzing a more inclusive inequality measure. We capture the incomes of the modestly affluent and those nearer to the midpoint as well as the most substantial incomes by analyzing state income data provided by the Internal Revenue Service (IRS). These tax statistics capture the highest incomes well (e.g., the largest published income category in the 2010 census was $200,000 plus, yet the IRS top category has been $1,000,000 plus for many years). According to Atkinson et al. (2011), while neither the census nor the IRS sources are without problems, tax data "in our judgment, can be used for distributional analysis" (p. 67). Although the IRS income statistics ignore some low incomes, we analyze these data because they provide the best opportunity to detect the factors that produced the most substantial U.S. income gap that has accelerated most rapidly since the late 1970s.[4]

Economic inequality and politics are interrelated. According to Thomas Piketty (2014, p. 577), for example, "A quick glance at the curves describing income and wealth inequality . . . is enough to show that politics is ubiquitous and that economic and political changes are inextricably intertwined and must be studied together." Yet only a few recent statistical studies have examined these relationships in the United States. In this article we report an analysis grounded in political economic concepts that we hope will fill part of this critical gap in the literature.

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about the extraordinary rapid pulling away of the very top. Those at the top are often highly educated, but so too are those just below them who have been left behind. . . . Only a very small slice of the new educational elite has entered the new economic elite" (2010*b*, p. 201).

[4] For reviews of sociological research on economic inequality, see Morris and Western (1999), Neckerman (2004), Neckerman and Torche (2007), and McCall and Percheski (2010). In addition to the Atkinson et al. (2011) review of economic research on top shares and the commanding treatment by Piketty (2014), see Levy and Murnane (1992), Danziger and Gottschalk (1995), DiNardo, Fortin, and Lemieux (1996), Gottschalk and Smeeding (1997), Gordon and Dew-Becker (2008), Limieux (2008), and Atkinson (2015) for summaries of the research in economics that mostly covers entire distributions. Because this analysis focuses on the United States, to save space we do not attempt to cover the cross-national literature, but see Scheve and Stasavage (2009) for an attempt at such an analysis in political science; Brady and Leicht (2008), however, find opposite results largely because they focus on the effects of political parties on the Right.

THEORY

In a single sentence Korpi (1985, p. 38) spells out why the power resource perspective should explain shifts in economic inequality. "Variations in the differences in power resources between the classes can be assumed to have significant consequences for distributive policies," such that shifts in inequality should be closely tied to changes in the distribution of political resources between classes. This perspective with its stress on changes in the relative resources of business and wage earners provides the conceptual basis for the hypotheses we test about the degree to which neoliberal national and state political regimes led to the acceleration in inequality and the degree to which the labor movement withstood this increase.

Walter Korpi's perspective leads to an expectation that ebbs and flows in the political assets of labor and shifts in the political strength of neoliberal forces aligned with business will yield strong explanations for changes in income inequality. We adopt the strategy followed by Brady and Leicht (2008) and focus on the political strength of the conservative probusiness neoliberal Republican party, but we assess changes in the strength of this political resource both within the states and at the national level. Shifts in strength of the most important wage earner political resource should help explain shifts in the income distribution as well. Yet there were no politically significant labor parties in the United States during the analysis period. To capture the influence of labor's primary political resource, we assess the influence of within-state union strength to determine whether changes in this critical power resource (Western and Rosenfeld 2011; Brady, Baker, and Finnigan 2013; Jacobs and Myers 2014) had opposite effects on within-state inequality.

Political Effects

A fateful political departure occurred when Ronald Reagan entered the White House in 1981 (Akard 1992; Jacobs and Myers 2014). Unlike the prior occupants from either party since 1945, Reagan successfully supported many neoliberal policies that reduced the federal government's regulation of economic affairs. Members of his administration deregulated financial markets (Hacker and Pierson 2010*a*; Bonica et al. 2013). Soon after he took office, Reagan underscored his opposition to unions by breaking the PATCO air controllers strike. In a sharp departure from tradition, he selected National Labor Relations Board members—charged with regulating disputes between labor and management—who were unsympathetic to labor (Tope and Jacobs 2009). These appointments and Reagan's pointed response to the PATCO strike sent a clear message that members of his administration might avert their eyes if employers used unfair labor practices to break strikes or undermine other union endeavors (Voss and Sherman 2000).

Although some deregulation took place in the years immediately before Reagan's presidency, Akard's (1992) account of legislative battles shows that almost all of management's political victories in the late 1970s were defensive. This pattern soon reversed after Reagan took office in 1981, as management began to gain long sought legislative and (de)regulatory triumphs. These victories undercut the unions' capacity to organize new workplaces (Tope and Jacobs 2009) and their ability to contest labor law violations by employers. These regulatory and legislative successes also let businesses engage in more profitable but financially precarious economic conduct (Tomaskovic-Devey and Lin 2011) with unfortunate later macroeconomic consequences.

The net effect of all these alterations was sharp increases in both executive earnings (Lin and Tomaskovic-Devey 2013) and stockholder rewards (Tomaskovic-Devey and Lin 2011). Reagan's weakening of unions had opposite effects on middle- and lower-income workers. Unions compress wage differences when they organize workplaces (Freeman and Medoff 1984; Card, Lemieux, and Riddel 2004; Western and Rosenfeld 2011). When they were strong, unions successfully lobbied for many public policies that increased incomes in the lower half of the income distribution (Greenstone 1977; Western and Rosenfeld 2011; Kristal 2013). Findings indicate that the Reagan neoliberal policies that undermined unions contributed to the stagnation in the real incomes of citizens near to or below the median (Jacobs and Myers 2014). In short, the deregulation of many industries and financial activities along with the deregulation of labor markets that advantaged employers led to the substantial acceleration in the growth of income inequality that had already begun in the years just before Reagan's presidency (Jacobs and Myers 2014).[5]

These distributive consequences did not contradict neoliberal views. In the Reagan era and thereafter, neoliberalism became the primary ideology that motivated U.S. policy elites from both political parties (Harvey 2005; Centeno and Cohen 2012)—although these presuppositions were much more important for Republicans especially during and after Reagan's presidency. This ideology rests on a simplified version of microeconomic theory that gives rise to a singular faith that citizens are rational agents who invariably respond appropriately to incentives.

[5] Many studies in labor economics conclude that unions raise covered wages by about 15%–20% (Freeman and Medoff 1984). In addition to their political efforts, unions benefit workers employed in nonunion firms because these firms pay more to avoid unionization. Although unionization may create differences in earnings between higher-paid unionized and lower-paid nonunionized employees, almost all studies find that the entire wage distribution in unionized firms and industries is more equal.

Substantial increases in the pecuniary rewards for socially beneficial behavior therefore should lead to significant increases in these acts. In this view, unequal distributions that involve great rewards for a few will produce valuable behavior that will advantage all. Ronald Reagan's close ideological ally Margaret Thatcher expressed this fundamental principle of their shared faith by declaring, "It is our job to glory in inequality and to see that talents and abilities are given vent and expression for the benefit of us all" (quoted by Wade 2012, p. 21). As these presuppositions suggest, a graph in Atkinson et al. (2011, fig. 8) shows that the income shares of the top 1% grew rapidly in the United Kingdom and the United States soon after Thatcher and Reagan took office.[6]

These political considerations lead to four testable hypotheses. First, *income inequality in the states should increase after Reagan and subsequent neoliberal Republican presidents entered the White House*.[7] Second, partly because almost all changes in public policy including those at the state level have significant distributional consequences (Kelly 2005; Brady and Leicht 2008), *a growth in neoliberal Republican political dominance in the states should produce additional expansions in income inequality in these jurisdictions*.[8] Third, since the income distribution is skewed such that there are far more voters who are not affluent compared to the small minority with substantial incomes, it is likely that presidential administrations will enact policies near to the end of their four-year terms that promote greater equality during presidential election years. Hence, *income inequality should decrease in years when presidents (or presidential candidates from their party) must face the electorate*. Fourth, because strong unions reduce income

[6] According to James Hightower, the Texas Agricultural Commissioner writing in 1987, "The economic agenda of the past seven years produced one of the quickest and most regressive redistributions of wealth in US history. For all of the impassioned rhetoric about removing government as a force in our financial affairs, the Reagan government injected itself more enthusiastically into the economy than any administration since Lyndon Johnson's Great Society. Indeed, Reagan's administration took so much money from the pockets of middle- and lower-income Americans and shoved it up to the wealthiest 10 percent in our society that a top-heavy structure now threatens to come crashing down on us" (as quoted by Phillips 1991, p. 74).

