Partisan Inequality?

Examining the Effects of International Capital Mobility on American Presidents' Impact on Income Inequality

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Master's Thesis in Political Science
FALL 2016

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Abstract

Denne masteroppgaven tar for seg den ekstreme økningen i inntektsulikhet i USA siden midten av 1970-tallet. Ved hjelp av deskriptive data, samt tidsserieanalyser av inntektsvekst og makroøkonomiske indikatorer, illustrerer jeg hvordan inntektsutviklingen har endret seg betraktelig etter 1974. Jeg argumenterer for at internasjonal kapital mobilitet har ført til økt inntektsulikhet ved å begrense handlingsrommet til demokratiske administrasjoner – i særdeleshet muligheten til å føre en ekpansiv finanspolitikk. Jeg finner at demokratiske presidenter skapte betydelig økonomisk vekst i perioden før internasjonal kapital mobilitet, og denne veksten kom spesielt de lavere inntektsgruppene til gode. Disse resultatene er i tråd med 'the partisan hypothesis', som forutsetter at venstrepartier fører en ekspansiv politikk for å styrke den økonomiske situasjonen til sine velgergrupper – som foretrekker høy vekst og lav arbeidsledighet. Etter kapital mobilitet har derimot demokratiske presidenters evne til å påvirke den økonomiske veksten blitt betydelig redusert, og den veksten som forekommer har i denne perioden størst positiv innvirkning på de høyere inntektsgruppene. Disse resultatene er i tråd med 'the capital mobility hypothesis', som forutsetter at evnen til å føre ekspansiv makroøkonomisk politikk begrenses når eierne av kapital har muligheten til å 'straffe' slik politikk ved kapitalflukt. Mine resultater står i skarp kontrast til Larry Bartels' argument (2008, 2016) om at økningen i ulikhet de siste fire tiårene skyldes forskjeller i makroøkonomisk politikk under demokratiske og republikanske presidenter. Tvert imot antyder mine funn at det er konvergensen mellom demokrater og republikanere som har deler av skylden for den økte ulikheten.

Acknowledgments

First of all, I would like to thank my thesis advisor, Jonathon Moses, for feedback and assistance on this thesis. His guidance has truly been above and beyond what can be expected. It has rarely been more than 24 hours between a submitted draft and the receipt of a fully annotated reply - regardless of weekends and travel plans. Furthermore, I also owe him thanks for turning me on to the topic of this thesis. It was through his suggestion that I read the two books which led me to investigate the impact of partisan politics on U.S. income inequality: *Buying Time* by Wolfgang Streeck, and *Unequal Democracy* by Larry M. Bartels. While the first was theoretically convincing (almost to the point of despair), the latter seemed to negate the entire premise of the first – and had impressive empirical evidence to back it up. However, they could not both be right. The following thesis is my attempt to reconcile the two.

I would also like to thank Arild Blekesaune for providing me with much needed assistance with the statistical analyses. Finally, I would be remiss not to thank my wonderful roommates, Anja and Mari, who have made life outside the university such a pleasure this semester – and given me the energy and enthusiasm necessary to complete this work.

Despite having received such wonderful assistance, some errors are sure to remain – for which I bear sole responsibility.

Contents

Introduction

Economic inequality in wealth and income has received increasing academic interest over the past decade (Piketty 2014; Stiglitz 2012). Rising inequality has been used to explain everything from different social ills (Wilkinson & Pickett 2010), weak economic growth, and not least the current backlash against political and economic elites. The rise of populist parties in mainland Europe, the decision by a majority of UK voters to leave the European Union, and the election of Donald Trump as the 45th president in the recent 2016 U.S. election are but a few examples. Unsurprisingly, commentators who fear the possible consequences of increased nationalism and isolationism are often quick to point out that a more equal distribution of the profits of globalization is necessary to preserve trust in, and support for, our current political systems (Habermas, 2016; Stiglitz 2012; 2015).

Regardless of whether inequality is in fact the causal factor behind all these recent developments, inequality is – and for most developed countries has been – increasing for the past several decades (Dadush, Dervis, Milsom & Stancil 2012). In the words of one observer, the 'growing inequality within most countries around the world is one of the critical issues facing the world today', not just because of its direct effects, but because '[w]e sense that it is morally wrong. We sense that it cannot be justified. We sense that it is dividing our societies and undermining our democracies' (Stiglitz 2015: 9). This thesis is not concerned with detailing or demonstrating the effects of inequality. It proceeds on the assumption that previous empirical work has demonstrated these effects to a sufficient extent, in turn warranting a study of what *causes* increases in inequality. Furthermore, it presupposes that rising inequality, all else being equal, is unjust (Rawls 1971: 54). Consequently, a clearer understanding of the causes of inequality is not only valuable as knowledge about how the world does work, but also as an enabling condition for achieving a more just distribution of goods in the future.

This thesis will be concerned primarily with different measures of inequality of *income*, as opposed to inequality of *wealth*. Among developed countries, the United States is the unchallenged trendsetter when it comes to increases in income inequality (Dadush *et al*. 2012: 2). From the mid-1970s to 2010, the top 0.1 percent of U.S. income earners increased their share of total national income from about 2

percent to 10 percent. Although this increase dwarves that of other countries, the 'central fact is that in all wealthy countries, including continental Europe and Japan, the top thousandth enjoyed spectacular increases in purchasing power from 1990–2010, while the average person's purchasing power stagnated' (Piketty 2014: 319-20). In other words, while inequality is on the rise in general, it started increasing earlier in the United States, and it has reached higher levels there than anywhere else in the developed world (Dadush *et al*. 2012: 15). Consequently, the US presents itself as a natural critical case to be examined when it comes to income inequality: Any factors proffered as causally related to levels of income inequality should at least be able to explain (some of) its increase in the U.S..

Although inequality has been on the rise for a while now, it represents a historical break from the mid 20th century. In fact, income inequality in the U.S. remained relatively stable from the end of the Second World War to the middle of the 1970s[1] (Bartels 2008: 8-9). One review of US income inequality chose to illustrate the change in distribution by relating the increase in average household income – 62 percent from 1979 to 2007 – to the increase in median household income – merely 35 percent in the same period (Dadush *et al*. 2012: 8). When the average value increases at a larger pace than the median value, it necessarily implies that a few households have become relatively richer compared to the total number of households. Data compiled by Piketty and Saez (2007) provide another stark illustration: The top 1 percent of income earners reaped 65.9 percent of the total income growth from 1973-2007.[2] A range of explanations have been offered to explain this shift in economic fortunes, such as skill-biased technological change, shifts in demography, structural changes to the US economy, increasing international trade, and the deregulation of finance. The central premise of Piketty's monumental *Capital in the 21st Century*, although not primarily aimed at answering the specific question of income inequality in America, is of course that the economic returns to capital is returning to a higher (normal) rate – exceeding the growth rate of the economy as a whole. Consequently,

[1] This holds for all measurements of inequality. However, some measures of inequality started increasing earlier than others. In general, "wide" inequality (that is, inequality between relatively wide groups of income earners, such as the ratio between income levels at the 80th and 20th percentile) started increasing in the mid-70s, whilst "narrow" inequality (e.g. the ratio of the top 1 and 0.1 percent to the median earner) took off a bit later, around the early to mid-80s.

[2] Piketty and Saez (2016), 'Table Incomegrowth'. This share includes income from capital gains.

one would imagine that those who rely on capital gains and other dividends for a substantial share of their income would see their incomes rise relative to wage workers. The thesis will touch upon some of these these explanations and more, but mostly tangentially. Its main task will be to critically assess a recent explanation that puts a lot more emphasis on national political factors than technical changes, economic "laws", or the globalization of world markets.

In 2008, Larry M. Bartels published *Unequal Democracy: The Political Economy of the New Gilded Age*. In it, Bartels attempted to answer the question of rising inequality with reference to factors on the nation state level. It appeared to him, 'as a student of American politics, that careful attention to public opinion, partisan politics, and public policy' might help explain 'how and why the economic fortunes of the affluent, middle-class, and poor people have diverged so dramatically in the contemporary United States' (Bartels 2008: ix). Bartels was driven by more than a search for understanding, as he had previously partaken in a task force whose members worried that rising economic inequality could exacerbate political inequality as well (*Ibid*.: 2). His book offers the rather surprising conclusion that what may be 'the most important single influence on the changing U.S. income distribution over the past half-century' is in fact 'the contrasting policy choices of Democratic and Republican presidents'. Surprising because, as reported above, rising income inequality affects almost all developed countries in the world. Yet Bartels reports that his projections 'suggest that income inequality would actually have *declined* slightly over the past 50 years' had 'the patterns of income growth characteristic of Democratic administrations been in effect throughout that period' (*Ibid*.: 30). In a recently published second edition (Fall 2016), Bartels modifies some of his claims (to an extent), yet he reiterates that 'income inequality would have been no greater in 2014 than it was in the late 1940s had the patterns of income growth characteristic of Democratic administrations operated throughout that period' (Bartels 2016: 34).

This thesis is dedicated to a critical examination of Bartels' explanation of income inequality in the United States in the past half-century. By employing both the same datasets, and expanding the analysis to new data where appropriate, I aim to show that Bartels fundamentally underestimates the difference in policymaking space available to U.S. incumbents in the first three decades after WWII, compared to that of the last four decades. Consequently, it does not make sense to talk about a single pattern of income growth as 'characteristic' of Democratic administrations from the

late 1940s to the early 2010s. On the contrary, I demonstrate that the pattern Bartels describes is in fact limited to a short period of interventionist policies during the 1950s and 60s, and that Democratic incumbents have had little – if any – significant impact on income inequality since the mid 1970s. I shall argue that the increase in international capital mobility since the breakdown of the Bretton Woods system in the mid 1970s led to restraints on government interventions in the US economy – robbing Democratic presidents of their most efficient tools for generating robust egalitarian growth. The data clearly show a watershed in Democratic growth patterns before and after the structural changes to the global economic system in the mid 1970s. Ignoring this phenomenon, or downplaying its importance, not only impairs our understanding of what has enabled income inequality to run rampant in the past four decades, but it obscures from view the measures which must be taken if we are serious about curbing present levels of inequality. I aim to fill a gap in the literature by assessing the relationship between literatures on partisan politics and international capital mobility. I am able to do so by deviating from what has become the standard methodological approach: Instead of a pooled cross-sectional time series analysis, I focus on changes in partisan effects within a single case.[3] Through this methodology, I am able to show how both the partisan hypothesis and the international capital mobility hypothesis are important factors in determining U.S. income inequality, and delineating the relationship between the two.

This thesis proceeds as follows: First, a quick overview of Bartels' main argument will be presented, and some empirical foundations of his claims will be reproduced. Secondly, a reexamination of his data will highlight "the puzzle" to be solved, namely the inability of recent Democratic incumbents to recreate the egalitarian growth patterns of the mid 20th century. Bartels' interpretation and use of the data will be discussed in some detail, to highlight divergences between our two accounts. I shall then offer a theoretical explanation, based on prior work concerning the implications of international capital mobility for inequality generally, and the limiting effect of international factors on national policymaking specifically. This explanation will be tested using descriptive statistics and a regression analyses of time

[3] Theoretically, a well-specified cross-sectional analysis should be able to pick up the same change in partisan differences. However, finding a suitable dependent variable which captures partisan differences across a range of different economies might be difficult. In any case, I have not been able to find a successful attempt in the extant literature.

series data on U.S. income inequality. The implications of my results will be discussed, and finally I shall of course highlight the (at times substantial) limitations of my analysis, complete with suggestions for further research.

A Short Introduction to Bartels' Argument

In order to introduce my research question in more detail, a short introduction of Bartels' argument is required. To be sure, there is a lot more evidence behind his claims than can be presented here, and a more thorough examination of his claims will be carried out below. Bartels developed the crux of his argument in the first edition of his book (2008), in which he relied on historical income data from 1947 to 2005. In the second edition (2016), newly available data is included, and some of the more demanding claims of the first edition are toned down. This thesis will focus mainly on the second edition of his book, as I will be employing the same dataset. However, I will refer to the first edition in cases where my arguments are supported by older data as well. Even though Bartels modifies some claims in the empirical chapters of the second edition, the book as a whole argues in the same vein as the first, namely that inequality is mainly caused by differential income growth rates under Democratic and Republican presidents. This view is apparent in the opening and closing chapters of both editions, and I shall therefore treat this as the central thesis of the work as a whole.

Bartels uses family income data from the United States Census Bureau[4] to illustrate that income growth has become more unequal in the United States. Specifically, he uses data on pre-tax market income for families, which includes 'wages, interests and dividends, and cash transfers such as Social Security payments', but not 'the value of government services such as Medicare and foods stamps' (Bartels 2016: 7n). The dataset consists of the upper income limit of each fifth of the family income distribution, as well as the lower limit of the top 5 percent, in 2014 dollars (US Census Bureau 2016). In 2014, a fifth of families earned less than $29,100 a year, while the cut-off for inclusion in the top 5 percent was $230,000. The dataset reaches back to 1947, and is adjusted for inflation. This enables a comparison of growth rates for each group over time. Figure 1 is a reproduction of Bartels' Figure 1.2 (2016: 10), showing cumulative income growth by income percentiles for two post-war periods. As is evident, income growth has been a lot slower and more unequal since 1974, with the 20th percentile experiencing virtually no growth in their

[4] U.S. Census Bureau, Current Population Survey (CPS), Annual Social and Economic Supplements, 'Table F-1. Income Limits for Each Fifth and Top 5 Percent of Families (All Races): 1947-2014.' Available from: http://www.census.gov/data/tables/time-series/demo/income-poverty/historical-income-families.html.

real incomes over the entire period (and the little growth that did occur was mostly due to longer working hours and changes in labor market participation). But, as Bartels notes, even for families at the 95th percentile income growth has been substantially reduced in the past decades (*Ibid*). Still, the striking feature of the figure is how clearly it illustrates a shift in income growth differentials before and after 1974 – which in turn led to growing levels of inequality over time.[5]

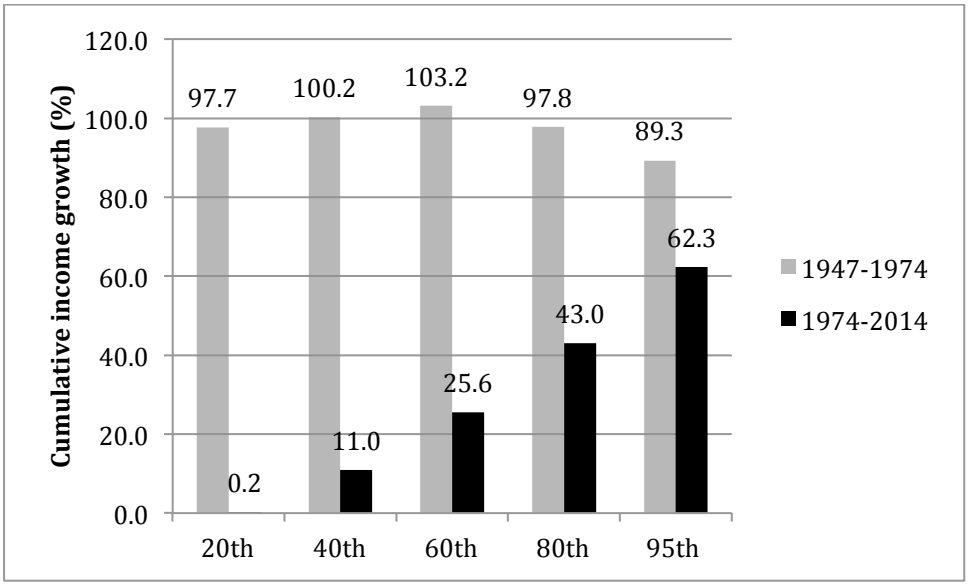

Figure 1: Cumulative Income Growth by Percentile, 1947-1974 and 1974-2014. Source: Bartels (2016: 10).

Bartels then calculates the average annual pre-tax income growth[6] for the five groups in the period 1948-2014, before dividing these averages between Democratic and Republican incumbents.[7] Table 1 (a reproduction of Table 2.1 in Bartels 2016: 36) shows the results of this exercise, and Bartels' main findings are of course the partisan differences reported in the final column. Apparently, income growth for families at the 20th percentile benefits substantially from a democratic incumbent, as evidenced by the on average 1.78 percent *annual* premium on the pre-market income growth rate. Furthermore, a quick comparison of the average growth rates under Democratic and Republican presidents reveal two distinct distributional patterns:

[5] The 1974 split in Bartels (2016: 10) is lifted form a similar presentation of the data in a 2002 publication from the Economic Policy Institute. It is not clear why the original authors decided to introduce a 1974 split, nor does Bartels provide any reason for doing so.
[6] Following Bartels (2016: 36), I compute percentage changes as 100 x $ln(Y_t/Y_{t-1})$, unless otherwise specified.
[7] Partisan control of the presidency is lagged by one year to allow time for the president's policies to affect income growth, see Bartels (2016: 37; 37n).

Under Democratic rule, the five income levels experienced more or less equal growth rates, with the exception of the top five percent who did a little worse. Under Republicans, on the other hand, average growth rates increased with each step up the income distribution (this is of course mirrored in the decreasing size of the Democratic growth rate premiums in the final column). Secondly, income growth was higher for all groups under Democratic administrations.

Table 1:
Real Income Growth Rates by Income Level
and Presidential Partisanship, 1948-2014.
Standard errors in parenthesis.

	All presidents	*Democratic presidents*	*Republican presidents*	*Partisan difference*
20th percentile	1.02 (.44)	1.98 (.68)	.20 (.56)	+1.78 (.87)
40th percentile	1.19 (.35)	1.91 (.51)	.57 (.46)	+1.34 (.68)
60th percentile	1.40 (.31)	1.98 (.46)	.90 (.41)	+1.08 (.62)
80th percentile	1.55 (.29)	1.99 (.44)	1.17 (.39)	+.82 (.59)
95th percentile	1.68 (.34)	1.77 (.55)	1.60 (.42)	+.17 (.68)
N	67	31	36	67

Source: Bartels (2016: 36).

The partisan disparities described above are illustrated graphically in Figure 2 (Figure 2.1 in Bartels 2016: 38). The results are striking, and seem to suggest that presidents do have a strong impact on the distribution of income in the United States:

> Affluent families have generally fared well regardless of which party controls the White House. However, the real incomes of middle-class families have increased more than twice as fast under Democratic presidents, while for working poor families, real income growth has been *ten times* as fast under Democrats as under Republicans. (*Ibid*: 37, emphasis in original).

