Institutions, Partisanship, and Inequality in the Long Run

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Abstract

It has been widely suggested by political scientists that institutions like centralized wage bargaining and factors like government partisanship are correlated with differences in income inequality between advanced industrial countries. There is empirical evidence for the period since 1970 to support each of these propositions. We make use of new data on top income shares to examine the effects of partisanship and wage bargaining over a much longer time period, nearly the entire twentieth century. Our empirical results provide little support for the idea that either of these two factors is correlated with income inequality over this period. We then show that a closer look at the introduction of centralized wage bargaining in individual countries during the 1930s and 1940s reveals that in countries that moved to centralize wage bargaining, income inequality was already trending downward well before the institutional change, and the move to centralized bargaining did not alter this trend. Our results suggest that there were alternative institutional paths to reduced income inequality during most of the twentieth century. This raises the possibility that commonly shared economic and political events, such as world wars and economic crises, may ultimately be more important for understanding the evolution of income inequality than are the institutional or partisan characteristics commonly thought to be decisive.

1 Introduction

Political scientists have a long-standing interest in examining cross-country differences in income inequality. Recent quantitative studies have added to this tradition by examining whether institutions like centralized wage bargaining and political factors like government partisanship are correlated with differences in income inequality between advanced industrial countries. Wage bargaining centralization, it is suggested, can affect the pre-tax distribution of income by compressing wage differentials. Factors like partisanship may influence the pre-tax income distribution in a dynamic fashion if governments of the left show a greater proclivity than governments of the right to pursue progressive income taxation, wealth taxation, and subsidies for goods like education. These policies may reduce future income inequality. To date, cross-country quantitative studies of income inequality and its political correlates have focused on the period since the beginning of the 1970s. There are two prominent reasons for this choice. First, the fact that income inequality has risen in some OECD countries but not others is of obvious substantive importance. Scholars have been interested in the question whether the high levels of inequality observed in the United States in recent years can be explained by the absence of centralized wage setting institutions and by the weakness of the political left when compared with trends in a number of continental European countries. A second reason for the focus of existing work is that even if scholars wanted to examine the political determinants of inequality over a longer time span, they have lacked the data to do so. The earliest data in the OECD's database of earnings dispersion, which has been used recently by Golden and Wallerstein (2006) and Rueda and Pontusson (2000), is from 1973. The earliest data from the Luxembourg Income Study, which has been used by Kenworthy and Pontusson (2005), dates from 1979.[1]

In this paper we make use of new data on top incomes, collected by a series of authors for a volume edited by Atkinson and Piketty (forthcoming). We also make use of a smaller number of long-run series on wage inequality. When combined with existing data on political

[1] For other recent studies that consider the relevance of government partisanship or labor market institutions for income inequality see Freeman (2007), Moene and Wallerstein (2002), Wallerstein (1999), Golden and Wallerstein (2006a), Alderson and Nielsen (2002), Pontusson, Rueda, and Way (2002), Kenworthy (2007), OECD (2004), and the reanalysis of the findings in Wallerstein (1999) by Golden and Londregan (2006).

institutions, as well as political data that we ourselves have coded, the top incomes data allows us to investigate whether institutions like centralized wage bargaining are correlated with levels of income inequality for periods prior to the 1970s. We also consider the effect of government partisanship over the long run. One reason for considering a longer time span is that data on top income shares over the course of the twentieth century suggest that there has been significantly more variation within countries over time than there has been between countries. If we are going to suggest that certain partisan and institutional factors explain the current difference in inequality between two countries such as the United States and Sweden, to provide one example, then these same factors ought to be able to account for variation both within and between these two countries. These factors in particular ought to be able to account for the fact that in terms of income inequality, the US and Sweden were actually much less different from each other in the 1950s than they are today. In addition, by considering a longer time span we are able to examine whether within-country changes in institutions like wage bargaining centralization have been associated with changes in inequality. Ideally, if a country adopted an institution like centralized wage bargaining in the 1930s or 1940s we would like to know not only whether the presence of this institution was associated with low inequality in the 1980s or 1990s, but also whether the initial introduction of the institution appears to have had a significant impact. If this is not the case then the existing institutional explanation may need to be revisited.

Our empirical results suggest that left government and centralized wage bargaining appear to have little causal effect on income inequality over the long-run. This raises several possibilities that will require further elaboration. First, it may be that long-run changes in income inequality are driven primarily by broad economic trends involving the "race between technology and education" and that political factors are ultimately of secondary importance.[2] A second possibility is that political factors do matter but not in the way that is often suggested. Rather than focussing on the formal centralization or decentralization of bargaining arrangements in two countries like the US and Sweden, it may be that a closer investigation would show that despite their formal institutional differences, within the US and Swedish labor markets during

[2] The original use of the phrase is from Tinbergen (1975) and has recently been used by Goldin and Katz (2007).

the 1950s similar norms or beliefs prevailed regarding acceptable levels of pay inequality. This conclusion is reinforced by the recent intriguing analysis of the US post-war experience with income inequality by Levy and Temin (2007). A third, complementary possibility is that politics matters for inequality, but it may be that the most prominent political effects on inequality are generated by cataclysmic events like wars rather than incremental changes in institutions.[3]

The remainder of this paper proceeds as follows. Section 2 introduces the top income shares data we will use and highlights the potential questions it raises for comparative political economy. We present only a very brief overview to aid in interpreting our statistical tests, and those looking for a full presentation of the top incomes data should consult Atkinson and Piketty (forthcoming) or the survey papers Piketty (2005), and Piketty and Saez (2006). Section 3 then considers two hypotheses regarding how partisanship and institutions may influence inequality. The first hypothesis is that governments of the left should be associated with lower income inequality given existing claims that left governments are more inclined to pursue policies favorable to low income constituencies. The second hypothesis is that centralized wage bargaining arrangements should be expected to also reduce inequality by compressing pay differentials between firms and sectors, as well as for different levels of a hierarchy within a firm. We also discuss a related argument that trade unionism may reduce inequality.

In section 4 we report the results of our empirical tests. These consider the correlation of centralized wage bargaining and government partisanship with three separate top incomes measures for the period 1916-2000. We also perform regressions for two separate sub-periods: 1916-1975 and 1976-2000. In our regressions we see no evidence of a statistically significant effect of left government on income inequality apart from with regard to very top incomes (the top 1%). Moreover, the substantive magnitude of this latter effect is very small, implying that it could account for only a small fraction of the large changes in income inequality that have occurred over time during the course of the twentieth century. For centralized wage bargaining we distinguish between the effect of three different types of wage bargaining (1) decentralized,

[3] The potential effects of war on inequality have been emphasized both by those who point to economic effects involving destruction of fortunes (Piketty, 2003,2001; Piketty and Saez, 2006, 2003), as well as by those who emphasize the political repercussions of wars (Gourevitch, 1986; Rogowski and MacRae, 2004).

meaning wages are set at the firm level or in the absence of collective bargaining, (2) sectoral level wage bargaining, and (3) centralized wage bargaining, meaning that wages are set at the peak or national level. Existing scholarship has placed the greatest emphasis on the effect of this third type of wage bargaining on inequality, with one reason being that peak level wage setting can be associated with the adoption of "solidaristic pay" policies. We find evidence that decentralized wage bargaining has been associated with higher levels of inequality than has sectoral wage bargaining, but this effect is driven almost entirely by events of the last three decades. During the first three-quarters of the twentieth century there is no evidence of a difference between decentralized and sectoral wage bargaining. However, given previous scholarship the most surprising part of our results is the lack of any evidence of an association of centralized, national level wage bargaining with income inequality. The conclusion applies with or without the inclusion of country fixed effects in our regressions, and it is applies across a number of different alternative specifications.

Our results regarding the absence of an effect of fully centralized (peak level) wage bargaining on inequality are surprising given the emphasis that has been placed on this factor in previous work. In Section 5 we extend our analysis by using individual country time series on both top income shares and wage inequality to take a closer look at whether the adoption of centralized wage bargaining in several countries during the 1930s and 1940s was associated with a downward structural break in income inequality. One would expect this if the idea of a causal effect of centralization on inequality is to be believed. The results for the four countries we consider that adopted centralized wage bargaining during this period are striking. In each of the four cases (Sweden, Denmark, the Netherlands, and Ireland) income inequality trended downwards after the move to centralized bargaining, but it had already been trending downwards well before this institutional change, and based on individual country regressions, we see little evidence that this trend was accentuated following the change. In addition, we show that in the immediate post-war period the trend in inequality in a country like Sweden, which moved to centralized bargaining, was very similar to that observed in the United States where the formal institutions of bargaining remained decentralized.

As noted above, our empirical results call for a reconsideration of existing conclusions regarding the effect of government partisanship and centralized wage bargaining on income inequality. In Section 6 we develop this point further by considering potential explanations for our results based on underlying economic or political processes. Finally, Section 7 concludes.

2 Data on Top Income Shares

The data we use to measure income inequality has been collected as part of a project that uses information from income tax returns to calculate the percentage of total pre-tax income earned by those at the top end of the income distribution in each country.[4] A number of papers on individual countries have already been published using this method for measuring income inequality.[5] In our analysis we make use of the standardized dataset compiled by Andrew Leigh (2007) that uses a number of adjustments to correct (to the extent possible) for heterogeneity in measurement that might otherwise influence inferences about the evolution of top income shares.[6] The idea of using tax data to measure income inequality in fact picks up on the method used by Kuznets (1953). This new data on income inequality has several advantages over existing measures of income inequality based upon household surveys. First, it results in inequality measures that are more homogeneous across countries, even though it should be emphasized that these top income measures are not completely homogenous due to differences in some countries in the unit for taxation (individual vs. household) or in the exact definition of what constitutes income. Second, the top income shares data provides us with a much longer run view of the evolution of income inequality in different countries when compared with data from the OECD or Luxembourg Income Study databases, or from the frequently used Deininger-Squire database.

[4] See Atkinson and Piketty (forthcoming), Piketty (2005), and Piketty and Saez (2006). This section draws heavily on these pieces.

[5] The full list of original papers is as follows: Australia (Atkinson and Leigh, 2007) France (Piketty, 2001, 2003) Germany (Dell, 2005) Ireland (Nolan, 2005) Japan (Moriguchi and Saez, 2006) Netherlands (Atkinson and Salverda, 2003) New Zealand (Atkinson and Leigh 2005) Spain (Alvaredo and Saez, 2006) Sweden (Roine and Waldenstrom, 2006), Switzerland (Dell 2005 and Dell, Piketty, and Saez (2007) United Kingdom (Atkinson, 2005) United States (Piketty and Saez, 2003),

[6] This includes in particular adjustments for the income unit, personal income total, income definition, dates for the tax year, and the age cutoff.

One constraint imposed by using tax data to measure inequality is that prior to World War II in most countries only a small fraction of households were subject to income taxation. This means that it is possible to use tax data to estimate the share of total income earned by the top 10% of households and by groups within the top 10% (top 1%, top 0.1%, etc.), but it does not allow one to produce a direct measure of developments at the bottom of the income distribution, such as the possibility of a growing gap between the bottom 10% and the median. This issue needs to be kept in mind during our subsequent analysis. However, below we will also present evidence to show that a measure based on the top incomes data which focuses on the income earned by those between the 90th and 99th percentiles of the distribution relative to those below is very highly correlated with existing data on earnings inequality. The correlation of top incomes measures with existing cross-country income inequality measures based on the gini coefficient has been explored extensively by Leigh (2007) who demonstrates that top incomes data presents a very good proxy for broader inequality measures.

Another constraint on our analysis is that while several papers from the top income shares project provide separate series for capital income and wage income, allowing one to more precisely test theories about the determinants of the two, we do not have separate capital and wage income series for all countries. As a result, for the moment we are restricted in this paper to investigating hypotheses about overall income inequality. However, since it is known that historically, only individuals at the very top of the distribution have derived income primarily from capital as opposed to labor income, when we use the top incomes measure that focuses on those in the 90th to 99th percentile relative to those below, we will in effect be controlling to a great extent for this problem.

Currently, data on top incomes is available for thirteen advanced industrial countries. Figure 1 presents data on the top 1% income share in a single graph that may be useful for identifying trends over time. Several things can immediately be noticed. First, as has been noted using numerous other data sources, the last thirty years have seen a significant increase in income inequality in the United States when compared with a number of continental European countries. What is distinct about the top income shares data, however, is that because it provides us with a view of inequality over a longer time horizon it also allows us to see the very considerable variation in levels of inequality that has occurred within countries over time. For the immediate post-war period, levels of inequality (as measured by the share earned by the top 1%) are remarkably similar across the different countries. When we consider broader measures, such as the top 10% income share changes over the course of the twentieth century are less dramatic than those for the top 1%, but they are still very significant. The reality of very significant changes in income inequality within countries over time is potentially troubling for comparative political economy explanations which suggest that certain relatively static features like electoral rules or wage bargaining arrangements account for differences in inequality. Iversen and Soskice (2005) suggest that there has been a striking degree of continuity in cross-national differences in patterns of income inequality and redistribution in advanced industrial countries over the last half century. As a result, they argue, it seems logical to think in terms of theories that focus on the political and historical factors that may have led countries to embark on a set path with respect to inequality. In fact, the top incomes data suggests that with regard to income inequality, there may actually be much more over time variation than has been previously recognized. If so, then it calls for broadening the inquiry to consider how political economy factors can account for both cross-country and over time variation.