[7] Hypotheses refer to change in the explanatory and dependent variables because the fixed-effects estimator we use only assesses relationships between changes in variables. But this does not imply that the variables should be in change score form, as this would introduce improper doubled changes in variables.

[8] State governments make many decisions about property rights and businesses taxation (Kelly 2005). Because markets rest on the exchange of property rights, some inevitably will lose and some will gain from these decisions (see Jenkins, Leicht, and Wendt [2006] who list multiple state development policies that differentially affect the profits of either large or small firms). The states control labor-management relationships with right-to-work laws and unemployment benefits and by enforcing other regulations that affect wages. See Brady et al. (2013) for a systematic analysis of such influences on state poverty rates.

inequality by compressing earnings differences within organized firms and by successfully lobbying for policies that benefit the less affluent, *expansions in union strength will lead to decreased income inequality in the states*. With few exceptions, however, union membership declined during the analysis period. We nevertheless expect to find a negative relationship attributable to downward shifts in union strength and the resulting growth in income inequality produced by reductions in union strength. These union findings, however, may not be as strong as those in Jacobs and Myers (2014) because this study focuses on the gap between the extremely affluent and all other income recipients. The four hypotheses in this and the prior paragraph capture the core of this analysis. If they are correct, the findings will support a claim that increases in business power resources along with a reduction in the most important worker power resource worked together to create the growth in income inequality in the states.

Minority Effects

According to William Julius Wilson, "The social problems of urban life in the United States are, in large measure, the problems of racial inequality" (1987, p. 20). Throughout its history the United States has been plagued by intense racial conflicts (Myrdal 1944; Tocqueville 1948). As a consequence of the severe discrimination against African-Americans in the past in all U.S. regions along with the lasting effects of the brutal repression in the Jim Crow South, when they enter the labor market, blacks face greater obstacles than whites. This harsh treatment in the past—along with the less intense but still damaging discrimination in contemporary labor markets documented by Pager (2003) and Bertrand and Mullainathan (2004)—created severe obstacles for blacks and Hispanics. Such barriers translate into reduced incomes for both groups. According to Bloome (2014, p. 1196), as a result of many countervailing trends "median black family income has remained around two-thirds the size of median white family income throughout the twentieth century and into the twenty-first" (Loury 1977; Isaacs 2008). It follows that shifts in the proportion of black residents in states should alter the income distribution in these units.

African-Americans (and probably Hispanics) have not accrued nearly as much personal wealth as whites (Oliver and Shapiro 1997). Because income from wealth can be substantial, discriminatory practices along with the current racial and minority wealth disparities suggest that increases in the proportion of African-Americans in states should have a strong positive relationship with income inequality. Yet this effect may have diminished during the analysis period, so we interact black presence with time. And because Hispanics suffer from discrimination as well, the results should show that increases in Hispanic presence also led to a growth in income inequality in the states.

Economic Determinants

The principal explanation in economics for the growth in both earnings and income inequality rests on schooling. Almost all economists believe technologically driven increases in the demand for employees with additional years of higher education produced greater earnings. This relationship allegedly occurred because these better educated employees became increasingly proficient with computers and other productive technologies (see Goldin and Katz [2008] for a detailed treatment; Card and DiNardo [2002], DiPrete [2007], and Limieux [2008] for criticisms; and Liu and Grusky [2013] for a sociological analysis).[9] Because economists believe employees are paid in accordance with their marginal product (or the unique increment each employee's labor adds to total firm revenues), if these hypotheses are correct, the greater productivity of college-educated workers should enhance their earnings and income and thereby create increased income inequality.[10]

This account suggests that there should be a positive relationship between college education rates and income inequality. Yet because this educational effect probably was most influential in the 1980s but weakened thereafter (Card and DiNardo 2002; Lemieux 2006), an interaction with time may support a hypothesis that higher education's positive influence on inequality declined after the 1980s.

Recent sociological scholarship has fruitfully emphasized the importance of financialization (Krippner 2005, 2011; Davis 2009; Tomaskovic-Devey

[9] This explanation worked best in the 1980s when "there was a pervasive growth in wage inequality throughout the wage distribution. By contrast, inequality growth since 1990 has been concentrated in the top end of the distribution" (Limieux 2008, p. 27). After 1990 educational effects should matter less since corporate executives and top earners in finance obtained a lion's share of these rewards. Political and social skills are critical determinants of success in such occupations, but it is doubtful that higher education confers significant improvements in these talents. And such educational effects should be ubiquitous at least in the developed world, but a growth in earnings inequality did not occur in France, Japan, and probably Germany (Limieux 2008). These anomalies suggest that politics and labor market institutions (which, of course, are shaped by politics) are at least as important as technological skills.

[10] This clever account is problematic. According to marginal product theory, if a firm pays less than an employee's marginal product, other employers will profit by offering that employee higher pay until the employee's pay is bid up to her marginal product. But most production within firms is based on highly interdependent relationships. This means even direct supervisors have difficulty assessing each employee's unique contribution. When this common condition is present, it is difficult to believe that external employers can assess the precise value of an individual's unique contributions in a different firm. Although tests have been supportive, they were based on atypical occupations such as real estate sales and professional baseball. Baseball, like real estate sales, is an individual endeavor with little interdependence between actors, such that productivity is readily measured. These defects are important as marginal product theory provides the ultimate basis for neoliberal claims that compensation set by markets is immutable and just.

and Lin 2011; Lin and Tomaskovic-Devey 2013). Many firms that had engaged in other activities began to make money from purely financial endeavors especially during the period when the growth in inequality was most pronounced. This change occurred partly because many financial activities were deregulated by Reagan appointees and by subsequent neoliberal administrations (Tomaskovic-Devey and Lin 2011; Bonica et al. 2013). Lin and Tomaskovic-Devey (2013) analyze cross-industry determinants and find that financialization produced increased income inequality probably by enhancing the "negotiating power of owners and executives in the compensation-setting . . . process" (p. 1294). Because Jacobs and Myers (2014) find added support for this hypothesis, we enter this determinant in our models.

An older but still popular explanation for shifts in inequality is the J-curve hypothesis put forward by Kuznets (1953, 1955; see Piketty [2014] for a critique) who stressed the nonlinear effects of economic development on economic inequality. We test this account by assessing the influence of economic development in the states. Because Kuznets focused on rural employment as well, we examine this effect in our models.

Inasmuch as growth in stock market prices should disproportionately increase the incomes of the most prosperous, fluctuations in this outcome should have a positive relationship with income inequality (Volscho and Kelly 2012; Atkinson 2015) especially if the inequality measure in question captures the highest incomes. Although earnings have become an increasingly important part of the most substantial incomes, income from stocks nevertheless remains a significant ingredient of these rewards (Atkinson et al. 2011; Piketty 2014).