To test whether these differences are more than mere coincidences, Bartels proceeds to drop different observations. All in all, he reports that the findings are robust to every test (*Ibid*. 39). Yet there are signs that some observations contribute substantially to the apparent partisan effect. For instance, the partisan difference in growth rates at the 20th percentile (where the deviation in growth rates between Democrats and Republicans are at their largest) drops to 1.10 with a *t*-statistic of 1.2 when omitting Lyndon Johnson from the 1948-2014 dataset[8] (Bartels 2016: 39n). In the first edition, Bartels also calculated the partisan differences and related *t*-statistics for the period before and after 1974, however this test is not replicated in the second edition (Bartels 2008: 36n).

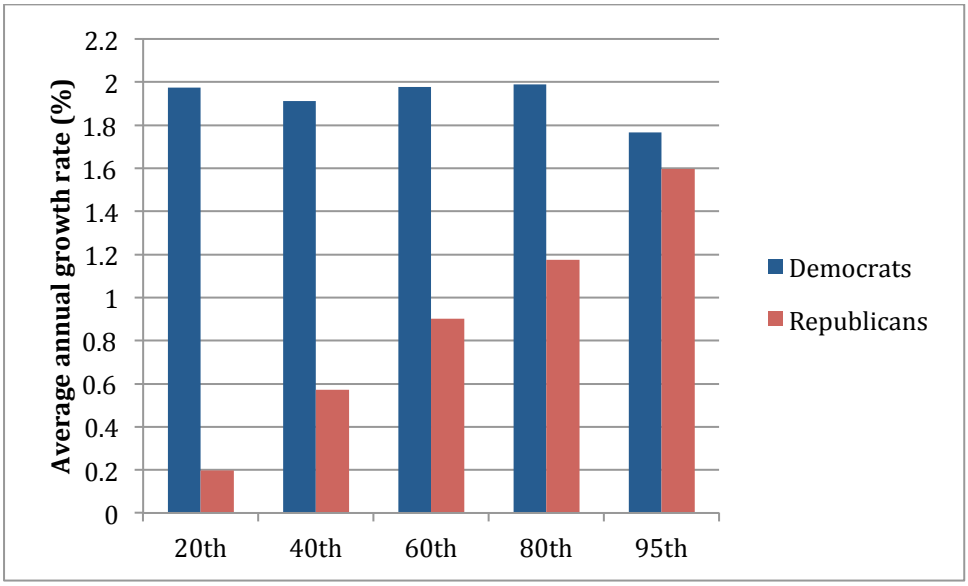

Figure 2: Real Income Growth by Income Level under Democratic and Republican Presidents, 1948-2014. Source: Bartels (2016: 10, 38).

Reviews of the first edition pointed out a disparity between the situation described in Figure 1, and Bartels' subsequent analysis: The striking thing about Figure 1 is of course the fact that income growth rates became so manifestly unequal after 1974. Some reviewers offered possible explanations for this shift, such as Pollin (2010), who argued that an ideological shift in policymaking circles must have taken place after the high inflation of the 1970s, while Page (2009) points out that

[8] Dropping Lyndon Johnson from the 1948-2005 dataset reduced the partisan difference from 2.21 to 1.49, with a *t*-statistic of 1.5 (Bartels 2008: 36n). In other words, expanding the dataset made it less robust to dropping Lyndon Johnson's two terms.

Democratic policies were not particularly redistributive, but on average only income-neutral, over the entire postwar period (as evidenced by the qualitatively equal growth rates for each income group under Democratic presidents in Table 1). Page wondered why Democrats did not do more to *create equality* instead of just maintaining the distribution of incomes as is. To be sure, even in the 1960s there was ample room for redistribution – as the top 1 percent of income earners received more than 8 percent of total national income throughout that decade.[9]

As a response to these criticisms, Bartels included a new sub-chapter titled 'Do Presidents Still Matter?' (Bartels 2016: 57-62). He starts the sub-chapter off with the recognition that 'there are good reasons to think that these patterns may have changed significantly over time', and mentions economic trends and shifts in ideology as possible causes of such a divergence (*Ibid*.: 57). To examine this question Bartels presents Figure 3 (Figure 2.5 in Bartels 2016: 59), which compares the differences in income growth rates between Democratic and Republican presidents from 1982-2014.[10] His interpretation of this graph is that 'substantial differences' remain, but he accedes that 'the nature of those differences has shifted over time' (*Ibid*: 58). However, he points out that Republicans seem to have contributed as much to inequality as they did in the previous period, and that Democratic presidents still offer substantial growth premiums, even if 'its implications for *inequality* have probably declined' (*Ibid*.: 61-2).

[9] Piketty and Saez (2007), 'Table A1'. The share does not include income from capital gains.
[10] It might strike one as odd that, even after including Figure 1 in his book, Bartels chooses 1982 as the tipping point when controlling for time effects. Bartels lists globalization, technological change, and shifts in parties' ideologies as possible explanations for a time shift, but he does not provide any explicit reasoning as to why 1982 would be the relevant (and concurrent!) starting point for these three developments (Bartels 2016: 57-8).

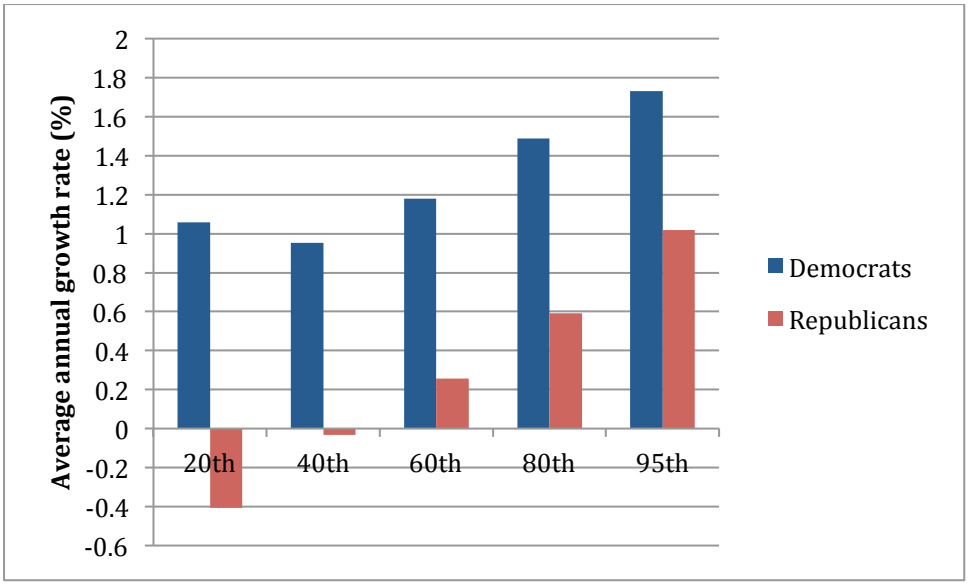

Figure 3: Real Income Growth by Income Level under Democratic and Republican Presidents, 1982-2014. Source: Bartels (2016: 59) and U.S. Census Bureau.

The Puzzle

Aside from mentioning economic trends such as globalization and possible ideological developments, Bartels makes no attempt to provide a theoretical explanation for the shifts in distributional patterns in the second half of the postwar era. He also repeats broad generalizations along the lines of the first edition, such as that increases in inequality since WWII is 'entirely limited to periods in which Republicans controlled the White House', and that 'continuous Democratic control [of the White House] would have produced little or no net increase in economic inequality since the late 1940s' since '[o]ver the past sixty-five years, Democratic presidents have generally presided over robust income growth for families across the economic spectrum' and have more or less 'consistently pursued high employment, high taxes, and economic redistribution from the rich to the poor' (Bartels 2016: 36, 70, 351, 352).

To be sure, I am not denying that there have been, and still are, substantial partisan differences in economic development between Democrats and Republicans. Many of them are diligently researched and convincingly presented in Bartels' book. Yet he consistently exaggerates the impact of partisan differences on U.S. income inequality throughout his book (and for good measure, in its title), as well as their effects on other macroeconomic outcomes. For instance, to claim that Democratic presidents have 'consistently' provided 'redistribution from the rich to the poor' is factually incorrect. One suspects that the strong emphasis throughout the book on the link between partisan control of the White House and increases in income inequality is tied to Bartels' strong sense that the rise in said inequality is unjust and should be ameliorated – and that national politics is the way to do so. After all, he describes the 'most important lesson of this book' as a 'very simple one: politics matters' (Bartels 2016: 363). I take no issue with that particular conclusion, but his definition of what counts as political is a decidedly parochial one: For instance, he asks whether the increase in inequality is 'really attributable to partisan politics rather than to accidental historical factors?' (*Ibid*.: 38). Although what is to be understood as 'accidental historical factors' is not elaborated, he subsequently celebrates the fact that women joining the workforce, more Americans getting a college education, changes in immigration and the age-composition of the population, changes in family size, and the increase in international trade flows have all been glacial changes,

because – 'from the standpoint of *political* analysis' – it makes them less likely to confound the impact of presidential incumbents (*Ibid*.: 45, emphasis in original). I fear that in attempting to preserve the conclusion that U.S. electoral politics matter, Bartels underestimates the magnitude and implications of the shift in distributional patterns before and after the mid-70s. And, more importantly, presidential incumbents' ability to affect those patterns. I also fear that limiting the scope of politics only to outcomes of public opinion polling and presidential elections is counterproductive if one is interested in identifying possible remedies for inequality – and perhaps saving partisan politics in general.

The stark difference in the pre/post 1974 distribution of income growth rates illustrated in Figure 1 of course begs the question whether the partisan distribution of income growth is similarly differentiated – yet Bartels does not investigate this split any further. In Figure 4 and 5, I have calculated the average growth rates for the different income groups based on the original 1948-2005 and the updated 1948-2014 datasets, and introduced a pre/post 1974 split. Before including any of the recession-era income growth rates under Obama, there is a marked difference in the *distributional* patterns of income growth under Democratic incumbents before and after 1974 (Figure 4). When Obama's first four years of impact on income growth is included, the pattern is even stronger: Democratic incumbents before 1974 produced consistently high and *redistributive* growth rates – with average annual rates sloping downwards with each step up the income distribution. In other words, Page's accusation of distributional complacency on the part of Democratic presidents do not apply to earlier incumbents. However, Democratic presidents after 1974 have not only presided over less redistribution or income neutrality: After 1974 the distributional pattern of Democratic incumbents is completely reversed (Figure 5). Consequently, while Democratic presidents used to contribute to a more *equal* income distribution, they now tend to contribute to a more *unequal* distribution. In addition, the Democratic growth premium afforded to all income groups has declined substantially.

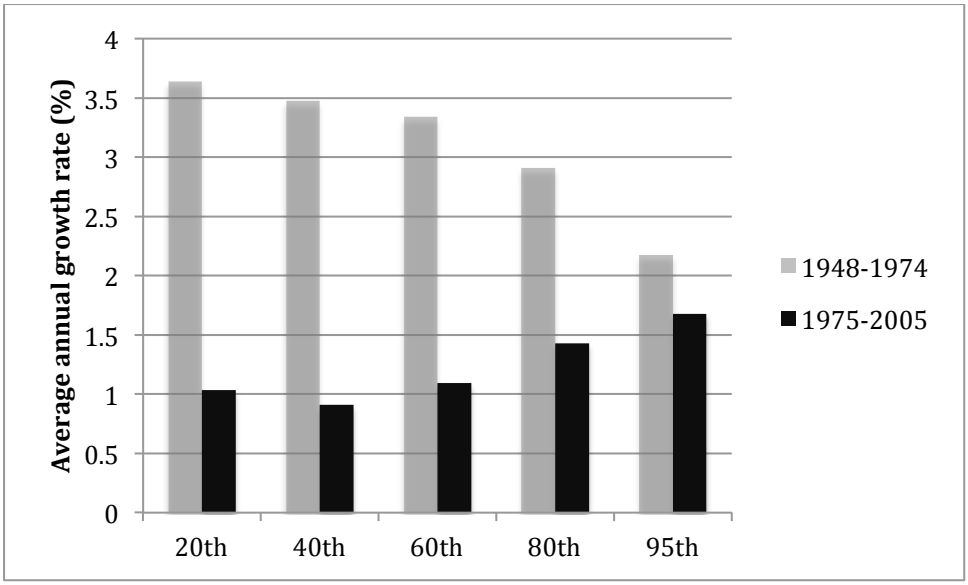

Figure 4: 2005 Dataset: Real Income Growth by Income Level under Democratic Presidents, 1948-1974 and 1975-2005. Source: U.S. Census Bureau.

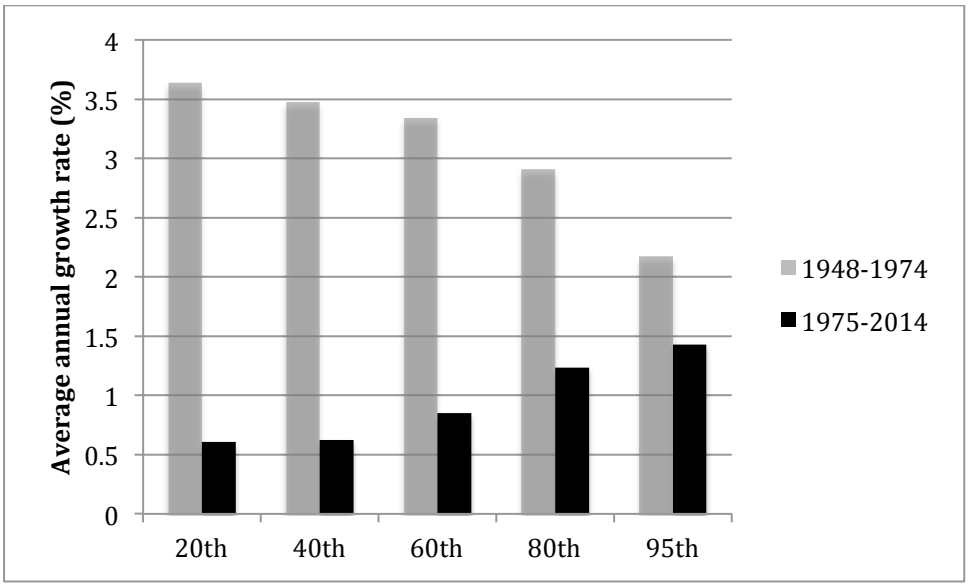

Figure 5: Updated 2014 Dataset: Real Income Growth by Income Level under Democratic Presidents, 1948-1974 and 1975-2014. Source: U.S. Census Bureau.

Although surprising in the light of Bartels' analysis, these findings are of course completely consistent with the explosion in unequal income growth evidenced in Figure 1.[11]

[11] The fact that this shift is consistent in both the 2005 and 2014 data sets implies that the shift is not driven by the recent recession, and that it was apparent already at the time of the first edition of Bartels' book.

The impact of this finding, or its status as a puzzle, might be influenced by how the post-1974 shift affected Republican growth rates. Calculating the growth rates for each income group under Republican incumbents pre/post 1974 yields an interesting result: Republicans' ability to spur income growth is similarly weakened,[12] but the distributional pattern of growth rates are identical for the two periods (Figure 6). In other words, both parties have presided over less overall income growth, but only Democrats have seen a substantial shift in the distributional patterns of that growth.

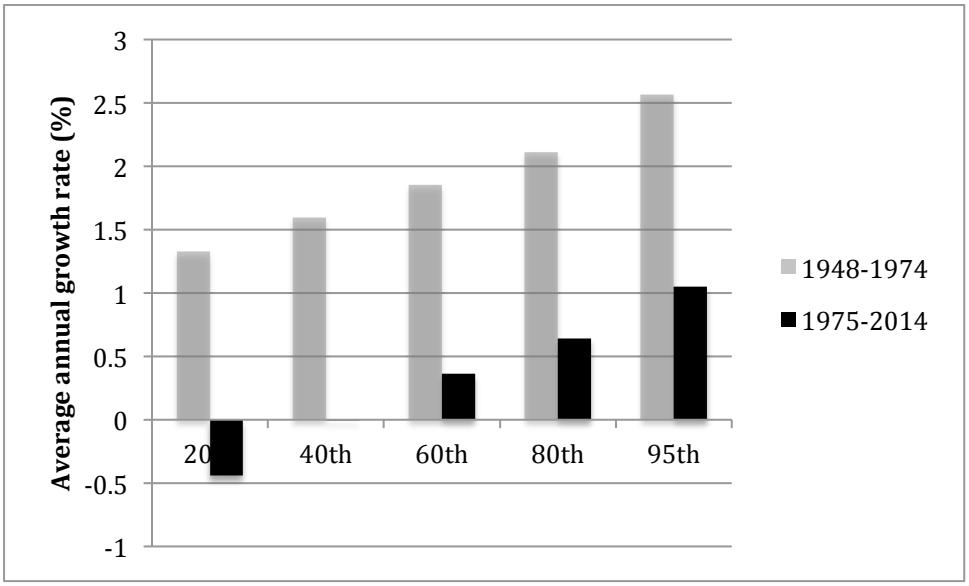

Figure 6: Real Income Growth by Income Level under Republican Presidents, 1948-1974 and 1975-2005. Source: U.S. Census Bureau.

In Figure 7, I calculate the partisan differentials in growth rates presented in Figure 2 above, but with separate graphs for the pre/post 1974 periods. Strikingly, Figure 7 shows that whereas Democratic incumbents achieved markedly more egalitarian (in fact redistributive) growth patterns than Republicans before 1974, distributional differences between Democratic and Republican incumbents are all but non-existent after 1974. To be sure, the growth rates for all income percentiles are

[12] Technically, an incumbent's ability to *affect* growth must of course be compared to a counter-factual scenario where no economic policies were introduced during the same period. However, based on the findings of Bartels and others, it seems natural to assume that federal policy initiatives have at least some bearing on economic performance. In any case, the differences between Democratic and Republican growth rates are, for most of the income levels, too large to be credibly attributed to chance.

consistently higher under Democrats (a Democratic growth premium remains), but that extra growth is no longer disproportionately going to the poor.