If we move next to considering the pre-1945 period, we observe in Figure 1 that levels of inequality were strikingly higher in many countries, and in several countries the Second World War appears to have been associated with a drastic reduction in levels of income inequality. For most countries the magnitude of pre-1945 changes in top income shares makes later changes seem small in comparison. This observation poses a challenge for comparative political economy. It suggests that if variation in factors like partisanship and wage bargaining centralization can account for variation observed between countries since 1970, but not for the larger changes that occurred before 1945, then one may want to reconsider the overall importance of these factors. The observation of a drop in inequality associated with World War II in the United States fits with what Goldin and Margo (1992) have referred to as "the great compression" where factors like wartime planning led to a drastic compression of the wage differential between the 90th and 10th percentiles of the wage distribution.[7] Piketty and Saez (2003), Piketty (1998), and Moriguchi and Saez (2005) document that for countries like France, the United States, and Japan the pre-1945 reduction in inequality was also attributable to a drastic fall in capital income for top earners as fortunes were reduced as a result of wartime inflation and taxation. As Piketty and Saez (2006, 2003) have noted, however, temporary features such as wartime wage controls and shocks to capital income cannot explain why, after a large negative shock during World War II, top income shares did not rise again once wage controls were dropped and fortunes had a chance to reconstitute themselves. These authors suggest that significant increases in the progressivity of income taxation that took place in many countries after 1945 may explain why income inequality did not rise after 1945. This raises the question, however, of why the imposition of very high marginal tax rates was politically sustainable at this time. We will return to this issue below.

Overall, the very significant over-time variation we observe in top income shares over the course of the twentieth century poses important questions for comparative political economy. It remains to be established whether the type of theories that have proven effective for explaining cross-country differences in inequality as they have emerged since the 1970s are also effective at explaining cross-country differences during earlier periods, as well as the important variation in inequality that has occurred within countries over time.

3 Potential Political Determinants of Inequality

Our goal is to investigate whether there is empirical support over a longer time horizon for the proposition that centralized wage bargaining and left government are correlated with low levels of income inequality. In what follows, we discuss each theory and briefly suggest how we intend to test it. In the next section we then present our empirical specification and political data in greater detail, together with our estimation results. In addition to evaluating these political economy hypotheses, our empirical tests will also attempt to control for the principal factors that economic theory suggests may drive inequality trends. Economic theory emphasizes the

[7] Their conclusions have recently been revisited by Kopczuk and Saez (2007).

supply and demand for skills, and authors have argued that factors like skill-biased technical change, migration, levels of education, and openness to trade may be important determinants of inequality.[8]

3.1 Government Partisanship

It is widely suggested that when controlling government, political parties that situate themselves on the left of the political spectrum will adopt redistributive policies including greater progressivity in income and estate taxation, more significant transfers, and greater public subsidies for goods like education when compared with their counterparts on the political right.[9] In a dynamic context we should expect these redistributive policies to have effects on pre-tax income inequality. More progressive income and estate taxation will have an effect on the accumulation of wealth that is invested to produce income in subsequent periods.[10] Likewise, public subsidies for education can have an important effect on human capital accumulation and thus future income. As a result, if we expect that governments of the left will engage in more redistribution, then we should also logically expect countries in which parties of the political left dominate to have lower levels of pre-tax income inequality. While the above arguments follow a convincing logic, it is also entirely plausible that ultimately it may make relatively little difference for the income distribution whether a country is governed by a party that has historically been on the political left. The strong common trend in the top 1% income share across countries over the course of the twentieth century may be explained by the fact that parties historically on the left or right shift their policy positions to fit current circumstances, constraints, or societal opinions. So, for example, the Eisenhower administration in the United States made no major attemp to reduce top marginal tax rates which, by today's standards, seem extremely high.

[8] See Acemoglu (2003) for a recent review, and Goldin and Katz (2007) for the effect on inequality of the interaction between technology and education.

[9] A number of contributions identify a relationship between partisanship and spending on transfers. See Huber and Stephens (2001) for a comprehensive overview. See Boix (1998) for evidence on the relationship between partisanship and spending on public education. See Pontusson , Rueda, and Way (2002) for results with regard to left government and top marginal tax rates.

[10] If partisanship determines the progressivity of the tax system then it might also have a more immediate effect on the pre-tax income distribution via changes in labor supply. A cut in top tax rates may induce top earners to work more, increasing their share of total income. Saez (2004) finds that only those with very high initial incomes exhibited this behavioral response.

3.2 Labor Market Institutions

While government partisanship may influence the pre-tax income distribution via redistributive policies, certain labor market institutions may have a direct effect on pre-tax inequality. A number of scholars have presented theoretical models and empirical evidence to suggest that in countries where wage negotiations tend to be centralized there will be lower levels of wage dispersion. One of the most powerful statements of this argument is presented by Moene and Wallerstein (2002). Centralized bargaining arrangements can reduce the dispersion of pay between different firms (when bargaining occurs at the industry level), between different industries (when bargaining occurs at the national level), as well as between different categories of wage earners. In countries such as Sweden centralized bargaining was accompanied by explicit policies emphasizing "solidaristic pay", paying equal wages for equal work, in addition to reducing differentials between employees at different levels of an organization. Authors like Wallerstein (1999) and Rueda and Pontusson (2000) have found empirical evidence of a negative correlation between centralization of wage bargaining and pay inequality in OECD countries. Rueda and Pontusson use data beginning in the early 1970s. Wallerstein (1999) considers pay inequality data over the period 1980 to 1992.[11] The literature has also suggested that the presence of centralized bargaining arrangements for the majority of workers in a country may also have knock-on effects on wage developments for white collar and higher salaried employees, even if these employees do not officially participate in the centralized arrangement.[12] Though quantitative studies of the impact of centralized wage bargaining have generally focused on the relationship between bargaining centralization and growing pay inequality since the 1970s, in a number of countries centralized wage bargaining arrangements first emerged during the crises of the 1930s and 1940s.[13] If this is the case, and wage bargaining centralization has had a significant effect on reducing inequality, then we should also expect to observe a negative correlation between wage bargaining centralization and top income shares for earlier periods, and we should also expect to observe that the introduction of centralized bargaining led to a

[11] A reanalysis of the Wallerstein (1999) results by Golden and Londregan (2006) concludes that wage bargaining centralization has had a statistically significant but small effect on pay dispersion.

[12] See Swenson (1989) for a discussion of this issue with reference to Sweden.

[13] Katzenstein (1985), Swenson (1989), Gourevitch (1986).

structural break in inequality.

The most common dependent variable in empirical studies of the correlation between inequality and centralized wage bargaining has been the 90/10 ratio. This represents the ratio between the earnings of a worker at the 90th and 10th percentiles of the earnings distribution, and the most commonly used cross-national source for this data is the OECD earnings database. For our analysis, it is important to determine the extent to which the top income measures upon which we focus in this paper can provide an appropriate dependent variable for investigating the effect of wage bargaining arrangements. The top incomes measures combine capital and labor income. An even bigger potential problem is that while studies of wage bargaining focus on developments at the 90th percentile of the distribution and below, the top incomes data allows only for tracking movements at the 90th percentile and above. As one considers increasingly higher levels of the income distribution it becomes less plausible that an individual's income would be directly affected by the presence of a centralized wage bargaining arrangement. When we investigate these questions more closely, however, we actually observe that the top incomes data, and in particular the broader measures within the top incomes dataset, appear to provide a remarkably good proxy for the 90-10 earnings ratio.

A first way to show this is to compare data from the OECD earnings database with a top incomes measure that we constructed as follows. Take total income y_{90-99} earned by individuals (or tax units) between the 90th and 99th percentiles of the income distribution. Then divide this total by the total income earned by all individuals (or tax units) between the bottom and the 99th percentiles of the distribution to get the ratio $\frac{y_{90-99}}{y_{0-99}}$ or what we call for the rest of the paper the *Top10-1* share This measure excludes the effect of trends in very top incomes, and we also know that over time, capital income has been heavily concentrated in the top 1%. Figure 2 plots the 90-10 earnings ratio from the OECD database for four time periods (1980-84, 1985-89, 1990-94, 1995-99) against the *Top10-1* income measure for our thirteen sample countries. As one would suspect from visual inspection of the graph, the pairwise correlation between the two variables is extremely high (0.84).[14] Moreover, this correlation remains strong even if we

[14] The correlation remains very high (0.74) if we exclude all observations from the United States and Sweden as potential outliers.

subtract country mean values from each variable and then compare within country variation in the 90-10 earnings and *Top10-1* income measure. In this case the pairwise correlation is 0.66. This further strengthens the case for using the *Top10-1* income share as a proxy measure. It is also worth noting here that in the case of Sweden, where we do actually have have both the *Top10-1* measure and a long-run series on wage inequality covering the entire twentieth century, the correlation between these two series is also very high (0.87). This can be seen in Figure 3 and will be described in detail below. Overall, this evidence suggests that given the absence of cross-country data on the 90-10 earnings ratio for periods before the 1970s, we can nonetheless use the *Top10-1* income measure as a very good proxy. This is certainly not to argue that the *Top10-1* measure is a perfect proxy for earnings inequality, and in particular for movements lower down the income distribution. The study by Kopczuk and Saez (2007) for example has recently shown using data from the US Social Security Administration how the 90/10 and 50/10 earnings ratios have exhibited different trends since the 1930s. With this said, neglecting to use the top incomes data for this reason would mean ignoring an important opportunity to analyze the impact of centralized wage bargaining arrangements over the long-run.

Leaving aside the question whether the degree of centralization of wage bargaining matters for inequality, it has also been suggested that high union membership will have a significant effect of reducing pay dispersion. Unions are frequently associated with features such as standardization of pay. However union membership may itself be endogenous to other factors. Acemoglu, Aghion, and Violante (2001) suggest that union membership may lower wage dispersion, but union membership is itself driven by the returns to skills in an economy. If economic change leads to an increasing return to highly skilled labor, then highly skilled workers may refuse to join unions where wages are set in tandem with low-skilled or unskilled workers.

4 Income Inequality, 1916-2000

4.1 Empirical Specification

This section empirically evaluates the hypotheses that labor market institutions and government partisanship are important determinants of income inequality. We show that there is no evidence of a statistically significant effect of partisanship on inequality, apart from with regard to very top incomes (the top 1%), and this latter result is small in substantive terms. With regard to wage bargaining, we document that there is little evidence in our full time series of a robust correlation between centralized wage bargaining and income inequality, contrary to much of the existing literature. Specifically, there is no evidence that national peak-level centralized wage bargaining is associated with lower levels of inequality than is sectoral level bargaining. We do find in our full time series using data from 1916-2000 that decentralized wage setting is associated with greater income inequality.[15] We demonstrate, however, that this correlation is driven almost exclusively by events in the last quarter of the century, and it is absent in the 1916-1975 subsample. We also present evidence that there is a significant correlation between trade unionism and three measures of income inequality. Throughout this section, our results are based on data from 13 advanced industrial democracies.

As discussed above, the three dependent variables for this analysis are *Top10-1*, *Top 10*, and *Top 1*. The first measure, as defined above, is equal to the percentage of national income earned by the top 10% minus the percentage to the top 1% divided by 100 minus the income earned by the top 1%, all multiplied by 100. The second and third measures are simply the percentage of national income to the top 10% and the top 1% of income earners. We average the data over 17 five-year periods from 1916 to 2000. Averaging across five-year time periods allows us to examine variation over time without specifying precisely how long it takes for changes in labor market institutions or government partisanship to affect income inequality. Even with five-year averages, there is evidence of serial autocorrelation in the inequality time series and we

[15] The starting year of our analysis is determined by data availability. Although we have some information on top income shares for the first decade of the twentieth century, it is rather limited. Consequently, we use only data from 1911 forward in our regression analyses and, as will be discussed further below, due to five-year averaging and the use of a lagged dependent variable, we lose the five years 1911-1915 in our actual anaysis. Including the data that we have from 1900-1910 produces similar results.

model this simply by adding a lagged dependent variable to each of our ordinary least squares specifications. To account for possible panel heteroskedasticity and panel correlations, we report panel-corrected standard errors for all coefficient estimates. One important potential problem with the analyses reported below is that some specifications include both a lagged dependent variable and country fixed effects in analyses with a relatively small number of time periods. Because the bias from fixed effects specifications with lagged dependent variables is decreasing in the number of time periods, this source of bias could be substantively important. Finally, we also conducted a set of tests for the existence of a unit root in the different top incomes measures. As discussed in detail in our technical appendix, for each top incomes measure, unit root tests exploiting the panel structure of our data rejected the null of all individual country series having a unit root, and we have chosen to perform our estimates in levels.

Upon constructing our datasets, there were non-trivial numbers of missing observations for various variables. The standard approach of deleting cases that have missing values for any of the variables—known as "listwise deletion"—can create two major problems for inference. One is inefficiency caused by throwing away information relevant to the inferences being made. Furthermore, inferences from listwise-deletion estimation can be biased if the observed data differs systematically from the unobserved data. The most general and extensively researched approach for dealing with a missing data problem like this is "multiple imputation" (Honaker and King 2006, King et al 2001, Schafer 1997). Multiple imputation requires a relatively weak assumption in this context that the process generating the missing data is random conditional on the data included in the imputation procedures (commonly referred to as assuming the data are *MAR)*. Multiple imputation yields consistent coefficient estimates and gives correct uncertainty estimates under the *MAR* assumption.[16] We implemented multiple imputation for our analysis and the appendix describes our imputation procedures in detail.

To test the hypotheses that government partisanship and labor market institutions are important determinants of income inequality for the data series from 1916-2000, we developed new measures of government partisanship and wage bargaining centralization. This was made nec-

[16] It is also necessary to assume the parameters describing the missing data process are distinct from parameters of the data model so that the missing data mechanism is ignorable.

essary by the fact that existing indices of partisanship and wage bargaining do not extend back to the period before World War II. As we discuss below, the principal conclusions of this paper remain very similar when we instead pursue the alternative strategy of estimating correlates of top incomes shares only for the post-1950 period where we can make use of existing measures of partisanship and wage bargaining.

