Political Institutions, Partisanship, and Inequality in the Long Run

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April 2007[1]

[1] We would like to thank Thomas Piketty for useful suggestions at the point we were developing this paper, as well as for sharing the data on top income shares. Jesper Roine and Daniel Waldenstrom kindly provided us with the data from their paper on top incomes in Sweden and Jonas Pontusson provided us with union density data. We would also like to thank Jim Alt, Thad Dunning, Miriam Golden, Peter Katzenstein, David Mayhew, Adam Przeworski, Ron Rogowski, and seminar participants at New York University, Notre Dame, Wisconsin, and Yale for comments on an initial draft. Xiaobo Lu and Kaj Thomsson provided invaluable research assistance.

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. The emergence of centralized wage bargaining and a predominance of left political parties may in turn, it is argued, be favored by an electoral system based on proportional representation. There is empirical evidence for the period since 1970 which shows that each of these variables is correlated with income inequality. We make use of new data on top income shares to test these propositions over a much longer time period, nearly the entire twentieth century. Our empirical results provide little support for the idea that centralized wage bargaining, left government, or proportional representation are 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. We argue that our results suggest that there were alternative institutional paths to labor and capital accommodation during most of the twentieth century, and it seems likely that commonly shared economic and political events, such as world wars and economic crises, may be more important for understanding the evolution of income inequality than 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 both by compressing wage differentials and by influencing division of profits between capital and labor. Factors like partisanship may influence the pre-tax income distribution in a dynamic fashion if governments of the left 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 in recent years but not others is of obvious substantive importance. Second, 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 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 and proportional representation are correlated with levels of income inequality for periods prior to the 1970s. We also consider the

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

effect of government partisanship over the long run. Use of long-run time series to consider political institutions and income inequality seems important for at least two reasons. First, 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. Convincing political economy hypotheses ought to be able to account for both cross-country variation and this important variation over time. Second, by considering a longer time span we are able to examine whether important 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. We would certainly expect this to be the case if the institutional hypothesis is to prove convincing.

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 by Atkinson (2005a), Piketty (2005), and Piketty and Saez (2006). Section 3 then considers three separate 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, and we suggest why we might expect this to be the case both with regard to disparities between the middle and bottom of the income distribution as well as between the top of the distribution and the rest. We also discuss a related argument that trade unionism may reduce inequality. Finally, our third hypothesis suggests that if, as has been argued, a system of proportional representation contributes both to the emergence of centralized wage bargaining and to political dominance of the left, then we should logically expect proportional representation to be associated with lower levels of inequality.

In section 4 we report the results of our empirical tests. The hypothesis that proportional representation is associated with lower income inequality receives little support when considering the long-run, and while there is evidence for the 1975-2000 period that partisanship is correlated with top income shares, this result disappears once we consider data from earlier periods. We also find little evidence of a negative correlation over the long-run between wage bargaining centralization and top income shares. We do, however, find evidence of a negative correlation between union density and income inequality that is robust to the inclusion of both country fixed effects and common time effects.

In Section 5 we use 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, often said to be a response to economic crisis, 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 up to 1970, the downward 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.

Our empirical results call for a reconsideration of existing conclusions regarding the effect of government partisanship and proportional representation on income inequality. They also call for a reinterpretation of the idea that an institution like centralized wage bargaining can have a causal effect on income inequality. In Section 6 we develop this point further by considering how centralized wage bargaining and low inequality each may have emerged as part of an underlying economic process, or as part of an underlying political process. Given the similarity in pre-1970 inequality trends between countries with and without centralized wage bargaining, we also discuss the possibility that the effect of labor market institutions on inequality has depended less on the formal degree of bargaining centralization than on other, more subtle features of the labor market that may have been present in countries like both the United States and Sweden during the 1950s and 1960s.

Finally, in concluding the paper we also suggest that while the comparative political economy literature that uses cross country data has, because of data limitations, generally considered outcomes from only a few recent decades, our results point to the importance of extending these inquiries back in time. They also indicate the importance of work like Katzenstein (1985), Swenson (1989, 2002), and Iversen and Soskice (2005) that examines the historical context in which corporatist bargaining arrangements emerged. In data sets focusing on only two or three decades, institutions often change little, making it difficult to distinguish whether correlations between institutions like wage bargaining centralization and outcomes like inequality are spurious or instead reflect causal relationships. A longer run investigation provides more opportunity to investigate institutions when they change, it allows for better consideration of how institutions might be endogenous, and ultimately it should also provide more opportunities for considering how to econometrically identify the causal effects of institutions.

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 income earned by those at the top end of the income distribution in each country.[2] A number of papers on individual countries have already been published using this method for measuring income inequality including Piketty and Saez (2003) on income inequality in the United States and the

[2] See Atkinson and Piketty (forthcoming), Atkinson (2005a), Piketty (2005), Piketty and Saez (2006), and Saez (2004). This section draws heavily on these pieces.

two publications by Piketty on income inequality in France (Piketty, 2003, 2001).[3] 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 two considerable advantages over existing measures of income inequality that are based upon household surveys. For one, 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. Some of the series include capital gains income while others do not.[4] The second main advantage is that 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.

One constraint imposed by using tax data to measure inequality is that since prior to World War II in most countries only a small fraction of households were subject to income taxation, it is possible 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 provide us with a direct measure of developments at the bottom of the income distribution, such as the possibility of a growing gap between the bottom 20% and the median. This issue needs to be kept in mind during our subsequent analysis. It is possible that in order to explain the evolution of top incomes, and in particular very top incomes (such as the top 0.1%) one may want to draw on additional theories such as those that have been applied to explain trends in compensation for CEOs.[5]

Another constraint on our analysis here is that while several papers from the top income shares project provide separate series for capital income and wage income, allowing one to more

[3] Other contributions include Saez (2005), Dell (2005), Moriguchi and Saez (2005), Atkinson (2005b), and Roine and Waldenstrom (2006).

[4] The series from the UK, Australia, New Zealand and Germany include capital gains. The nine remaining series used here do not include capital gains income. Roine and Waldenstrom (2006) show that for Sweden when capital gains are included one actually observes a significant increase in inequality since the 1980s.

[5] See Atkinson (2003) for an extended discussion of this issue. Further, in this paper, we augment our analysis of top income shares with examination of wage inequality measures that are available for some countries for much of the twentieth century.

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.

In our empirical analyses below we will estimate the determinants of the income share for the top 1%, the top 10% and the top 10% minus the top 1% (the 90th to 99th percentiles of the income distribution). The 90th percentile-10th percentile ratio is used as a dependent variable in many comparative political economy studies of inequality and we should expect the top 10% income share to be closely correlated with the 90/10 ratio. Examining the determinants of the top 1% income share allows us to ask whether factors like government partisanship are relevant for examining changes at the very top of the income distribution. This is substantively important because papers on inequality trends for the US, France, Japan, and Sweden all point to the fact that changes in the share of income earned by the top 1% account for the lion's share of change observed in overall levels of income inequality over the course of the twentieth century.[6]

Currently, data on top incomes is available for thirteen advanced industrial countries.[7] Figure 1 presents the data 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 UK and the United States when compared with a number of continental European countries like Germany or France. 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. This is potentially troubling for comparative political economy explanations which suggest that certain relatively static features

[6] Roine and Waldenstrom (2006) on Sweden, Moriguchi and Saez (2005) on Japan, Piketty and Saez (2003) on the US, and Piketty (2003) on France.