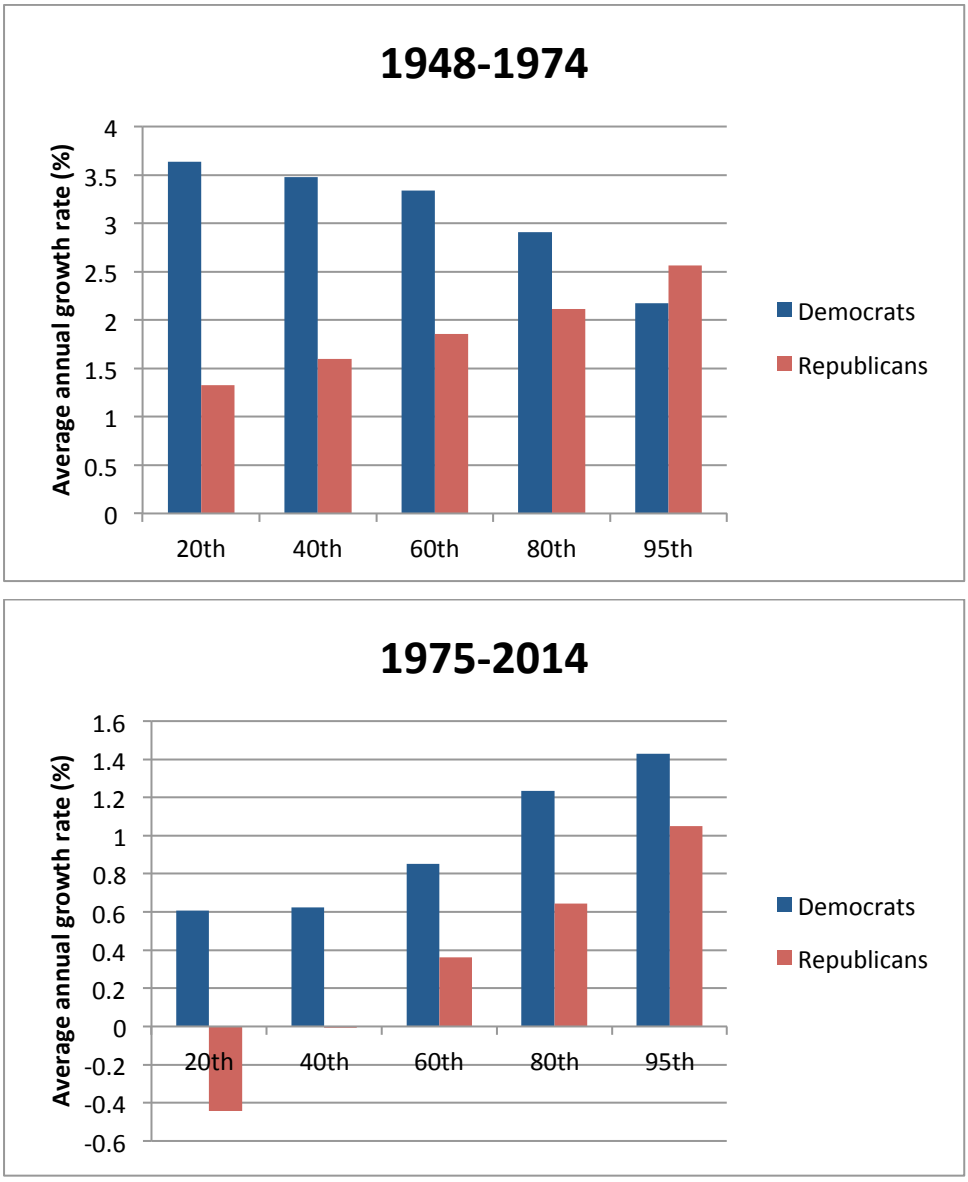

Figure 7: Real Income Growth by Income Level under Democratic and Republican Presidents, 1948-1974 and 1975-2014. Source: U.S. Census Bureau.

As mentioned in the introduction, however, the bulk of the increase in income inequality has been driven by a veritable explosion of top incomes – at a level way beyond those that are captured by the U.S. Census Bureau's income tables. By employing data on individual tax filings gathered by Piketty and Saez (2007), I am able to calculate the same annual average growth rates for this top stratum of income

earners – namely the top 95th, 99th, 99.5th, 99.9th and 99.99th percentile of the income distribution.[13]

Figure 8 shows the average annual income growth rates for the five groups under Democratic incumbents before and after 1974, excluding capital gains and dividends. The results are qualitatively similar to Figures 4 and 5 above, although the pre-1974 distributional pattern was less markedly redistributive for the top income levels. In general, each step up the income distribution meant lower average annual income growth under Democratic incumbents. The shift in distributional patterns before and after 1974 is especially striking at the very top of the distribution. The top 0.1 and 0.01 percent has fared particularly better under recent Democratic incumbents, with the top 0.01 percent jumping from basically no income growth in the 14 years of Democratic rule before 1974 to an average annual growth of more than 4.5 percent in the subsequent 17 years of Democratic incumbents. Seemingly, the 95th percentile of individual filings are not experiencing much growth under Democrats relative to the very top, but the annual growth rate is in fact comparable to that of families in the top 5 percent reported in Figure 5.

[13] 'Table A4: Top fractiles income levels (excluding capital gains) in the United States (adjusted for price inflation)' and 'Table A6: Top fractiles income levels (including capital gains) in the United States adjusted for price inflation'. Unlike the Census Bureau data, this data is compiled from individual tax filings – not reported income for families as a whole. Thus, the income limit of the top 5 percent of families and the top 5 percent individual filings are not comparable (not surprisingly, incomes for the top 5 percent of families is substantially higher, as a family usually consists of multiple income earners). Furthermore, this data does not include government transfers (such as Social Security and unemployment benefits). The nominal incomes are adjusted for inflation and reported in 2015 dollars. See Piketty and Saez (2007) for complete information about the dataset.

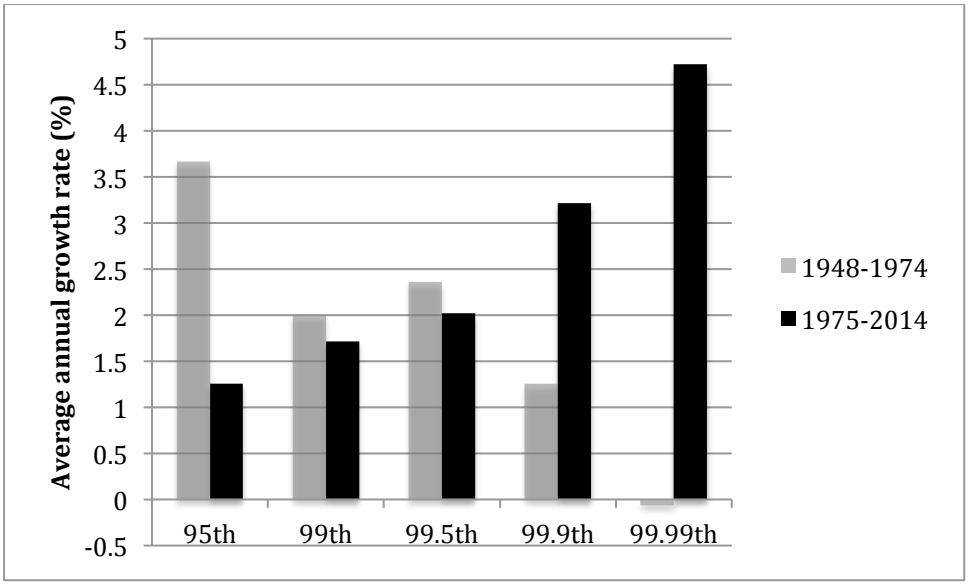

Figure 8: Real Income Growth by Income Level under Democratic Presidents, 1948-1974 and 1975-2005. Excluding capital gains. Source: Piketty and Saez (2007) 'Table A4'.

Figure 9 recreates the comparison of Democratic and Republican annual average growth rates before and after 1974 for the top income groups. Again, the shift in Democratic distributional patterns is striking. Before 1974, the top 0.01 percent of income earners could expect an average increase in real pre-tax income (excluding capital gains) of more than 1.5 percent per annum under Republican incumbents, while Democrats offered the prospect of zero income growth. After 1974 however, the same income group would come to fare *better* under Democrats than Republicans – and in any case substantially better than any other income group regardless of the partisan affiliation of the presidential incumbent. Disregarding the Democratic income *premium*, the *distributional* patterns are in essence identical between Democrats and Republicans after 1974.

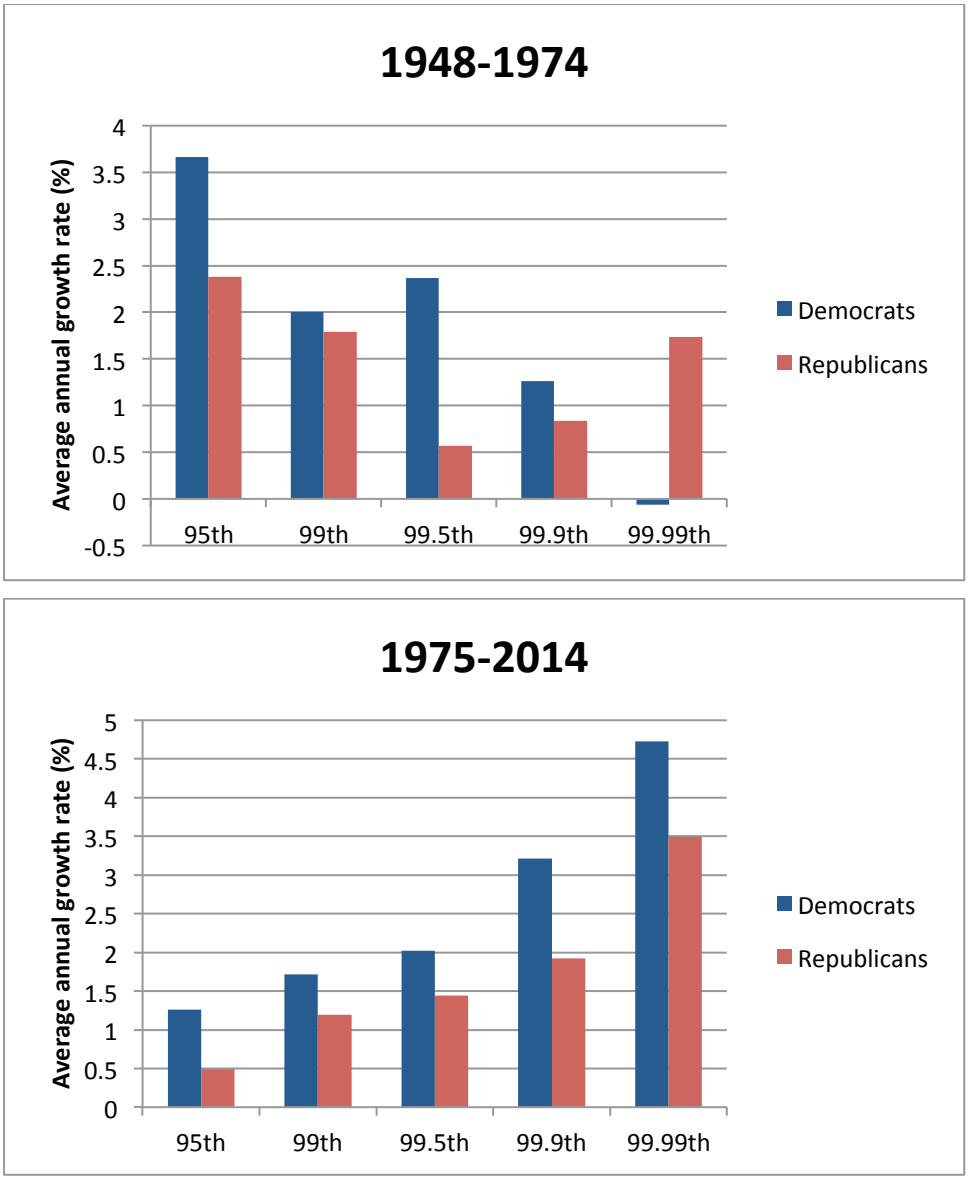

Figure 9: Real Income Growth by Income Level under Democratic and Republican Presidents, 1948-1974 and 1975-2014. Excluding capital gains. Source: Piketty and Saez (2007) 'Table A4'.

If we include income from capital gains for the top income groups, the shifts in distributional patterns under Democrats are even more striking. Figure 10 shows the average annual income growth rates for the five groups under Democratic incumbents before and after 1974, including capital gains and dividends. Again, the changing fortune of the top 0.01 percent of income earners is instructive. Now that capital gains are included, we see that this top stratum did in fact experience some growth in income under Democrats before 1974 – averaging about 1.2 percent annually (slightly below the Republican average of 1.1 percent, not shown). However, after 1974 this group of top earners averaged an annual increase in real incomes of

about 7.2 percent under Democratic presidents (against only 1.2 percent under Republicans in the post-1974 period).

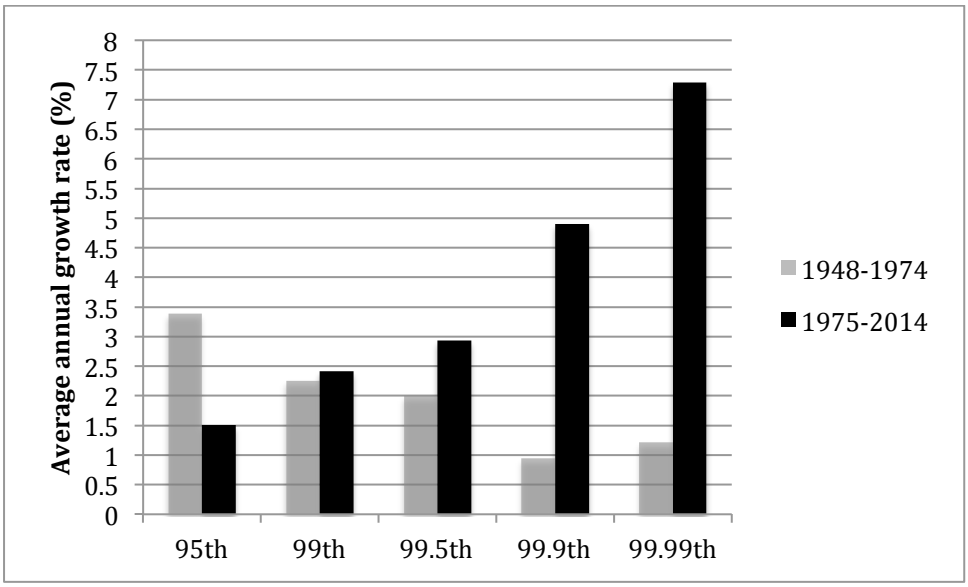

Figure 10: Real Income Growth by Income Level under Democratic Presidents, 1948-1974 and 1975-2005. Including capital gains. Source: Piketty and Saez (2007) 'Table A6'.

To summarize, Republican incumbents' impact on the *distribution* of income growth is consistent throughout the post-war period. However, Democratic incumbents have had their impact reversed since the mid 1970s. This reversal has been especially striking at the very top of the income distribution. The puzzle then, is this: What explains the radical shift in Democratic incumbents' ability to produce equalizing income growth rates? The explanation offered in this thesis is that the re-emergence of international capital mobility has contributed to income inequality in several important ways, one of which is to diminish Democratic incumbents' ability to pursue expansionary macroeconomic policies. We now turn to a theoretical exposition of why this might be so.

How Capital Mobility Affects Inequality

The argument of this thesis is that Bartels overemphasizes the partisan effects on inequality in the United States since the 1970s, and that a more convincing explanation can be found in the increasing mobility of capital in the same period – both in explaining the increases in inequality and the shift in patterns of partisan distributions of income growth outlined above. In this subsection I shall elaborate the theoretical basis for assuming that international capital mobility would impact income inequality negatively. I shall argue that capital mobility tends to do so both directly – by shifting the market returns of production in favor of capital – and indirectly – by shifting political power in the favor of capital, thereby diminishing policymakers' ability to implement expansive and redistributive macroeconomic policies. The main emphasis in this thesis is on the latter, indirect effect, and as such it will receive the more thorough theoretical treatment. The theoretical discussion will lead to a number of specific hypotheses about the relationship between capital mobility and inequality of incomes in the United States. I shall end by reviewing the extant literature, highlighting methodological and theoretical errors which precludes it from providing satisfying answers to my hypotheses. These hypotheses will then be subjected to empirical testing in the next section.

The direct effect of capital mobility

The argument for a direct effect of international capital mobility on inequality is a simple one, and it relies on the same logic as the Heckscher-Ohlin Stolper-Samuelson model (Stolper & Samuelson 1941). In short, the model predicts that increases in levels of trade will benefit the abundant factor of production, and in time lead to the equalization of factor prices. That model depends on the assumption that returns to certain factors of production will vary between economies (in the case of the Heckscher-Ohlin model, due to different endowments of such factors) – which of course is borne out by our everyday experience. In such a scenario, increases in trade will benefit the abundant factor in an economy, as the products eligible for exportation will be those that rely more heavily on that factor of production (Rogowski 1987: 1122).

In this stylized model, both factors of production are locked in separate states connected by (varying levels of) trade. However, as international capital mobility

allows one factor of production to move more or less freely across borders, it allows capital to take advantage of these differences in returns. Thus, 'because capital mobility can substitute for trade' it too 'can have effects on the income distribution similar to those of trade' (Obstfeld & Taylor 2004: 12). To understand why this is so, it is important to note that both factors of production are in the last instance owned by individuals – that is, an individual can be a wage laborer, an owner of capital, or both. Whereas more trade will increase the returns to capital in a capital abundant state, international capital mobility allows an individual owner of capital to move her investments to a country with a higher demand for capital – yielding higher returns. However, a wage laborer faces a lot more restrictions on mobility due to barriers of law, language, and the cost of relocation. Thus, capital does not only have the ability to move in search of higher returns, but it can use this credible threat of "exit" (Hirschman 1970: 15-20; 20ff) to shortchange labor in wage negotiations. Consequently, an individual whose income is predominantly dependent on – or to a large degree reflects – returns to capital will be better off than an individual who relies on wage labor in a world of free capital mobility – all else equal. In the words of Frieden:

> It can hardly be bad for capitalists to have more investment options than before, which is what capital mobility gives them. By the same token, increasing the options of capital presumably reduces those of labor by making it less costly for capital to move rather than accede to labor demands. (Frieden 1991: 343)[14]

Thus, international capital mobility can be expected to increase the incomes of those whose remuneration is directly or indirectly linked to the returns to capital – such as business owners, high-level managers, and people employed in the financial industry (who tend to be among the highest-paid individuals), and decrease the incomes of those who rely on wage labor (who tend to be among the lower paid individuals).