The variable *Wage Bargaining Centralization* is an index constructed by the authors that takes a value equal to 1 if wages are primarily determined in a decentralized setting, either without collective bargaining at all or bargaining at the plant level, equal to 2 if wages are primarily determined at the industry-level, and equal to 3 if there is national centralized wage setting. We consulted a number of sources to code each country including Campbell (1992); Ebbinghaus and Visser (2000); Blum (1981); Wallerstein, Golden, & Lange (1997); Iversen (1999); OECD (2004); Swenson (1989, 2002); and Golden and Wallerstein (2006b).[17] As discussed above, the literature suggests that more centralized wage bargaining decreases income inequality through a number of possible mechanisms, and so there is an expectation of a negative partial correlation between *Wage Bargaining Centralization* and income inequality. Although the use of indexes similar to *Wage Bargaining Centralization* are common in the literature, it is not clear that each of the unit intervals should be expected to have the same impact on inequality. Consequently, we constructed two dichotomous indicator variables, *Centralized Wage Bargaining* and *Decentralized Wage Bargaining*, based on the values of the index and included these measures to evaluate the hypothesis that greater centralization is associated with lower inequality. *Centralized Wage Bargaining* is equal to 1 if there is national, peak-level centralized wage setting and is equal to 0 otherwise. *Decentralized Wage Bargaining* is equal to 1 if wages are primarily determined in a decentralized setting, either without collective bargaining at all or bargaining at the plant level and is equal to 0 otherwise. Since the omitted category is sectoral-level bargaining, the expectation is that *Centralized Wage Bargaining* should be negatively correlated with each of the measures of inequality while *Decentralized Wage Bargaining* should be positively correlated with these measures.

[17] See Appendix for further description of this variable.

We also include an alternative measure of the extent of labor market organization by adding the variable *Union Density* equal to the percent of the total dependent labor force that are members of unions (less the self-employed).[18] In the literature, this variable is used both to measure how organized the labor market is and to measure the influence of the left. Under either interpretation, however, we expect the variable to be negatively correlated with our income inequality measures. To measure government partisanship, we constructed a dummy variable, *Left Executive*, equal to one if the country had a Prime Minister and/or President from a left party in a given year and zero otherwise.[19] Given the expectation that left governments set policies favorable to lower income voters, this variable should also be negatively correlated with the measures of income inequality.

The economic literature on income inequality suggests a number of control variables that should be included to estimate the partial correlations between *Left Executive*, *Centralized Wage Bargaining*, *Decentralized Wage Bargaining*, *Union Density* and our measures of income inequality. For our analysis of the complete data series, we include *GDP per capita*, *Trade Openness*, *Secondary Education Share*, and *Female Participation*, indicating the proportion of women in the economically active population, in all regressions.[20] We also include a control, *Non-Democracy*, equal to one if the country is experiencing a non-democratic year and zero otherwise and a control, *Universal Suffrage*, equal to one for all years after which the country had universal suffrage (male and female) and zero otherwise. In all specifications, we include dichotomous indicator variables for the time period of the observation. The time periods allow us to control for common shocks to income inequality in all countries.

4.2 Results

Table 1 reports the coefficient estimates for the ordinary least squares regression of each of our measures of income inequality on its one-period lag, the key variables of interest, *Left*

[18] The primary source for the period after 1950 is Golden and Wallerstein (2006b) The primary source for the period before 1950 is Kjellberg (1983). When both these sources were missing, we used Visser (1989). Additional sources used were OECD (2004); May, Walsh, Harbridge, & Thickett (2003); and Wallace (2003).

[19] Coded based on information in Caramani (2000) and McDonald (2002). See Appendix for further description.

[20] The complete definitions and sources for these variables are described in the Appendix.

Executive, *Centralized Wage Bargaining*, *Decentralized Wage Bargaining*, and *Union Density,* and the controls. For each dependent variable two specifications are reported, one without and one with country fixed effects. The fixed effects specifications are particularly important because they address to some extent the concern that much of the existing literature on centralized wage bargaining and income inequality identifies of off cross-sectional variation and therefore may be biased due to unobserved or unmeasured country characteristics.

The coefficient estimates in Table 1 provide no evidence for an important role for *Centralized Wage Bargaining* in determining national levels of income inequality. In the specifications with or without country fixed effects, the coefficients are imprecisely estimated and not statistically significant. Given the presence of the variable *Decentralized Wage Bargaining* in the model, it is important emphasize that the correct interpretation of this result is that there is no evidence in this data that national peak-level centralized wage bargaining is associated with less income inequality than is sectoral-level bargaining.[21] One potential concern about this finding is that the OLS estimator may biased if income inequality influences wage bargaining centralization. Specifically, it is plausible that countries experiencing greater equality find it easier to adopt centralized institutions. If this were case, however, the OLS estimator would be biased but in a negative direction. This means that if anything, we have over-estimated the negative effect of centralization on income inequality. Since this null result contradicts an extensive empirical literature based on analyses for the last two to three decades of the twentieth century, in the next section we will revisit this finding by looking at individual cases in greater detail.

The results in Table 1 do indicate, a positive and statistically significant correlation between decentralized wage bargaining and each of the three measures of income inequality. These estimates indicate that decentralized wage setting is associated with more income inequality than sectoral-level bargaining. The result is consistent with the hypothesis that more organized labor markets have less income inequality even if, as our results indicate, peak-level bargaining does not have an effect over and above sectoral wage setting. The long run impact of decentralized

[21] The absence of a significant correlation between *Centralized Wage Bargaining* and the income inequality measures does not, however, depend on the inclusion of the *Decentralized Wage Bargaining* variable. There is no partial correlation for *Centralized Wage Bargaining* and income inequality with or without the indicator variable for decentralized wage setting.

wage setting implied by these estimates is about a 2 to 3 percentage point increase in each of our inequality measures. This is economically important, but it does not suggest that wage bargaining institutions can account for the large changes over time in income inequality during the twentieth century. Perhaps most importantly, as we show below, this partial correlation is not statistically significant for the 1916 to 1975 period and is driven by outcomes after the mid-1970s. There is, therefore, little evidence in this data that wage bargaining institutions played a significant role in the substantial changes in income inequality during the first three quarters of the twentieth century, though a role at the end of the century is certainly a possibility.

The results for the trade unionism measure indicate a negative correlation between union density and our three top income measures. In the specifications with and without country fixed effects, the coefficient estimates for the *Union Density* are negative and statistically significant at the 0.01 level for the *Top10-1* and *Top10* measures of income inequality. For the *Top1* variable, the coefficient estimates for *Union Density* remain negative but are less precisely estimated (p-values equal to 0.11 and 0.14). The inclusion of both country and period fixed effects mean that the fixed effects estimates are identifying off of within-country variation over time, controlling for common shocks to income inequality experienced by all the countries included in the sample. In some respects, this is the strongest quantitative evidence of a causal effect for union density on income inequality in the existing literature. That said, the estimates may reflect changes over time within countries in unobserved factors that influence both the evolution of union participation and income inequality. It also may be biased due to the possible influence of inequality on the development of trade unionism.[22] This later point is especially important as the direction of the bias can be predicted to be negative, thus indicating that our OLS results would tend to overestimate the negative effect of union density on income inequality.

The coefficient estimates in Table 1 do not suggest an important role for government partisanship in determining national levels of income inequality. In the specifications with or without country fixed effects, the coefficients are imprecisely estimated and not statistically significant. This summary should be qualified somewhat for the *Top1* measure of income inequality. The

[22] See the discussion in Acemgolu, Aghion, and Violante (2001) on this point.

coefficient estimate is negative for the specifications with and without fixed effects with p-values equal to 0.064 and 0.128 respectively. This suggests some modest evidence that left partisan control may be associated with less income inequality but that any such effect seems to operate on the incomes at the very top of the income distribution. Moreover, the substantive magnitude of the estimated effect of partisanship is very small when compared to the massive changes in the Top 1% income share that have occurred over the course of the twentieth century.[23]

To evaluate the robustness of our empirical results, we conducted a number of sensitivity analyses. First, we reestimated each specification for the 1976-2000 and 1916-1975 periods separately. Previous research on the impact of labor market institutions on income inequality has focused almost exclusively on the prior period due to data availability. It is instructive to determine whether there is heterogeneity in the relationship between these variables over time and thus how sensitive the estimates reported in Table 1 are to the sample period. The results for these regressions, reported in Tables 2 and 3, indicate that there is little evidence of a correlation between *Centralized Wage Bargaining* and *Left Executive* and our measures of income inequality in either sample. The results for *Decentralized Wage Bargaining* suggest that the evidence for the importance of organized labor markets in determining income inequality is driven by the empirical record for the 1976 to 2000 period, which has informed the current literature on this question. The Table 2 results for 1976-2000 indicate a positive and statistically significant correlation between *Decentralized Wage Bargaining* and our three measures of inequality, but the Table 3 estimates for 1916-1975 are smaller in magnitude and much less precisely estimated with p-values ranging between 0.172 and 0.767. In sum, there is no evidence that the centralization of wage bargaining played a significant role in the large changes in income inequality over the first 75 years of the twentieth century.[24] For *Union Density*, there is also some evidence of

[23] In the interest of space, we do not discuss the results for the control variables in any detail. There is some evidence that richer and more open countries have greater income inequality and that greater female labor force participation is associate with lower inequality. These estimates, however, are based on specifications including the labor market organization and partisanship variables and since these measures may be consequences of the economic variables, they may not be proper controls for evaluating the impact of the economic measures. The results in Table 1 also indicate that non-democracy is associated with greater inequality.

[24] Along these lines, we also reestimated these specifications for the 1951-1975 period and found no evidence of a significant positive correlation between decentralization and the income inequality measures. Similarly, there was no evidence of a negative correlation for peak-level national wage setting.

heterogeneity across the samples with a stronger correlation in the earlier period.

As a second robustness check, we considered the possibility that the cumulative experience of left partisanship and centralized wage bargaining is what matters most for accounting for variation in income inequality. We constructed variables that measured the proportion of years in the last twenty that each country had values of the variables *Left Executive, Centralized Wage Bargaining*, and *Decentralized Wage Bargaining* equal to one. The results for the wage bargaining measures are essentially the same. There is no evidence of a correlation between the cumulative version of *Centralized Wage Bargaining* and the income inequality measures while a positive correlation is observed for the cumulative *Decentralized Wage Bargaining* variable [25] For partisanship, the hypothesized negative partial correlation is not observed for the *Top10-1* and *Top10* measures.[26] There is further evidence that left partisanship may reduce the share of income that goes to the very highest earners as the cumulative partisan measure is negatively and statistically significantly correlated with the *Top1* variable in the specification with fixed effects (p-value equal to 0.043). As before, however, the substantive magnitude of this effect remains very small compared to the very large changes that have occurred in the Top 1% income share over the course of the twentieth century.

As a third robustness check, we estimated specifications that dropped the union density measure to evaluate whether its inclusion might attenuate our estimates for centralized wage bargaining and partisanship. We found, little evidence that this was the case. The estimates were substantially the same for our key variables of interest.

Fourth, we considered the possibility that our estimates might be biased due to omitted variables.[27] One possibility is the possible influence of proportional representation on income inequality. The primary mechanisms by which PR is thought to influence income inequality involve making left partisanship and/or centralized wage bargaining more likely (Iversen and

[25] The partial correlation is somewhat weaker for the cumulative version of the *Decentralized Wage Bargaining* variable but it is still significant at the 0.10 level in the specifications without fixed effects and the 0.05 level in the specifications with fixed effects.

[26] For these measures without fixed effects, the cumulative partisan coefficient is actually also statistically significant at the 0.05 level but the sign is positive and therefore inconsistent with the usual partisan hypothesis.

[27] We also reestimated each specification dropping one of our thirteen countries at a time. This also did not substantially change our estimates for any of the main variables.

Soskice, 2006). Even if PR did have these effects on partisanship and the adoption of wage setting institutions, it still might not have an effect on income inequality given the findings in our paper. That said, there may be other mechanisms by which PR influences income inequality and so we tried controlling for it in our analysis. The inclusion of PR had little impact on our estimates and the coefficient on PR was only statistically significant in specifications without fixed effects for the *Top10-1* and *Top10* measures of income inequality.

Finally, we considered the possibility that our results may be biased due to poorly measured variables. To address this possibility, we investigated each hypothesis further with alternative data, arguably better measured, that is available from standard sources for the period 1950 to 2000. In this analysis, we measure government partisanship as equal to the government's left-right position as determined by the weighted (by seats in parliament) left-right positions of the parties in government and we use Golden and Wallerstein's coding of wage bargaining centralization to form our indicator variables for labor market organization. Perhaps the most striking finding in this analysis is that we find some evidence of a partial correlation between this measure of partisanship and income inequality for the more recent 1976-2000 period but very little evidence for this correlation in the 1951-1975 period. This result resonates both with the existing literature that has focused on the more recent period and with our main results that do not find evidence of a robust correlation between partisanship and income inequality. The results also are consistent with our main findings for wage bargaining centralization. For both periods, there is no significant partial correlations between peak-level centralized bargaining and the income inequality measures. Further, the positive correlations between decentralized wage setting and income inequality are observed most strongly in the 1976-2000 period and are not present at all in any of the fixed effects specifications.[28]

5 Institutional Turning Points

Our regression results based on a pooled analysis of thirteen countries suggest that when one considers a long time horizon (one extending before the 1970s) there is relatively weak evidence

[28] See Appendix for a more complete description of this analysis.

that the presence of a "left" government has been associated with lower income inequality. Nor is there clear evidence of an association between centralized wage bargaining and low levels of income inequality. Given the conclusions of previous scholarly work, however, it makes sense to take a closer look at the relationship between inequality and the establishment of peak level centralized wage bargaining at the country level. Doing so may suggest if our findings are influenced by the fact that we are pooling across a heterogeneous set of countries. It can also allow us to investigate whether the initial introduction of centralized wage bargaining in several countries was associated with a drop in inequality.