[7] Australia, Canada, the United Kingdom, France, Germany, Ireland, Japan, the Netherlands, New Zealand, Switzerland, the United States, Sweden, and Spain. In addition, in our individual country analyses in Section 5 we also use a long-run measure of income inequality in Denmark that is not a top income measure. We do not use this Denmark series in our pooled regressions.

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 with the top incomes data 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 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. Piketty and Saez (2003), Piketty (1998), and Moriguchi and Saez (2005) document that for countries like France, the United States, and Japan the post-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. Another possibility is that in addition to reducing income inequality, the negative shock to top incomes around the time of World War II also altered the politics of redistribution, helping to make the shock permanent.

Overall, the very significant over-time variation we observe in top income shares over the course of the twentieth century also 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

We focus on three political hypotheses about the determinants of income inequality, each of which has received empirical support based on data from the period since 1970. As described above, these involve the effect on inequality of labor market institutions, government partisanship, and electoral rules. Our goal is to investigate whether there is empirical support for each of these hypotheses when considering data over a longer time horizon. 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 cross-country inequality trends. Economic theory emphasizes the importance of the distribution of skills within and across countries, and authors have argued that factors like skill-biased technical change, migration, and openness to trade may be important determinants of inequality.[8]

[8] See Acemoglu (2003) and Glaeser (2006) for review articles. Becker and Gordon (2005) suggest that too much emphasis has been placed on skill-biased technical change as opposed to phenomena that might better explain developments at the very top of the income distribution, such as the "economics of superstars". Williamson (1997) provides historical evidence on the importance of factors like migration for explaining inequality trends.

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 before 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. 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.

It should be noted that if one observes empirically that there is a correlation between a measure of government partisanship and income inequality, there nonetheless may be difficulties in attaching a causal interpretation to this finding. First, one needs to ascertain that a measure of partisanship is actually independent of the redistributive policies that are thought to have influenced the subsequent distribution of income. If instead knowledge of the policies subsequently implemented has influenced the classification of a government on a left-right scale, then there would be an obvious bias. Leaving this question aside, any observation that governments of the left tend to be associated with lower levels of income inequality immediately begs the question of what factors lead to political parties of the left controlling government in some countries at certain times but not in others. This is obviously a very broad question, but one possibility we consider below is that strength or weakness of the left is influenced by electoral rules, and in particular the presence or absence of proportional representation.

[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. It should be noted that the existing results with regard to partisanship and tax progressivity are based on use of the top marginal tax rate as a proxy for the overall progressivity of the system. Piketty and Saez (2006b) show that a careful calculation of tax progressivity can render surprising results.

Our empirical tests will consider two alternative measures of government partisanship. The first is a classification of governments on a left-right scale that has been produced by McDonald (2002) using data produced by Budge et al. (2001) that measures partisan political orientation based on party manifestos. This data is available for the period since 1950, and it has the advantage of classifying parties according to a common metric. For the pre-1950 period we lack either manifestos-based measures of partisan orientation or those based on expert surveys. As a simple alternative, we have produced a dummy variable that takes a value of 1 for each year in which a country's head of government was from a party on the left of the political spectrum. As one would hope, it turns out that there is a high pairwise correlation between these two variables for the post-1950 years (0.55), and as a result we have some confidence that our dummy variable is a useful proxy for differences in partisan control.

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 all as between different categories of wage earners. Authors like Wallerstein (1999) and Rueda and Pontusson (2000) have found strong 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.[10] The literature has 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

[10] 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.

higher salaried employees, even if these employees do not officially participate in the centralized arrangement.[11] 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 crisis of the 1930s.[12] Sweden subsequent to the Saltsjöbaden agreement of 1938 is an emblematic example here. 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 structural break in inequality.

If centralized wage bargaining primarily has an effect on reducing wage dispersion, then we might expect it to matter most immediately for income inequality below the top of the income distribution, rather than for the top 1%, where capital income will be a larger share of total income. Centralized wage bargaining arrangements should logically have more importance for explaining the share of income earned by the top 10% of the distribution. However, it also seems very likely that wage bargaining centralization should serve as a good proxy for the presence of institutions that have a broader effect on reducing income inequality in a society, even when we are considering the share of income earned by the top 1% of the distribution. In many countries where centralized wage bargaining emerged, it arose as part of a national accord between unions and employers regarding distributional issues both between wage earners in different sectors and between wage earners and owners of capital. Sweden after the Saltsjöbaden agreement is again an emblematic case. Przeworski and Wallerstein (1988) have presented a model investigating how centralized bargaining by workers (they refer to centralized union confederations) can alter the share of total surplus directed towards wage earners, which should imply a reduction in income inequality.[13] It is recognized that wage bargaining centralization and "corporatist"

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

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

[13] There are actually two counteracting effects here, because a centralized union confederation may have more bargaining power, and thus be able to extract a greater share of the surplus, but it will also internalize the macroeconomic effect of its own wage demands, which can be a force for wage moderation. See also Rueda and Pontusson (2000) and Swenson (1989) on the relevance of centralized wage bargaining for this issue.

bargaining more generally have tended to go hand in hand, and corporatist bargaining is often suggested to create pressures towards equalization of incomes. However, it should be noted that it remains to be specified through exactly what causal mechanism this would operate (other than the one identified by Przeworski and Wallerstein (1988)).

In order to measure the degree of wage bargaining centralization across countries and over time, we will make use of data collected by Golden, Lange, and Wallerstein (2006b) that cover the 1950-2000 period. For the pre-1950 period, since we believe that it is particularly interesting to consider the impact of centralized wage bargaining when it was first introduced, we have used several sources to code our own index of the degree of wage bargaining centralization. Our own measure turns out to be highly correlated with the Golden, Lange, and Wallerstein measure (pairwise correlation coefficient of 0.63). One important feature of the wage bargaining arrangements is that if one considers only recent decades, with a few notable exceptions, the overall picture is one of relatively static cross-country differences. Based on the variable we have coded, for the period 1975-2000, 88% of the variance is accounted for by cross-sectional as opposed to within country variation. This means that in any regression framework we will be essentially unable to distinguish between the effect of wage bargaining centralization and the effect of unobserved country heterogeneity. When we consider the 1916-2000 period, however, the portion of the variance in wage bargaining centralization accounted for by cross-sectional variation drops to 44%. So, an empirical investigation over a long time horizon will allow us a test of whether wage bargaining centralization matters, even when controlling for unobserved country heterogeneity.

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. The tendency of unions to bargain for across-the-board wage increases, rather than differential increases for each worker, should also reduce pay inequalities over time. The extent to which these outcomes are attributable to the formal arrangements implied by unions, as opposed to the norms or expectations promoted by unions is an open question.[14] There is cross-country evidence for recent decades showing a negative correlation between union density and pay inequality. There is also micro-econometric evidence that unions reduce wage dispersion (Card, 1996; Blau and Kahn 1996). However, it should also be emphasized that Blau and Kahn (1996) find that union membership has a most pronounced effect on reducing earnings dispersion between the middle and bottom segments of the income distribution. In our empirical analysis to follow we use data on union density from the Golden and Wallerstein (2006b) dataset, as well as data collected by Kjellberg (1983) covering several countries for the pre-1950 period. One final point regarding union membership is that it 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.