Unemployment should enhance inequality because low-wage workers are first to be laid off when the job market softens. In comparison to the earnings of the highest paid employees, however, a growth in the unemployment rate should lead to greater reductions in the pay of midlevel employees and thereby widen the gap between employees with the highest earnings and their less affluent counterparts.

As Wilson points out in many of his scholarly works (see, e.g., Wilson 1987), manufacturing firms pay wages close to the median, such that the long secular decline in these jobs can be expected to produce greater inequality. Manufacturing firms in the past offered many relatively well-paying jobs to workers with high school degrees. But now when such jobs are lost, workers subjected to these economic shifts often cannot obtain new employment that pays as much. The long slow reduction in manufacturing employment should lead to increased income inequality. Finally, expansions in women's labor market participation should compress family income differences, as studies (Cancian and Reed 1999; Western et al. 2008) indicate that this growth disproportionately increased the incomes of less affluent families.

METHOD

Sample and Estimation

Sample.—To obtain consistent longitudinal data without measurement departures, we analyze income inequality in the U.S. states from 1978 to 2011 (see the appendix for sources).[11] Republican strength in the state legislatures typically is the most influential determinant of this party's within-state political influence. Yet the Nebraska legislature is nonpartisan. This means the most potent indicator of Republican strength cannot be calculated for this state. To provide sufficient time for the hypothesized relationships to be completed, we lag the explanatory variables by at least a year, as longer lags reduce explanatory power (most political variables are lagged by two years to give changes in public policy enough time to matter, but these longer lags do not alter the number of cases). In addition to missing data from Nebraska, there are two unique missing values that occur at the start of two state series. One-year lags, 49 states, two additional missing values, and 33 years combine to yield an almost perfectly balanced 1,615 state-year sample except when we drop the 11 ex-Confederate states to discover whether the results depend on this singular region.

Estimation.—Omitted variable bias clearly is the most important threat to nonexperimental findings. We seek to eliminate this disturbance in several ways: first, we estimate with fixed effects because this method removes the confounding effects of any unmeasured determinants that do not change.[12] State cultural differences, for example, are likely to influence income inequality. Yet this determinant probably is mostly time invariant. This means that a large part of this difficult-to-measure explanatory factor is eliminated in fixed-effects analyses (Western 2006).[13]

In addition to this valuable procedure, we enter a linear time trend for each state along with its square in all models to capture any additional unmeasured determinants that operate in either a linear or a nonlinear manner. See Friedberg (1998), Ziliak et al. (2000), and Gundersen and Ziliak

[11] Data from earlier years are inconsistent with the more accurate state data series that begin in the late 1970s, mostly because the agencies that collected these data altered their reporting or measurement practices.

[12] See Halaby (2004) for a detailed treatment of the many advantages of pooled time-series designs along with the benefits conferred by fixed-effects estimation. Lieberson (1985) highlights the virtues of time-series designs. He asserts that causal assertions rest on claims about the effects of changes within cases, but cross-sectional analyses force differences between cases to be used to proxy within-case changes. This demands a heroic assumption. "If [we can] assume the values of a dependent variable were all initially the same, *only then are there grounds to believe that situations observed cross-sectionally will at a later . . . time provide a reasonable* [picture of] *causation*" (Lieberson, p. 180).

[13] We estimate with the Stata (ver. 13.1) procedure areg, which performs conventional case-specific mean-centered calculations to obtain fixed effects. In two final models, we

(2004) for precedent for using such case-specific linear and squared trend terms entered to reduce omitted variable bias in analyses of state data. We also include national-level explanatory variables such as measures that capture the presence of Republican presidents. Perfect collinearity, however, means we cannot include year-specific dummy variables in these models despite the clear advantages of this specification.

Measurement

Income inequality and a corrective explanatory variable.—The dependent variable is measured with the natural log of the Theil inequality measure computed on IRS income data by Frank (2014). Most continuous variables are logged unless this transformation ruins a variable's explanatory power. We log Theil and other indicators to correct modest nonlinearities. Theil often has been used to assess inequality in sociological research (Breen and Salazar 2011; Bloome 2014). This measure is based on group comparisons much like those in Gini (Allison 1978), although Theil is an entropy measure calculated with the average of logs of reciprocals of income shares weighted by income. In comparison to Gini, Theil exhibits greater sensitivity to differences in the upper tail of distributions (Allison 1978). This attribute makes Theil more suitable for this study especially because Gini is most sensitive to differences nearest to distribution midpoints.

Atkinson et al. (2011) claim that the Current Population Survey (CPS) and IRS income data have roughly equal but dissimilar advantages and disadvantages. Yet the IRS data best suit our purposes largely because these statistics provide a superior enumeration of high incomes. Another IRS data advantage is their sensitivity to income from capital gains—which is ignored by the CPS (Atkinson et al. 2011). There are no penalties for misstating incomes on census forms, but taxpayers risk criminal sanctions for understating their incomes to reduce their taxes.

One potential problem with the IRS data concerns taxpayer responses to changes in the progressivity of the tax codes—which may alter measures of inequality based on these data (Atkinson et al. 2011). We deal with this possible hazard with an explanatory variable that purges the dependent variable of alterations in tax code progressivity by using the marginal rates stipulated in the federal income tax codes. A marginal tax rate is the added

estimate with a feasible generalized least squares (FGLS) approach (the Stata command is xtgls) that also fits panel-data models. This FGLS estimator removes the effects of serial correlation with an AR(1) correction, and it corrects heteroskedasticity as well (see Greene [2012] or Maddala and Lahiri [2006] for discussions). To obtain fixed-effects estimation, we enter state-specific dummy variables in these xtgls models because Stata does not provide a mean-centered FGLS estimator. Both dummy and mean-centered approaches, of course, yield the same estimates.

amount a taxpayer must pay for a dollar increase in her income. If a tax is progressive, such that the taxes paid by affluent citizens are larger proportions of their incomes, marginal rates on higher incomes must be greater than they are on more modest incomes.

Changes in the tax codes that make the marginal rates increasingly unequal therefore produce greater equality in after-tax distributions. As the federal marginal rates on larger incomes always are higher than these rates on smaller incomes, increases in the inequality of the marginal rates will yield a more progressive tax. We create a tax progressivity index based on federal marginal tax rates on family incomes at half of each state's real median income, incomes at the median, and the marginal rates on incomes at two, four, six, and eight times state median incomes in each year (see Schmidt [1993] for the calculation of these rates; note that the results are robust if we select different income levels). We assess the degree of inequality in these six marginal rates with their variance since we are most interested in the effects of the highest tax rates on the most substantial incomes.[14]

As this variance is computed on federal marginal rates, changes that lead to progressive or regressive shifts in the income tax should remove the effects of taxpayer reactions to these shifts in tax policy that would affect their reported incomes. Inasmuch as greater inequality in the marginal rates must produce a more progressive tax, we expect negative signs on this variable (see Piketty and Saez [2003] and Saez [2004] who stress the importance of tax progressivity).

Measurement of theoretically derived explanatory variables.—To capture national political effects, we enter either a dummy variable coded 1 for all Republican presidents in this period or two dummy variables. One is coded 1 for Reagan's tenure, and the second is coded 1 for the two other Republican presidencies (Bush 1 and Bush *2*). To assess Republican strength in the states, we include a dummy variable coded 1 for the presence of a Republican governor along with the natural log of the percentage of Republicans in both houses of the state legislature. We expect positive coefficients on these explanatory variables because increases in this neoliberal party's strength should produce increased inequality. We capture presidential election years with a dummy coded 1 and expect a negative relationship. To assess union strength, we enter the natural log of the percentage of nonagricultural workers in unions and expect this coefficient to be negative as well.