In this section we consider evidence from individual country cases in order to provide a closer look at the effect of centralized wage bargaining, and in particular shifts towards greater centralization, on top income shares. Seminal work in the field of political economy has emphasized the importance of the crises of the 1930s and 1940s in leading to the adoption of corporatist bargaining arrangements in several states. For Katzenstein (1985) the economic crisis of the 1930s led to the development of corporatist bargaining in the smaller European states, but not their larger neighbors, because of different political conditions, one of which was the prior adoption of proportional representation. For Gourevitch (1986 ch.4) the crisis of the 1930s and the subsequent wartime experience helped lead to formal centralized bargaining arrangements in some states, but he also emphasizes how these twin crises drove all states by 1945 towards a politics of accommodation between business and labor irrespective of the formal arrangements for bargaining. His arguments regarding the post-war period in the US closely parallel the more recent analysis of Levy and Temin (2007).

Sweden in the early 1950s is the most frequently cited example of a major shift towards formal centralized bargaining, but the literature also identifies similar historical turning points in countries like Denmark and the Netherlands. In this section we investigate whether these suggested historical turning points are associated with structural breaks in income inequality as measured by top incomes shares. Previous quantitative studies of centralized wage bargaining and its effects have not considered whether the steps to centralized wage bargaining that occurred in the 1930s, 1940s, and 1950s had a significant impact on outcomes. We argue that there is in fact little evidence that the initial move to centralize wage bargaining led to a downward break in top income shares. For each country we instead observe a pattern where inequality was already trending downwards before bargaining was centralized, and inequality continued to trends downwards at the same rate after this point. This raises questions about the extent to which this institutional change had an independent causal effect on income inequality, or whether centralized wage bargaining and lower income inequality were instead both outcomes driven by some underlying process at the country level. In what follows we first present visual evidence for Sweden on the extent to which the move to centralized bargaining constituted a structural break. We then consider this issue more formally using data from Sweden, Denmark, the Netherlands, and Ireland - four countries that established centralized bargaining at the national level in the middle of the twentieth century.

5.1 Was There a Structural Break?

Among our thirteen sample countries there are three that established national level wage bargaining in a democratic context during the 1930s, 1940s, or 1950s. In addition, we also have a separate income inequality series for Denmark that will allow for examining whether the shift to centralized bargaining in that country was associated with a downward break in income inequality.[29] For Sweden, Denmark, and Ireland we also have long-run series on wage differentials between more highly skilled and less skilled workers. For Sweden we have a long-run series covering the entire twentieth century compiled by Ljungberg (2006) that builds on the earlier work of (Jungenfelt, 1966) and represents the ratio of the wage for technicians (at age 42-45) to the average wage in manufacturing. For Denmark, we have a series measuring the pay ratio between skilled and unskilled manual workers drawn from Johansen (1985) that covers the years between 1870 and 1965. Finally, for Ireland we have data compiled by O'Rourke (1994) showing the ratio between wages for skilled and unskilled workers in three sectors of the Irish economy between 1926 and 1984. Each of these series can be used to examine whether centralization of bargaining produced a break in inequality. An examination of these wage series is also useful

[29] Sorensen (1993) used tax statistics to investigate income inequality over the long run in Denmark, and we will use his measure of income inequality in the structural break tests below.

because it demonstrates that they are highly correlated with the data on top income shares.

The Swedish national employers federation (SAF) and the union confederation (LO) took a significant step towards centralized wage bargaining with the Saltsjöbaden accord of 1938 which was an agreement to manage potential labor disputes at the confederal level. From 1953 the SAF and LO bargained in a centralized fashion with pay set according to a "solidaristic" wage policy stating that those performing similar work should receive similar pay irrespective of the firm or sector in which they worked, and that wage disparities within firms should be reduced. In Denmark a national collective bargaining agreement was reached in 1934 between the LO union federation and the DA employers federation. This introduced a degree of centralization into the process, though it was not until the late 1950s that the LO and DA acquired authority to sign legally binding agreements on wages and other issues.[30] In the Netherlands a government decree of 1945 created the Foundation of Labor, a bipartite organization including top employers associations and trade union federations (Windmuller, 1957). The Foundation had a prominent role in wage-setting. Finally, though not having the same history of "democratic corporatism" as the other three countries, in 1946 Ireland also established a centralized system of wage bargaining with a national wage round (Blum 1981 p.291).

Figure 3 plots the *Top10-1* income share for Sweden, indicating two potential break points, the Saltsjöbaden accord of 1938 and the establishment of a solidaristic wage policy in 1953. We focus on this particular top income share because it is the top income measure that we might expect would be the most likely to be influenced by the establishment of centralized wage bargaining. The figure also makes use of the wage inequality series from Ljungberg (2006). Two things are apparent from this graph. First, the *Top10-1* share and the wage inequality series are very highly correlated and follow a common downward trend until the beginning of the 1980s. Second, there is no apparent evidence for either of these series that the Saltsjöbaden accord of 1938 produced an acceleration of this downward trend. One would draw a very similar conclusion if the break date for the centralization of bargaining was set, at a different point, such as the adoption of a solidaristic pay policy in the early 1950s. Interestingly, there is also evidence

[30] See the discussion in Iversen (1999 ch.5) as well as Ebbinghaus and Visser (2000).

from Svensson (2004) that the gender wage gap declined in Sweden before equal pay became an official policy of Sweden's central labor confederation. In other words, we see a similar pattern to overall wages; institutional (or policy) change took place only after the changes in inequality that institutional change is often presumed to have caused were well underway.

As an addendum to the above discussion, it should also be emphasized that while inequality was trending strongly downwards well before the institutional changes in Sweden, Denmark, the Netherlands, and Ireland, this was also true of most countries that did not adopt centralized wage bargaining at this time, as can be seen from the strong overall time trend in Figure 1. One way to illustrate this is to consider two countries that are often portrayed as polar opposites in comparative political economy - the United States and Sweden. While Sweden moved to adopt centralized bargaining at mid-century, the United States never had a centralized system of wage bargaining, unless one considers wartime price controls between 1941 and 1945. Figure 4 plots the *Top10-1* income share in the United States and Sweden over the twentieth century. The two series are strikingly close during the immediate post-war period up until the 1970s. With this evidence in mind, it seems difficult to understand how wage bargaining institutions can explain the gap that emerged between the two countries after 1975 unless one asserts that once it is introduced, centralized wage bargaining has an effect only after an extremely long time lag.

5.2 Tests for a Structural Break

The visual evidence for Sweden suggests that both the *Top10-1* income share and a measure of wage inequality declined following institutional changes, but inequality was already declining before this date and there is little evidence of an acceleration of this process. We now investigate this question more formally using available data on both top incomes and wages for Sweden, Denmark, the Netherlands, and Ireland.[31] Each series is modeled as a function of a linear trend, allowing for a break at the time that wage bargaining was centralized. We report the results of

[31] For Sweden we use data on the *Top10-1* income share from 1903 to 2000 and wage data for the 1900 to 2000 period. For Denmark the MEC inequality series covers the period 1871 to 1965 and the wage series we use is for the period 1870-1965. For the Netherlands we use data on the *Top10-1* income share for 1914 to 1999. For Ireland we use wage data covering the period 1926-1984 but unfortunately lack a top income series other than the Top 0.1% share prior to the 1970s.

this regression, as well as regressions setting the break point at five year intervals up to fifteen years before or after the date that wage bargaining was centralized. Formally, in the equation below the variables T_1 and T_2 refer to separate time trends. T_1 is set equal to t for all years previous to the chosen break point. and it is set at $T_1 = 0$ for all t subsequent to the break. T_2 takes a value of zero for all t prior to the break and a value of t for all t after the break. If the adoption of centralized wage bargaining resulted in a structural break in inequality we would expect to find that the coefficient γ is more negative than β.

$$\text{Top}X_t = \alpha + \beta T_1 + \gamma T_2 + \varepsilon$$

For Sweden we set $t = 1953$ with the initiation of a solidaristic wage policy, though we obtained very similar results when instead setting $t = 1938$, the date of the Saltsjöbaden accord. For Ireland we set $t = 1946$ and for the Netherlands $t = 1945$. Finally, for Denmark we set $t = 1934$ representing the step towards centralization with the accord between the LO and DA federations.[32]

The estimates of the equation are presented in Table 5.[33] There is strong evidence of a general downward trend in inequality in both the top incomes and the wage series, but there is very little evidence here of an acceleration of this downward trend following the adoption of centralized wage bargaining. This is particularly clear for Sweden, and we also observe that neither of the two Danish series provide evidence of a significant trend shift at any point up to fifteen years before or after the institutional change of 1934. For the Irish wage series there is some evidence of a downward trend shift around the time of the first national wage round in 1946, but the difference between β and γ is small and it is not statistically significant.[34] The Netherlands top income series is the single case where the difference between β and γ

[32] The choice of this date for Denmark was in part dictated by the fact that our Danish series end in 1965, making it difficult to assess whether an alternative centralization date of the late 1950s was associated with a structural break in inequality. We should add to this that a visual inspection shows that neither of the two Danish series trends downwards from the late 1950s.

[33] We have chosen to control for serial correlation here by using Newey-West standard errors. We obtained similar conclusions regarding the absence of a break in trend (but also less precise estimates of the β and γ coefficients) when including an AR(1) term, or terms for higher order autocorrelation in the regression.

[34] We failed to reject the null of the two coefficients being identical $p = 0.39$.

is negative, statistically significant and occurs around the time of an important institutional change in wage bargaining arrangements. This parallels the observation by Windmuller (1957) that the establishment of the Foundation of Labor in the Netherlands led to a reduction in the skill premium. However, the drop in the skill premium that he pointed to was small.[35] One might respond that the absence of evidence for a structural break results from the fact that we have not correctly identified the date at which wage bargaining became fully centralized. But the Table 5 results show that in almost all cases, if we move the break date either forwards or backwards by up to fifteen years, there continues to be little evidence of an acceleration in the rate at which inequality was declining. Finally, we also obtain very similar results from these tests if we drop from each country series later years corresponding to a period where bargaining arrangements became decentralized after the prior establishment of centralization.[36]

We can also adopt a more formal procedure for each of the six series, testing whether there is a break in both the trend and mean of the series against the null hypothesis that the series is characterized by a single trend. The most familiar way to test this hypothesis is a Chow test. Quandt (1960) suggested that in the absence of prior knowledge where a break occurs, one could perform a Chow test on each point in the series, and choose the point with the largest Chow statistic as the most likely break point. Andrews (1993) provided critical values that can be combined with Quandt's method to determine whether there is a statistical break in the series at a specific point.[37] We performed the Quandt-Andrews test for structural breaks on each of the series producing results very similar to those we drew based on the Table 5 regressions. Based on this test, there is no evidence of a structural break at any point in any of the six series.

6 Implications of our Results

Our empirical tests suggest that certain hypotheses about the political correlates of inequality that are supported by data for recent decades find much less support when looking at top income

[35] Prior to 1945 there was a 25% average difference in pay between skilled and unskilled workers. After 1945 the Foundation of Labor pursued a policy of reducing this ratio to 20%.

[36] For Sweden this includes the period 1983-2000. For the Netherlands this includes the period from 1959 to 1999, and for Ireland this includes the period from 1981 to 1984.

[37] The standard critical values for a Chi^2 test are not applicable unless the break date is known ex ante.

shares over the rest of the twentieth century. This result is not simply attributable to the fact that if we take a sufficiently long-run view, political factors will always wash out. If we look at earlier periods over the short-run we continue to find little support for the existing arguments. In this section we review our findings regarding government partisanship and wage bargaining centralization, and we then briefly consider three possible explanations for our results.

To begin, our results point first to a need to reconsider the relationship between government partisanship and inequality, and one logical next step would be to examine whether our conclusion regarding partisanship also holds when looking at redistributive policies like progressive income and estate taxation for periods before the 1970s. One might find here that in a period like the 1950s and 1960s all governments, irrespective of partisan orientation, implemented highly progressive tax policies. The recent work by Piketty and Saez (2006) shows that a detailed comparison of the progressivity of the tax system in the United States, the UK, and France over time reveals surprising results as governments dominated by parties of the left have not had a tendency to pursue more progressive taxation.[38] One may question whether our results regarding partisanship are simply attributable to measurement error. Perhaps existing indicators of partisanship, based on either a simple left-right dichotomy or on manifesto coding, are simply insufficient in tracking, for example, the extent to which the Eisenhower administration in the US had a stance on redistributive policy markedly different from that of subsequent Republican administrations. The alternative conclusion one might draw based on our empirical results is that if partisanship has been highly correlated with the extent of redistributive policy (and thus income inequality) in recent decades, over the long-run, the principal story may be that parties of both the left and the right tend to be pushed in the same direction with regard to redistribution by underlying forces or common processes. The question would, of course, then become what these underlying forces or processes are.

In addition to our findings regarding partisanship, we also observe little evidence of a robust correlation between top income shares and centralized wage bargaining. This conclusion is reinforced when we look at individual country time series. Here we observe that income

[38] The work of Atkinson and Leigh (2007) also raises interesting questions in this regard.

inequality did decline after the introduction of centralized wage bargaining, but it was already declining beforehand, and it continued to decline at a similar rate afterwards. Moreover, inequality was also declining in countries that never adopted centralized wage bargaining. This raises questions about the extent to which it is more accurate to think of centralized wage bargaining as an institution that poses constraints on actors and which produces lower inequality, or alternatively whether centralized wage bargaining was simply a policy outcome produced by an underlying process that influenced both inequality and the formal setup of labor market institutions. Our pooled estimation results also raises important questions about how countries without centralized wage bargaining arrangements may nonetheless have experienced dramatic declines in income inequality through the immediate post-war period.