[14] In his essay, Frieden argues that the dominant domestic cleavage under capital mobility will in fact be between the open and closed sectors, with workers and executives in the open sector finding common ground in a quest for increased competitiveness. I use him here only to outline the mechanism by which capital mobility, *ceteris paribus*, is expected to increase the returns to capital.

The indirect effect of capital mobility

The direct effect outlined above can be described as purely economical, in the sense that it does not take into account the effect of politics in determining the pre-tax market incomes of various groups. However, as political scientists we should at least be open to the possibility that, as Bartels notes in his conclusion: 'politics matters' (2016: 363). For instance, a political scientist might be tempted to suggest that redistribution through taxes should have an effect on income inequality. However, it is important to remember that Bartels work is based on *pre*-tax income levels – thus my analysis will similarly be limited to policies that might affect market incomes prior to direct redistribution through taxes.[15] Yet there are many other policy tools which will affect expected returns to different factors, such as regulations, minimum wage laws, and not least macroeconomic policy decisions. The exposition of the indirect effect of capital mobility will therefore proceed in two stages. First, the claim that politics does indeed influence pre-tax market incomes will be substantiated with reference to a train of thought known as the "partisan hypothesis". Then, international capital mobility's limiting effects on the partisan hypothesis will be outlined.

Bartels explicitly positions his contribution on income inequality within a literature concerned with the impact of partisan politics on macroeconomic outcomes, spearheaded by Douglas Hibbs (Bartels 2016: 35). Hibbs argued that governments of the left and the right pursue distinct macroeconomic policies which benefit their core constituencies. In short, these constituencies are 'class-defined' (1977: 1468), with differing preferences regarding the trade-off between inflation and employment, where

> the objective economic interests as well as the subjective preferences of lower income and occupational status groups are best served by a relatively low unemployment-high inflation macroeconomic configuration, whereas a comparatively high unemployment-low inflation configuration is compatible with the interests and preferences of upper income and occupational status groups. (Hibbs 1977: 1467).

If this is in fact the case, and given that

[15] The Census Bureau data does however include social transfers such as Social Security, which of course captures a lagged redistribution through taxes on the previous year's income.

> political authorities in the post-Keynesian age have considerable influence on macroeconomic outcomes, we would expect to observe (*ceteris paribus*, of course) a relatively low unemployment-high inflation macroeconomic configuration under leftist regimes and conversely under rightist regimes. (Hibbs 1977: 1471).

In other words, through macroeconomic policies of fiscal and monetary expansion leftist governments create high growth and low unemployment, while running a calculated risk of increasing inflation. This will benefit those low income groups whose incomes depend on having a job at all, and who will benefit the most from a rapidly growing economy. The costs of inflation might also be offset by the reduction in the value of debt, which is prevalent at lower income levels (Long 2015).[16] Conversely, rightist governments will pursue a tight monetary policy which keeps inflation in check and thus preserves the present value of capital to the benefit of its holders, running a risk of higher-than-trend unemployment due to lacking demand (Hibbs 1992: 362-3). Today, this analysis of American politics might sound surprisingly class-based, yet a famous mainstream economist at the time quipped that: 'You don't have to be a Marxist to realize that the class struggle and the class conflict-of-interest is being fought every four years in the election' (Samuelson 1977: 33).[17]

In short, Hibbs' argument can be restated as follows: Parties do in fact represent distinct constituencies, and they use government policy to successfully influence macroeconomic outcomes to the benefit of one constituency or the other – which of course is completely analogous to Bartels' argument in his chapter on 'The Partisan Political Economy' (2016: 33ff). Clearly, then, the partisan hypothesis relies on two important causal links: The first between government policy and macroeconomic outcomes – i.e. that policies are *effective*. The second between government policy and the partisan constituency the incumbent represents – i.e. that politics are *representative of fixed constituencies*. The hypothesized impact of international capital mobility on both these causal links will be discussed in turn.

[16] This offsetting of costs of inflation was poined out to me by Jonathon Moses.
[17] Hibbs himself is in fact wary of defining the Democratic Party as "leftist", and in his cross-sectional analysis of 12 industrial countries from 1960-69 he codes the United States as having zero years of 'Socialist-Labor' parties in the executive government (1977: 1474). Yet he does find, in a time series analysis of unemployment and inflation in the United States from 1948-1972, that Democratic presidents presided over lower levels of unemployment than Republicans (*Ibid*.: 1485-6).

International capital mobility and national policy autonomy

The partisan hypothesis presented above describes an economy in which government administrations 'can (and do) influence the rate of unemployment and inflation by manipulation of monetary and fiscal policy instruments' (Hibbs 1977: 1468). However, standard economic theory tells us that in an open economy – where there are no capital controls and the exchange rate is floating – fiscal policy will not be effective at expanding the domestic economy. This ineffectiveness is caused by an influx of capital in response to a fiscal expansion, which in turn causes the exchange rate to appreciate, shifting domestic consumption to imported goods. This shift will crowd out demand for domestic production, pushing the domestic economy back down to its pre-expansion level of output. An expansive monetary policy, however, will be effective as the interest rate reduction causes an outflow of capital and a subsequent reduction in the exchange rate. When the exchange rate floats, then, a permanent reduction in the domestic interest rate is achieved through exchange rate adjustments. Consequently, governments in open economies will have to rely on monetary policy to expand their economies (see for instance Oatley 1999: 1008-10; Moses 1994).

Now, as pointed out by Oatley, among others, this does not in itself preclude governments from pursuing *distinct* policies – leftist governments could still be more expansionary in their monetary policy choices than rightist governments. In that limited sense, it might still be correct to say that 'international financial integration does not eliminate the autonomy necessary to pursue distinct partisan macroeconomic policies' (Oatley 1999: 1004; see also Potrafke 2009). Yet the crux of the partisan politics hypothesis, as I understand it, is not primarily its assumptions regarding the ability of leftist and rightist governments to pursue *distinct* policies regardless of their *outcome*. Following Hibbs' foundational account above, it seems obvious that the aim of partisan politics is to enact policies which benefit core (class-based) constituencies. With that understanding of what counts as relevant autonomy in mind, one can imagine that monetary policy leaves considerably less leeway for administrations to shape macroeconomic policy to benefit their preferred constituencies. Whereas government investment or tax cuts[18] are easily tailored to specific income groups,

[18] As noted above, the direct taxation of income is not pertinent to the empirical analysis in this thesis. However, one would suspect that the overall distribution of the tax burden will influence future investment decisions, thus affecting subsequent pre-tax market incomes.

monetary expansion is a considerably less precise tool. As monetary policy works through expanding the money supply and easing credit, the decision about where to invest is taken by private actors (usually banks), and not the government. In fact there is no guarantee that these actors will choose to increase liquidity in the domestic economy at all, as they could decide to increase their balance sheets in off-shore markets instead.

It seems plausible, then, that a combination of capital mobility and floating[19] exchange rates impairs the ability of leftist governments to direct the benefits of macroeconomic expansions towards their core constituencies, as it necessitates a shift from fiscal to monetary policy instruments.[20] However, according to this account they should still be able to generate economic growth overall by relying on expansive monetary policies.

Proponents of the international capital mobility hypothesis (Webb 1991; Cerny 1994; Moses 1994) points out another way in which the free movement of capital might restrict the effectiveness of macroeconomic policies pursued by the left. They argue that, similar to the threat of exit outlined above, owners of capital will respond by moving – or threatening to move – their capital out of the domestic economy as a response to macroeconomic policies that might hurt their economic interests (Garret 1995: 667). Since expansive economic policies are usually perceived to carry with them the threat of higher inflation, and since inflation is especially harmful to owners of capital (Hibbs 1977; Samuelson 1977) – one would expect an outflow of investment which precludes leftist governments from pursuing their preferred policies (Obstfeld & Taylor 2004: 9-10). That is, in a scenario where

> … capital is internationally mobile, as it was by the late 1970s, the payments imbalances that emerge when different countries pursue different macroeconomic policies are too large to be ignored or managed; governments can reduce payments imbalances and stabilize their external economic positions only by coordinating their monetary and fiscal policies. (Webb 1991: 309-10).

[19] See Moses (1994) for an analysis of policy autonomy under capital mobility and fixed exchange rates.
[20] In other words, even if monetary policy is effective at expanding the total output of an economy, it might be less apt at distributing the profits from such an expansion towards the core constituencies of leftist parties.

Barring such international coordination, necessitated and governed as it is by the threat of exit from owners of capital, domestic governments will have to take the interests of capital into account when formulating fiscal and monetary policies - as a large scale outflow of capital will be detrimental for the prospects of growth in any economy. Furthermore, Streeck (2014) provides a comprehensive and convincing account of the dependency of modern industrialized states on the owners of internationally mobile capital – or the 'marktvolk' as he dubs them. In short, as states have come to rely on borrowing money by issuing government bonds instead of taxing capital and corporations (because the threat of exit provides a downward pressure on tax levels as well)[21] to fund their expansions, the potential buyers of that debt have considerable influence on the shape and size of government budgets (see Moses 1994 for the implications of this dependency for small open economies).

Thus owners of internationally mobile capital may influence policymaking indirectly by threat of exit, either from the national economy in general or from the bond markets, affecting either economic growth or the yield on government bonds.[22] In other words, owners of mobile capital can choose to make the country poorer, the government, or both.[23] Thus the 'implications of interdependence for fiscal and monetary policies are clear: governments no longer possess the autonomy to pursue independent macroeconomic strategies effectively, even if they were to seek to do so' (Garrett & Lange 1991: 543).[24] It seems that the ability of capital to leave (and enter) the domestic economy can be expected to influence policy autonomy in two distinct ways. First, by shifting the policy tools that are expected to be effective in steering the economy from fiscal to monetary policy, and second, by restricting governments' *de facto* ability to pursue – even through monetary expansion – policies which are potentially hurtful to the interests of capital. In the words of Makin,

[21] A pertinent example of this dynamic is the surreal situation that arose when Ireland protested the verdict of the European Commission that it had broken rules on state aid by taxing Apple at an effective corporate tax rate of 0.005 percent in 2014 (European Commission 2016). Normally, a state should be content when courts find that private corporations owe them money.
[22] That is, the interest rate the government has to pay to the owner of the bond.
[23] Describing the current debt crisis in developed nations, Piketty remarks that 'The rich world is rich, but the governments of the rich world are poor' (Piketty 2014: 540).
[24] Garrett and Lange do in fact argue against this strong interpretation of the capital mobility hypothesis, yet – or perhaps therefore – their presentation of the hypothesis is particularly succinct and accessible (Garret & Lange 1991: 542-3).

> … with globally integrated financial markets, foreign investors are able to pass judgment quickly on governments' economic policies. If such policies in any particular economy are perceived as unsound, international finance is suddenly withdrawn, with immediate implications for its exchange rate, interest rates, expenditure and production. (Makin 2000: 13).

This last point can both be construed as operating on the causal link between policy and outcomes (*effectiveness*), as an attempted expansion is counteracted by capital flight, and on the link between governments and constituencies (*representation*), as governments are forced to take the interests of international capital into account when devising policy.

International capital mobility and the domestic balance of power

So far, my discussion of the implications of capital mobility has been limited to the threat of exit – both on effectiveness and representation. But to make this treatment complete, and to avoid being trapped by 'the economist's bias in favor of exit' (Hirschman 1970: 17), we should also consider the potential impact of international mobility on the ability of capital to use "voice" (*Ibid*.: 30ff) as an instrument for securing its interests. This final argument rests on the assumption that domestic state politics is affected by 'the character of the international and transnational environment' (Cerny 1994: 321). Furthermore, I will rely on Rogowski's fundamental logic when explaining political cleavages by reference to changes in the international economic system (1987). Rogowski relies on the economic insight described above, namely 'that in any society protection benefits—and liberalization of trade harms—owners of factors in which that society is poorly endowed, relative to the rest of the world, as well as producers who use the scarce factors intensively' (Rogowski 1987: 1122). Furthermore, Rogowski makes the following assumption about the connection between economy and polity: 'that those who enjoy a sudden increase in (actual or potential) wealth and income will thereby be enabled to expand their political influence as well', and that they will use said influence to preserve or expand the situation which led to an increase in their wealth (*Ibid*.: 1123). This connection between wealth and influence is strongly supported by recent empirical research in the United States (Bartels 2016: 235ff; Bonica, McCarty, Poole & Rosenthal 2013;

Franko, Kelly & Witko 2016; Gilens 2012; Gilens & Page 2014). Although similar in design, this argument is slightly different from the "exit"-argument above, as it does not rely on the creation of a new constituency of internationally mobile capital, but merely strengthens the hand of an existing constituency operating within the domestic political system.

This leaves a second theoretical argument for why international capital mobility might affect policymaking on a national level: Owners of capital may use their increased relative wealth to influence politics directly through lobbying and campaign donations, similarly to owners of abundant factors under increased trade in Rogowski's model. However, the point about capitalists' impact on policymaking only holds if capitalists are assumed to have preferences that diverge from that of wage laborers, as per the partisan hypothesis. To recap, the main interests of capitalist should be to curb inflation to preserve the value of their wealth, to limit public deficits that must be financed by either taxes or debt (which in turn must be repaid through taxes or inflation), and at best an ambivalent attitude towards growth. Piketty's now-famous argument, that the rate of return to capital tends to exceed the growth of income and output, suggests that rentier capitalists would gladly forego rapid expansions of the economy to preserve their (relative) wealth (2014: 571). They should be similarly ambivalent towards unemployment levels, and certainly unwilling to accept inflationary pressures in order to lower them.[25]

Wage laborers would presumably have the opposite interests. Although inflation is by no means a welcome phenomenon, it is surely preferable to being unemployed. Furthermore, a higher stable rate of inflation would benefit workers who are in debt.[26] Similarly, higher incomes and output should also be welcome, on the undemanding assumption that wage earners prefer more income to less, and more consumption to less consumption. Wage earners should therefore be more positively inclined towards policies that promote higher growth rates and employment levels, although not without an eye towards inflation eating away their incomes.

If we put these interests into the model of political cleavages above, it is clear that an increase in political influence on the part of capital owners should lead to less expansive macroeconomic policy. Furthermore, as policy is shifted to accommodate

[25] If anything, owners of capital should prefer a level of unemployment that keeps the cost of labor as low as possible.
[26] Again, I owe this clarification to Jonathon Moses.

the interests of capitalists, their returns should increase relative to those of wage earners, cementing their political influence and exacerbating income inequality at the same time.[27]

Formulating hypotheses

The discussion above shows that international capital mobility can be expected to influence inequality both directly and indirectly. Directly, an owner of capital residing and paying taxes in the United States can maximize her returns by moving capital abroad in case of low returns (or stringent demands from labor) at home. Indirectly, international capital mobility could affect income inequality negatively by way of the increased influence of capital owners – both domestic and foreign – on national policymaking space. But in order to deduce specific hypotheses for the impact of capital mobility on inequality in the United States, we must first answer two related questions: What defines capital mobility, and when does it make sense to talk about the United States as 'having' international capital mobility?

There are four main ways to measure the concept of capital mobility in the literature: As the absence of barriers to capital mobility, as the volume of international transactions (inflows and outflows), as the price sensitivity of interest rates across borders, or as the decoupling of national savings and investment rates (Cerny 1994: 320-30; Makin 2000: 2; Oatley 1999: 1006). In this thesis, I shall rely on a version of the first operationalization. From the theoretical discussion above, it is clear that it is the *ability* of capital to respond to higher returns in another economy, or policies that it perceives as harmful, which is the defining feature of capital mobility. This is evident in most presentations of the capital mobility hypothesis, such as the one in Garrett (1995: 667): '[T]he easier it is for asset holders to move their capital offshore the stronger the incentives for governments to pursue policies that will increase rates of return on domestic investment'. The problem with measuring flows is that any given increase or decrease in flows may be caused by any number of factors which are unrelated to the ease with which capital *could* move in response to a policy proposal (Garrett 1995: 660). Similarly, price sensitivity seems less apt because the calculation of exchange rate risks and other factors is often hard to quantify – and may itself be a consequence of perceived political 'risks', such as fear of expansive

[27] This assumes that capital owners and those whose salaries are directly linked to the performance of invested capital receive at least part of the increasing returns to capital in the form of income, capital gains, or other dividends.

policies which might decrease the value of the foreign currency. Finally, the decoupling of domestic investment and savings rates presupposes that a high correlation between the domestic savings rate and investment rate *necessarily* implies that capital cannot move in response to policy changes or decreasing returns – however one can imagine that this relationship may hold even if capital has the potential to move, such as a scenario of high risk assessments with regards to foreign markets. Looking at barriers to mobility directly, then, seems to be both the most straightforward and theoretically valid way of measuring international capital mobility for the purposes of this thesis.

There is agreement in the literature, both among the more and less sympathetically inclined towards the capital mobility hypothesis, that exchange controls are one of the most important forms of capital controls available to governments (Henning 1996: 176-8; Makin 2000: 1, 9; Obstfeld & Taylor 2004: 29, 39). In the U.S. case, this is aptly illustrated by the fact that its capital controls *proper*, which were implemented by Lyndon Johnson to reduce capital outflows in connection with the Vietnam War and the Great Society Program, were lifted in January 1974 (Helleiner 1994: 111), mere months after the industrialized countries finally gave up on the Bretton Woods exchange rate system, in 1973 (Obstfeld & Taylor 2004: 38-9). According to Obstfeld & Taylor, the breakdown of Bretton Woods 'allowed the explosion in international financial markets experienced over the [following] three decades' (2004: 39). References to the end of the Bretton Woods era as the starting point for a period of international capital mobility – at a level not seen since before the Great Depression – are commonplace in the literature (Cerny 1994: 319-29; Henning 1996; Makin 2000: 1; Helleiner 1994). This should not come as a surprise, as one of the intensions of the restrictive Bretton Woods system was to 'prevent disequilibrating flows [of capital] responding to international interest rate differentials' (Helleiner 1994: 39).