[14] The variance is based on the sum of squared differences from the mean, such that this marginal tax inequality measure is most sensitive to the largest components of skewed distributions. But the variance is not scale invariant. Because these marginal tax rates do not exhibit substantial change, a scale-invariant measure is unnecessary. And because entering the mean of state marginal tax rates in models not shown has no effect on the theoretical implications, our use of this non-scale-invariant measure will not create inaccuracies.

We measure higher education with the natural log of the percentage of state residents older than 24 who completed four or more years of college and expect a positive sign. To capture potentially diminishing over time effects, we interact this variable with a year counter and predict a negative sign on this term. Financialization is captured with the percentage of workers employed in such enterprises. This operationalization departs from convention, but we cannot locate the standard measure (financial profits) for states. State economic development is measured with real per capita personal income, as alternatives such as real gross state product per capita or real median household income have no explanatory power. The degree to which a state's economy remains rural is captured with the percentage employed in rural occupations (e.g., crop and animal production, forestry, logging, fishing, hunting, and trapping) and by population density—which should have predominantly negative but diminishing nonlinear effects.

We assess stock values with the Standard and Poor's 500 index. Employment is measured with state joblessness rates, and we assess manufacturing employment with the log of the percentage employed in this manner. Women's labor force participation is assessed with the relevant percentage using Bureau of Labor Force Statistics; we assess minority presence with the log of the percentage of blacks and with the percentage of Hispanic residents.[15] We interact black presence with a year counter as well to determine whether this effect diminished during the analysis period and expect a negative sign on this term.

Specification

One of the more general specifications of these fixed-effects models is

$$\begin{aligned}\text{INEQUALITY} = {} & \mathbf{b}_0 + \mathbf{b}_1\text{REPUB-GOV}_{t-2} + \mathbf{b}_2\,\%\text{REPUB-LEG}_{t-2} \\ & + \mathbf{b}_3\text{REPUB-PRES}_{t-2} + \mathbf{b}_4\text{PRES-ELECT}_{t-1} + \mathbf{b}_5\,\%\text{UNION}_{t-1} \\ & + \mathbf{b}_6\text{FINANCE-EMP}_{t-1} + \mathbf{b}_7\,\%\text{BLACK}_{t-1} + \mathbf{b}_8(\%\text{BLACK} \times \text{YR})_{t-1} \\ & + \mathbf{b}_9\,\%\text{HISPANC}_{t-1} + \mathbf{b}_{10}\text{INC-PER-CAP}_{t-1} + \mathbf{b}_{11}\text{INC-PER-CAP}^2_{t-1} \\ & + \mathbf{b}_{12}\,\%\text{COLLEG}_{t-1} + \mathbf{b}_{13}(\%\text{COLLEG} \times \text{YR})_{t-1} + \mathbf{b}_{14}\text{DENSITY}_{t-1} \\ & + \mathbf{b}_{15}\text{DENSITY}^2_{t-1} + \mathbf{b}_{16}\,\%\text{POVERTY}_{t-1} + \mathbf{b}_{17}\,\%\text{RURAL-EMP}_{t-1}\end{aligned}$$

[15] Logging the percentage of Hispanics eliminates this variable's explanatory power in models not shown.

$$+ \mathbf{b}_{18}\%\text{WOMEN-LBR-FRC}_{t-1} + \mathbf{b}_{19}\%\text{MANU-EMP}_{t-1}$$
$$+ \mathbf{b}_{20}\text{STOCK-VAL}_{t-1} + \mathbf{b}_{21}\%\text{UNEMPLOYED}_{t-1}$$
$$+ \mathbf{b}_{22}(\%\text{UNEMPLOYED} \times \text{YR})_{t-1} + \mathbf{b}_{23}\text{INEQ-MARGNL-TX}$$
$$+ \mathbf{b}_{24-73}(49\ \text{STATE TRNDS}) + \mathbf{b}_{74-123}(49\ \text{STATE TRNDS}^2) + e. \quad (1)$$

All required main effects are present in these models, as the state-specific trends capture the time main effect for the interactions between time and race or those between time and the college education rate.

ANALYSES

Table 1 shows the means, standard deviations, ranges, and the expected signs. Figures 1 and 2 graph the associations between two core explanatory variables and inequality using mean-centered variables like those used by our fixed-effects estimation approach (the correlation between union strength and inequality is .734, while the correlation between four-year college completion rates and this outcome is .707).[16] While these two relationships are substantial, they may not persist after other determinants are held constant.

We begin the multivariate analyses with a restricted baseline model that focuses on political variables and union strength only. Model 1 in table 2 includes the indicator variables that measure the presence of a Republican governor and the presence of a Republican president. We also assess Republican strength with the percentage of Republicans in both houses of the state legislature. This baseline model includes the dummy variable coded 1 for presidential election years as well as the union density measure.

In model 2 we add employment in finance, black and Hispanic presence, and economic development along with its square to the explanatory variables in model 1. In model 3, we add the two college education measures, employment in rural occupations, female labor market participation, man-

[16] The three states with the most growth in income inequality in the analysis period as measured by the range in their (unlogged) Theil scores were Connecticut (1.215), New York (1.168), and Nevada (1.103). The three with the least were South Carolina (.494), Mississippi (.495), and West Virginia (.500). If we rank by the coefficient of variation (or the standard deviation divided by the mean), the three states with the most growth were Wyoming (.420), Connecticut (.387), and New York (.376), and the three with the least were New Mexico (.229), West Virginia (.229), and Mississippi (.230). As one might expect because the increase in the most substantial incomes had the greatest effect on the growth in inequality, the largest expansion in this outcome occurred in the most affluent states.

TABLE 1
EXPECTED SIGNS, MEANS, AND STANDARD DEVIATIONS

Variable	Expected Sign	Mean	SD	Min	Max	Range
Income inequality (Theil)		.74	.20	.35	1.63	1.28
1 if Republican governor	+	.47	.50	.00	1.00	1.00
% Republicans state legislature	+	44.09	17.12	1.91	89.29	87.38
1 if presidential election	+	.24	.43	.00	1.00	1.00
1 if Republican president	+	.61	.49	.00	1.00	1.00
% in unions	−	14.49	6.91	2.30	38.30	36.00
% employed in finance	+	5.59	1.29	3.25	12.41	9.16
% black	+	10.01	9.41	.22	37.21	36.99
% Hispanic	+	6.86	8.50	.40	46.44	46.04
Real personal income per capita	−	28.26	6.81	14.69	52.58	37.90
% 4 years+ of college	+	22.25	5.70	10.04	40.40	30.36
Density	−	168.74	224.10	.68	1,134.12	1,133.43
% below poverty line	+	.13	.39	.29	.27	.24
% employed in rural occupation	+	.72	.68	.01	3.64	3.62
% women in labor market	−	58.55	5.21	38.30	71.20	32.90
% employed in manufacturing	−	14.79	6.93	2.20	34.67	32.47
S&P 500 stock value	+	680.41	461.17	105.42	1,479.83	1,374.41
% unemployed	+	6.07	2.14	2.30	18.00	15.70
Variance marginal tax rate	−	.01	.01	.00	.04	.04

NOTE.—$N = 1{,}615$; variables are not logged.

ufacturing employment, the S&P stock value index, and the two variables that capture joblessness.