A first possible explanation for our results is that political factors have had little influence on the evolution of income inequality over the long-run, as income differentials have instead been driven by exogenous economic forces involving the race between technology and education.[39] When we consider the case of wage bargaining, It may be that exogenous changes in the relative demand for different skills in an economy influence the willingness of highly skilled individuals to participate in centralized bargaining arrangements that pool skilled workers with unskilled workers, and at the same time the presence or absence of centralized bargaining in turn determines the degree of pre-tax income inequality. In this case wage bargaining centralization could lead to lower inequality, but it would be only a proximate cause. Acemoglu, Aghion, and Violante (2001) have used this type of theoretical framework to suggest why participation in trade unions and income inequality may both depend on technological changes that alter relative demand for highly skilled labor. Svensson (2005) has recently proposed exactly this type of argument for understanding the emergence and subsequent dismantling of centralized wage bargaining in Sweden. Depending on the precise assumptions made, this type of model could be consistent with the empirical observation of income inequality trending downwards prior to a shift towards more centralized bargaining. However, this type of model would not be able to explain why, from relatively similar prior levels of inequality in the 1930s, some states chose

[39] By stating this we would, of course, be distinguishing this argument from one where political factors involving partisanship or institutions would themselves influence both technology and education.

to adopt centralized wage bargaining while others did not, yet almost all states experienced a significant reduction in inequality in the immediate post-war period.

A second possible explanation for our results is that income inequality was endogenous to an underlying *political* process that we have not captured with our data. The combined experiences of economic crisis and major war during the 1930s and 1940s may have been particularly important in this regard. As we noted above, Piketty (2003, 2001) and Piketty and Saez (2006, 2003) have emphasized the effect of wartime taxation and destruction on large fortunes, suggesting a direct effect of war in narrowing inequality. For the period after the war the establishment of high top marginal tax rates may have prevented pre-war fortunes from being quickly reconstituted. This leaves open the question of why the post-war consensus on progressive income taxation was politically sustained. It may be the case that the post-war decrease in inequality would not have occurred without a change in the political climate that was itself the produce of wartime experience and mobilization. This political change may have made progressive income taxation and other new redistributive policies like the US GI Bill politically feasible.

There is also at least one further potential explanation for our results regarding centralized wage bargaining. Centralized wage bargaining may at certain times (like the 1980s) appear to have mattered for inequality because differences in the formal degree of centralization between countries like the United States and Sweden also corresponded with underlying differences in norms or beliefs about pay inequality. But at other junctures, such as the 1950s, underlying norms about pay inequality may have been much more similar across countries irrespective of differences in the degree of centralization in their bargaining arrangements. The important recent work by Levy and Temin (2007) on post-war US inequality is particularly interesting in this regard, as it suggests how wage setting norms in the US in the immediate post-war period may have been quite similar to those operating in a number of continental European countries despite sharp differences in the formal degree of wage bargaining centralization.[40] The difficult question is how, if one wanted to pursue this line of reasoning, would it be possible to identify the existence of beliefs about pay inequality independent of their effects.

[40] The recent work by Kopczuk and Saez (2007) also raises the possibility that changing pay norms may help explain the evolution of earnings inequality in the United States during this period.

7 Conclusion

In this paper we have suggested that while explaining post-1970 differences in income inequality between OECD countries is an important task, convincing comparative political economy hypotheses should also be able to account for inequality trends in earlier time periods. For an institutional hypothesis to be convincing it should also be possible to demonstrate that institutional change leads to significant shifts or breaks in inequality in individual countries. We have found little evidence that the government partisanship and wage bargaining centralization can account for variation in inequality over the long-run. When we take a closer look at the evolution of inequality and of wage bargaining in individual countries, and in particular those countries that established a centralized system of wage bargaining in the middle of the twentieth century, we continue to see little evidence of an effect on inequality. In those countries that adopted centralized wage bargaining, income inequality was indeed lower after this institutional change, but inequality was trending downwards well before the change, and the institutional change was not associated with either a one-time shift downwards or with a change in this trend. This raises questions about the extent to which we can say that centralized wage bargaining is an institution that has a causal effect on inequality, or alternatively whether centralized bargaining is simply an outcome that has, along with income equality, evolved over time in response to an underlying political or economic process. More generally, our results also suggest that while scholars in comparative political economy have been most active in developing hypotheses that explain cross-country differences in inequality, they have focused less on developing effective arguments to account for changes in inequality over time. In other words, while it is important to seek explanations for the current difference in inequality between the United States and Sweden, political scientists also ought to attempt to explain why in the 1950s and 1960s these countries appeared much less different in terms of inequality than they do today.

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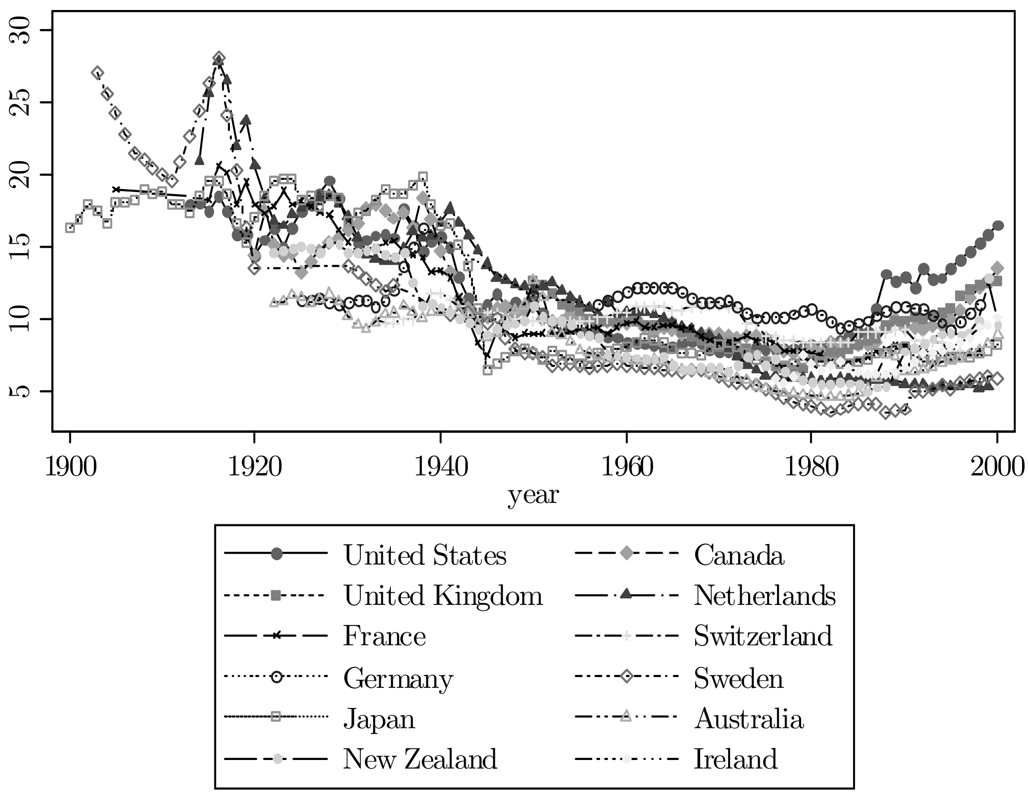

Figure 1: Share of Income Earned by Top 1%. Sources: Atkinson and Piketty (forthcoming), Moriguchi and Saez (2005), Roine and Waldenstrom (2006). The graph reports the adjusted series constructed by Leigh (2007). Data for the Top 1% share in Spain are available from Alvaredo and Saez (2006) but only for the period 1981-00 and are not included in the graph.

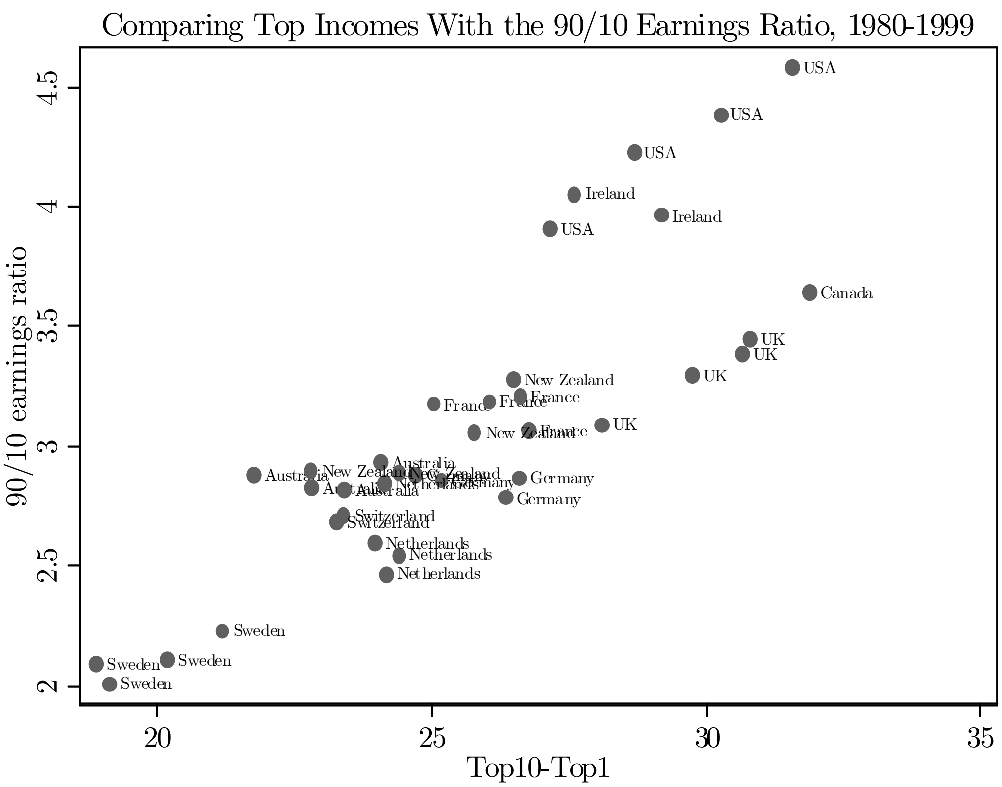

Figure 2: Comparing Top Incomes with the 90/10 earnings ratio. Each country is shown for four separate sub-periods 1980-84, 1985-89, 1990-94, and 1995-99. Earnings data reported in OECD (2004). *Top 10-1* income share is from the sources listed in Figure 1.

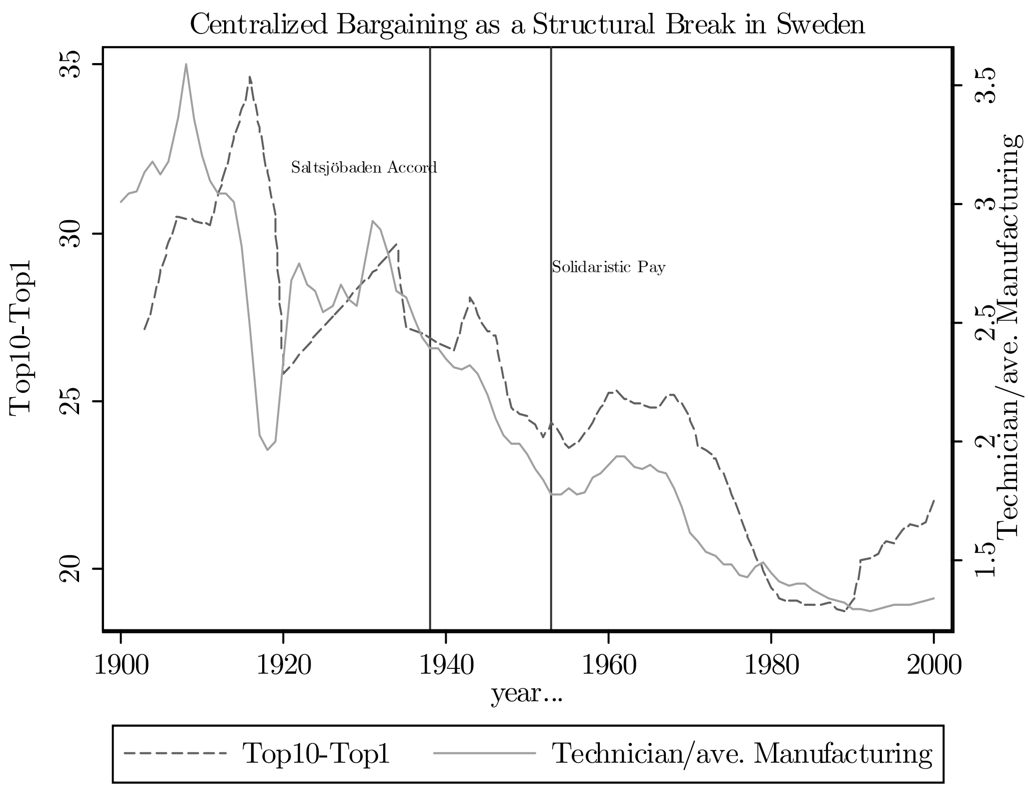

Figure 3: Centralized Bargaining as a Structural Break in Sweden. Vertical lines indicate dates at which wage bargaining becomes more centralized.Top incomes data from Roine and Waldenstrom (2006) with the *Top10-1* share on the left axis. The ratio for technicians to the average wage in manufacturing is from Ljungberg (2006) and is on the right axis.