It is clear then, that any presumed effects of international capital mobility on inequality in the United States, both direct or indirect, should become apparent after the move to a floating exchange rate in 1973 and the subsequent removal of remaining capital controls in January 1974. Although any strict cut-off will be arbitrary to some extent, 1974 appears to be as strong a candidate as any for hypothesis formation. Based on the theoretical discussion above, we can formulate the following hypotheses for the U.S. case:

H1: *Income inequality in the United States should increase after 1974, both as a direct effect of higher returns to capital, and as an indirect effect of capital's influence on domestic policy.*

H2: *Partisan differences in macroeconomic policy outcomes between Democratic and Republican administrations should be substantially diminished after 1974.*

As seen above, the reduction in partisan differences is over-determined in the sense that there are three separate mechanisms which yield the same hypothesized outcome as H2. These can be further specified as the following:

H2.1: *Partisan differences in macroeconomic policy outcomes should diminish after 1974 as a result of a shift from fiscal to monetary policy instruments. Specifically, this should limit the Democrats' ability to target expansions towards their core constituencies.*

H2.2: *Partisan differences in macroeconomic policy outcomes should diminish after 1974 as a result of decreased macroeconomic policy autonomy due to the potential for capital flight (exit). Specifically, this should limit Democrats' ability to create expansions of the economy in general.*

H2.3: *Partisan differences in macroeconomic policy outcomes should diminish after 1974 as a result of the increased political influence of owners of capital (voice). This effect is not necessarily limited to macroeconomic policy instruments.*[28]

Since the argument of my thesis relates to partisan inequality, and for brevity and clarity in the following, the empirical testing will focus on H2. Furthermore, as H2.2 and H2.3 are difficult to differentiate when it comes to macroeconomic outcomes (as diminishing partisan differences could be caused by voice, exit, or both) I will treat

[28] Some (Alvarez, Garret & Lange 1991; Garrett & Lange 1991; Garrett & Lange 1995; Garrett 1995) have argued that domestic institutions are likely to mediate the transmission of increased economic resources into political influence. Luckily, the United States is arguably closest to the ideal of "economic pluralism", which these authors hypothesize will come closest to parity between economic resources and influence.

them as one for the remainder of the essay. To be sure, examining the relative strengths of these two causal mechanisms could provide a fruitful avenue for further research. Before proceeding to the empirical results, a short overview of previous work on these (and related) hypotheses are in order.

Prior Research

To be sure, I am not the first to hypothesize that international capital mobility, and economic globalization in general, has had adverse impacts on the ability of national policymakers to manipulate their economies in order to achieve a more just distribution of profits. Already 20 years ago, it was almost considered 'a cliché to claim that economic internationalization has undermined the traditional redistributive agenda of the left' (Garrett 1995: 682). One scholar went even further, warning that the cliché had become so universal that it risked undermining its own importance (Cerny 1994: 324). However, previous attempts to test this hypothesis are limited in their capacity to cast light on the topic of this thesis. The following review proceeds somewhat chronologically, which nicely illustrates the importance of the temporal dimension to this research question.

The work of Hibbs (1977) is obviously relevant for H2, as he is regarded as the founder of the partisan hypothesis in American politics. In his article, Hibbs finds a significantly lower level of unemployment under Democratic presidents in the period 1948-1972, in addition to suggestive results in a cross-sectional analysis of 12 industrial countries from 1960-9 showing an unemployment/inflation trade-off. According to Alvarez, Garrett, and Lange, however, Hibbs findings were later disputed, and subsequent empirical tests have been unable to reproduce similar effects (1991: 540). According to Garrett and Lange, the 'common wisdom' at the time of their writing was that international capital mobility was to blame for this difficulty in reproducing partisan differences. However, they criticize the capital mobility literature for not taking labor market relations into account, and devise hypotheses by separating economies into pluralist ("weak-labor") and corporatist labor relations. Their argument is that expansive policies are efficient at producing growth only in economies with a strong corporatist bond between leftist governments and peak labor associations, although they admit that these countries have had to shift from demand to supply-side policies (Garrett & Lange 1991: 545-563). They conclude that 'contrary to common wisdom, this combination of [economic interdependence, increased competition, and economic decline] has not resulted in a pervasive trend toward convergence around neoliberalism' (*Ibid*.: 563).

The obvious problem with this explanation is that it does not allow for any change within economies – which means that an economy defined as corporatist

remains so throughout the period under review (1968-1987). This means that Garrett and Lange did not in fact study 'partisan separation', because changes in government within an economy is not reflected in their study (Garrett & Lange 1991: 563). Consequently, they certainly did not study changes in partisan effects over time – which is exactly what the capital mobility hypothesis entails – and admits at much when they write that their study

> …does not, however, shed light on the effects that changes in government within individual countries over much shorter periods of time have had on economic strategies. Why do some governments, such as the 1974-79 Labor government in Britain and Mitterrand's administration in France, initially pursue partisan policies but subsequently turn dramatically away from them? (Garret and Lange 1991: 563).

Arguing in the same vein, Alvarez, Garrett and Lange hypothesized that

> In countries with densely and centrally organized labor movements, leftist governments can promote economic growth and reduce inflation and unemployment. Conversely, in countries with weak labor movements, rightist governments can pursue their partisan-preferred macroeconomic strategies and achieve similarly beneficial macroeconomic outcomes (Alvarez, Garrett & Lange 1991: 539).

If this is the case, and on the assumption that the United States would end up in the "weak labor" category, we would expect to see that Republicans did better at producing beneficial macroeconomic outcomes in the United States, and that this relationship holds over time. In his book, Bartels has already shown that this is not the case (2016: 52-7). Below, I shall show that – contrary to the labor relations argument – Democratic presidents did in fact pursue expansive policies in a weak labor economy with beneficial results.

A common approach to testing the capital mobility hypothesis has been to employ cross-sectional time series data of a select group of OECD countries (Garrett 1995; Midtbø 1999; Oatley 1999; Potrafke 2009). The problem with this approach is that it does not adequately control for differential timing when it comes to liberalization of capital markets. Whereas the United States removed its capital controls in 1974, France and Spain had capital controls in place until the late 1980s,

and Greece did not remove theirs until the 1990s (Potrafke 2009: 118). Similar differentials exist when it comes to switching to floating exchange rates. There also appears to be a pattern where small open economies with a corporatist labor market structure have 'had relatively closed financial markets' until quite recently (Garrett 1995: 660). Although the authors are clearly aware of these factors, precious little has been done to avoid comparing government activism in small open economies which only recently went through capital market liberalization, to larger economies with a longer history of capital mobility. Instead of controlling for an effect of capital mobility on partisan politics *within* economies, the studies are in fact – at least for most of the period under review – simply comparing partisan effects *between* economies. Furthermore, some studies are so temporally limited that they cannot be expected to pick up the full impact of the pre/post capital mobility shift in most economies (for instance, Potrafke 2009 only looks at social expenditure in OECD countries from 1980 to 2003).

Finally, and perhaps most importantly, expansive macroeconomic policy, or policies associated with the "Keynesian Welfare State" (Garrett 1995: 657) are consistently operationalized as either government spending and/or deficits (Garret 1995: 659; Oatley 1999: 1003; Potrafke 2009: 105; see Midtbø 1999 for a rare exception to this trend). Yet, just as with the definition of *distinctive* partisan politics above, there seems to be a mismatch between this operationalization and the theoretical concept it is meant to capture. An illustrious example is that the governments of Thatcher and Reagan have little impact on Potrafke's results because neither did much to reduce social transfers (2009: 118-9), similarly, as described in Bartels (2016: 136ff), Bush left behind a sizable deficit due to tax cuts for the rich and wealthy. Still, something does not feel right about labeling these administrations as proponents of the Keynesian welfare state. Oatley is refreshingly upfront about this, and argues explicitly against using outcomes as dependent variables because it 'tests joint hypotheses: (a) governments retain the ability to target fiscal and monetary policy at their most preferred domestic economic objectives and (b) fiscal and monetary policies generate the desired macroeconomic outcomes' (1999: 1012). By just focusing on 'policy instruments' such as budget balances, Oatley seems to think that he escapes this problem. He notes, correctly, that:

> For example, if no systematic relationship is found between partisanship and, say, rates of unemployment, it could be because expansionary fiscal and monetary policies are ineffective at reducing and raising unemployment, or it could be because mobile capital prevents governments from adopting policies that, in the absence of capital mobility, would otherwise be effective. (Oatley 1999: 1012-3).

Consequently, one would imagine, Oatley fancies that his approach allows him to accurately capture only (a). However, his own approach relies on untested assumptions as well, namely that the policy instrument which correlates with a certain party in government was voluntarily adopted by that party with the intention of benefitting its constituency. How, then, are we to interpret the fact that Oatley reports policy convergence in the 1990s, not as a consequence of more financially prudent leftist governments, but because 'rightist governments moved into deficit' (1999: 1017)? Either we are all – unwittingly – living in a neo-Keynesian era, or one must have to accept that there are potential explanations for moving into deficits beyond pursuit of an expansive macroeconomic policy: Streeck (2014) offers one, Oatley, by blaming the recession in the 1990s for increased deficits, offers another. This difficulty highlights the *importance* of including outcomes as dependent variables.

Although Oatley's logic is sound, he adopts a solution which does not solve the problem – at least not without throwing the baby out with the bathwater. The entire point of partisan politics, both as a subject of study and as a pursuit, is presumably its differential impact on the well-being of core constituencies. Oatley's results, due to his methodological approach, leaves us non the wiser about the question: 'Are leftist governments equally capable to promote the well-being of their constituencies under international capital mobility?'. His reservations against testing joint hypotheses are well founded, but the solution should not be to forego outcomes as dependent variables altogether, but to search for instances where there *does* appear to be a correlation between partisan politics and outcomes, and use these as baselines for our comparisons.[29]

[29] Sticking to the approach of Oatley *et. al.* is paramount to accepting a view of politics where leftist governments' only ambition is to deteriorate public finances without any regard to its effects on macroeconomic performance or distributional outcomes. This seems to be a strain on credibility that equals, if not surpasses, that of assuming government policies to be effective - at least in some scenarios, some of the time. Furthermore, it is also counter to Keynes' own view on how to use macroeconomic policy instruments in a booming economy.

One notable recent exception to the methodology described above appears in Keller and Kelly's analysis of how financial deregulation has helped create income inequality in the United States (Keller & Kelly 2015). In short, they look at U.S. income inequality in the period 1914-2010 using the Piketty and Saez (2007) top incomes database. They first establish a connection between financial deregulation and inequality, and then a connection between Democratic power in Washington and declining deregulation. However, they find that this partisan effect on deregulation has 'diminished since the early 1980s' (i.e. that the parties have converged on matters of financial deregulation), which they explain with 'globalization, the increasing availability of credit, and shifts in campaign finance' (*Ibid*.: 428). Yet, they operationalize 'globalization' by a measure of trade openness, and thus rely on slightly different theoretical argument than mine above (i.e. that increased competitiveness leads to deregulation) (*Ibid*.: 431; 437-8). Furthermore, they limit their analysis to a single policy instrument (regulation of finance) and a single macroeconomic outcome (inequality). Thus, even though their results are certainly in line with my hypotheses – especially the fact that their post-1981 dummy is the strongest variable in explaining regime change – they do not settle the question of whether international capital mobility has diminished overall macroeconomic policymaking space.[30]

Two recent additions to the literature do in fact employ a longitudinal case study approach with U.S. income inequality as the dependent variable (Jacobs & Myers 2014; Jacobs & Dirlam 2016). Unfortunately, neither provide much insight into the puzzle presented above. The most recent of the two employs pooled cross-sectional time series data of 1,615 state-years from 1978 to 2011 to analyze the rise in income inequality in 49 U.S. states since 1980 (Jacobs & Dirlam 2016: 478). They do report a few results which are in line with the hypotheses above, among others that the decline in well-paying manufacturing jobs has contributed to inequality, which is in line with H1 to the extent that capital mobility has increased the ability of U.S. companies to move their production lines elsewhere. However, like Bartels, their main focus is on explanatory variables at the nation state level. Specifically, they

[30] They also focus their analysis on Democratic control of the Senate, as they find little evidence of a presidential effect on regulatory policy (Keller & Kelly 2015: 436-7). This is of course counter to the partisan hypothesis as developed by Hibbs (1977), and results in Bartels (2008; 2016), Jacobs & Myers (2014), and Jacobs & Dirlam (2016).

emphasize the importance of the neoliberal policies and ideology which were heralded by Ronald Reagan's arrival in the White House (*Ibid*.: 472-5). There are three main weaknesses in Jacobs and Dirlam's account: First of all, the neoliberal agenda of Ronald Reagan does not help explain why Democratic presidents opted for less expansive policies and less redistribution. If the only problem since the 1980s was that Republican presidents became *more* Republican – in the sense that their resolve to check inflation and reducing public investment became stronger – we would expect an *increase* in the partisan income growth differentials. Secondly, if the election of Ronald Reagan heralded a new governing ideology which impacted Republicans and Democrats alike (which Jacobs and Dirlam allude to in their discussion (*Ibid*.: 493-4)), this cannot explain the sharp turn away from the traditional Democratic macroeconomic policy mix which occurred under Jimmy Carter (see the discussion of Carter's presidency below). Finally, when Jacobs and Dirlam criticize previous studies for examining the presence of Democratic, and not Republican, presidents – even though 'cross-national research shows that when the absolute effects of Right versus Left parties are compared, parties on the Right have more substantial positive effects on inequality than Left parties' negative effects' (*Ibid*.: 493) – they ignore the possibility that this may not always have been the case. That they do find a positive correlation between Republican presidents and inequality after 1980, and especially under Ronald Reagan, is of course in line with the hypothesized impact of Republican presidents under the partisan hypothesis. The descriptive results above suggest that this relationship would also hold for Republican presidents *before* Ronald Reagan.

In the same vein, Jacobs and Myers focus on 'shifts in the resources of the anti-union neoliberal factions within both U.S. political parties' in their explanation of increased income inequality (2014: 754). Although I am sympathetic to the argument that reducing the political resources of a social group will lead to worse distributional outcomes for said group (see H2.3 above), this initial shift in political resources cannot simply be left to a *deus ex machina*.[31] While Jacobs and Myers' solution, namely to use a neoliberal ideological shift in both parties after Ronald Reagan as an increase of the political resources of businesses, is plausible, it runs into the same difficulties as apply to Jacobs and Dirlam above. However, Jacobs and Myers are

[31] The theoretical foundation in both Jacobs and Myers (2014) and Jacobs and Dirlam (2016) is the 'power resource theory' as outlined in Korpi (1985).

more upfront about defining Clinton as a neoliberal, referencing his signing of NAFTA, welfare reforms, and deregulation of the financial industry – which they use to explain the fact that in their statistical models, Clinton contributed more to inequality than did Ronald Reagan (*Ibid*.: 756, 767). Whether their main finding, that 'unions could influence inequality only before the neoliberal departure that was initiated by Reagan and sustained by subsequent administrations' (*Ibid*.: 767), is caused by an autonomous and ubiquitous shift in political ideologies – or is in fact a consequence of a shift in the domestic political power balance due to the increased mobility of capital – will be difficult to adjudicate within the scope of this thesis.[32]

Compared to the literature reviewed above then, Bartels' approach is definitely an improvement over much of the previous research in that he does in fact presuppose, and empirically substantiate, the existence of partisan differentials in macroeconomic outcomes over time. His error is to assume, in his first book, that these differentials would not be impacted by the veritable revolution of the international economic order which took place in the middle of his time series. In the second edition of his book, Bartels does note the apparent shift in effects of Democratic incumbents before and after 1981, but downplays the importance of this finding because none of the differences between time periods are statistically significant (Bartels 2016: 62). In effect, Bartels seems to be arguing that if the parameter estimates are large enough in one time period to be averaged over an extended time period without losing significance, this counts as evidence that the causal mechanisms involved in the two time periods are continuous. If we take political science seriously, we should reject that line of reasoning vehemently. On the contrary, the strength of statistical evidence in one time period has no bearing on our knowledge about another time period (or any other case, be it temporal or spatial) unless there is a convincing theoretical argument that the time periods are identical in the aspects relevant for our study. In other words, it is only on the condition that we have a sound theoretical basis for assuming that the two time periods are identical in the relevant respects, that we can apply Bartels' logic. As I have argued extensively above, and to the contrary, we have every reason to suspect that there *is* a difference

[32] To be sure, the two accounts are not necessarily mutually exclusive. In fact, H2.3 is an attempt at reconciling the two, on the implicit assumption that the ability of an ideology or worldview to gain traction in policymaking circles is (at least in part) a function of the political resources available to those who espouse – and presumably benefit from – those ideas.

in Democratic administrations' ability to affect macroeconomic outcomes after 1974. It is now time to test this suspicion against the data.

Bartels' Case for Partisan Inequality – Growth is Good

A reader familiar with Bartels' book will be forgiven for thinking that my puzzle is somewhat overstated, and that Bartels' argument is stronger than suggested by the discussion above. In order to do justice to Bartels' impressive work, and to set the stage for my explanation of the *shift* in distributional patterns, it is necessary to present how Bartels identifies and explains the observed partisan differences. To test the robustness of the descriptive differences reported above, Bartels employs several regression analyses. First, to show a significant effect of Democratic presidents on the annual average income growth rates of the different groups, and then to show that this is in large part explained by Democrats' ability to produce higher GDP growth and lower unemployment – in line with the partisan hypothesis. Finally, Bartels presents an overview of economic policies over the past six and a half decades, complete with case studies on tax policy and the eroding minimum wage to substantiate these statistical findings. I shall argue that Bartels findings, where they are in fact robust, lend themselves to my alternative explanation – namely that of decreased economic policymaking space due to the increased mobility of capital.

One of the most suggestive robustness tests of the partisan hypothesis by Bartels relates to what is known as a president's "honeymoon period". This term refers to a newly elected president's substantial influence on policy in his first year in office (Bartels 2016: 40). Since an incumbent's influence on income growth is lagged by one year, one would assume this "honeymoon period" to manifest itself in the second year of a presidential term – or the first year for which the incumbent gets credit in Bartels' and my analysis. Bartels shows that the Democratic income premiums are almost exclusively limited to the "honeymoon period" of each term – in fact there is no statistically significant difference in income growth rates between Democrats and Republicans in other years (*Ibid*.). This is highly suggestive of the fact that presidential policies do have substantial impacts on income growth, as the likelihood that the differences in income growth rates between Democrats and Republicans are not only coincidental, but also by chance relegated to the honeymoon period of each term, is remote. One hopes even Oatley might be persuaded by such a strong and consistent finding. Bartels presents these findings graphically in his Figure 2.2, which contrasts the Democratic growth premium (i.e., the differences in annual average growth rates between Democratic and Republican incumbents) in the

"honeymoon period" with those of the three remaining years of each president's term (*Ibid*.).