The results in model 1 show that except for the Republican governor measure, all political variables and union strength are significant predictors of state income inequality with signs in the expected direction. Because the dependent variable is logged, the coefficients on the three dummy variables show the percentage change in the dependent variable that is attributable to these explanatory variables. For example, the coefficient on the Republican president variable suggests that the policies enacted by these three administrations (the Reagan, George H. W. Bush, and George W. Bush presidencies) led to an 11.6% growth in inequality. When both the dependent and continuous explanatory variables are in log form, the coefficient on such explanatory variables can be interpreted as a percentage as well. For example, these findings suggest that if Republican strength in a state legislature grew by 10%, inequality in that state would rise by almost 1.8%. While within-state union strength is significant, it is less influential than the political effects. These initial findings suggest that the national political variables have a substantial influence on income inequality, but we need to enter additional controls.

Political variables continue to explain inequality in model 2. This model suggests that a growth in financial employment along with increases in mi-

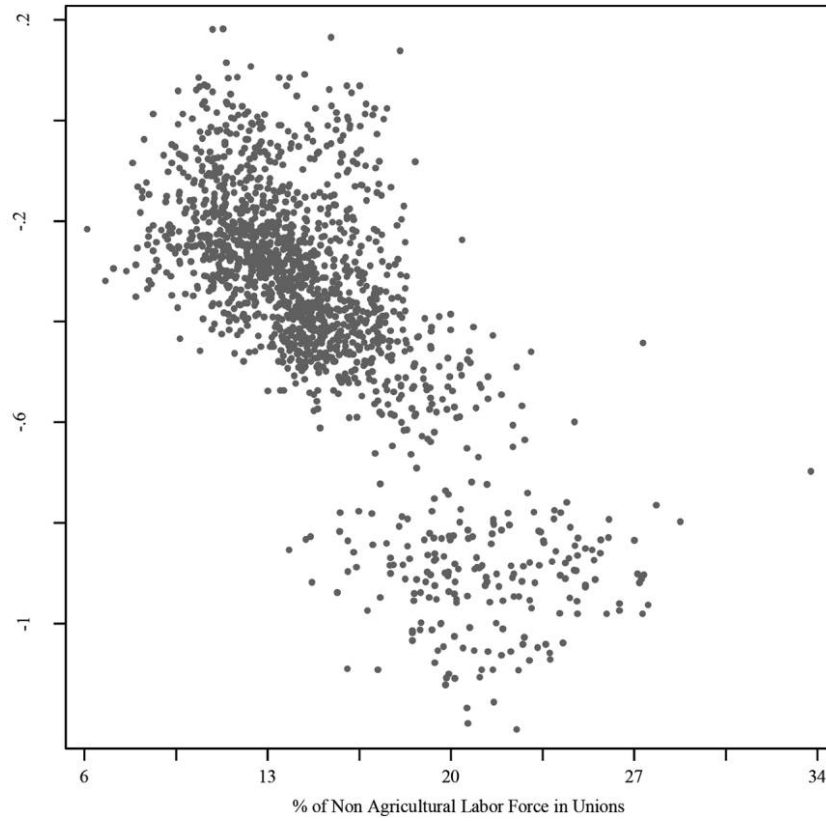

FIG. 1.—Association between union strength and income inequality

nority presence led to greater inequality, but economic development reduced this outcome. These results show that the positive effects of race diminished over time. The same political, union, financial, and minority findings persist in model 3, but we now find that reduced employment in manufacturing led to greater inequality. Higher stock values and college education rates again have positive effects, but the positive relationship between higher education and inequality also diminishes as time passes. Women's labor market participation, rural employment, and joblessness, however, have no explanatory power.

These preliminary results support the political resource theory that provided the primary impetus for this research, especially because the national political variables have such potent effects. Although they are significant, within-state union strength and state political variables are less influential. Such results are plausible because in contrast to the federal government, states have less control over critical economic policies.

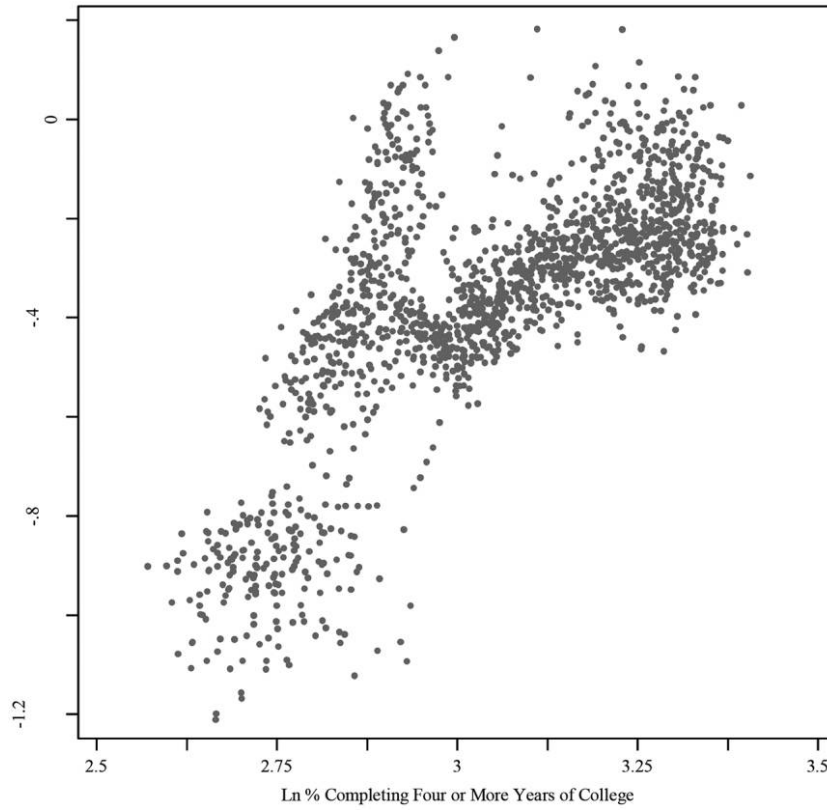

FIG. 2.—Association between college completion rates and income inequality

In model 4 in table 2 we retain the explanatory variables entered in model 3, but we now discover whether presidential effects vary by president. Instead of using one indicator to capture the combined influence of all three Republican presidents, we now code Reagan's term with one dummy variable, and we add a second dummy variable coded 1 for the two Bush presidencies.[17] Population density and its square along with the percentage below the poverty line are included as well. Model 5 retains these two presidential dummy variables along with the variables entered in model 3. In this model, however, we enter the marginal tax dispersion measure to control for changes in the

[17] There are important reasons for using two dummy variables and not forcing the coefficients on Reagan and the other Republican presidents to be identical. If we enter three dummy variables each coded for a different Republican president's years in office, models (not shown) have less explanatory power, and they do not pass the Ramsey-RESET test that detects specification errors.

TABLE 2
POOLED TIME-SERIES FIXED-EFFECTS ANALYSES OF THE DETERMINANTS OF STATE INCOME INEQUALITY

	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
Explanatory Variable	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
1 if Republican governor$_{t-2}$	−.01	.01	−.01	.01	−.01	.01	−.01	.01	−.01	.01	−.01	.01
ln % Republicans in legislature$_{t-2}$	.18***	.03	.18***	.03	.14***	.03	.06**	.02	.06*	.02	.08**	.03
1 if presidential election$_{t-1}$	−.08***	.01	−.07***	.01	−.08***	.01	−.08***	.01	−.07***	.01	−.06***	.01
1 if Republican president$_{t-2}$	.12***	.01	.09***	.01	.10***	.01	.22***	.01	.18***	.01	.18***	.01
1 if Bush 1 or Bush 2 president$_{t-2}$	...	...	...	...	...	...	.02*	.01	.00	.01	−.00	.01
ln % in union$_{t-1}$	−.18***	.04	−.15***	.04	−.16***	.03	−.08*	.03	−.09***	.03	−.09*	.04
ln % employed in finance$_{t-1}$	...	...	.58***	.09	.43***	.09	.20*	.08	.09	.08	.20*	.08
ln % black$_{t-1}$	...	...	1.29***	.32	1.41***	.30	.96***	.26	.81**	.26	.97***	.26
ln % black$_{t-1}$ × year	...	...	−.04***	.01	−.05***	.01	−.03**	.01	−.02*	.01	−.03**	.01
% Hispanic$_{t-1}$	...	...	.06***	.02	.05***	.02	.05***	.01	.07***	.01	.06***	.02
Real personal income per capita$_{t-1}$	...	...	−.40**	.15	−.56***	.17	.00	.15	.15	.15	.06	.16
Real personal income per capita$^2_{t-1}$	...	...	.06**	.02	.06**	.02	−.01	.02	−.04	.02	−.03	.02
ln % 4 years+ college$_{t-1}$	...	...	...	...	5.03***	.48	1.93***	.45	1.38**	.44	1.14*	.49
ln % 4 years+ college$_{t-1}$ × year	...	...	...	...	−.16***	.01	−.06***	.01	−.04**	.01	−.03*	.01