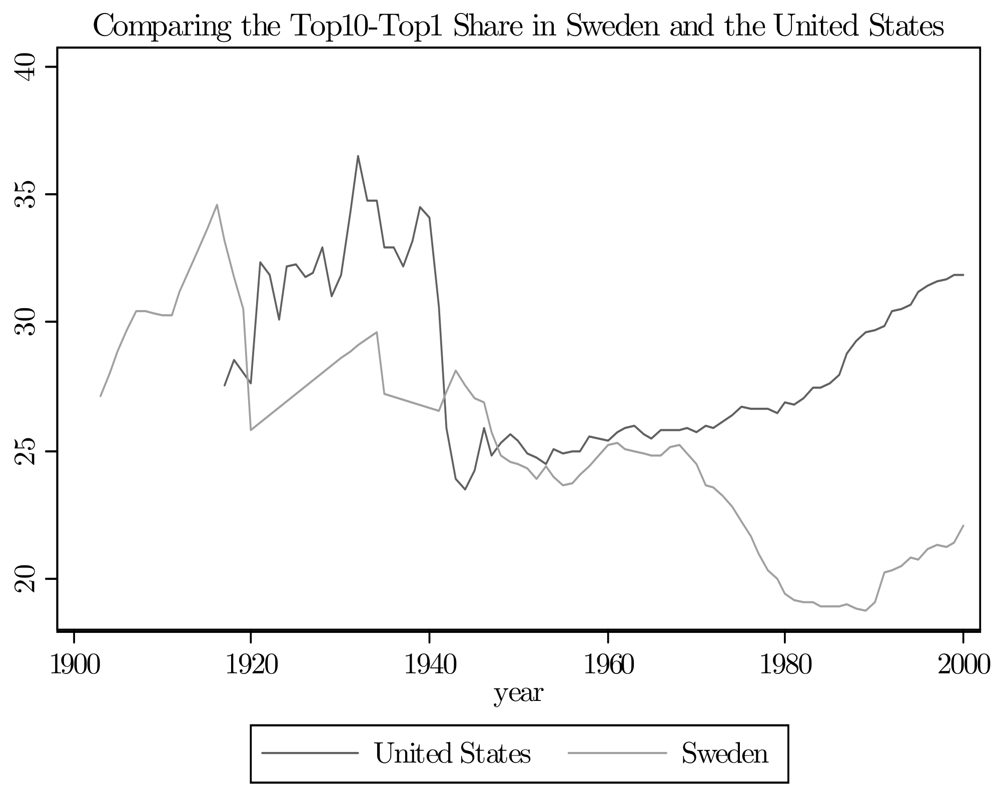

Figure 4: Comparing the Top10-Top1 Income Share in the United States and Sweden. Top incomes data for Sweden from Roine and Waldenstrom (2006) and for the United States from Piketty and Saez (2003).

	Top10-1		*Top10*		*Top1*	
TopX$_{t-1}$	0.531	0.376	0.547	0.458	0.577	0.542
	(0.070)	(0.078)	(0.073)	(0.076)	(0.069)	(0.068)
GDP per capita	0.028	-0.022	0.104	0.137	0.111	0.170
	(0.051)	(0.085)	(0.068)	(0.114)	(0.048)	(0.078)
Trade Openness	0.019	0.025	0.023	0.027	0.009	0.007
	(0.010)	(0.014)	(0.013)	(0.018)	(0.008)	(0.011)
Secondary Education Share	0.486	-1.407	-0.285	-1.898	-0.920	-0.944
	(1.150)	(1.416)	(1.407)	(1.825)	(0.857)	(1.193)
Female Participation	-14.881	-11.252	-21.728	-17.848	-12.064	-11.238
	(5.963)	(5.213)	(7.699)	(6.915)	(4.370)	(4.705)
Centralized Wage Bargaining	0.202	0.406	0.113	0.166	-0.114	-0.241
	(0.490)	(0.541)	(0.621)	(0.713)	(0.385)	(0.447)
Decentralized Wage Bargaining	1.064	1.247	1.541	1.659	0.839	0.871
	(0.402)	(0.413)	(0.495)	(0.520)	(0.301)	(0.339)
Union Density	-0.061	-0.077	-0.069	-0.084	-0.017	-0.021
	(0.013)	(0.019)	(0.018)	(0.023)	(0.011)	(0.014)
Left Executive	0.330	-0.313	-0.029	-0.628	-0.455	-0.523
	(0.362)	(0.340)	(0.463)	(0.420)	(0.296)	(0.280)
Non-Democracy	1.303	1.659	2.217	2.557	1.486	1.549
	(0.808)	(0.813)	(1.109)	(1.135)	(0.716)	(0.738)
Universal Suffrage	1.213	1.183	0.865	0.472	-0.275	-0.626
	(0.429)	(0.482)	(0.505)	(0.591)	(0.335)	(0.411)
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Country Fixed Effects	No	Yes	No	Yes	No	Yes
Number of Countries	13	13	13	13	13	13
Number of 5-Year Periods	17	17	17	17	17	17
Total Observations	219	219	219	219	219	219

Table 1: Labor Market Institutions, Government Partisanship, and Income Inequality: 1916-2000. The Table reports the results of OLS regressions for the three measures of income inequality, *Top10-1*, *Top10*, and *Top1* on *Centralized Wage Bargaining*, *Decentralized Wage Bargaining*, *Union Density*, *Left Executive* and various control variables for the 17 5-year periods between 1916 and 2000. Ireland was not an independent country until 1922 and so is not included in the analysis until that 5-year period. The table reports multiple imputation estimates of the OLS coefficients for each variable and their PCSEs in parentheses. A constant term is included in each regression but not reported in the table.

	Top10-1		*Top10*		*Top1*	
Centralized Wage Bargaining	0.656	0.598	0.609	0.818	0.118	0.015
	(0.687)	(0.661)	(0.952)	(0.922)	(0.527)	(0.700)
Decentralized Wage Bargaining	1.587	1.260	2.134	2.177	1.344	1.087
	(0.562)	(0.571)	(0.718)	(0.716)	(0.459)	(0.493)
Union Density	-0.031	-0.048	-0.017	-0.057	0.012	-0.006
	(0.018)	(0.032)	(0.024)	(0.043)	(0.009)	(0.027)
Left Executive	0.549	0.056	0.430	0.166	-0.028	-0.083
	(0.493)	(0.382)	(0.598)	(0.524)	(0.289)	(0.289)
Baseline Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Country Fixed Effects	No	Yes	No	Yes	No	Yes
Number of Countries	13	13	13	13	13	13
Number of 5-Year Periods	5	5	5	5	5	5
Total Observations	65	65	65	65	65	65

Table 2: Labor Market Institutions, Government Partisanship, and Income Inequality: 1976-2000. The Table reports the results of OLS regressions for the three measures of income inequality, *Top10-1*, *Top10*, and *Top1* on *Centralized Wage Bargaining*, *Decentralized Wage Bargaining*, *Union Density*, *Left Executive* and various control variables for the 5 5-year periods between 1976 and 2000. The table reports multiple imputation estimates of the OLS coefficients for each variable and their PCSEs in parentheses. A constant term is included in each regression but not reported in the table.

	Top10-1		*Top10*		*Top1*	
Centralized Wage Bargaining	0.422	0.919	0.312	0.485	-0.146	-0.388
	(0.700)	(0.774)	(0.832)	(1.022)	(0.558)	(0.665)
Decentralized Wage Bargaining	0.799	0.724	1.038	0.659	0.458	0.156
	(0.601)	(0.582)	(0.740)	(0.749)	(0.420)	(0.523)
Union Density	-0.094	-0.107	-0.119	-0.127	-0.041	-0.041
	(0.022)	(0.029)	(0.026)	(0.034)	(0.015)	(0.020)
Left Executive	0.400	-0.355	0.140	-0.541	-0.406	-0.399
	(0.470)	(0.434)	(0.537)	(0.515)	(0.350)	(0.377)
Baseline Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Country Fixed Effects	No	Yes	No	Yes	No	Yes
Number of Countries	13	13	13	13	13	13
Number of 5-Year Periods	12	12	12	12	12	12
Total Observations	154	154	154	154	154	154

Table 3: Labor Market Institutions, Government Partisanship, and Income Inequality: 1916-1975. The Table reports the results of OLS regressions for the three measures of income inequality, *Top10-1*, *Top10*, and *Top1* on *Centralized Wage Bargaining*, *Decentralized Wage Bargaining*, *Union Density*, *Left Executive* and various control variables for the 12 5-year periods between 1916 and 1975. Ireland was not an independent country until 1922 and so is not included in the analysis until that 5-year period. The table reports multiple imputation estimates of the OLS coefficients for each variable and their PCSEs in parentheses. A constant term is included in each regression but not reported in the table.

	Break set relative to centralization date						
	−15	−10	−5	0	5	10	15
Sweden (Top10-1)							
(1903-2000)							
β	-.101	-.105	-.093	-.116	-.129	-.120	-.110
	(.036)	(.032)	(.028)	(.029)	(.027)	(.024)	(.022)
γ	-.120	-.121	-.118	-.123	-.126	-.124	-.123
	(.016)	(.016)	(.016)	(.016)	(.016)	(.015)	(.015)
Sweden (wage ratio in percent)							
(1900-2000)							
β	-1.63	-1.70	-1.84	-2.13	-2.35	-2.29	-2.18
	(0.46)	(.040)	(0.36)	(0.33)	(0.30)	(0.27)	(0.26)
γ	-2.00	-2.00	-2.02	-2.08	-2.12	-2.10	-2.08
	(0.17)	(0.18)	(0.18)	(0.17)	(0.17)	(0.16)	(0.16)
Netherlands (Top10-1)							
(1914-1999)							
β	-.168	-.157	-.131	-.045	-.046	-.083	-.117
	(.050)	(.052)	(.046)	(.030)	(.033)	(.034)	(.028)
γ	-.145	-.143	-.137	-.111	-.111	-.121	-.132
	(.017)	(.019)	(.019)	(.014)	(.015)	(.015)	(.012)
Denmark (MEC)							
(1871-1965)							
β	-.183	-.171	-.173	-.164	-.158	-.165	-.181
	(.092)	(.068)	(.049)	(.038)	(.031)	(.027)	(.025)
γ	-.208	-.211	-.212	-.217	-.222	-.225	-.221
	(.032)	(.026)	(.022)	(.020)	(.018)	(.017)	(.016)
Denmark (wage ratio in percent)							
(1870-1965)							
β	-.437	-.447	-.417	-.401	-.397	-.397	-.391
	(.060)	(.046)	(.041)	(.037)	(.031)	(.026)	(.024)
γ	-.259	-.255	-.261	-.261	-.254	-.238	-.217
	(.043)	(.039)	(.040)	(.040)	(.039)	(.036)	(.032)
Ireland (wage ratio in percent)							
(1926-1984)							
β	-.491	-.474	-.361	-.262	-.076	-.123	-.352
	(.100)	(.118)	(.156)	(.145)	(.091)	(.085)	(.096)
γ	-.397	-.405	-.370	-.326	-.236	-.256	-.365
	(.051)	(.063)	(.079)	(.080)	(.054)	(.052)	(.059)

Table 4: Testing for a Trend Shift at Different Break Dates. Newey West Standard Errors.

A Appendix

A.1 Data Description

This section of the appendix defines each of the variables employed in the analysis, identifies the sources used to construct each measure, reports the descriptive statistics, and for selected variables gathered by the authors, reports the value of the measure for each country-year observation.

A.1.1 Variable Definition and Sources

Centralized Wage Bargaining is equal to 1 if there is national centralized wage setting and is equal to 0 otherwise. This variable is derived directly from *Wage Bargaining Centralization* described below. See the description for *Wage Bargaining Centralization* for the sources for this measure.

Decentralized Wage Bargaining is equal to 1 if wages are primarily determined in a decentralized setting, either without collective bargaining at all or bargaining at the plant level and is equal to 0 otherwise. This variable is derived directly from *Wage Bargaining Centralization* described below. See the description for *Wage Bargaining Centralization* for the sources for this measure.

Female Participation is equal to the number of women in the economically active population as a share of the total economically active population. The source is Mitchell (2003).

GDP per capita is equal to GDP in millions of 1990 International Geary-Khamis dollars divided by population in thousands. The source for the GDP and population data is Maddison (2003).

Left Executive takes a value of 1 for those country-years where the head of government (President in a presidential system, Prime Minister/Chancellor in a parliamentary system) was from a "left" party and 0 otherwise. If there was a "left" executive for only part of the year then the variable still takes a value of 1. The exception here is Switzerland which has a seven member Federal Council and no equivalent to a single head of government. For Switzerland the variable takes a value equal to the proportion of the seven seats on the Federal Council held by individuals from left parties. The identity of "Left" political parties is judged based on the information in Caramani (2000) as well as by inference from the partisanship scores in McDonald (2002). Table 1 reports the country-years for which a left executive is coded as present.

Country	Coding for *Left Executive*
Australia	Left Prime Ministers (Labour Party) 1904, 1908-17, 1929-32, 1941-49, 1972-75, 1983-96
Canada	Left Prime Ministers (Liberal Party) 1900-11, 1921-30, 1935-57, 1963-79, 1980-84, 1993-2000
France	Left Prime Ministers (Socialist Party, SFIO, Front Populaire, Republican Socialist Party) 1909-11, 1913-17, 1920-22, 1925-26, 1929, 1932-33, 1936-38, 1946-47, 1956-57, 1981-86, 1988-93, 1997-2000
Germany	Left Chancellors (SDP) 1918-20, 1928-30, 1969-74, 1974-82, 1998-00
Ireland	Left Chairmen of Provisional Government (1922-1937) or Left Prime Ministers (1937-2000) (CG, Fine Gael) 1922-32, 1948-51, 1954-57, 1973-77, 1981-87, 1994-97
Japan	Left Prime Ministers (Social Democratic Party, Japan New Party) 1947-48, 1993-96
Netherlands	Left Prime Ministers (PVDA) 1945-46, 1948-58, 1973-77, 1994-2000
New Zealand	Left Prime Ministers (Labour) 1935-49, 1957-60, 1972-75, 1984-90, 1999-2000
Spain	Left Prime Ministers (Socialist Party) 1931-1936, 1982-1996
Sweden	Left Prime Ministers (Social Democrat) 1920-26, 1932-76, 1982-91, 1994-2000
Switzerland	Left party representation on the seven member Federal Council 1 out of 7 1943-54, 2 out of 7 1959-2000
United Kingdom	Left Prime Ministers (Labour Party) 1924, 1929-1935, 1945-51, 1964-1970, 1974-79, 1997-2000
United States	Left Presidents (Democrat) 1913-21, 1933-52, 1960-68, 1977-80, 1992-2000

Table 1: Coding for *Left Executive*

Non-Democracy is equal to 1 if the country is experiencing a non-democratic year and 0 otherwise. The variable is coded a 1 for the following country years: France 1940-1944, Germany 1900-1918, 1933-1945, Japan 1900-1945, Netherlands 1940-1944, Spain 1939-1975.