3.3 Proportional Representation

If the arrival of left governments in power and the adoption of centralized wage bargaining institutions are endogenous outcomes, one interesting possibility is that the emergence of both of these outcomes is favored by the presence of proportional representation, rather than a majoritarian system for elections. The argument that proportional representation favors consensual or corporatist politics has a distinguished pedigree and has been emphasized in important works by Lijphart (1968) and Katzenstein (1985). One common element in corporatist systems is to have centralized wage bargaining institutions. A number of authors have also argued that proportional representation will lead to a greater likelihood of observing a governmental coalition that includes parties of the left. This is the case for Iversen and Soskice (2006) who consider a model of redistribution where there are three classes of citizens (indexed according to income) and political parties cannot commit in advance to platforms.[15] Ticchi and Vindigni (2003)

[14] See Swenson (1989) for an emphasis on the latter set of factors.
[15] Iversen and Soskice (2005) consider this argument in a historical context.

have presented a model that leads to a similar conclusion regarding greater likelihood of the left dominating under proportional representation. When considering the weakness of support for redistribution in the US compared to many European countries, Alesina, Glaeser and Sacerdote (2001) have also emphasized the effect that proportional representation can have on the emergence of socialist parties.

It should also be acknowledged that ultimately, electoral rules may themselves be endogenous to broad economic trends. One possibility raised by Rogowski and MacRae (2004) and Ticchi and Vindigni (2003) is that exogenous shifts in returns to skills may simultaneously produce both a shift in pre-tax inequality and a shift in electoral rules. So, for example, if some exogenous economic event leads to a narrowing of the gap between the highly skilled and the unskilled in an economy, then this may simultaneously produce a reduction in income inequality and political pressures for the adoption of proportional representation. Rogowski and MacRae suggest that an exogenous reduction in inequality may explain the shift to proportional representation in a number of industrialized countries during the early twentieth century.

For our empirical analysis, we have accurate information for the entire twentieth century regarding the extent to which elections were based on proportional representation (Golder 2005, Caramani 2000, Mackie and Rose 1991). While there is little variation for recent decades in the set of countries with proportional representation, when we consider a longer time horizon we are able to observe some country variation that may be useful for establishing the potential effects of proportional representation on inequality. We can investigate the effect of proportional representation in those countries that adopted and then abandoned it (France) and in those countries that adopted and abandoned and then adopted it again (Germany). Overall, however, even in our 1916-2000 sample a very significant fraction of the variation in our proportional representation variable remains accounted for by cross-sectional variation (68%). Another problem for analyzing the effect of proportional representation is that in those countries that opted for PR it appeared at a sufficiently early date that we cannot effectively use the top income shares data to examine whether the establishment of PR was associated with a downward structural break in top income shares. In the end, these two issues would pose greater problems for interpretation of our statistical results if we did observe a significant negative correlation between top income shares and proportional representation when omitting country fixed effects from our regressions. As we describe below, this is not the case.

4 Empirical Specification and Results

This section empirically evaluates the evidence for the hypotheses that labor market institutions, government partisanship, and electoral rules are important determinants of income inequality. We document that there is little evidence in our full time series of a significant correlation between our measures of centralized wage bargaining, government partisanship, and electoral rules with any of the three measures of income inequality. We also present evidence, using data from 1916–2000, that there is a significant correlation between trade unionism and three measures of income inequality.[16] We then investigate each hypothesis further with better data for the periods of 1951-1975 and 1976-2000. We find evidence of a partial correlation between wage-bargaining centralization and income inequality in analyses identifying off of cross-country variation. These correlations are most clearly observed for the period 1976-2000 which has been widely studied in the existing literature. Further, we find evidence of a partial correlation between union density and income inequality. In these analyses, partisanship is correlated with income inequality for the 1976-2000 period but not for the earlier 1951-75 period. There is little evidence in these data of a correlation between electoral institutions and any of the measures of income inequality. In summary, in this section we present some evidence of a partial correlation between trade unionism and income inequality but do not find robust evidence consistent with the conjectured role for centralized wage bargaining, partisanship, and electoral institutions in determining income inequality. Throughout this section, our results are based on data from 13 advanced industrial democracies.[17]

[16] 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.

[17] Our cases include Australia, Canada, France, Germany, Ireland, Japan, Netherlands, New Zealand, Spain, Sweden, Switzerland, UK, and the United States.

As discussed above, the three dependent variables for this analysis are *Top 1*, *Top 10*, and *Top10-1* which are equal to the percentage of national income earned by the top 1%, the top 10%, and the top 10% minus the top 1%.

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.[18] 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.[19] 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 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.[20] 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 the top 10% income share, as well as the "top 10%- top1%" share.[21] For

[18] 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.

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

[20] 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.

[21] For the top 10% share $Chi^2(18) = 41.8$, p= 0.001 and when including a time trend in the regressions $Chi^2(18) = 28.9$, p= .0498. For the "Top 10%-Top 1%" share $Chi^2(18) = 52.9$, $p < 0.001$ and when including a time trend in the regressions $Chi^2(18) = 36.7$, $p < .005$.

the top 1% share we rejected the null when not including time trends in the individual country regressions, but the results were less conclusive when including time trends.[22] 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, centralization of wage bargaining, and proportional representation 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.

Based on this evidence, the analyses that follow are estimated in levels. Further, we average our data over 17 five-year periods from 1916 to 2000. Averaging across five-year time periods follows much of the economic growth literature and allows us to examine variation over time without specifying precisely how long it takes for changes in labor market institutions, electoral 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 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.

Upon constructing our datasets, there were non-trivial numbers of missing observations for

[22] $\mathrm{Chi}^2(22) = 35.1$, p= 0.0375 when not including a time trend in the regressions and $\mathrm{Chi}^2(22) = 30.92$, p= .0977 when including a time trend.

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 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.[23]

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.[24] Inclusion of data from multiple top income measures in the model was useful, because in many cases where one top income measure (top 10%, top 1%, etc.) is not observed, another measure is observed, and we can expect these different measures to be highly correlated. Altogether we imputed 10 complete

[23] 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.

[24] The imputation procedures were implemented using *Amelia II: A Program for Missing Data* (Honaker, King, and Blackwell 2006). The imputation model was multivariate normal with a ridge prior. The model also included lagged and lead values of a number of variables as well as country-specific time trends.

data sets. The exact imputation algorithm we used is Honaker and King's bootstrapping-based EM algorithm (2006).

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.

4.1 Income Inequality, 1916-2000

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

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 centralized (national) wage setting. We consulted a number of sources to code each country including Campbell (1992), Ebbinghaus and Visser (2000), Blum (1981) and Golden and Wallerstein (2006b).[25] As discussed above, the literature suggests that more centralized wage bargaining decreases income inequality through a number of possible mechanisms, and so we expect a negative partial correlation between *Wage Bargaining Centralization* and each of the measures of income inequality. 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

[25] See Data Appendix and Table 8 for further description of this variable.

(less the self-employed).[26] 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.[27] 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. Finally, we constructed a dummy variable, *Proportional Representation*, equal to one if elections to the lower house of the legislature were contested under proportional representation rules and equal to zero otherwise. The sources for this variable for the years before 1946 were Mackie and Rose (1991) and Caramani (2000) and for the years after 1946 was Golder (2005). It too is expected to be negatively correlated with 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*, *Wage Bargaining Centralization*, *Union Density*, *Proportional Representation* and our measures of income inequality. For our analysis of the complete data series, we include *GDP per capita* and *Trade Openness* in all regressions.[28] 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. These may be correlated with, and perhaps even determine, our key explanatory variables.