Density$_{t-1}$	...	...	...	...	...	...	−1.38***	.33	−1.09***	.33	−1.52***	.37
Density$^2_{t-1}$	...	...	...	...	...	...	.07***	.02	.06**	.02	.08***	.02
% below poverty line$_{t-1}$	...	...	...	...	...	...	.49**	.18	.46*	.18	.65**	.21
ln % employed in rural occupation$_{t-1}$	...	...	...	...	−.00	.03	.01	.02	.02	.02	.02	.02
% women in labor market$_{t-1}$	...	...	...	...	−.00	.00	−.00	.00	−.00	.00	−.00	.00
ln % employed in manufacturing$_{t-1}$	...	...	...	...	−.30***	.09	−.30***	.08	−.32***	.08	−.33***	.08
ln S&P 500 stock value$_{t-1}$	...	...	...	...	.17***	.03	.08***	.02	.09***	.02	.01***	.03
ln % unemployed$_{t-1}$	...	...	...	...	.04	.05	.10*	.04	.12**	.04	.08	.05
ln % unemployed$_{t-1}$ × year	...	...	...	...	−.004*	.00	−.01***	.00	−.01	.00	−.01***	.00
Variance marginal tax rates	...	...	...	...	...	...	...	...	−6.61***	.84	−6.28***	.98
Intercept	−1.20***	.15	−3.79***	.51	−15.87***	1.25	−6.04***	1.28	−4.36***	1.28	−2.93*	1.43
R^2 (corrected)	.78***		.79***		.81***		.86***		.87***		.87***	

NOTE.—N = 1,615 state-years in models 1–5, N = 1,253 in model 6 without the 11 ex-Confederate states; robust SEs. All models include state-specific time trends and their square. To show coefficients, real personal income per capita and its square have been divided by 10,000, while density has been divided by 100.

* $P \leq .05$ (two-tailed tests).

** $P \leq .01$.

*** $P \leq .001$.

progressivity of the federal tax codes. In model 6 we retain the variables in model 5, but we drop the 11 ex-Confederate states to determine whether the results depend on these singular jurisdictions.

After we recode the presidential variables, all prior findings hold in model 4, but economic development no longer is significant. This change in how presidential administrations are coded produces a considerable increase in model explanatory power. There is an even more important result, however: model 4 and the other models that employ two dummy variables to capture presidential effects now pass the Ramsey-RESET test. This test—which determines whether omitted variables or other specification problems are present—suggests these difficulties are not distorting the findings. The findings in model 4 also show that shifts in population density and the percentage below the poverty line explain fluctuations in income inequality. After we enter the variance in marginal tax rates in model 5, this addition produces a single difference in the results: financial employment becomes nonsignificant. After we remove the 11 ex-Confederate states in model 6, all results persist except for the unemployment rate, which now has a nonsignificant effect on inequality.

We need to correct the estimates for serial correlation. In models 7 and 8 in table 3 we reestimate models 5 and 6 using a generalized least squares approach that corrects this disturbance with an AR(1) term. Although this estimator adjusts for heteroskedasticity in a different way, just one relationship changes. Financial employment—which had been significant in models 2, 3, 4, and 6 but nonsignificant in model 5—again becomes significant in model 8 with the expected positive sign.

If we compare the estimates of the amount of change in inequality attributable to a 10% change in significant continuous variables using the coefficients from model 5, the results suggest that college education rates had the greatest influence on inequality. The relevant point estimate indicates that if the percentage of state residents with four or more years of college grew by 10%, this shift would produce about a 13.8% increase in income inequality. The significant interaction term, however, suggests this effect diminished in later years. The same 10% expansion in a state's percentage of black residents would yield about an 8.1% expansion in inequality, but again the interaction effect suggests this relationship decreased in the later part of the sample. The estimates also suggest that a 10% upward movement in Hispanic presence would produce about a 7.1% expansion in inequality. A nonsignificant interaction with time (in models not shown) implies that this relationship remained constant throughout the analysis period.

The remaining within-state effects are less potent. The results suggest that a 10% reduction in manufacturing employment would yield a 3.15% growth in inequality, but the same percentage decrease in union strength

TABLE 3
Pooled Time-Series Generalized Least Squares Fixed-Effects Analyses of the Determinants of State Income Inequality with Corrections for Heteroskedasticity and Serial Correlation

Explanatory Variable	Model 7		Model 8	
	Coefficient	SE	Coefficient	SE
1 if Republican governor$_{t-2}$	−.01	.01	−.01	.01
ln % Republicans in legislature$_{t-2}$	.07***	.02	.09***	.02
1 if presidential election$_{t-1}$	−.07***	.01	−.07***	.01
1 if Reagan president$_{t-2}$	.24***	.01	.23***	.01
1 if Bush 1 or Bush 2 president$_{t-2}$	.02*	.01	.01	.01
ln % in union$_{t-1}$	−.11***	.03	−.11**	.04
ln % employed in finance$_{t-1}$	.03	.07	.17*	.08
ln % black$_{t-1}$	.76***	.23	.95***	.24
ln % black$_{t-1}$ × year	−.02**	.01	−.03***	.01
% Hispanic$_{t-1}$	.07***	.01	.05***	.01
Real personal income per capita$_{t-1}$	−.00	.14	−.13	.15
Real personal income per capita$^2_{t-1}$	−.02	.02	−.01	.02
ln % 4 years+ college$_{t-1}$	1.63***	.36	1.34***	.40
ln % 4 years+ college$_{t-1}$ × year	−.05***	.01	−.04**	.01
Density$_{t-1}$	−.93***	.28	−1.23***	.30
Density$^2_{t-1}$	.05**	.02	.06***	.02
% below poverty line$_{t-1}$	.59**	.19	.77***	.22
ln % employed in rural occupation$_{t-1}$	.03	.02	.03	.02
% women in labor market$_{t-1}$	−.00	.00	−.00	.00
ln % employed in manufacturing$_{t-1}$	−.33***	.07	−.36***	.07
ln S&P 500 stock value$_{t-1}$	.06**	.02	.07**	.03
ln % unemployed$_{t-1}$	.12***	.03	.07	.04
ln % unemployed$_{t-1}$ × year	−.01***	.00	−.01***	.00
Variance marginal tax rates	−1.97**	.76	−1.51	.87
Intercept	−5.92***	1.10	−5.36***	1.21

Note.—N = 1,615 state-years in model 7 and 1,253 in model 8 without the 11 ex-Confederate states; robust SEs. All models include state-specific time trends and their square. To show coefficients, real personal income per capita and its square have been divided by 10,000, while density has been divided by 100. The Stata FGLS routine used to estimate these models does not supply an R^2.