Secondary Education Share is equal to the number of students enrolled in secondary education divided by an estimate of the total population between ages 12 and 17. We estimated the total population between ages 12 and 17 by using data on the population for the 10-14 and 15-19 age ranges as provided by Mitchell (2003), based on the assumption of a uniform distribution of ages within each of these two sub-groups. The variable is lagged by 10 years to better indicate the skill attainment of the labor force. The source for education enrollments is Mitchell (2003).

Top1 is equal to the percentage of national income earned by the top 1% of income earners. The original source for this data is Atkinson and Piketty (forthcoming) for all countries except Japan, Sweden and Spain. The data from Japan come from Moriguchi and Saez (2005). The data for Sweden are from Roine and Waldenstrom (2006). The data for Spain are from Alvaredo and Saez (2006). We use Leigh's (forthcoming) adjusted series for all 13 countries.

Top10 is equal to the percentage of national income earned by the top 10% of income earners. The original source for this data is Atkinson and Piketty (forthcoming) for all countries except Japan, Sweden and Spain. The data from Japan come from Moriguchi and Saez (2005). The data for Sweden are from Roine and Waldenstrom (2006). The data for Spain are from Alvaredo and Saez (2006). We use Leigh's (forthcoming) adjusted series for all 13 countries.

Top10-1 is equal to the percentage of national income earned by the top 10% of income earners less the percentage of national income earned by the top 1% of income earners divided by 100 minus the income earned by the top 1%, all multiplied by 100. The source for this data is Atkinson and Piketty (forthcoming) for all countries except Japan, Sweden and Spain. The data from Japan come from Moriguchi and Saez (2005). The data for Sweden are from Roine and Waldenstrom (2006). The data for Spain are from Alvaredo and Saez (2006). We use Leigh's (forthcoming) adjusted series for all 13 countries.

Trade Openness is equal to the sum of total imports and exports in current US dollars (millions) divided by GDP in current US dollars (millions). The source for this variable is Barbieri (2002).

Union Density is equal to the percent of the total dependent labor force that are members of unions (less the self-employed). The variable is spliced from several series. The primary source for after 1950 is Golden and Wallerstein (2006b) The primary source before 1950 is Kjellberg (1983). When both these sources were missing, we used Visser (1989). Additional sources used were OECD (2004); May, Walsh, Harbridge, & Thickett (2003); and Wallace (2003).

Universal Suffrage is equal to 1 for those country-years for which the country had universal suffrage (male and female) and 0 otherwise. We used the information in Mackie and Rose (1982) and Caramani (2000) to determine whether a country had universal suffrage in a given year. The more detailed information in Caramani was the preferred source and we used Mackie and Rose (1982) for those countries not covered by Caramani.

Wage Bargaining Centralization is equal to 1 if wages are primarily determined in a decentralized setting, either without collective bargaining at all or bargaining at the plant level, equal to 2 if wages are primarily determined at the industry-level, and equal to 3 if there is national centralized wage setting. Each country-year was coded based on data from a number of sources including Campbell (1992); Ebbinghaus and Visser (2000); Blum (1981); Wallerstein, Golden, & Lange (1997); Iversen (1999); OECD (2004); Swenson (1989, 2002); and Golden and Wallerstein (2006b). Table 2 reports the value of this variable for each country-year.

Country	Coding for *Wage Bargaining Centralization*
Australia	1 1900-1953 2 1954-1991 1 1992-2000
Canada	1 1900-2000
France	1 1900-1950 2 1951-2000
Germany	1 1900-1932 3 1933-1945 1 1946-1950 2 1951-2000
Ireland	1 1922-1945 3 1946-1980 1 1981-1986 3 1987-2000
Japan	1 1900-2000
Netherlands	1 1900-1926 2 1927-1945 3 1946-1959 2 1960-2000
New Zealand	2 1900-1989 1 1990-2000
Spain	1 1900-1937 3 1938-1985 2 1986-2000
Sweden	1 1900-1904 2 1905-1951 3 1952-1982 2 1983-2000
Switzerland	1 1900-1944 2 1945-2000
United Kingdom	1 1900-1913 2 1914-1979 1 1980-2000
United States	1 1900-2000

Table 2: Coding for *Wage Bargaining Centralization*

A.1.2 Descriptive Statistics

Variable	Mean	Standard Error of Mean
Centralized Wage Bargaining	0.142	0.023
Decentralized Wage Bargaining	0.437	0.033
Female Participation	0.309	0.010
GDP per capita	9.475	0.387
Left Executive	0.392	0.027
Non-Democracy	0.085	0.018
Secondary Education Share	0.387	0.019
Top1	10.684	0.258
Top10	34.859	0.364
Top10-1	27.178	0.239
Trade Openness	40.282	1.635
Union Density	34.608	1.065
Universal Suffrage	0.768	0.028
Wage Bargaining Centralization	1.704	0.047

Table 3: *Summary Statistics.* Multiple-imputation estimates of the mean of each variable and the standard error of this estimate. The unit of observation is the 5-year periods from 1916 to 2000 used in the main analyses in the paper. There are 219 observations for each variable.

A.2 Unit Root Tests

One important issue for our empirical tests is whether our top income share variables have a unit root. This would preclude a standard regression in levels, unless we could identify a variable or set of variables with which the top income share is cointegrated. One problem with conventional unit root tests is that unless conducted over a long span of data in terms of time, then they will have low power when attempting to distinguish between a series that has a unit root and a series that does not have a unit root but which is highly persistent.[1] One way to increase the power of such a test is to consider a longer span of data, though considering a longer time period increases the risk that there is a structural break in the series, which should bias the test against a finding of stationarity. A complementary possibility is to increase the span of data by conducting the test on a panel of countries.[2] We pursued this approach using the panel unit root test proposed by Maddala and Wu (1999). The Maddala-Wu test combines the significance levels from independent unit root tests for each country series in order to generate

[1] Shiller and Perron (1985) demonstrated that increasing the time span has a dramatic effect on the power of a unit root test while increasing the frequency of observations within a set time span does not.

[2] See Breitung and Pesaran (2005) for a discussion of unit roots in panels.

a statistic for testing against the null that all country series are non-stationary. In our case we used the Philips-Perron test to generate the individual country statistics. Unlike a related test proposed by Im, Pesaran, and Shin (2003), the Maddala-Wu test has the advantage of being possible to implement in an unbalanced panel.[3] Since a test of this type is dependent on the assumption that the individual country test statistics are independently distributed, before performing the test we first demeaned each country series by subtracting the period average for the given top income share. Based on the Maddala-Wu test, we rejected the null that all series are nonstationary for each of the three series.[4] It should be emphasized that we can only conclude from these tests that a significant fraction of the thirteen country series used here is stationary, not that all individual country series are stationary. With this word of caution, we will proceed by estimating regressions in levels. In addition to the test results reported above, one further motivation for this choice is that if we remain uncertain whether the top income shares variables have a unit root, we do know that any bias generated by the presence of a unit root would be a bias in favor of finding that variables like partisanship and the centralization of wage bargaining are significantly correlated with top income shares. Since the principal empirical finding of this paper is that there is less evidence of correlations between these variables and inequality than has been previously believed, any failure on our part to take account of the top income share series being I(1) would only reinforce our conclusions.

A.3 Multiple Imputation

Upon constructing our dataset, there were non-trivial numbers of missing observations for various variables for particular countries and years. The standard approach of deleting cases that have missing values for any of the variables—known as "listwise deletion"—can create two major problems for inference. One is inefficiency caused by throwing away information relevant to the statistical inferences being made. Furthermore, inferences from listwise-deletion estimation can

[3] Note that the stationarity tests discussed here used observed data only and did not employ any imputation methods for missing data such as those discussed and employed below for our main regression analyses. Due to the extent of missing data for Spain, it is omitted from the stationarity diagnostic tests.

[4] For the *Top 1* share $\text{Chi}^2(22) = 38.2$ p=0.01. For the *Top 10* share $\text{Chi}^2(20) = 34.9$, p= 0.02. For the *Top 10-1* share $\text{Chi}^2(20) = 34.3.3$, p< 0.02.

be biased if the observed data differs systematically from the unobserved data.

The most general and extensively researched approach for dealing with a missing data problem like this is "multiple imputation" (King et al 2001, Schafer 1997). Multiple imputation requires a relatively weak assumption in this context that the process generating the missing data is random conditional on the data included in the imputation procedures (this is commonly referred to in the literature as assuming the data are *MAR)*. Multiple imputation yields consistent coefficient estimates and gives correct uncertainty estimates under the *MAR* assumption.[5]

The approach has several variations but always involves three main steps. First, some algorithm is used to impute values for the missing data. In this step, m ($m>1$) "complete" data sets are created consisting of all the observed data and imputations for the missing values. The second step simply involves analyzing each of the m data sets using standard complete-data statistical methods. The final step combines the parameter estimates and variances from the m complete-data analyses to form a single set of parameter estimates and variances. Importantly, this step systematically accounts for variation across the m analyses due to missing data in addition to ordinary sample variation.

The first step in our multiple-imputation procedures was to create imputations in the missing data cells for all the variables included in the analysis plus some additional information that we determined would be helpful in predicting the missing data for a total of 17 variables. The imputations were conducted on the country-year dataset with 1,159 observations before taking five-year averages. The imputation model also included a lagged and lead value by one year of the *Top1* and *Top10* variables as well as country fixed effects. The imputation model was multivariate normal with a slight ridge prior.[6] Altogether we imputed 10 complete data sets. The exact imputation algorithm we used is Honaker and King's bootstrapping-based EM algorithm (2006).[7]

We evaluated the quality of imputations in a number of ways. For example, we checked to see

[5] It is also necessary to assume the parameters describing the missing data process are distinct from parameters of the data model so that the missing data mechanism is ignorable.

[6] Specifically, the ridge prior was set to 29 which is 2.5% of the 1,159 country-year observations in the imputation model.

[7] The imputation procedures were implemented using *Amelia II: A Program for Missing Data* (Honaker, King, and Blackwell 2006).

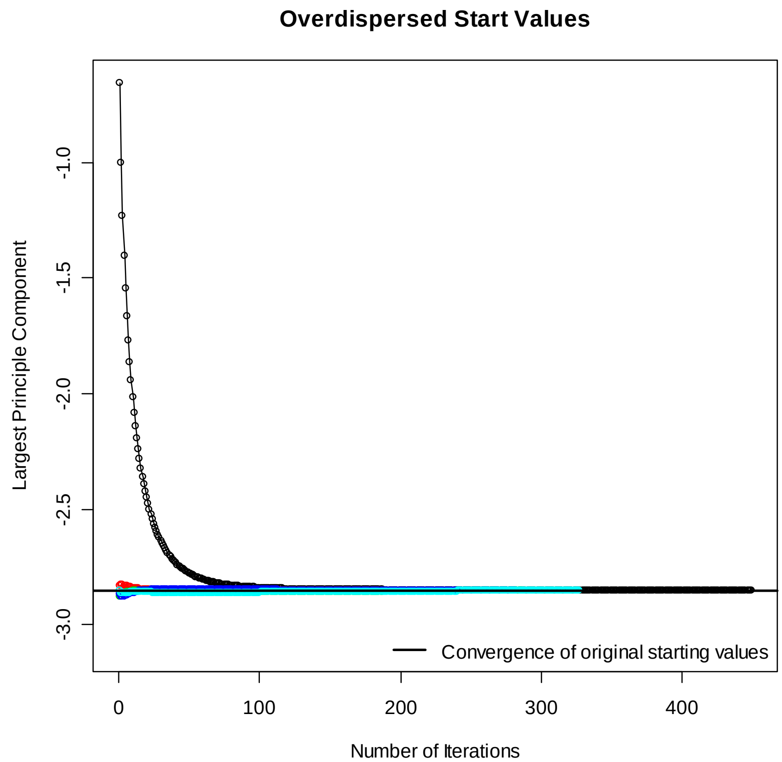

Figure 1: *Convergence to Single Mode.* This figure plots a summary statistic of the parameter space defined by the largest principle component against the number of iterations for 5 EM chains from different starting values.

whether there was evidence that the likelihood for the imputation model was poorly behaved. If it were, the EM algorithm might have problems finding a global maximum and the starting values could affect the imputations. To check this, we ran the EM algorithm from multiple, dispersed starting values and evaluated whether the resulting EM chains converged to the same value. To represent the high dimensional parameter space, we evaluated convergence relative to the largest principle component of the final mode (see Honaker, King, and Blackwell (2006) for further description). Figure 1 plots the largest principle component against the number of iterations in each of 5 EM chains from different starting values. All the EM chains are converging to the same mode which indicates that the imputations are unlikley to be sensitive to the starting values for the EM algorithm.

Although it is impossible to check whether the imputations are close to the unobserved values, it is nonetheless useful to provide some indication of how well the imputation model fits the data. One method for doing this is overimputing. This diagnostic sequentially treats each of the observed values of a variable of interest as if it had actually been missing. For each observed value, a confidence interval can be constructed indicating what the distribution of imputations would have been for that observation had it actually been missing. Then the observed value can be compared with this interval to determine whether these intervals generally include the observed value. If so, this is evidence that the imputation model fits the data. Figures 2 and 3 plot the observed and imputed values for the variables *Top10* and *Top1* respectively. The lines indicate the 90 percent confidence intervals for the imputations with the mean imputation indicated by the dot. The color of the line reports the fraction of missing observations in the pattern of missingness for that observation. For the *Top10* variable, Figure 2 shows that the imputations are relatively tightly distributed around the observed values indicating a good fit.to the data. For the the *Top1* variable, Figure 3 shows that there is greater uncertainty associated with the imputations as indicated by the longer 90 percent confidence intervals. Further, the model does not do very well at predicting cases for which the percentage of income to the top one percent is extremely high. That said, about 90 percent of the confidence intervals contain the observed value and so the model fits reasonably for this variable as well.