[26] The source for this variable is Golden and Wallerstein (2006b). This variable is missing for the early part of our sample but the imputation model included two alternative union density measures. These measures although not as available for the later years of our sample included data for many country years during the first half of the century. The source for the alternative measures is Kjellberg (1983).

[27] Coded based on information in Caramani (2000) and McDonald (2002). See Data Appendix and Table 7 for further description of this variable.

[28] The sources for this data were Maddison (2003) and Barbieri (2002). Also note that alternative specifications that included additional control variables such as GDP per capita squared, the share of economic activity in agriculture, and the female labor force participation rate yielded similar results to those reported below.

Table 1 reports the coefficient estimates for the regression of each of our measures of income inequality on its one-period lag, the key variables of interest, *Left Executive*, *Wage Bargaining Centralization*, *Union Density*, and *Proportional Representation,* and the controls. For each dependent variable two specifications are reported, one without and one with country fixed effects.

The coefficient estimates in Table 1 indicate 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, while in the expected negative direction, are imprecisely estimated and not statistically significant. Essentially, there is no evidence in this data for a centralized wage bargaining effect for the 1916-2000 period. One potential concern about this finding is that the OLS estimator is biased because 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 wage bargaining 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, we will revisit this finding by looking at individual cases in greater detail including considering, where available, alternative measures of inequality.

The results for the trade unionism measure, however, indicate a consistent negative correlation between union density and our three top income measures. In the specifications without country fixed effects, the coefficient estimates for the *Union Density* are negative and statistically significant at the 0.05 level for the *Top10*, *Top1*, and *Top10-1* measures of income inequality. In the fixed effects specifications in Table 1, the coefficient estimates for *Union Density* remain negative and statistically significant for the *Top10* and *Top10-1* measures. The estimate for the *Top1* variable is also negative but less precisely estimated (p-value equal to 0.17). The inclusion of both country and period fixed effects mean that the estimate is 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.[29] 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. We will evaluate the robustness of this partial correlation further below.

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. Essentially, there is no evidence in this data for a partisanship effect for the 1916-2000 period.

The coefficient estimates in Table 1 for the *Proportional Representation* variable are statistically and substantively insignificant across all specifications for the *Top10* and *Top1* measures and only marginally significant for the *Top10-1* measure (p-values of 0.11 and 0.13). Importantly, however, the main hypotheses about how electoral institutions matter for the distribution of income have to do with its influence on the development of wage bargaining centralization and dominance of the left. Consequently, to test the hypothesis properly, we are primarily interested in whether there is a negative partial correlation between *Proportional Representation* and the income inequality measures in specifications that exclude measures of labor market institutions and government partisanship. Table 2 reports the results for this specification. In the regressions that exclude country-fixed effects, the coefficient estimates for the *Proportional Representation* variable are negative across all three income inequality measures, but only for the *Top10-1* variable is the estimate statistically significant (p-value equal to 0.04). Once country fixed effects are included, the *Proportional Representation* coefficient estimates across all specifications are not statistically insignificant at conventional levels. Again, there is little evidence in this data for a proportional representation effect for the 1916-2000 period.

To evaluate the robustness of these results, we conducted a number of sensitivity analyses.

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

First, we considered the possibility that the cumulative experience of left partisanship, centralized wage bargaining, and proportional representation 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, Wage Bargaining Centralization* and *Proportional Representation* equal to one. In specifications without country fixed effects, the cumulative measure for proportional representation and centralized wage bargaining did perform somewhat better, particularly for the *Top10-1* measure of inequality.[30] However, as suggested above, these estimates are at best weak evidence of the influence of these factors as there are likely unobserved characteristics of these countries correlated with both centralized wage bargaining and income inequality that bias the cross-sectional estimates. Across all three measures of income inequality, none of the cumulative measures are statistically significant in the specifications including country fixed effects.

Second, we estimated specifications that dropped the union density measure to evaluate whether its inclusion might attenuate our estimates for centralized wage bargaining and partisanship. Although we found no evidence of the later, the coefficient for centralized wage bargaining was larger and statistically significant in the analyses without fixed effects and employing the *Top10* and *Top10-1* measures of income inequality. The exclusion of the union density variable, however, did not alter the estimates substantially in the fixed effects analyses.

Third, we reestimated each specification dropping one of our thirteen countries at a time. This did not change our conclusions for partisanship, centralized wage bargaining, and electoral institutions. Particularly in our preferred fixed effects specifications, there is little evidence in our data that our failure to find robust partial correlations between these variables and income inequality is due to a single influential outlier country. In contrast, we found that the significant coefficients for union density reported in Table 1 are somewhat sensitive to the inclusion of all cases. For the *Top10* and *Top10-1* measures, dropping Sweden in the fixed effects specifications results in less precise estimates with p-values equal to 0.15 and 0.14. For the *Top1* measure, the

[30] For the *Top10-1* measure without fixed effects, the cumulative proportional representation coefficient is statistically significant at the 0.06 level and the cumulative centralized wage bargaining coefficient is significant at the 0.10 level. 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.

estimates are sensitive to the inclusion of the UK, US, and Sweden. Dropping any one of these cases results in significantly smaller and less precisely estimated coefficients for union density.[31]

4.2 Income Inequality, 1950-2000

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, labor market institutions, and electoral institutions by identifying off of variation over time. All three 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 the better measures 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. 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). The *Proportional Representation* variable is

[31] Other sensitivity tests conducted included removing the measure of proportional representation from the baseline specification reported in Table 1 and adding control variables for the agricultural share of economic activity and female employment. The results for these specifications were qualitatively the same as those reported in Tables 1 and 2 particularly for the preferred fixed effects specifications. We also estimated specifications for which the measure of wage bargaining institutions was dichotomized. For one specification, the dichtomous variable was set equal to one if the country had national, peak-level bargaining and 0 otherwise while for the other specification an analogous indicator variable for decentralization was included. The centralization variable was not signficant in any specifications and the decentralization variable was not significant in the fixed effects specifications. These results again are consistent with the claim that there is little evidence in this data of a robust correlation between wage bargaining arrangements and income inequality.

identical to the one defined above.

In addition to better measured variables for our key hypotheses, it is also possible to add a fuller set of control variables to the analysis. Specifically, we add two 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.

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 in Tables 1 and 2 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 3 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 statistically significant.[32] Furthermore, in unreported specifications with fixed effects, the coefficient estimates are also positive and at least marginally statistically significant for *Government Partisanship*.[33] 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 certainly evident in the data for this recent period.

Again focusing our attention on the 1976-2000 period, there is also evidence of a negative correlation between wage bargaining centralization and income inequality. For the specifications

[32] The coefficient estimate for *Government Partisanship* in the specification with *Top10-1* as the dependent variable is somewhat less precisely estimated and the p-value for the 1976-2000 specification is 0.102.

[33] The p-values are 0.051, 0.102, and 0.110 for the *Top10*, *Top1*, and *Top10-1* measures of income inequality respectively.

without fixed effects reported in Table 3, the coefficient estimate is negative and statistically significant across all three measures of income inequality.[34] These results are consistent with much of the existing literature that has also used data from this time period.[35] Further, there is evidence of a negative partial correlation between the union density variable and the *Top10* and *Top10-1* measures of income inequality in the 1976-2000 period. This correlation is also observed and even more precisely estimated 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 during this period.