Although I will not reproduce that graph, it is interesting to note that the shift in distributional patterns is robust to the same test. Figure 11 is a replication of Bartels' Figure 2.2, only instead of contrasting honeymoon years with other years, it contrasts the Democratic income growth premium in honeymoon years before and after 1974. The results are strikingly similar to those presented in Figures 7 and 9 above, and show how the distributional effects of the growth premium in honeymoon years have all but vanished (although each income level still does substantially better in Democratic honeymoon years than Republican ones). This graph is a useful way to illustrate that where Democratic presidents used to differ from Republicans on both the *level* and *distribution* of income, they are now merely replicating the distributional pattern of Republican incumbents at a higher absolute level of growth. These changes are of course in line with those hypothesized in H2.1 and H2.2.

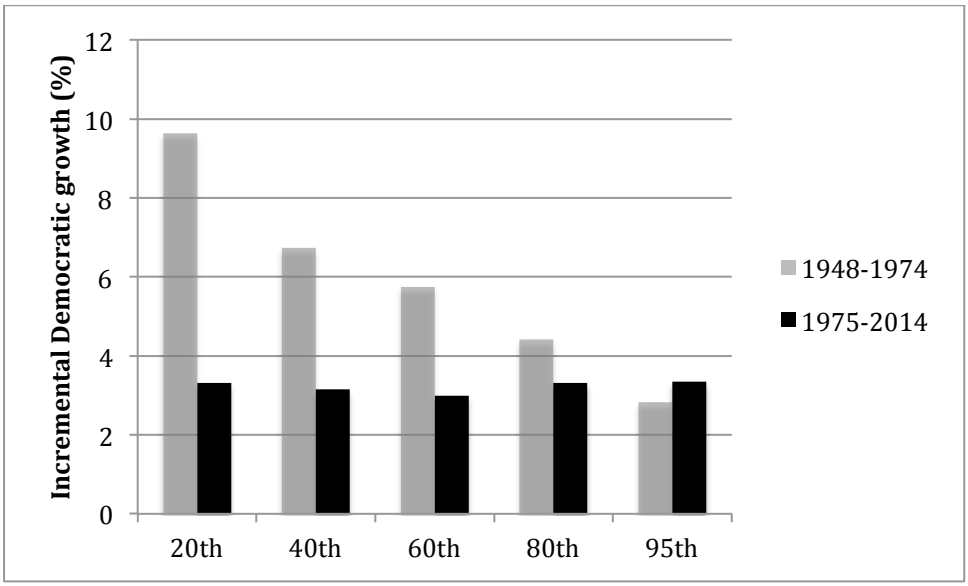

Figure 11: Democratic Income Premium in Honeymoon Years, 1948-1974 and 1975-2014. Source: U.S. Census Bureau.

Bartels also tests the hypothesis that the distributional patterns he describes are merely a consequence of a 'cycle of equilibration' where 'Democrats pursue expansionary policies in reaction to Republican contractions and Republicans produce contractions as an antidote to Democratic expansions' (*Ibid*.: 41). After comparing the Democratic income premium in first terms with terms where the incumbent succeeded

himself or a member of his party, Bartels finds that the Democratic premium is actually bigger in second and subsequent terms than in first terms (*Ibid*.: 41-3). The problem with this test is that there has only been one instance of a Democratic president succeeding himself or a member of his party since 1974 (Clinton's final term) for which Bartels has the relevant data (Obama's second term, with a one year lag for economic policies to take effect, started in 2014 and will end with 2017). In other words, Bartels is in effect comparing the growth under Truman's 6 years in office, Johnson's 6 years[33] and Clinton's last 4 years with that of Kennedy, Carter, Clinton and Obama's first terms. With the exception of Clinton's last term and Kennedy's first, this corresponds to a pre-/post 1974 split of the presidencies, which probably explains why growth was substantially lower in the latter. An internal comparison of Clinton's two terms shows that annual average income growth at the 20th percentile was a full percentage point higher in his *first* term than his second (2.5 versus 1.4 respectively). To be sure, this cannot be taken to refute Bartels' claim that partisan effects might be larger in subsequent terms in general, but it is worth highlighting that his finding relies entirely on growth rates under the first three post-war Democratic presidents.

So far the discussion has been limited to descriptive statistics, at times with a precariously low number of observations, and without controlling for the myriad of other factors that are likely to affect the distribution of income. However, Bartels also uses regression analyses of the family income data to show that there are partisan differences in income growth which are statistically significant. More specifically, Bartels' uses a seemingly unrelated regression analysis to take account of the fact that the residuals of each separate regression analysis will be highly correlated – since factors outside the model that affect one income group's income level are likely to also affect other income groups (*Ibid*.: 43; Zellner 1962). In other words, Bartels runs a series of separate regressions for the annual income growth of each group, with a regression model that corrects the estimated parameters to account for cross-correlation in residuals between the regressions.

[33] Alternatively, Johnson's 4 years after his re-election. Bartels does not offer specifics on how he counts the instances of less traditional transfers of power, i.e. that of Kennedy/Johnson and Nixon/Ford.

In his basic regression model, Bartels includes control variables for changes in oil prices[34] and labor force participation[35], as well as two trend terms[36] to control for the various glacial social and economic trends discussed above (Bartels 2016: 43-6). Bartels also controls for autocorrelation by including a lagged version of the dependent variable (i.e. the previous year change in annual income level for each group), as well as for a lagged version of income growth at the 95th percentile. Finally, the main independent variable is of course a dummy for Democratic control of the presidency. Table 2 presents the results from my replication of this analysis, using the same estimation method. [37]

The main takeaway from this analysis is of course the significant positive effect of democratic incumbents on the annual average income growth of the various groups. At the 20th percentile, holding the control variables constant, annual income growth is about 2 percent higher than under a Republican incumbent. Democratic incumbents have significant positive effects for each group at the standard level of significance, except for the 95th percentile. Furthermore, the Democratic premium is reduced by each step up the income distribution, in line with Bartels' previous findings. It is also noteworthy that families at the lower end of the income distribution benefit substantially from higher labor force participation, and that this effect, while significant at all income levels, decreases towards the top. This is most likely due to the fact that women joining the work force are likely to contribute substantially to the

[34] Following Bartels (2016: 44n) I use annual percentage changes in the price of oil (West Texas Intermediate) compiled by Dow Jones & Company, available at the Federal Reserve Bank of St. Louis (https://fred.stlouisfed.org/series/OILPRICE/). This dataset was discontinued in 2012, so data for the last two years are based on monthly data from the U.S. Energy Information Administration, also available at the Federal Reserve Bank of St. Louis (https://fred.stlouisfed.org/series/MCOILWTICO).

[35] Following Bartels (2016: 45n) I calculate the percent change in the average annual level of non-institutionalized civilians who are employed or seeking employment, as reported by the Bureau of Labor Statistics (http://data.bls.gov/timeseries/LNS11300000).

[36] Following Bartels (2016: 46n) I use a simple trend running from 0 in 1948 to 1 in 2014, and a quadratic trend which is simply the first trend variable squared.

[37] In order to assess Bartels' findings and expand on his analysis I have replicated his regressions using the same datasets and model specifications. To allow for comparison between the model specifications used by Bartels and my improved models, I will only report parameters from my replications in the text. The results of my analyses are qualitatively identical to Bartels' results, although my variable for changes in oil prices has a bit more explanatory power than his, whereas his labor force variable has a bit more power than mine. Appendix A contains the parameter estimates from Bartels' basic model (Table 2.2, p. 44) for comparison.

total income of a poorer family, while the need for women to work – and their incomes relative to their husband's – is likely to be smaller in wealthier families.

Table 2:

Statistical Analysis of Income Growth, 1949-2014

Annual real pre-tax income growth (percent) for families at various points in the income distribution. Parameter estimates from seemingly unrelated regression (standard error in parenthesis, * = p<0.05 ** = p<0.01). Partisan control is lagged by one year.

	20th percentile	*40th percentile*	*60th percentile*	*80th percentile*	*95th percentile*
Democratic president	2.09** (.73)	1.40** (.51)	1.30** (.48)	1.09* (.46)	.52 (.58)
Oil prices (lagged)	-.0338* (.0166)	-.0363** (.0120)	-.0386** (.0112)	-.0306** (.0108)	-.0350** (.0135)
Labor force (percentage change)	4.23** (1.32)	4.02** (.95)	2.65** (.89)	2.25** (.85)	2.73** (1.05)
Lagged dependent	-.219** (.080)	-.267** (.070)	-.294** (.072)	-.336** (.084)	-.034 (.111)
Lagged 95th growth	.403** (.147)	.264* (.108)	.227* (.101)	.249* (.104)	–
Linear trend	-14.42* (6.13)	-16.13** (4.44)	-11.22** (4.12)	-8.14* (3.96)	-5.06 (4.90)
Quadratic trend	11.70 (6.19)	13.03** (4.48)	7.79 (4.15)	5.38 (4.00)	3.67 (4.96)
Intercept	3.02 (1.30)	4.25 (.95)	4.04 (.88)	3.62 (.85)	2.98 (1.05)
S.E.	2.82	2.00	1.88	1.83	2.25
Adjusted R^2	.38	.49	.43	.33	.25
N	66	66	66	66	66

Source: U.S. Census Bureau.

Not surprisingly, all income levels are adversely affected by spikes in the price of oil. Seeing as this effect is more or less equal across income levels, shocks in oil prices do not contribute much to income inequality.[38] Furthermore, the lagged dependent variable shows that each income level is negatively affected by income growth in the previous year, which suggests 'some tendency towards equilibration' (Bartels 2016: 46) – however this does not apply to families at the 95th percentile. Finally, the trend variables show that income growth has slowed substantially for all families over the period, but that this effect is less pronounced for the wealthiest families, thus these families 'have been surprisingly insulated from the structural shifts in the U.S. economy that have eroded income growth among less affluent families' (*Ibid*.). All in all,

> [t]hese statistical results provide strong evidence that the striking differences in the economic fortunes of middle-class and working poor families under Democratic and Republican administrations are not an artifact of the different conditions under which Democratic and Republicans have happened to hold the reins of government, but a reflection of the fundamental significance of partisan politics in the political economy of the postwar United States. (Bartels 2016: 47)

Bartels' theoretical exploration of the causal links between a Democratic president and more egalitarian income growth are analogous to Hibbs' argument, in short that Democratic administrations have led interventionist macroeconomic policies to boost growth and employment, while Republicans have focused on avoiding inflation and reducing federal spending. Bartels illustrates this policy divergence with a brief summary of macroeconomic policy from Eisenhower to Reagan, which itself draws heavily on work by Hibbs (*Ibid*.: 48-54). Empirically, Bartels substantiates the partisan hypothesis by a comparison of macroeconomic performance between Democratic and Republican incumbents. The regression results in Table 3 and 4 report the results from my replicating the analysis in Bartels' tables 2.3 and 2.4 respectively, where the first analysis aims at showing a significant partisan difference in macroeconomic performance, and the latter then incorporates

[38] Although, in terms of spending, increased expenditure on gas and heat will disproportionately affect poor families who are forced to allocate a larger portion of their budget to bare necessities.

these macroeconomic indicators in the analysis of differential income growth reported in Table 2 (*Ibid*.: 55-6).

Table 3:
Statistical Analysis of Macroeconomic Performance, 1949-2014

Growth in real GDP per capita (percentage change in annual average), the level of unemployment (as a percentage of the labor force) and inflation per year (percentage change in annual average). Parameter estimates from seemingly unrelated regression (standard error in parenthesis, * = p<0.05 ** = p<0.01). Partisan control is lagged by one year.

	Real per capita GDP growth (%)	*Unemployment (%)*	*Inflation (%)*
Democratic president	1.66** (.50)	-.66** (.21)	.55 (.45)
Lagged dependent	-.274** (.075)	.861** (.043)	.484** (.108)
Oil prices (lagged)	-.0345** (.0110)	.0160** (.0048)	.0106 (.0121)
Labor force (percentage change)	2.52** (.94)	-1.23** (.39)	1.48 (.83)
Linear trend	.86 (4.18)	2.09 (1.82)	10.38* (4.35)
Quadratic trend	-1.58 (4.21)	-2.42 (1.82)	-9.56* (4.37)
Intercept	1.88 (.90)	.88 (.42)	-.72 (.82)
S.E.	1.95	.85	1.77
Adjusted R^2	.27	.72	.56
N	66	66	66

Sources: Bureau of Labor Statistic; Bureau of Economic Analysis.

The results reported in Table 3 offer strong support for the partisan hypothesis, and seem to negate my H2. Controlling for historical trends,[39] changes in the price of

[39] Among other things, these trend variables (particularly the quadratic trend) should pick up some of the effects of the explosion in cross-border capital flows in the latter half of the period.

oil, and the size of the U.S. labor force still leaves a significant positive impact of a Democratic presidency on real GDP growth: More than 1.5 percent annually. Unsurprisingly, spikes in oil prices hurt the U.S. economy while a larger share of the population joining the labor force boosts growth, all else equal. Similarly, Democratic incumbents are significantly correlated with lower unemployment – with Democratic presidents presiding over more than half a percentage point lower levels of unemployment on average. However, there is no significant evidence that Republicans are in fact more capable of fighting inflation. It is also interesting to note that while GDP growth is self-correcting (that is, a boon one year is correlated with a slump in the next) both inflation and, especially, unemployment are reinforced by their previous levels (suggesting longer periods of sustained rise/fall).[40]

However, it is by combining the results of Table 2 and 3 that Bartels completes his argument in favor of the 'partisan inequality' hypothesis. Table 4 incorporates the three macroeconomic indicators in the analysis of income growth for the various groups. As Bartels notes, the Democratic premium is now more or less evenly distributed between the income groups (Bartels 2016: 54-5) (and only reaches conventional levels of significance for families at the 80th percentile). The effects of GDP growth however, mirrors the distributional effects attributed to Democratic incumbents: Families at the lower end of the distribution reap significant benefits from an increase in real GDP – with families at the 20th percentile converting a 1 percentage point increase in GDP growth into a 0.8 percentage point annual increase in their real income levels. Families at the very top, however, are not significantly affected by GDP growth at all. The other macroeconomic factor related to Democratic incumbents, namely lower levels of unemployment, behave in the same way. The impact of lower unemployment is disproportionately geared towards the middle and working classes, but has no impact on income for families at the top. The last macroeconomic indicator, however, has the opposite distributional properties. While inflation does tend to affect all income levels negatively, that effect is stronger the higher up the income distribution. For the wealthiest families, a 1 percentage point increase in inflation will reduce the income growth rate by half a percentage point, all

[40] Consequently, a standard test of autocorrelation (the *corrgram* command in STATA) reveals the presence of autocorrelation for these two variables, yet the same tests reveal that a one-year lag is sufficient to control for this effect.

else equal. These results are a strong confirmation of the model of partisan politics developed by Hibbs.

Table 4:
Statistical Analysis of Income Growth, Including Macroeconomic Conditions, 1949-2014

Annual real pre-tax income growth (percent) for families at various points in the income distribution. Controlling for growth in real GDP per capita (percentage change in annual average), the level of unemployment (as a percentage of the labor force) and inflation per year (percentage change in annual average). Parameter estimates from seemingly unrelated regression (standard error in parenthesis, * = p<0.05 ** = p<0.01). Partisan control is lagged by one year.

	20th percentile	*40th percentile*	*60th percentile*	*80th percentile*	*95th percentile*
Democratic president	.72 (.59)	.57 (.39)	.59 (.35)	.77* (.39)	.67 (.55)
GDP growth (%)	.781** (.144)	.532** (.094)	.485** (.084)	.314** (.092)	.163 (.126)
Unemployment (%)	-.536* (.230)	-.450** (.147)	-.383** (.134)	-.210 (.144)	.075 (.196)
Inflation (% change)	-.130 (.133)	-.253** (.088)	-.283** (.080)	-.372** (.089)	-.533** (.124)
Oil prices (lagged)	-.0072 (.0141)	-.0112 (.0093)	-.0129 (.0087)	-.0058 (.0095)	-.0071 (.0131)
Labor force (% change)	2.22* (1.07)	2.77** (.72)	1.63* (.65)	1.95** (.72)	3.35** (1.00)
Lagged dependent	-.138 (.086)	-.226** (.071)	-.251** (.072)	-.318** (.086)	-.007 (.105)
Lagged 95th growth	.173 (.118)	.106 (.083)	.091 (.076)	.182* (.091)	–
Linear trend	-7.32 (5.39)	-7.32* (3.60)	-2.52 (3.28)	.66 (3.63)	4.38 (5.05)
Quadratic trend	6.01 (5.33)	5.41 (3.55)	.19 (3.24)	-2.64 (3.59)	-5.33 (4.99)
Intercept	4.28 (1.54)	5.33 (1.03)	4.87 (.93)	3.86 (.99)	1.99 (1.32)
S.E.	2.12	1.38	1.28	1.43	1.94
Adjusted R^2	.65	.76	.74	.59	.44
N	66	66	66	66	66

Sources: Bureau of Labor Statistic; Bureau of Economic Analysis; Census Bureau.

Including these macroeconomic outcomes in the statistical analysis also impacts the control variables in the basic model. Changes in oil prices are no longer significantly correlated with income growth rates, and the impact of the trend variables are similarly reduced – as is the "trickle down" effect captured by the lagged variable for income growth at the 95th percentile. Perhaps more surprisingly, the impact of increases in the labor force has been reversed after controlling for macroeconomic factors: In the new model, income growth at the top is more positively related with increases in the civilian labor force. This could be due to the fact that very wealthy families are more likely to be employers than employees – or at least more likely to have their remuneration linked to the performance of a company than the median market wage – in which case a larger labor force would translate into lower costs of production and thus higher profits.

As mentioned above, reviews of the first edition have already pointed out the possibility of a time effect which might affect these results, and in response Bartels included a new sub-chapter to investigate that possibility (*Ibid*.: 57-62). One commentator (Pollin 2010) points out that inequality, measured as the ratio of family incomes at the 80th percentile to families at the 20th percentile, did not decline under Bill Clinton's two terms – despite strong growth. Similarly, the 80/20 ratio did not decline under Obama's first term either (in fact it increased slightly, from 4.28 in 2010 to 4.43 in 2014). These observations are all in line with H2.1. Bartels admits that 'changes in the parties' ideologies and economic policies may have blunted traditional patterns of income growth' and that, in fact, 'recent Democratic presidents have not managed to *decrease* income inequality—as their Democratic predecessors did—but have merely stemmed the rate of increase' (Ibid.: 58-9). Yet, Bartels warns that 'partisan comparisons of this sort over a relatively short period of time—33 years spanning five Republican and three Democratic terms—are especially subject to potential biases stemming from differences in the circumstances under which each party held the White House' (*Ibid*.). To control for such biases Bartels recreates the analysis reported in Table 2, but with separate variables for Democratic incumbents before and after 1981, and a new control variable for the period after 1981 (Bartels 2016: 60). For the sake of brevity, Table 5 only reports the relevant parameter estimates from my replication of that exercise.