* $P \leq .05$ (two-tailed tests).

** $P \leq .01$.

*** $P \leq .001$.

would enhance inequality by almost 1%. The findings also indicate that state governments have less influence than the federal government. For example, the coefficient on Republican proportions in the legislatures suggests that a 10% growth in Republican strength would produce about a .56% increase in inequality. The coefficient on Reagan's presidency indicates that after many other effects are held constant, this administration's neoliberal policies led to an 18% increase in inequality. These two coefficients, of course, are not directly comparable (owing to differences in the periods covered by

these dissimilar variables). Yet such different magnitudes imply that national policies have stronger effects on income inequality.

Alternative accounts and diagnostic test results.—Other measures of economic development such as real median incomes or real per capita gross state product are nonsignificant. The findings persist when measures such as state population and percentage urban are entered in additional models not shown, while trade and foreign direct investment measures have no discernible effects. Following results reported by Jacobs and Myers (2014), we find that Republican strength in Congress never matters. Theoretically plausible interactions and other quadratic specifications, moreover, have no influence.[18]

The FGLS estimator we use can only correct cross-sectional serial correlation when panels are perfectly balanced. If we drop the first year to create a completely balanced panel with no missing values, the theoretical implications are identical. Such results suggest that cross-sectional serial correlation does not affect these findings. Panel unit root tests show that this disturbance is not present. As long as explanatory variables are lagged and serial correlation is corrected, simultaneity will not alter the results in properly specified models. The results of the Ramsey-RESET test for specification errors support suppositions that these models are correctly specified. The extensive controls that include state-specific time trends and their square in fixed-effects models, the many factors held constant, the random cloud of points when the residuals are graphed on predicted values, and the substantial model explanatory power all suggest that these findings are accurate.

DISCUSSION

Although the primary theoretical impetus for this study involved a focus on political explanations, other accounts help explain the growth in inequality after the late 1970s. The results indicate that inequality in the states increased after increases in four-year college completion rates. In light of the attention given to the skill-biased technical change story about the influence of higher education on inequality (Card and DiNardo 2002; Goldin and Katz 2008; Liu and Grusky 2013), it is interesting that the interaction between this variable and time suggests that this powerful influence was most important during the initial years in our analysis, but its effects declined after the early 1980s. The relative strength of this effect, when it is compared to the weaker influence of the other variables, nevertheless justifies the considerable atten-

[18] Model explanatory power is not affected much by state fixed effects. If we enter 49 state-specific dummy variables in a regression model otherwise equivalent to model 5 (which is estimated with areg), the R^2 corrected for degrees of freedom is .8651, or a value identical to the same R^2 reported for model 5. If we drop the 49 state-specific dummy variables in this regression, the R^2 corrected for degrees of freedom falls to .8561.

tion given to this account.[19] Higher education rates initially were a potent influence on inequality, although this effect diminished later.

Other effects stressed in the literature are less influential. The financialization indicator we use explains income inequality in five of eight models. While the coefficients on this variable are nonsignificant in the most comprehensive models that include a correction for tax progressivity (see models 5 and 7), financialization remains significant in the two models that exclude the ex-Confederate states. These inconsistent results probably are attributable to the less than ideal employment measure we were forced to use. The conventional and probably superior way to assess this determinant is with profits in finance rather than employment in these activities, but we have not found data on state-level financial profits.

Despite the emphasis in Kuznets (1953, 1955) and by sociologists who stress nonpolitical explanations for inequality, neither state economic development nor the degree to which states were dominated by rural occupations has explanatory power in the more exhaustive models (see models 4–8). Such results support Piketty's (2014) skepticism (see Atkinson 2015 as well) about the Kuznets J-curve thesis on the effects of economic development. Female participation in the labor market is nonsignificant. Unemployment has mixed effects, as this variable is significant in three of six models.

Employment in manufacturing, however, mattered in all of the six models that included this determinant. Manufacturing employment has a fairly strong relationship with inequality, as the point estimates suggest that a 10% growth in this employment would produce about a 3.1% reduction in inequality. While the fluctuations in stock values explain inequality, stock values have weaker effects, as the results show that a 10% change in this indicator leads to slightly less than a 1% change in inequality. Both minority presence indicators, however, have potent effects on inequality, although neither is as influential as the higher education effect.[20]

[19] While we are skeptical about the common practice in the labor economics literature of using postsecondary education as a measure of skills that receive high rewards in the marketplace, we acknowledge that such effects rest on both the demand and supply of these skills (Goldin and Katz 2008), so our education indicator is an imperfect measure of this account. A systematic analysis of supply and demand, however, is well beyond the scope of this study.

[20] The literature suggests that trends in differences in the earnings of blacks and whites or between women and men do not account for the acceleration in economic inequality since the 1970s. These between-group earnings differences have been narrowing, although the reductions in earnings differences between the genders or between the races have slowed (Blau and Kahn 2007; Leicht 2008). In contrast to these modestly egalitarian trends, earnings differences within each gender or racial group (e.g., between black males or between black females or between white women) have expanded (Leicht 2008). Since women and blacks continue to be paid less, findings that shifts in the presence of these lower-paid groups help explain changes in inequality nevertheless are plausible.

The results suggest that the reduction in union strength led to increased income inequality in the states, but the point estimates indicate that this variable was less effective than other determinants. This modest relationship is plausible because almost all of the years in our analysis involved the period after the Reagan administration weakened unions (Tope and Jacobs 2009). Jacobs and Myers (2014) show that although union strength had negative effects on income inequality before Reagan's presidency, this determinant had no influence on inequality during and after the Reagan years. Remember as well that this analysis focuses mostly on the gap between citizens with extremely high incomes and all other recipients. It seems likely that shifts in union strength are less likely to influence this gap. The period-specific effects found by Jacobs and Myers along with the high income gap we analyze here probably explain why union strength had just a modest negative influence on income inequality in this investigation.

We are more interested in political effects. With but one exception the findings support this focus, but the presence of a Republican governor never is significant in any model. Although the coefficients on Republican strength in the state legislatures reach this threshold in all models, the results suggest that this positive influence on inequality was less potent than the effects of national Republican administrations. The strength and consistency of the coefficients on Reagan's presidency after so many other factors are held constant provide clear evidence that the neoliberal departure orchestrated by this administration was an essential determinant of the post-1980 acceleration in inequality.

This administration made the tax codes increasingly favorable to the most affluent (Bartels 2008; Volscho and Kelly 2012; Atkinson 2015). Reagan and his political allies deregulated many industries including finance (Lin and Tomaskovic-Devey 2013); the Reagan administration significantly weakened unions (Tope and Jacobs 2009) and reduced spending on compensatory programs (Phillips 1991). Reagan and his allies undermined other policies designed to protect the least affluent from market instabilities (Phillips 1991; Hacker and Pierson 2010*a*). These policies almost certainly help explain why our findings and those reported by Jacobs and Myers (2014) indicate that Reagan's neoliberalism was a forceful determinant of the sharp acceleration in U.S. inequality that began soon after he took office.[21]

Our findings therefore support the four hypotheses that provided the principal motivation for this research: First, with many alternative determinants held constant, the results always suggest that the Reagan neoliberal

[21] We follow Jacobs and Myers (2014) who did not attempt to estimate the effects of specific policies that may alter income inequality. The independent effects of the many narrowly targeted neoliberal policies that together accumulated to produce growth in such an all-encompassing outcome like aggregate inequality are difficult to detect (Bonica et al.

policies were influential determinants of the post-1980 growth in inequality. Second, the findings support a claim that expansions in neoliberal political strength in the states led to greater income inequality, but this account did not prove to be as empirically potent as the prior hypothesis about national politics. These results also show that the reduction in union strength within the states led to increased income inequality. And finally, as one might expect from the explanatory power of national politics, the findings always show that income inequality was reduced in the last year of presidential terms probably to maximize the vote for either the incumbent or the nominee from the incumbent's party.