Table 4, excluding the partisanship and labor market variables, reports results evaluating the hypothesis that proportional electoral systems decrease income inequality. For the 1976-2000 period, the coefficient estimates for the *Proportional Representation* variable are negative as hypothesized but are not statistically significant across all three income inequality measures. This finding is replicated in our unreported fixed effects specifications. The later finding, however, is not too surprising given the modest number of changes in electoral institutions for our sample of countries during this short period.

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 3 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. Moreover, in unreported fixed effects specifications for the period, none of the predicted correlations are evident.

The findings for our measures of labor market organization are also informative. Without fixed effects, the coefficient estimates for the *Bargaining Level–GW* variable are negative across

[34] The p-values are 0.027, 0.107, and 0.059 for the *Top10*, *Top1*, and *Top10-1* measures of income inequality respectively.

[35] It is worth mentioning, however, that in unreported specifications using fixed effects, the coefficient estimates for the *Bargaining Level–GW* variable are small in magnitude and not statistically significant. This test, however, relies on a short time series with limited over time variation in the wage bargaining measure.

all three income inequality measures but only statistically significant for the *Top1* measure (p-value is equal to 0.055). In these same specifications, the estimates for the *Union Density* variable are negative as hypothesized and relatively precisely estimated.[36] In the unreported fixed effects specifications for this period, there is little evidence of a correlation between wage-bargaining centralization and the income inequality measures for this period, but the partial correlation for the union density variable remains for the *Top10* and *Top10-1* measures of inequality. Overall, there is certainly some evidence for this period consistent with the hypothesis that more organized (i.e. unionized) labor markets are associated with lower levels of inequality, but the evidence is weaker than for the 1976-2000 period.

The coefficient estimates for the *Proportional Representation* variable reported in Table 4, excluding the partisanship and labor market institutions variables, vary in sign across the three measures of income inequality and are statistically insignificant for the 1951-1975 period. Further, none of the estimates for the *Proportional Representation* variable are significant in the unreported fixed effects specifications for this period.

5 The 1930s as an Institutional Turning Point

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 that either proportional representation or presence of a "left" government have 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 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.

[36] The p-values are 0.007, 0.001, and 0.116 for the *Top10*, *Top1*, and *Top10-1* measures of income inequality respectively.

We have argued that one might expect centralized wage bargaining to be associated with lower top income shares both because of wage compression and because previous authors have highlighted that centralized wage bargaining often occurs as part of national level discussions between employers and unions that should be expected to affect the distribution of both labor and capital incomes in a society. 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 economic crisis of the 1930s 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 prior political conditions, one of which was the 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.

Sweden with the Saltsjöbaden accord of 1938 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 moves to centralized wage bargaining that occurred in the 1930s and 1940s 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 consider 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 either the 1930s or the 1940s.

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 or 1940s. In addition, we also have a separate income inequality series for Denmark that will allow for examining whether the shift to centralized bargaining in 1934 in that country was associated with a downward break in income inequality.[37] Finally, for both Sweden and Denmark 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 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). 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 because it demonstrates that they are highly correlated with the data on top income shares.

[37] 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. This is not a measure of the share of total income accruing to a top segment of the income distribution, and as a result it cannot be directly compared to the other country series used in this paper. It instead is based on an estimate of the "maximum equalization coefficient" which is the share of total incomes that would need to be transferred from those above the mean level of income to those below the mean level of income in order to achieve a completely equal distribution. Even if it is not directly comparable to the top incomes series, the Danish inequality series is potentially useful for investigating the trend in inequality in Denmark over time and whether this was influenced by the establishment of centralized wage bargaining. It should be noted that this inequality measure suffers from the opposite problem when compared with the top income shares. When used as proxies for overall income inequality the latter may miss important trends at the bottom of the income distribution. By focusing only on distributional issues between those above and below the mean, the maximum income coefficient ignores inequality trends within top income groups.

The Swedish national employers federation (SAF) and the union confederation (LO) initiated national bargaining with the Saltsjöbaden accord of 1938.[38] In Denmark a similar national collective bargaining agreement occurred in 1934 between the LO union federation and the DA employers federation (Ebbinghaus and Visser, 2000). 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). 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 2 plots the Top 1% income share for Sweden, indicating the potential break point associated with the Saltsjöbaden accord, and it also makes use of the wage inequality series from Ljungberg (2006). Two things are apparent from this graph. First, the Top 1% share and the wage inequality series are 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 the break date for the centralization of bargaining was set, at a different point, such as the adoption of the Rehn-Meidner plan in the early 1950s.[39]

As an addendum to the above discussion, it should also be emphasized that while income 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. Sweden moved to adopt centralized wage bargaining in 1938; the United States never had a centralized system of wage bargaining, unless one considers wartime price controls between 1941 and 1945. Figure

[38] Note that alternative key dates for the centralization of wage bargaining in Sweden, e.g. 1952 or 1956, lead to qualitatively similar patterns for the relationship between centralization and income inequality as those discussed below focusing on 1938.

[39] Interestingly, there is also evidence that the gender wage gap was also declining in Sweden before equal pay became an official policy. See Svensson (2004).

3 plots the Top 1% income share in the United States and Sweden over the twentieth century. The two series can be described as following a common downward trend 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 top 1% income share and a measure of wage inequality declined after 1938, 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 for Sweden, Denmark, the Netherlands, and Ireland. 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 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.

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

The estimates of the equation are presented in Table 5.[40] There is strong evidence of a downward trend in inequality, but almost no evidence here of an acceleration of this downward trend following the adoption of centralized wage bargaining. The one potential exception to this is Ireland. 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 one would say that wage

[40]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.

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.

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 over time. 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.[41] We performed the Quandt-Andrews test for structural breaks on each of the six series producing results very similar to those we drew based on the Table 5 regressions. With the exception of the top 0.1% series for Ireland, 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 which are supported by data for recent decades (1976-2000) find much less support when looking at top income 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 an earlier period over the short-run (1951-1975) we continue to find little support for the existing arguments. 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 result 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 all governments, irrespective of partisan orientation, implemented highly progressive tax systems.

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

With regard to proportional representation, our results provide little support for the idea that this choice of electoral rule has an identifiable effect on inequality. One important reason for this may be that even if the adoption of proportional representation is, in fact, more likely to favor subsequent adoption of centralized wage bargaining and subsequent control by the parties on the left, neither of these latter two factors seem to have a robust correlation with top income shares over time. It should be emphasized that the top income shares data do not allow us to conduct an effective test to determine whether the initial introduction of proportional representation in a number of countries around the end of World War I was associated with a structural break in inequality, but even so, for the hypothesis to be convincing we would still expect to see a significant difference between PR and non-PR countries in the data we do have available.

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 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, and 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.