Table 5:
Statistical Analysis of Income Growth with 1981 Dummy, 1949-2014

Annual real pre-tax income growth (percent) for families at various points in the income distribution. Parameter estimates from seemingly unrelated regression (standard error in parenthesis, * = p<0.05 ** = p<0.01). Partisan control is lagged by one year.

	20th percentile	*40th percentile*	*60th percentile*	*80th percentile*	*95th percentile*
Democratic president (1949-81)	2.37* (.99)	1.49* (.70)	1.26 (.66)	.80 (.63)	-.21 (.78)
Democratic president (1982-14)	1.95 (1.13)	1.38 (.81)	1.39 (.76)	1.49* (.73)	1.47 (.90)
Post-1981	.87 (1.56)	.38 (1.12)	.01 (1.04)	-.12 (1.01)	-.44* (1.24)
Intercept	3.04 (1.39)	4.29 (1.01)	4.10 (.94)	3.82 (.90)	3.43 (1.10)
S.E.	2.82	2.00	1.88	1.83	2.22
Adjusted R^2	.38	.49	.43	.34	.27
N	66	66	66	66	66

Source: U.S. Census Bureau.

Bartels considers that 'there is a good deal of statistical evidence of higher income growth rates under Democratic presidents in both halves of the postwar era' (Bartels 2016: 61). One may agree or disagree with that statement depending on one's attitude towards conventional levels of significance. Regardless, it is obvious from the results that the distributional pattern of that growth has shifted since 1981 towards a markedly less distributive one – in line with H2.1 and 2.2. Before 1981, families at the 20th percentile experienced about 2.5 percentage points higher income growth under Democratic incumbents than the richest families, but that premium withered to merely half a percentage point after 1981. Even though Bartels warns us against making 'too much of these differences between periods, given the limitations of the data on which they are based' he proceeds to predict the level of inequality[41] that would pertain under continuous Democratic control of the White House using these regression estimates (Bartels 2016: 61; 71). This projected level of inequality is barely

[41] Again, measured as the 80/20 income ratio.

above that which existed in the 1960s and 70s, leading Bartels to conclude that 'the marked escalation of inequality over the course of the postwar era would simply not have occurred under a steady diet of Democratic presidents and policies' (*Ibid*.: 74).

Surprisingly, Bartels provides no basis for why 1981 should be the relevant cut-off when investigating a shift in presidents' ability to affect income growth rates – except that it is an even split of the dataset. But there is, as argued above, a sound theoretical basis for expecting that the mid 1970s is a more relevant turning point. To test whether H.2 in general, and specifically H2.1 and H2.2/3 stands up to Bartels' control variables, I have created a new regression analysis with the hypothesized 1974 split instead. My results, reported in Table 6, provide strong support for the argument that partisan differences in distribution of income have all but disappeared in the last three decades – and thus presents a serious obstacle to Bartels' interpretation of the data.

Whereas the democratic income premium is statistically significant for each income level except the wealthiest families before 1974, none of the premiums reaches conventional levels of significance after that period (and it is families at the 80th percentile who come closest, with a *z*-statistic of 1.38). Furthermore, the premiums have a strong equalizing effect on the income distribution before 1974, as they range from almost 4 percent annually at the 20th percentile to merely 1.5 percent at the 80th percentile. Since 1974, the premiums have been much more evenly distributed, although the data still suggests that families at the 20th percentile have fared better under some Democratic incumbents. In other words, empirical support for the partisan hypothesis is very strong – but only in the pre-capital mobility U.S. economy.

To be sure, Bartels is right that one should be careful about making sweeping generalizations based on a few observations. However, he is guilty of doing just that when he averages the substantial and equalizing income growth under Truman, Kennedy, and Johnson over the Carter administration (in the second analysis), and not least over all the Democratic presidencies since WWII (in the first analysis). In fact, the contrast could not be sharper between Carter and his predecessors – under whom annual income growth rates averaged -0.85, -0.45, -0.21, 0.41, and 0.45 for families at the 20th, 40th, 60th, 80th and 95th percentile respectively. Incidentally, Carter's experience in the White House is also an excellent argument in favor of using a dichotomous operationalization of capital mobility instead of one based on flows or

other measures. In the mid 1970s, when Carter ran for election, the value of 'cross-border transactions of bonds and equities in the United States' only made up about 4 percent of GDP. In the early 1990s, these transactions equaled a whopping 150 percent of GDP (Oatley 1999:1005-6). Using the level of net portfolio flows to operationalize capital mobility would therefore imply that mobility was somehow 37 times *more* present in the early 1990s than in 1975. Yet the economic reality that faced Carter in the late 1970s is instructive of how constrained he really was under a floating exchange rate and no capital controls: Initially, Carter attempted to run a unilateral expansion (after failing to enlist West-Germany and Japan) of the U.S. economy as per the Democratic playbook. The expansion was accompanied by a large external deficit and inflationary pressures at home, which in turn led to foreigners losing confidence in the dollar's value and 'enormous flight from the dollar in the increasingly powerful global financial markets' (Helleiner 1994: 131-2). Carter briefly considered re-instating capital controls, but opted for an

> … anti-inflation program that included cutbacks in government spending and an increase in the interest rate. When these measures did not satisfy the financial markets or foreign governments, he became persuaded of the need for more decisive austerity measures to restore confidence in the dollar. In August 1979, to signal his determination, Carter appointed Paul Volcker—a renowned "hard money" man […] to head the Federal Reserve Board. Vice-President Mondale said that this appointment was made "to reassure the financial markets, to buy back legitimacy and to reassure our major trading partners and our partners in the international financial institutions". (Helleiner 1994: 133).

This account is hardly commensurate with Hibbs' or Bartels' description of the typical Democratic policy mix, nor does it square well with Bartels' description of Carter's policies as 'surprisingly consistent with traditional Democratic tendencies and priorities' (2016: 51). It is, however, completely in line with the hypothesized response of mobile capital to domestic expansion laid out in my theoretical discussion above.

Furthermore, while one should of course be careful not to make too much out of differences between short time periods, the analysis reported in Table 6 is not only better at explaining the variance in income growth rates compared to Bartels' 1981

split, it also surpasses the analysis reported in Table 2 for each income level. Nor is it the lack of observations since 1974 (40 years, 17 under Democrats) that prevents Democratic incumbents from having a significant egalitarian impact on wages, it is simply the consequence of a shift in distributional patterns of income growth after that period.

Table 6:
Statistical Analysis of Income Growth Before and After ICM, 1949-2014

Annual real pre-tax income growth (percent) for families at various points in the income distribution. Parameter estimates from seemingly unrelated regression (standard error in parenthesis, * = p<0.05 ** = p<0.01). Partisan control is lagged by one year.

	20th percentile	*40th percentile*	*60th percentile*	*80th percentile*	*95th percentile*
Democratic president (1948-74)	3.87** (1.09)	2.63** (.76)	2.35** (.73)	1.50* (.71)	.33 (.89)
Democratic president (1975-14)	.92 (.90)	.57 (.64)	.60 (.61)	.83 (.60)	.68 (.75)
Oil prices (lagged)	-.0264 (.0162)	-.0310** (.0114)	-.0346** (.0109)	-.0277* (.0107)	-.0332* (.0134)
Labor force (% change)	4.97** (1.29)	4.56** (.92)	3.10** (.88)	2.66** (.86)	3.04** (1.06)
Lagged dependent	-.250** (.081)	-.296** (.069)	-.319** (.072)	-.365** (.085)	-.081 (.115)
Lagged 95th growth	.420** (.147)	.289** (.069)	.242* (.102)	.241* (.105)	–
Linear trend	-5.65 (7.20)	-10.07* (5.12)	-6.09 (4.86)	-2.79 (4.78)	.04 (5.99)
Quadratic trend	8.47 (6.20)	10.88* (4.42)	5.92 (4.19)	-3.10 (4.12)	1.21 (5.16)
Post-1974	-2.02 (1.57)	-1.45 (1.11)	-1.19 (1.06)	-1.66 (1.05)	-1.93 (1.30)
Intercept	.75 (1.48)	2.70 (1.05)	2.73 (1.00)	2.68 (.98)	2.44 (1.23)
S.E.	2.67	1.89	1.80	1.78	2.22
Adjusted R^2	.44	.55	.48	.37	.27
N	66	66	66	66	66

Source: U.S. Census Bureau.

Testing the Capital Mobility Hypothesis – Is Growth Still Good?

The regression analyses reported above makes a strong case for H2, and especially the inability of Democrats to target income growth to their traditional core constituency after capital mobility (H2.1). However, this begs the next questions: Whether macroeconomic performance under Democrats has shifted since 1974 – and whether the finding that strong GDP growth is good for all, but even better for the least well-off, holds across time. According to H2, and H2.2/3 in particular, we should expect that the Democrats' ability to affect growth would have similarly declined. As an empirical test of this shift, I have calculated the effects of Democratic incumbents on macroeconomic outcomes before and after international capital mobility. The statistical results reported in Table 7 are all in the hypothesized direction. Whereas Democratic incumbents had a strong positive impact on real GDP growth rates before 1974, there is little evidence that they have had any impact on growth rates since then. Similarly, although the reported parameter is just shy of conventional levels of significance, my results strongly suggest that Democrats' influence on unemployment levels have been correspondingly reduced (from a *z*-statistic of -1.9 before 1974 to -0.7 since then).[42] On the contrary, whereas there was no correlation to speak of between higher inflation and Democratic incumbents before 1974, there is a tendency towards higher inflation under recent Democratic incumbents (although the *z*-statistic for this parameter estimate is only 1.4), which is in line with capital outflows as a response to Democratic policies (H2.2).

[42] This analysis is putting a lot of strain on relatively few observations. Removing the post-1974 dummy in the analysis of unemployment levels (which in any case is a far cry from significance in the unemployment model) pushes the *z*-statistic for Democratic incumbents before 1974 to -2.01, making the Democratic impact significant at the 0.05 level. Similarly, splitting the dataset into two periods (instead of using dummies to control for time) also yields a strong negative effect of Democratic incumbents on unemployment levels before ICM (with a *z*-statistic of -3.52). The *z*-statistic for Democratic incumbents on unemployment levels falls to -2.12 after ICM with a split dataset, i.e. in the hypothesized direction.

Table 7:
Statistical Analysis of Partisan Macroeconomic Performance Pre and Post ICM 1949-2014

Growth in real GDP per capita (percentage change in annual average), the level of unemployment (as a percentage of the labor force) and inflation per year (percentage change in annual average). Parameter estimates from seemingly unrelated regression (standard error in parenthesis, * = p<0.05 ** = p<0.01). Partisan control is lagged by one year.

	Real per capita GDP growth (%)	*Unemployment (%)*	*Inflation (%)*
Democratic president (1948-74)	2.80** (.79)	-.65 (.34)	-.46 (.70)
Democratic president (1975-14)	.39 (.64)	-.21 (.28)	.81 (.58)
Lagged dependent	-.317** (.074)	.843** (.047)	.541** (.117)
Oil prices (lagged)	-.0315** (.0112)	.0114** (.0050)	.0098 (.0127)
Labor force (percentage change)	3.12** (.97)	-1.39** (.41)	1.52 (.85)
Post-1974	-.05 (1.09)	.38 (.50)	-1.21 (1.06)
Linear trend	2.28 (5.05)	1.34 (2.24)	10.87* (4.71)
Quadratic trend	-1.06 (4.36)	-2.54 (1.94)	-9.26* (4.32)
Intercept	1.19 (1.04)	1.07 (.52)	-.42 (.97)
S.E.	1.94	.87	1.75
Adjusted R^2	.28	.71	.57
N	66	66	66

Sources: Bureau of Labor Statistic; Bureau of Economic Analysis.

Still, one might suspect that the apparent partisan effects in the period before 1974 are themselves a coincidence due to historical factors that are not captured by the control variables. For instance, it might be the case that Democrats disproportionately held control of the White House in the immediate post-war period,

when growth might have been stronger for reasons beyond partisan control. Or Democrats could have been lucky by only occupying the White House in a few years of extraordinary growth, skewing the results. Fortunately, there is little in the data which would support such a view. First of all, the number of observations is evenly distributed between Democrats and Republicans (14 and 13 years respectively) and their reigns are intermittent. In the six years of Democratic control from 1948-1953, GDP growth averaged 3.0 percent annually, against 0.9 percent over the following 8 years of Republican control. Yet growth was even stronger during the second Democratic spell – averaging an impressive 3.7 percent annually – before falling to 1.6 percent in the subsequent 5 years of Republican rule. It is worth noting that the increase in Democratic growth rates under Johnson is in line with H2, as his are the only Democratic terms during which the U.S. employed capital controls after WWII (Helleiner 1994: 86-7). Thus it seems like Democratic administrations have in fact been able to create egalitarian income growth in the past – by way of boosting economic growth and reducing unemployment.

Finally, the question remains whether the distributional characteristics of growth have changed along with Democratic incumbents' abilities to affect it. The results reported in Table 8, while certainly pushing the limit of what can be reliably tested given the low number of observations, suggests that they have. In Table 8, I have tested for effects of international capital mobility on the interplay between macroeconomic outcomes and income growth for the five groups. Parameter estimates for some control variables are left out to make the table size manageable. Again, the results show that the mechanisms described by Bartels – that Democrats are good for income equality through their ability to produce egalitarian growth – only hold in the first three decades after WWII. Whereas GDP growth had a significant positive impact on incomes of the three lowest groups before 1974, and a stronger positive effect for each step down the income ladder, there is barely a significant positive effect of increased growth on incomes after 1974 – and that effect is no longer tilted towards the lower income groups. One interpretation of these results is that the *type* of economic growth matters for its impact on the income distribution. In other words, all growth is not equal. This is, again, in line with hypothesis H2.1 and helps explain the shifts in Figure 5 and 10 above. The rapid growth under fiscally expansive Democrats lifted all boats, and tended to lift the smallest boats faster than the rest. The sluggish growth after 1974, when Democrats

had to rely on monetary expansions, was much more evenly distributed, and had less impact on incomes overall. Similarly, the impact of increases in unemployment levels on family incomes have decreased markedly over time – perhaps due to stabilizing social transfers that were introduced in the mid 1960s.

The results are even more striking for the inflation variable. The parameter estimates suggest that families at the 20th percentiles actually *benefitted* from inflation before international capital mobility. This correlation is strongly significant, but by necessity spurious, as the direct effect of increased inflation on *real* incomes will be to reduce them. In other words, there must be something that is causing inflation which is at the same time so beneficial to the poorest families that it outweighs the direct negative effect of increased inflation on real wages at that level. Yet, whatever that cause may be, it was no longer driving inflation in the next four decades, as the negative effects of inflation on income levels were much more evenly distributed in that period. The fact that the burden of inflation was so exceptionally unequally distributed in the immediate post-war period puts into sharp relief the conflict of interests between rich and poor when it comes to trading the threat of inflation for the benefit of growth and employment. These results are, again, strongly suggestive of Hibbs' model of partisan politics before capital mobility, and completely in line with the international capital mobility hypothesis since then – specifically H2.1.

If we accept that increased government activism in the economy will eventually lead to inflationary pressures (Samuelson 1977), this might also explain the weak parameter estimates of Democratic incumbency in the pre-1974 period. Whereas Democrats might not have been able to produce strong income growth every single year they were in office (a hypothesis supported by the descriptive "honeymoon"-results above), it is reasonable to assume that bursts of growth were consistently paired with upwards inflationary pressure. Thus the inflation variable may in fact be picking up the effects of expansionist Democratic macroeconomic policy more accurately than the Democratic president variable itself. If we accept this explanation, the fact that inflation was no longer correlated with positive income growth at the bottom, and that its negative effects were more equally distributed overall, would support the hypothesis that Democrats have become more limited in their ability to promote growth – and to target it at core constituencies – due to international capital mobility's effect on their policy toolbox. In other words, Democrats have to use other policy tools to affect income growth rates, which is

probably what the Democratic incumbent variable is picking up in the post-1974 period.

Although not reported in the table, it is interesting to note that the positive effects of increases in the labor force are very strong in the pre-1974 period. In the first three decades these effects are largest at the 20th percentile (an average increase of 5.1 percent in annual average income per 1 percentage point increase in the labor force), and decreasing with each step up the income distribution (to a 1.5 percent increase for families at the 80th percentile). However, families at the 95th percentile received an income premium of 4 percent for each percentage point increase in the labor force. Since 1974 the positive effects of increases in the labor force have decreased substantially for each group in the income distribution (to 1.9, 2.7, 1.1 and 1.5 for the 20th, 40th, 60th and 80th percentiles respectively) – except for families at the 95th percentile, who still saw a 3 percent increase in their average annual income for each percentage point increase of the labor force.[43] This might be explained by the fact that adding another income earner to families at the lower percentiles boosted incomes substantially when the minimum wage was higher (in real terms), whereas the effect of an added earner has decreased over time as the real value of the minimum wage has eroded (see Bartels 2016: 198ff).

[43] The post-1974 parameter estimates are only statistically significant for families at the 40th percentile and 95th percentile, whereas the pre-1974 estimates were significant for all groups but families at the 80th percentile.

Table 8:
Statistical Analysis of Macroeconomic Outcomes, Pre and Post ICM 1949-2014

Annual real pre-tax income growth (percent) for families at various points in the income distribution. Controlling for relevant macroeconomic outcomes. Parameter estimates from two separate seemingly unrelated regressions (standard errors in parenthesis, * = p<0.05 ** = p<0.01). Partisan control lagged by one year.