Wider Implications

How do our findings compare with those from other investigations that assessed the political factors that affected economic inequality? Some studies examine the presence of Democratic rather than Republican presidents and find negligible presidential effects. Yet our results, and those reported by Jacobs and Myers (2014), show that especially after 1980, Republican administrations had strong positive effects on U.S. inequality. Such findings should not be surprising as cross-national research shows that when the absolute effects of Right versus Left parties are compared, parties on the Right have more substantial positive effects on inequality than Left parties' negative effects (Brady and Leicht 2008). The Brady and Leicht results and ours suggest that researchers who code for the presence of Democratic presidents and ignore Reagan's presidency will be less likely to find that national neoliberal administrations matter despite clear evidence for this relationship.

This coding is mistaken partly because the mostly moderate officeholders in the Democratic party became heavily reliant on Wall Street and other business contributions (Hacker and Pierson 2010*a*), especially after they lost their support from unions—which, of course, were drastically weakened especially in the 1980s (Dark 1999; Tope and Jacobs 2009).[22] Perhaps for this

2013). Perhaps none of these narrowly focused policies would be sufficiently influential by itself to have statistically perceptible independent effects on such a wide-ranging outcome. In any event, to focus on just one or a few specific policies while ignoring others that are at least as important but are difficult or impossible to measure would produce biased estimates.

[22] Phillips (1991, p. 32) quotes the Republican campaign operative Lee Atwater, who spoke in 1988 on this change in the Democratic party: "Traditionally the Republican party has been elitist, but one of the things that has happened is that the Democratic party has become the party of elites." For example, Bendavid (2007) describes how much Rahm Emanuel, who successfully chaired the Democratic Congressional Campaign committee in 2006, stressed money. "Most of Rahm's time was spent meeting with wealthy lawyers or financiers . . . even as Nancy Pelosi and other Democrats were also raising enormous

reason, many of the moderate Democratic officeholders in this Center Left party did not actively resist Republican neoliberal policies that advantaged the affluent and led to the acceleration in income inequality (Phillips 1991; Akard 1992; Hacker and Pierson 2010*a*).

If the first two Democratic presidents after the 1960s (Carter and Clinton) mostly straddled this issue or halfheartedly supported some Republican neoliberal policies, it would be even more unlikely that these ideologically moderate Democratic administrations would have had any effects on income inequality. But certainly after 1980 the evidence in this study and the results reported by Jacobs and Myers suggest that the intensely neoliberal Reagan administration successfully endorsed many political outcomes that led to the substantial acceleration in economic inequality. It therefore is vital to code presidential effects correctly.

What about the contrasts between our findings and those in Jacobs and Myers (2014)? Jacobs and Myers analyzed time-series data that began in 1951 and ended in 2010. This long interval meant that Jacobs and Myers could detect relationships that differed significantly between non-neoliberal and neoliberal political eras. Using period-specific interactions to capture contrasts in the effects of these dissimilar political regimes, Jacobs and Myers find that any increases in union strength would have reduced inequality, but only before the Reagan administration weakened unions (Dark 1999). After the Reagan neoliberal departure, these findings show that union strength had no effect on inequality. And their results show that the comparatively moderate Republican presidents before Reagan did not influence inequality. Soon after Reagan entered the White House and instituted many potent neoliberal policies, however, income inequality accelerated.

Yet the Jacobs and Myers time-series approach had costs. An analysis based on 60 cases will have limited statistical power such that weaker but still influential determinants are less likely to be statistically significant. The present analysis, however, is based on 1,615 state-years. This greater statistical power means that we can evaluate additional explanatory factors. And a pooled time-series design means that we can estimate with fixed effects. These advantages provide greater protection against omitted variable bias, and they make our results less vulnerable to type 2 errors. But we cannot detect statistically significant contrasts in the effects of neoliberal administrations and their more moderate counterparts. The shorter analysis period in this study will not let us detect such contrasts because sufficient years before the Reagan or after the last Bush presidency are not present in these state data.

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sums" (pp. 156–57). Emanuel told the campaign staff, "The first third of your campaign is money, money, money. The second third is money, money, and press. And the last third is votes, press, and money" (p. 157).

The more important point, however, is that strong neoliberal political effects on income inequality appear in both studies despite their dissimilar research designs. In contrast to the results in the Jacobs and Myers investigation, additional factors such as shifts in higher education and minority presence along with the reduction of employment in manufacturing had influential effects on inequality in this study.

Yet probably our most theoretically important results concern fluctuations in the distribution of political resources. The mutually supportive findings in this investigation and in the Jacobs and Myers study illustrate the explanatory power of Korpi's power resource perspective when income inequality is at issue. Both investigations show that increased differences between management's and labor's political resources combine to produce a growth in income inequality.

Politically based expansions in the power resources of employers—during a time when significant reductions in labor's primary political resource take place—therefore provide an influential explanation for the escalation in economic inequality. Other research, moreover, shows that the decrease in union strength that reduced a core labor political resource was at least partly attributable to the same neoliberal political departure that began in 1981 (Tope and Jacobs 2009). Probably the most important conclusion from this research is that when intensely neoliberal national administrations that are sympathetic to employers and unfavorable to labor take office, income inequality is likely to expand by nontrivial amounts.

As Polanyi (1941), Piketty (2014), and many others have pointed out, markets that seem to be completely unregulated in reality are closely controlled by political regimes. In addition to many other critical influences, political choices about taxes help decide the substantial contrasts in the rewards of the haves and have-nots. Governments determine access to the courts and the disputants who are advantaged after this access is granted (Galanter 1974). Governments regulate labor-management relations along with personnel policies, and they decide how enterprises are administered. Governments establish rules that control financial and other transactions while supplying the public goods essential for economic activity.

Because these and other political decisions combine to determine differences in the rewards of winners and losers, researchers who seek to explain changes in economic inequality ignore politics at their peril. As Piketty suggests (2014, p. 577), research based on a political economic approach is indispensable if we are to fully understand the forces that shape economic inequality. This momentous change in the distribution of economic rewards deserves greater attention in sociology, but it cannot be accurately explained from only a technological perspective.

APPENDIX

TABLE A1

DATA SOURCES

Variable	Source
Theil measure of income inequality based on IRS data	Frank (2014)
Republican governor and % Republicans in the legislature	Klarner (2011)
Republican presidents and presidential elections	Coded from historical knowledge
Union strength	The primary union membership source is Troy and Sheflin (1985); data after 1985 come from the *US Statistical Abstract*, various years
% employed in finance	Bureau of Labor Statistics, U.S. Department of Labor
% black and % Hispanic	Population estimates, U.S. Census Bureau
Real per capita income	Current Population Survey, annual social and economic supplements, U.S. Census Bureau
College attendance rate	Statistical abstracts, various years
Employment in rural occupations	Bureau of Labor Statistics, U.S. Department of Labor
% women in the labor market	Bureau of Labor Statistics, U.S. Department of Labor
% employed in manufacturing	Bureau of Labor Statistics, U.S. Department of Labor
S&P stock value index	Bloomberg financial information
% unemployed	Bureau of Labor Statistics, U.S. Department of Labor
Marginal tax rates	Statistics provided by Stata (as found in Stata vers. 13 reference chapter on the command "egen"); see Schmidt (1993) for information on how this statistic was computed
Density	U.S. Geological Survey and population estimates, U.S. Census Bureau
Poverty	Population estimates, U.S. Census Bureau

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