A first possibility here is that centralized wage bargaining and income inequality were both endogenous to an underlying *economic* process. 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 would 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. Their argument of course directly accounts for the partical correlation between union density and income inequality observed in our data but suggests that this does not necessarily reflect a causal relationship. A similar formal framework could be developed with regard to centralized wage bargaining. 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 might not be able to explain why, from relatively similar prior levels of inequality in the 1930s, some states chose to adopt centralized wage bargaining while others did not.[42] In addition, this type of model would still presumably predict that we would observe different inequality trends after the 1930s when comparing countries that opted for centralized versus decentralized bargaining institutions. This is not in fact the case

A second possibility is that wage bargaining arrangements and income inequality were both endogenous to a underlying *political* process that we cannot capture with our data. Gourevitch (1986 ch.4) argues that while the crisis of the 1930s and the wartime crisis initially helped set European countries on very different trajectories, the long-term effect was to generate a common response in advanced industrial states, a politics of accommodation between business and labor. While this accord was clearly articulated in a formal fashion in states like Sweden, after 1945 there was a strong similarity in outcomes in the five countries he considers (France, Germany, Sweden, the UK, and the USA) irrespective of their formal institutional arrangements. In sum, we might suggest that by 1945 a desire to avoid future crises helped push business and labor towards a politics of compromise that one might expect to be associated with lower income

[42] Prior to Sweden's adoption of centralized wage bargaining in 1938 the top 1% income share was 12.3%. Prior to the Netherlands adopting centralized bargaining in 1946, the top 1% share was 12.9%. In contrast with these two cases, in the US the top 1% share in 1945 was 11.1% and the analogous figure for Canada was 10.1%. A more systematic regression exercise also reveals that there is little correlation between the initial level of income inequality and the likelihood of adopting centralized wage bargaining.

inequality.[43] In some states this politics of accommodation was characterized by centralized bargaining arrangements, but centralization was not a necessary ingredient for this outcome. This account, while partially convincing, also contains gaps. It does not suggest why income inequality was trending sharply downwards well before 1945. Nor does this line of argument explain why, as a response to crisis, some countries chose to cement political compromises through formal centralized bargaining arrangements while others did not pursue this option. Where Gourevitch's argument does seem to fit the top incomes data particularly well is in focusing on commonalities across countries during one time period (the immediate post-war era), as opposed to emphasizing relatively static institutional differences between countries.

A third and final possibility one might consider is that wage bargaining arrangements do have a causal effect on income inequality, but that in focusing exclusively on the formal degree of centralization and by trying to identify a key moment when institutions changed we have been missing important aspects of the process. If we consider the Swedish example, Swenson (1989 p.50) has emphasized that the Saltsjöbaden accord of 1938 was an important event, but it was "in many respects a codification of existing practices." His evidence also shows that in the decades after 1938 a number of incremental changes occurred in Swedish wage bargaining arrangements. One implication of this observation for our empirical exercise might be to say that we have simply not adopted a sufficiently fine grained empirical measure of centralization. The alternative is to suggest that if Swedish wage bargaining arrangements changed frequently, if they responded to pressures from actors with differing objectives, and if they often involved practices that were not actually codified, then it seems much more natural to speak of these as policy outcomes rather than institutions with causal effects.

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 hy-

[43] We should emphasize that Gourevitch himself does not make any specific predictions about income inequality in his 1986 book.

potheses should able to account for inequality trends in earlier time periods, as well as over a longer time span. 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. When considering wage bargaining centralization, government partisanship, and the presence of an electoral system based on proportional representation, we have found little evidence that the these factors can account for variation in inequality over the long-run, particularly when we control for unobserved country effects and common time effects. 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 1930s or 1940s, 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 two very different countries had very similar top income shares.

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A Data Appendix

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

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. The source for this variable is Golden and Wallerstein (2006b).

Education Years is equal to the average years of schooling. The source for this variable is Barro and Lee, `http://www.nber.org/pub/barro.lee/`.

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

Government Partisanship is equal to the left-right position of the parties in government weighted by seats in parliament. The source for this variable is McDonald's (2002) classification using Budge et al. (2001) data from party manifestos.

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 7 reports the country-years for which a left executive is coded as present.

Migrant Share is equal the percent of the population that is foreign born. The source for this variable 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`.

Non-Democracy is equal to 1 if the country is experiencing a non-democratic year and 0 otherwise.

Proportional Representation is equal to 1 if elections to the lower house of the legislature were contested under proportional representation rules and equal to zero otherwise. The sources for this variable for the years before 1946 were Mackie and Rose (1991) and Caramani (2000) and for the years after 1946 was Golder (2005).

Top1 is equal to the percentage of national income earned by the top 1% of income earners. 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. The data for Spain are from Alvaredo and Saez (2006).

Top10 is equal to the percentage of national income earned by the top 10% of income earners. 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. The data for Spain are from Alvaredo and Saez (2006).

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. 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. The data for Spain are from Alvaredo and Saez (2006).

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 source for this variable is Golden and Wallerstein (2006b)[44]

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 three 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), and Golden and Wallerstein (2006b). Table 8 reports the value of this variable for each country-year.

[44] The imputation model included two alternative union density measures. These measures although not as available for the later years of our sample included data for many country years during the first half of the century which are not available in the Golden and Wallerstein (2006b) data. The source for the alternative measures is Kjellberg (1983).

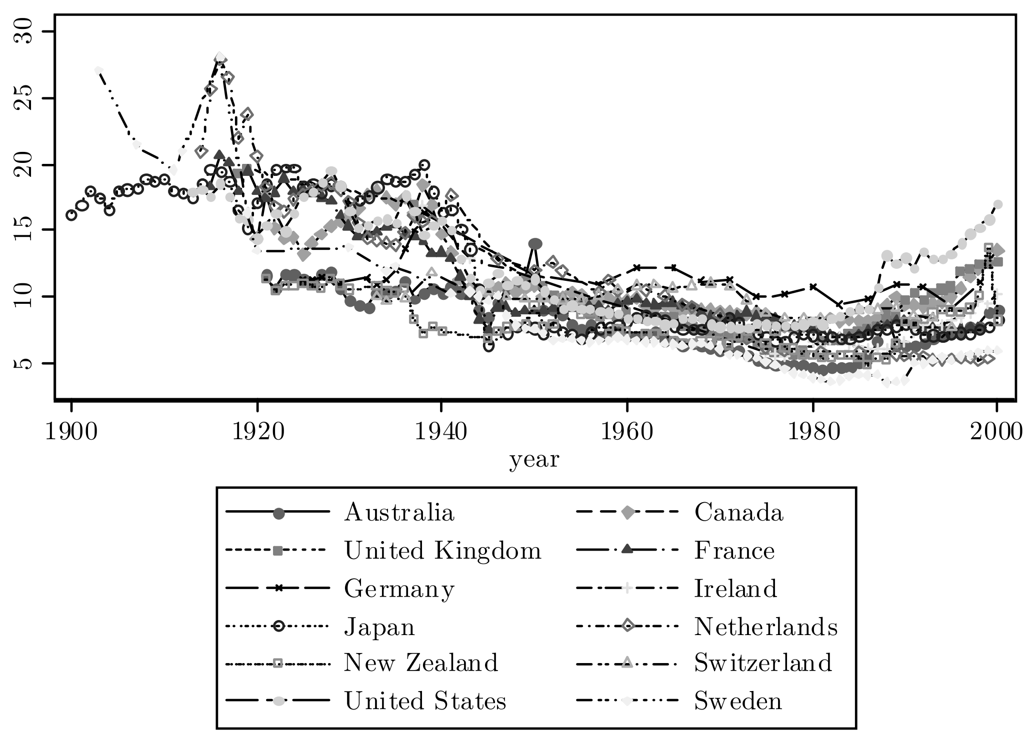

Figure 1: Share of Income Earned by Top 1%. Sources: Atkinson and Piketty (forthcoming), Moriguchi and Saez (2005), Roine and Waldenstrom (2006). Data for the Top 1% share in Spain are available from Alvaredo and Saez (2006) but only for the period 1981-00 and are not included in the graph.