The second step in our multiple-imputation analysis was to run the ordinary least squares regressions described above separately on each of the 10 final data sets. The last multiple-imputation step was to combine the 10 sets of estimation results to obtain a single set of estimated parameter means and variances. The single set of estimated means is simply the arithmetic average of the 10 different estimation results. The single set of estimated variances is more complicated than a simple average, because these variances account for both the ordinary within-sample variation and the between-sample variation due to missing data. See King et al. (2001) and Schafer (1997) for a complete description of these variances.

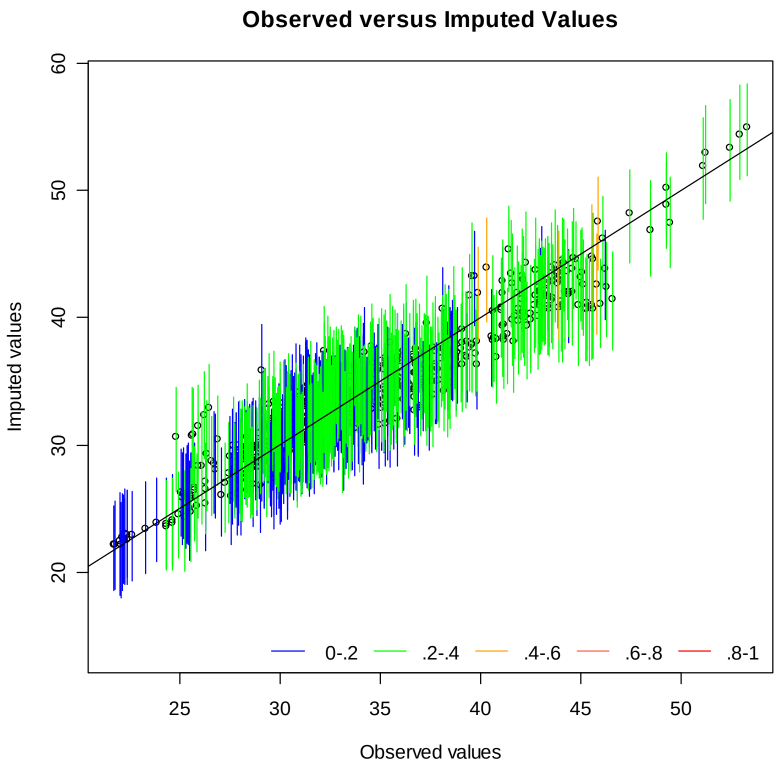

Figure 2: *Observed versus Imputed for* Top10*.* Lines indicate 90 percent confidence intervals for imputations and dots indicate mean imputations. The color of the line represents the proportion of missing observations in the pattern of missingness for that observation.

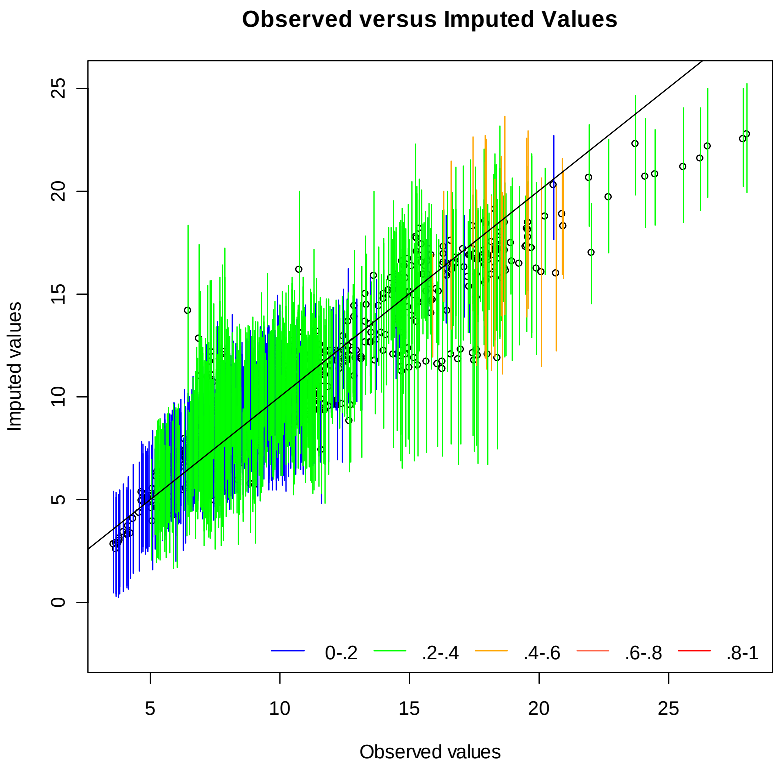

Figure 3: *Observed versus Imputed for Top1.* Lines indicate 90 percent confidence intervals for imputations and dots indicate mean imputations. The color of the line represents the proportion of missing observations in the pattern of missingness for that observation.

A.4 Additional Results

The analysis of the full time series from 1916-2000 has the distinct advantage of allowing a test of the hypothesized role of government partisanship and labor market institutions by identifying off of variation over time. These factors vary significantly over time in our data and it is reasonable to expect these changes to be correlated with changes in income inequality over time. Perhaps the most obvious objection to the analysis is that the results may be biased due to poorly measured variables. In this subsection, we revisit each hypothesis by examining evidence from 1950-2000 using alternative measures, arguably better measured, that are available for this period.

To measure wage bargaining centralization for this period, we use Golden and Wallerstein's coding (2006b). The variable *Bargaining Level–GW* is equal to 1 for plant-level wage setting, 2 for industry-level wage setting, 3 for central wage setting without sanctions, and 4 for central wage setting with sanctions. Following our main analysis, we constructed two dichtomous indicator variables based on the values of this index. *Centralized Wage Bargaining–GW* is equal to 1 if *Bargaining Level–GW* is equal to 3 or 4 and is equal to 0 otherwise. *Decentralized Wage Bargaining-GW* is equal to 1 if *Bargaining Level–GW* is equal to 1 and is equal to 0 otherwise. We also include the *Union Density* variable described above. To measure government partisanship, we use McDonald's (2002) classification of governments on a left-right scale constructed using data produced by Budge et al. (2001) that measures partisan political orientation based on party manifestos. The variable *Government Partisanship* is equal to the government's left-right position as determined by the weighted (by seats in parliament) left-right positions of the parties in government. Note that this variable increases as governments move to the right and so we expect positive coefficient estimates for the variable (the opposite of expectations for the *Left Executive* measure).

In addition to better measured variables for our key hypotheses, it is also possible to use alternative measures of the overall skill levels in a country. The first is *Education Years* equal to the average years of schooling in a country and the second is *Migrant Share* equal to the percent of the population that is foreign born.[8]

Our empirical objective for this analysis is twofold. First, we want to replicate, to the extent possible, the results reported in the previous literature using our measures of income inequality. To do this, we focus on the period from 1976-2000. Second, we want to evaluate whether these findings are also evident in the earlier period from 1951-1975. This will allow us to assess generally the robustness of the results reported for 1916-2000 and to evaluate specifically the extent to which the results from the full time series are biased by poor measurement.

For the 1976-2000 analysis, the results for the government partisanship hypothesis generally mirror the findings in the current literature. Income inequality increases as governments move to the right. Table 4 reports the relevant coefficient estimates for specifications without fixed effects. Across all three measures of income inequality, the estimates for 1976-2000 for *Government Partisanship* are positive and marginally statistically significant.[9] Furthermore, in unreported specifications with fixed effects, the coefficient estimates are also positive and at least marginally statistically significant for *Government Partisanship*.[10] There is evidence consistent with the hypothesis that governments of the left and the right are correlated with levels of income inequality for the 1976-2000 period. Whether this correlation is due to a causal relationship is, as discussed above, not altogether clear, but the correlations that are predicted by partisan theories are evident in the data for this recent period.

Again focusing our attention on the 1976-2000 period, there is also some evidence of a correlation between wage bargaining institutions and income inequality. The coefficient estimates for *Decentralized Wage Bargaining-GW* reported in Table 4 are positive and statistically significant indicating that decentralized wage setting is associated with greater inequality. It is worth mentioning, however, that in unreported specifications using fixed effects, the coefficient estimates for the *Decentralized Wage Bargaining-GW* variable are small in magnitude and not

[8] Our specification here drops the variable *Secondary Education Share* due to the inclusion of these variables. The source for *Education Years* is Barro and Lee, `http://www.nber.org/pub/barro.lee/`. The source for *Migrant Share* is the United Nations, Population Division of the Department of Economic and Social Affairs of the United Nations Secretariat, Trends in Total Migrant Stock: The 2005 Revision, `http://esa.un.org/migration`.

[9] The p-values are 0.078, 0.056, and 0.115 for the dependent variables *Top10-1, Top10,* and *Top1* respectively.

[10] The p-values are 0.055, 0.042, and 0.083 for the *Top10-1*, *Top10*, and *Top1* measures of income inequality respectively.

statistically significant. This test, however, relies on a short time series with limited over time variation in the wage bargaining measure. With or without fixed effects, there is no evidence of a signficant partial correlation between *Centralized Wage Bargaining–GW* and any of the three measures of income inequality, consistent with the results reported in the main analysis.

Further, there is evidence of a negative partial correlation between the union density variable and the *Top10-1* and *Top10* measures of income inequality in the 1976-2000 period. This correlation is also observed in unreported results including fixed effects for each country. These estimates strengthen the evidence that union density is associated with lower levels of income inequality.

Finally, to shed further light on the question, we examine an analogous set of estimates for the 1951-75 period, again using the improved measures available for the last half of the twentieth century. Perhaps the most striking result in these analyses is that the results consistent with a government partisanship effect for the 1976-2000 period disappear. Table 4 reports the coefficient estimates for *Government Partisanship* for the 1951-1975 period. The coefficient is the wrong sign for two out of three of the income inequality measures and statistically insignificant for the third.[11]

The findings for our measures of labor market organization are also informative. Again, there is little evidence of a signficant partial correlation between *Centralized Wage Bargaining–GW* and income inequality.[12] The coefficients for *Decentralized Wage Bargaining-GW* are positive and marginally statistically significant in the specifications without fixed effects for the *Top10-1* and *Top10* measures of income inequality.[13] The magnitude of these estimates, however, are about half the size as those for the 1976-2000 sample and less precisely estimated. In these same specifications, the estimates for the *Union Density* variable are negative as hypothesized

[11] A small caveat to this finding is that in one of the three unreported fixed effects specifications, there is some evidence of a positive and significant partial correlation for the partisanship measure. This estimate is for the *Top1* measure and resonates with the results in the main analysis that if there is a partisan effect at all it is probably on the incomes to the very highest earners. Further, it is again important to keep in mind that the fixed effects results here rely on fairly short time series.

[12] The partial exception here being a negative, significant coefficient in the specification without fixed effects for the *Top1* measure. But again, none of the coefficients for *Centralized Wage Bargaining-GW* are significant in the specifications with fixed effects including the one for the *Top1* measure of inequality.

[13] These results also are not robust to the inclusion of fixed effects.

and at least marginally statistically significant for the *Top10-1* and *Top10* variables.[14] Overall, there is modest evidence for this period consistent with the hypothesis that more organized (particularly unionized) labor markets are associated with lower levels of inequality, but the evidence is weaker than for the 1976-2000 period.

[14] This result does hold up in the fixed effects specifications.

	Top10-1		*Top10*		*Top1*	
	1951-75	1976-00	1951-75	1976-00	1951-75	1976-00
$TopX_{t-1}$	0.660	0.578	0.516	0.661	0.658	0.911
	(0.133)	(0.096)	(0.123)	(0.100)	(0.149)	(0.083)
GDP per capita	-0.098	-0.090	-0.092	-0.062	0.008	-0.009
	(0.089)	(0.082)	(0.089)	(0.102)	(0.055)	(0.056)
Trade Openness	-0.002	0.013	0.016	0.010	0.011	-0.003
	(0.014)	(0.008)	(0.016)	(0.010)	(0.008)	(0.005)
Education Years	-0.075	-0.015	-0.218	0.014	-0.173	0.066
	(0.464)	(0.292)	(0.461)	(0.401)	(0.249)	(0.216)
Migrant Share	0.034	-0.034	0.026	-0.033	-0.004	-0.004
	(0.103)	(0.078)	(0.110)	(0.101)	(0.079)	(0.048)
Female Participation	-3.365	-5.769	-3.699	-6.950	-0.309	-0.876
	(9.260)	(6.628)	(10.834)	(8.243)	(5.844)	(4.422)
Centralized Wage Bargaining–GW	0.114	-0.035	-0.755	-0.185	-1.118	0.280
	(0.811)	(0.618)	(0.698)	(0.909)	(0.428)	(0.454)
Dec*entralized Wage Bargaining–GW*	0.908	2.172	0.969	2.527	-0.162	1.077
	(0.538)	(0.393)	(0.562)	(0.528)	(0.311)	(0.354)
Union Density	-0.051	-0.046	-0.069	-0.048	-0.005	-0.008
	(0.027)	(0.014)	(0.027)	(0.017)	(0.017)	(0.007)
Government Partisanship	-0.015	0.022	-0.010	0.033	0.008	0.012
	(0.014)	(0.013)	(0.013)	(0.017)	(0.008)	(0.008)
Period Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Country Fixed Effects	No	No	No	No	No	No
Number of Countries	13	13	13	13	13	13
Number of 5-Year Periods	5	5	5	5	5	5
Total Observations	65	65	65	65	65	65

Table 4: Labor Market Institutions, Government Partisanship, and Income Inequality: 1950-2000. Table reports the results of OLS regressions for the three measures of income inequality, *Top10-1*, *Top10*, and *Top1* on *Centralized Wage Bargaining–GW*, *Decentralized Wage Bargaining-GW*, *Union Density*, *Government Partisanship* and various control variables for the 5 5-year periods between 1951 and 1975 and 1976 and 2000. The table reports multiple imputation estimates of the OLS coefficients for each variable and their PCSEs in parentheses. A constant term is included in each regression but not reported in the table.