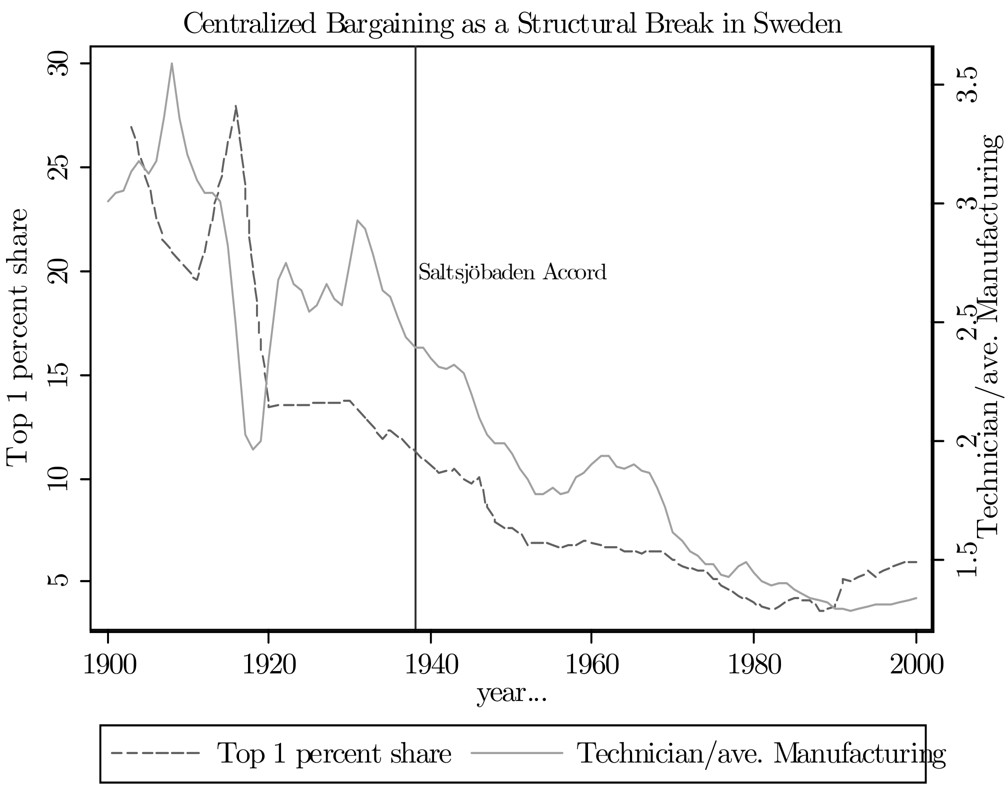

Figure 2: Vertical line indicates date at which wage bargaining is centralized.Top incomes data from Roine and Waldenstrom (2006) with top 1% share on left axis and top 10% share on right axis. The ratio of the for technicians to the average wage in manufacturing is from Ljungberg (2006).

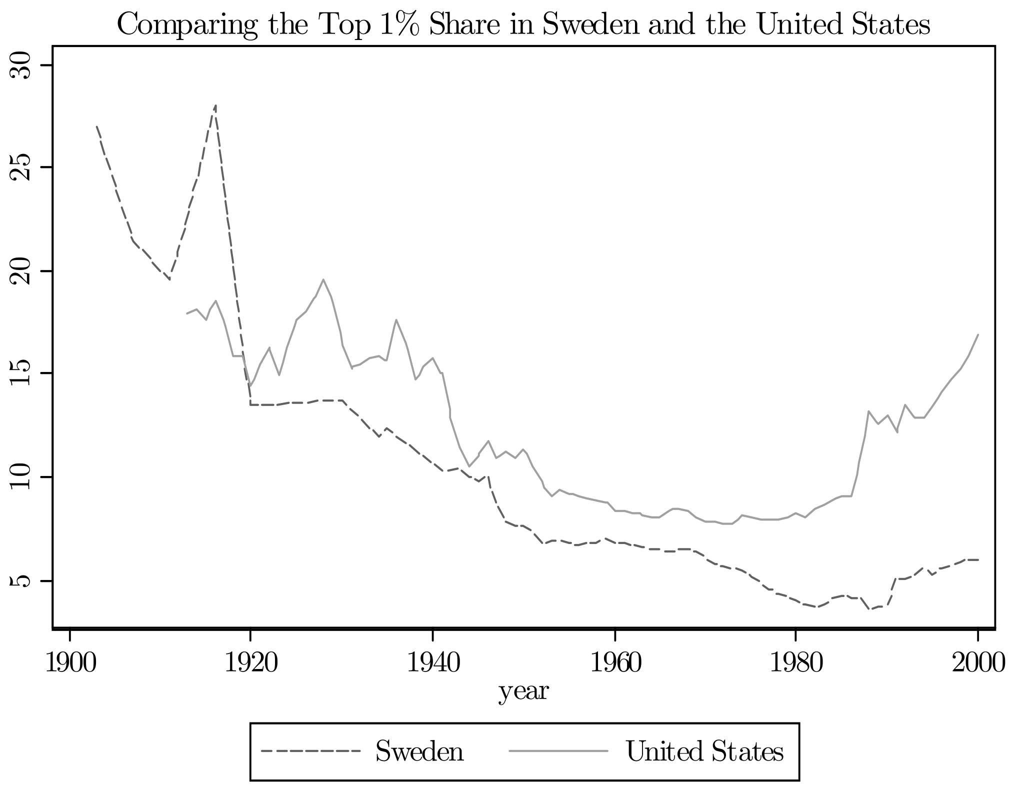

Figure 3: Top incomes data for Sweden from Roine and Waldenstrom (2006) and for the United States from Atkinson and Piketty (forthcoming).

	Top10		*Top1*		*Top10-1*	
$TopX_{t-1}$	0.668	0.410	0.603	0.440	0.499	0.218
	(0.070)	(0.102)	(0.081)	(0.091)	(0.088)	(0.109)
GDP per capita	-0.067	-0.026	0.052	0.101	-0.158	-0.133
	(0.069)	(0.138)	(0.049)	(0.087)	(0.054)	(0.107)
Trade Openness	0.008	-0.034	0.004	-0.029	0.008	-0.006
	(0.011)	(0.024)	(0.006)	(0.016)	(0.009)	(0.016)
Proportional Representation	-0.357	-0.266	0.118	0.623	-0.611	-1.176
	(0.486)	(1.088)	(0.304)	(0.556)	(0.378)	(0.762)
Wage Bargaining Centralization	-0.347	-0.224	-0.143	-0.156	-0.303	-0.098
	(0.330)	(0.433)	(0.202)	(0.288)	(0.249)	(0.312)
Union Density	-0.044	-0.114	-0.022	-0.042	-0.038	-0.085
	(0.016)	(0.051)	(0.009)	(0.029)	(0.012)	(0.033)
Left Executive	0.002	-0.616	-0.116	-0.312	0.263	-0.330
	(0.508)	(0.538)	(0.325)	(0.343)	(0.362)	(0.361)
Non-Democracy	0.387	0.269	0.804	1.024	-0.385	-0.986
	(0.930)	(1.095)	(0.588)	(0.677)	(0.740)	(0.862)
Universal Suffrage	0.015	-0.866	-0.412	-1.212	0.491	0.524
	(0.541)	(0.700)	(0.356)	(0.458)	(0.391)	(0.505)
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 Evidence. The Table reports the results of OLS regressions for the three measures of income inequality, *Top1*, *Top10*, and *Top10-1* on *Wage Bargaining Centralization*, *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		*Top1*		*Top10-1*	
$TopX_{t-1}$	0.775	0.472	0.676	0.459	0.625	0.270
	(0.058)	(0.095)	(0.069)	(0.090)	(0.084)	(0.105)
GDP per capita	-0.053	0.039	0.042	0.125	-0.130	-0.076
	(0.069)	(0.138)	(0.048)	(0.088)	(0.056)	(0.103)
Trade Openness	0.0003	-0.045	0.0002	-0.034	0.002	-0.016
	(0.011)	(0.027)	(0.006)	(0.017)	(0.009)	(0.018)
Proportional Representation	-0.600	-0.287	-0.080	0.568	-0.742	-1.135
	(0.425)	(1.083)	(0.262)	(0.540)	(0.349)	(0.777)
Non-Democracy	0.151	-0.003	0.683	0.858	-0.549	-1.127
	(0.884)	(0.965)	(0.560)	(0.587)	(0.707)	(0.787)
Universal Suffrage	-0.431	-0.573	-0.602	-1.109	-0.057	0.653
	(0.521)	(0.661)	(0.345)	(0.450)	(0.397)	(0.474)
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 2: Proportional Representation and Income Inequality: 1916-2000 Evidence. Table reports the results of OLS regressions for the three measures of income inequality, *Top1*, *Top10*, and *Top10-1* on *PR* 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		*Top1*		*Top10-1*	
	1951-75	1976-00	1951-75	1976-00	1951-75	1976-00
TopX $_{t-1}$	0.511	0.674	0.468	0.891	0.646	0.579
	(0.157)	(0.139)	(0.153)	(0.152)	(0.157)	(0.141)
GDP per capita	-0.033	-0.096	0.074	-0.020	-0.068	-0.112
	(0.124)	(0.096)	(0.060)	(0.058)	(0.104)	(0.070)
Trade Openness	0.022	-0.003	0.002	-0.005	0.015	0.006
	(0.018)	(0.014)	(0.008)	(0.007)	(0.012)	(0.011)
Education Years	-0.106	0.109	-0.201	0.093	0.059	0.016
	(0.406)	(0.331)	(0.187)	(0.184)	(0.347)	(0.186)
Migrant Share	0.016	-0.040	0.025	0.003	-0.005	-0.064
	(0.078)	(0.074)	(0.035)	(0.028)	(0.066)	(0.059)
Proportional Representation	-0.270	0.078	0.520	0.064	-0.626	-0.402
	(0.590)	(0.581)	(0.285)	(0.358)	(0.489)	(0.502)
Bargaining Level–GW	-0.457	-1.087	-0.248	-0.419	-0.165	-0.581
	(0.347)	(0.488)	(0.129)	(0.258)	(0.337)	(0.304)
Union Density	-0.080	-0.036	-0.031	-0.001	-0.039	-0.029
	(0.029)	(0.023)	(0.009)	(0.011)	(0.024)	(0.013)
Government Partisanship	-0.020	0.043	0.002	0.019	-0.015	0.022
	(0.016)	(0.019)	(0.007)	(0.009)	(0.013)	(0.013)
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 3: Labor Market Institutions, Government Partisanship, and Income Inequality: 1950-2000 Evidence. Table reports the results of OLS regressions for the three measures of income inequality, *Top1*, *Top10*, and *Top10-1* on *Bargaining Level–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.

	Top10		*Top1*		*Top10-1*	
	1951-75	1976-00	1951-75	1976-00	1951-75	1976-00
$TopX_{t-1}$	0.803	0.926	0.739	1.018	0.784	0.804
	(0.143)	(0.095)	(0.159)	(0.083)	(0.136)	(0.131)
GDP per capita	-0.026	-0.039	0.007	-0.017	-0.016	-0.043
	(0.150)	(0.123)	(0.070)	(0.062)	(0.104)	(0.084)
Trade Openness	0.002	-0.006	-0.003	-0.003	0.006	0.001
	(0.016)	(0.014)	(0.009)	(0.006)	(0.010)	(0.011)
Education Years	-0.111	0.059	-0.137	0.142	0.003	-0.071
	(0.452)	(0.389)	(0.186)	(0.189)	(0.345)	(0.232)
Migrant Share	0.029	-0.029	0.031	-0.005	-0.001	-0.046
	(0.098)	(0.084)	(0.047)	(0.031)	(0.071)	(0.070)
Proportional Representation	-0.392	-0.725	0.224	-0.420	-0.671	-0.643
	(0.611)	(0.623)	(0.266)	(0.264)	(0.504)	(0.561)
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: Proportional Representation and Income Inequality: 1950-2000 Evidence. Table reports the results of OLS regressions for the three measures of income inequality, *Top1*, *Top10*, and *Top10-1* on *PR* 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.

	Break set relative to centralization date						
	-15	-10	-5	0	5	10	15
Sweden (top 1%)							
β	-.091	-.114	-.190	-.241	-.276	-.291	-.316
	(.190)	(.141)	(.096)	(.077)	(.064)	(.054)	(.042)
γ	-.174	-.192	-.202	-.212	-.219	-.224	-.229
	(.027)	(.031)	(.033)	(.033)	(.032)	(.029)	(.026)
Sweden (wage diff.)							
β	-.036	-.030	-.020	-.016	-.017	-.018	-.021
	(.011)	(.007)	(.006)	(.005)	(.004)	(.004)	(.003)
γ	-.022	-.022	-.021	-.020	-.020	-.020	-.021
	(.001)	(.001)	(.002)	(.002)	(.002)	(.002)	(.001)
Netherlands (top 1%)							
β	-.141	-.251	-.242	-.207	-.239	-.246	-.251
	(.054)	(.054)	(.057)	(.060)	(.054)	(.047)	(.040)
γ	-.201	-.223	-.222	-.212	-.222	-.225	-.226
	(.020)	(.025)	(.028)	(.030)	(.029)	(.027)	(.025)
Denmark (MEC)							
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 diff.)							
(x 100)	-.437	-.447	-.417	-.401	-.397	-.397	-.391
	(.060)	(.046)	(.041)	(.037)	(.031)	(.026)	(.024)
	-.259	-.255	-.261	-.261	-.254	-.238	-.217
	(.043)	(.039)	(.040)	(.040)	(.039)	(.036)	(.032)
Ireland (top 0.1%)							
β	-.153	-.061	-.022	-.027	-.019	-.050	-.074
	(.029)	(.030)	(.028)	(.029)	(.018)	(.023)	(.023)
γ	-.096	-.076	-.061	-.061	-.056	-.068	-.078
	(.013)	(.012)	(.012)	(.013)	(.010)	(.014)	(.015)

Table 5: Testing for a Trend Shift at Different Break Dates. Newey West Standard Errors.

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, 1981-85, 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 6: Coding for *Left Executive*

Country	Coding for *Wage Bargaining Centralization*
Australia	1 1900-1953 2 1954-2000
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 1900-1945 3 1946-2000
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 1905-1937 3 1938-2000
Switzerland	1 1900-1944 2 1945-2000
United Kingdom	1 1900-1913 2 1914-2000
United States	1 1900-2000

Table 7: Coding for *Wage Bargaining Centralization*