	20th percentile	*40th percentile*	*60th percentile*	*80th percentile*	*95th percentile*
Democratic president (1948-74)	-.726 (.907)	-.060 (.675)	-.313 (.650)	-.671 (.815)	.360 (1.064)
Democratic president (1974-14)	1.121* (.512)	.795* (.376)	.953** (.347)	1.190** (.383)	1.150* (.547)
GDP growth (1948-74)	1.174** (.208)	.435** (.153)	.391** (.146)	.306 (.177)	-.270 (.209)
GDP growth (1974-14)	.149 (.145)	.314** (.153)	.337** (.097)	.259* (.106)	.175 (.209)
Unemployment (1948-74)	-1.160** (.448)	-1.103** (.313)	-1.141** (.298)	-.996** (.386)	-.097 (.513)
Unemployment (1974-14)	-.597* (.240)	-.564** (.171)	-.492** (.158)	-.233 (.172)	-.038 (.239)
Inflation (1948-74)	.589** (.206)	-.281 (.152)	-.479** (.144)	-.580** (.189)	-1.060** (.124)
Inflation (1974-14)	-.608** (.163)	-.487** (.120)	-.563** (.111)	-.537** (.122)	-.612** (.175)
Intercept (1948-74)	-.05 (2.92)	8.14 (2.13)	9.35 (2.08)	7.70 (2.64)	8.18 (3.39
Intercept (1974-14)	21.00 (7.87)	19.50 (5.71)	21.04 (5.29)	15.33 (5.84)	11.77 (8.32)
S.E. (1948-74)	1.50	1.13	1.06	1.39	1.84
S.E. (1974-14)	1.30	.98	.88	.97	1.39
Adjusted R^2 (1948-74)	.88	.87	.84	.68	.62
Adjusted R^2 (1975-14)	.71	.78	.79	.70	.57
N (1948-74)	26	26	26	26	26
N (1974-14)	41	41	41	41	41

Sources: Bureau of Labor Statistic; Bureau of Economic Analysis; Census Bureau.

Yet, as has been noted above, the most important driver of U.S. income inequality since the mid-1970s has been a veritable explosion of top incomes. To see whether the shifts in the distributional effects of macroeconomic outcomes reported in Table 8 holds for the very top income earners, I have run the same regressions using the annual real pre-tax income growth – including capital gains – for the 95th, 99th, 99.5th, 99.95th and 99.99th income percentiles. The results are reported in Table 9. Although most of the results do not reach conventional levels of significance, there are a few suggestive findings worth noting.[44] The Democratic incumbent variable is insignificant in both time periods and for all income levels. Yet there are suggestions of a shift from a net negative to a net positive effect, in line with the results in Table 8. When it comes to GDP growth, there is a significant and positive correlation for all income levels in both periods. Yet at the very top of the income distribution, the relationship between GDP growth and income growth has strengthened substantially, in the case of the top 0.01 percent of income earners, from around 2.7 percent for each percentage point increase in the GDP growth rate before 1974 to a 5.4 percent increase since then. These results are thus in line with the findings in Table 8, and my hypothesis H2.1.

The results also highlight the differences in class interests when it comes to macroeconomic outcomes, along the lines of the partisan hypothesis. Whereas the impact of higher unemployment was uniformly negative for real income growth in Table 8 (although not always significant), the data suggests an inverted relationship at the very top of the income distribution – the relationship between unemployment and income growth is in fact positive at the 99.99th percentile, but this is only significant at the 0.10-level. If we continue to interpret the inflation variable as picking up on expansionary policies, these results are also in line with my hypotheses: The parameter estimates for the inflation variable are uniformly negative before 1974 – although this relationship is not statistically significant at every income level, and is sharply declining towards the top of the income distribution. However, after 1974, there are signs of a decrease in the negative relationship between inflation and real

[44] This is probably due to the fact that income growth rates at the top income levels fluctuate a lot more than income at lower levels. Furthermore, due to the inclusion of several non-significant control variables, the regression model for the 99.99th percentile in the pre 1974 period is not significant (out of 9 included parameters, only the GDP parameter estimate is significant at the 0.05 level).

income growth, and the parameter estimate is in fact strongly positive (though not significant) at the 99.99th percentile.

To sum up, then, the Democratic growth premium is *completely* explained by macroeconomic factors before international capital mobility, whereas the income premium that appears in the later period is most likely due to a shift of policy tools away from interventionist macroeconomic policy – and a concurrent shift from fiscal to monetary policy instruments. One telling sign of this shift is the simple fact that the regression model is substantially better at explaining the variance in income growth rates for the lower half of the income distribution than the wealthier groups before capital mobility. Yet after capital mobility, the model becomes better at explaining top income growth after capital mobility. In the case of the family income analysis, this appears to be mainly due to an increase in the parameter effects for Democratic incumbents. In other words, my hypotheses – namely that Democratic presidents are less efficient at creating income *equality* under capital mobility – and that this is most likely due to a diminished macroeconomic policy space – is held up by the data.

Table 9:
Statistical Analysis of Top Income Growth, Before and After ICM 1949-2014

Annual real pre-tax income growth (percent) for individual tax filings at various points of the income distribution. Estimates from some control variables omitted. Parameter estimates from two separate seemingly unrelated regressions (standard errors in parenthesis, * = p<0.05 ** = p<0.01). Partisan control lagged by one year.

	95th percentile	*99th percentile*	*99.5th percentile*	*99.9th percentile*	*99.99th percentile*
Democratic president (1948-74)	-1.858 (1.134)	-.454 (1.867)	-1.653 (2.106)	-1.961 (3.538)	-1.846 (6.102)
Democratic president (1974-14)	1.283 (.767)	1.223 (1.544)	2.071 (2.436)	2.848 (3.715)	4.691 (6.001)
GDP growth (1948-74)	.893** (.251)	1.425** (.416)	2.057** (.468)	2.013* (.798)	2.659* (1.349)
GDP growth (1974-14)	.801** (.225)	1.847** (.455)	2.090** (.718)	3.669** (1.095)	5.364** (1.767)
Unemployment (1948-74)	-1.474* (.574)	1.506 (.943)	1.121 (1.063)	2.242 (1.786)	2.891 (3.080)
Unemployment (1974-14)	-.243 (.331)	.562 (.663)	1.049 (1.045)	1.049 (1.045)	4.231 (2.538)
Inflation (1948-74)	-.607* (.268)	-.775 (.467)	-.656 (.512)	-.938 (.877)	.064 (1.448)
Inflation (1974-14)	-.437 (.259)	-.071 (.525)	-.139 (.827)	-.139 (.827)	2.917 (2.037)
Intercept (1948-74)	8.06 (3.72)	-6.68 (6.11)	-8.89 (6.83)	-12.27 (11.54)	-31.55 (19.73)
Intercept (1974-14)	6.99 (12.11)	-27.15 (24.51)	-39.89 (38.71)	-94.21 (58.90)	-193.85 (95.05)
S.E. (1948-74)	2.00	3.33	3.74	6.30	10.81
S.E. (1974-14)	2.05	4.23	6.57	10.28	16.12
Adjusted R^2 (1948-74)	.73	.62	.67	.45	.29
Adjusted R^2 (1975-14)	.52	.46	.36	.35	.38
N (1948-74)	26	26	26	26	26
N (1974-14)	41	41	41	41	41

Sources: Bureau of Labor Statistic; Bureau of Economic Analysis; Census Bureau, and Piketty & Saez (2007).

Does Politics Still Matter?

The above results can – and perhaps to some extent should – be disheartening to students of political science. At first glance, they seem to imply that politics is no longer particularly influential when it comes to influencing important macroeconomic outcomes such as growth, unemployment, and inequality. Furthermore, it probably does not engender enthusiasm at the prospects of political participation for the average voter. Yet there is a dangerous fallacy at work in such a response, namely that of defining anything beyond presidential elections as somehow *a*political. But just as much as the Bretton Woods system of limited international capital mobility was a political construct, so is the current system of highly mobile capital (Helleiner 1994). Attempts to qualify the partisan hypothesis, as my thesis certainly does, should not be read as arguments that politics no longer matter. To the contrary, they should be seen as arguments for the importance of politics. Developments that strengthen and empower certain social classes are sure to have important consequences of a political nature. Recognizing these changes is an absolute must if one is truly impassioned about preserving the importance of politics in a world of international capital mobility – ignoring them, or worse, claiming that they are beyond the scope of politics, is a sure-fire way of achieving the opposite.

On a higher level of abstraction, the problem can be restated as one of levels of governance: The internationalization of trade and finance has created a global marketplace where one factor of production is extremely mobile. However, economic policies are still devised and implemented at the nation state and lower levels (the EU's recent foray into controlling member states' tax policies can be construed as a first attempt at remedying this situation). As one observer puts it, this process of globalization comes close to completing Polanyi's *Great Transformation* ([1944] 2001) – creating a market for capital which is beyond the control of individual national governments (Cerny 1994: 319). Labor, especially low-skilled labor with little international mobility, is struck twice – both by less interventionist policies and less social welfare spending and by the simple fact that market distribution of profits again is tilting towards capital (Piketty, 2014). This is the political challenge facing our time – and its consequences, both realized and potential, are everywhere apparent.

Limits of my Analysis and Avenues for Future Research

I have argued throughout that my chosen methodology – namely a time series analysis of a single case, using a dichotomous operationalization of international capital mobility – is the most appropriate when attempting to answer the research question at hand. However, it has obvious deficiencies. First of all, the results are not necessarily generalizable to any other economy. I have already touched upon the labor market relations argument (to which I am partial, although not to the extent that weak labor organizations necessarily preclude the efficacy of Keynesian policies *tout court*) in the theoretical discussion, and there are certainly a myriad of others that can be held up as limitations on generalizability. However, the design is straightforward and can easily be replicated in other cases as long as one is able to identify a relevant starting point for the period(s) of capital mobility. Secondly, and for the same reason, the results are not particularly robust to dropping certain observations (an obvious example is Lyndon Johnson's two terms). This implies an increased risk of spurious correlation between partisan control and the dependent variables. Finally, due to the low number of observations there is a limit to the number of relevant control variables which can be introduced. This is of course the price to pay for relying on time series analysis from a single case for such complicated phenomena as macroeconomic outcomes.

To control for these weaknesses, I have run a few robustness checks. Most importantly, both Bartels (2016) and Keller & Kelly (2015) single out 1981 as a relevant breaking point. Running the analysis in Table 8 with a 1981-split instead, thereby excluding Ford and Carter from the period of international capital mobility, yields a barely significant Democratic impact on GDP after 1981 – but this estimate is half that of the 1949-81 period, and almost a third of the effect from 1949-74. The results are otherwise qualitatively identical to the 1974-split, with macroeconomic outcomes explaining the equalizing effects of Democratic incumbents before 1981, and an (evenly distributed) Democratic premium – independent of macroeconomic outcomes – after 1981. Crucially, the effect of inflation on the lowest income group behaves in exactly the same way with a 1981 split: A positive and strongly significant relationship before 1981, but a negative and significant relationship after 1981. As the explained variance is slightly higher in my original model, and taking into account that there is no theoretical or empirical reason to assume that Carter's macroeconomic

policy was *not* restricted by capital mobility, I conclude that 1974 stands the test as a relevant breaking point for international capital mobility for the United States. The parameter estimates from the analysis with a 1981 split is reported in Appendix B.[45]

Furthermore, I have tested the robustness of my regression analyses to the inclusion of a Vietnam war-dummy, coded 1 from 1959 to 1975.[46] The dummy rarely reaches significance, although it does have a significant effect on the parameter estimates in Table 7. Specifically, inclusion of the Vietnam dummy reduces the effect of Democratic presidents on GDP growth slightly in the pre-ICM period (from 2.9 to 2.4), yet the partisan effect is still highly significant (with a *z*-statistic of 3.69). The dummy has virtually no effect on Democrats' impact on unemployment levels.

Finally, I have repeated the regression analysis reported in Table 8, but substituted the percentage change in annual average income growth for the five groups with the percentage change in the 80/20, 95/60, 99/60, and 99.99/60 income ratios as dependent variables. The parameter estimates for this analysis is reported in Appendix C. The first two ratios are simply arrived at by dividing the family income limit of the 80th and 95th percentiles with the 20th and 60th family income limits respectively. However, as mentioned above, these ratios do not capture the full extent of American income inequality, as it is the top 1 and 0.01 percent who have seen the greatest increase in their income levels. To try and capture this increase, I have calculated the latter two ratios by dividing the income limit for the top 1 and 0.01 percent of *individual tax filings* by the income limit for the 60th percentile of family incomes. Although these numbers are not nominally comparable in themselves (as individual tax returns are not necessarily filed for an entire family), the fact that they are computed in real terms over the same period of time means that the relationship between them serve as a meaningful measurement of inequality. Crucially, both data series include income from capital gains. These ratios aptly illustrate how American income inequality has developed since the 1970s. From 1974 to 2014, the 80/20 income ratio increased by 42 % (from 3.11 to 4.43) while the 95/60 ratio increased by 29 % (from 2.17 to 2.80). These can both be construed as measures of what I refer to as "wide inequality" above, i.e. differences in income between relatively large income

[45] The analysis reported in Table 9 is similarly robust to the 1981-split.
[46] I.e. to control for the effect of war on economic expansion (GDP growth), inflation, and employment levels, on the assumption that wars are not primarily fought (or extended) by Democratic incumbents in order to benefit their core constituencies.

groups. The 99/60 ratio barely beat the 80/20 with a 43 % increase in the same period (from 3.58 to 5.12), while the 99.99/60 ratio increased by a whopping 263 % (from 33.70 to 122.28).[47] Having argued in favor of using outcomes as dependent variables, and taking into account that Bartels uses his results to *project* the 80/20 ratio (2016: 71), it seems appropriate to see whether the suggested impact of capital mobility is robust to this test.

Before reading too much into the parameter estimates, it is important to note that the model is a lot better at explaining the changes in inequality as measured by the 80/20 and 95/60 ratios (with an explained variance of .80 and .70 respectively), than the "narrow inequality" measured by the 99/60 and 99.99/60 ratios (as evidenced by an explained variance of .39 and .25 respectively) before 1974. After 1974, the model loses a lot of its explanatory power vis-à-vis the 80/20 ratio, but improves its explained variance for the 99.99/60 ratio somewhat. This general lack of explanatory power vis-à-vis "narrow" inequality is to be expected, for at least two reasons. First of all, narrow inequality started to increase half a decade later than "wide inequality", which suggests that the shift in macroeconomic policies under Democrats did not immediately affect the top income groups. Secondly, as illustrated by the percentage change in the 99.99/60 ratio reported above, the increase in "narrow" inequality has been of a different order of magnitude than the more "wide" measures of inequality. I have argued above that employing a dichotomous variable to measure capital mobility is the most theoretically valid when testing for an impact on policymaking autonomy. However, it might be the case that an operationalization which captures total flows or cross-border price sensitivity is better at capturing the *direct* effects of capital mobility on inequality – and that this direct effect is more important when explaining the increase in "narrow" inequality.[48]

[47] Author's calculations. Data from Piketty and Saez (2016), 'Table A6: Top fractiles income levels (including capital gains) in the United States adjusted for price inflation'. It is important to remember that these ratios are based on family incomes at the 60th percentile, which are higher than the more commonly used household income limit for the same percentile (Bartels 2016: 8n). In other words, this number will to some extent "underestimate" inequality.

[48] I.e., one might assume that the returns to capital relative to domestic labor will increase more rapidly as information technology advances, thus allowing owners of capital to take advantage of even smaller interest rate differentials around the world. If that is the case, a dichotomous operationalization of capital mobility would not be very apt at explaining increases in the 99.90/60 ratio, while a continous variable which captures the increase in marginal price sensitivty should be expected to have more purchase.

Before capital mobility, the parameter estimates for the 80/20 and 95/60 ratios are completely in line with the findings above: An increase in the GDP growth rate of 1 percentage points translates into a 1 percentage point decrease in the 80/20 ratio, as does a 1 percentage point increase in inflation. These results are significant at the 0.01 level. The impact of both variables is weaker for the 95/60 ratio, but remain significant at the 0.05 level. However, the strongest finding, which I interpret as support of H2.1, is the change in the GDP variable for the most narrow measure of inequality, namely the 99.99/60 ratio. Before 1974, increases in the GDP growth rate has no significant impact on the 99.99/60 ratio. In other words, rapid growth – which Democrats regularly supplied – did nothing to increase the *relative* income level of the super rich. After 1974, however, increases in GDP growth are strongly and positively correlated with increases in the 99.99/60 ratio: A 1 percent increase in GDP growth translates into a 5 percent increase of the 99.99/60 ratio. That correlation is significant at the 0.01 level.[49] Of course, there could be (and definitely are) other factors which help explain why the very wealthy are capturing a lot more of the profits after 1974, beyond the direct and indirect effects of capital mobility outlined in this thesis. Nonetheless, these results are in line with the logic that the shift in effective macroeconomic policy instruments from fiscal to monetary expansion has led to less redistributive growth after 1974.

Although these test are in no way exhaustive, they do show the impressive robustness of these findings, based as they are on relatively few observations. Still, the strength of the findings do not provide any help in the quest for generalizability beyond my single case. Thus the only way to sufficiently alleviate concerns of spurious relationships is by expanding the analysis to other cases – preferably economies with a less pronounced role in the world financial markets, and which did not fight a major war for a large part of the post-war period.

Due to the limited scope of this thesis, and the data available, I have not been able to test the full extent of the four hypotheses, nor to compare their relative strengths. Although this has not been strictly necessary to provide an overall answer to the research question, it does leave many unanswered questions for future research. For instance, the direct impact of capital mobility on income inequality has not been

[49] The same is true for the 99/60 ratio, although in this case the positive correlation between GDP growth and inequality existed prior to 1974, but the correlation is slightly stronger after capital mobility.

tested empirically beyond the fact that the increase in inequality coincides with the period of international capital mobility – as well as some suggestive results from the robustness tests. Similarly, I have not been able to separate the impact of increased mobility of domestic capital specifically from the increased reliance on international capital markets to fund budget deficits generally, as outlined by Streeck (2014). Neither has the relationship between macroeconomic policy instruments and inequality been tested directly. I suspect that a direct test of these relationships would yield interesting results, and perhaps even shed some light on why the capital/income ratio is climbing towards levels which have not been seen since previous periods of high international capital mobility (Piketty 2014: 165; Obstfeld & Taylor 2004: 15-7).

Consequently, this thesis should be seen as providing a *de minimis* test of the capital mobility hypothesis for the U.S. case. This in the sense that, if the capital mobility hypothesis holds, we should *at least* expect to find a decrease in partisan differences on macroeconomic policy outcomes after capital mobility. The larger – and no doubt more daunting task – of separating the effects of capital mobility on this shift in partisan politics from other changes to the U.S. polity since 1974 still remains. As pointed out by Pollin (2010), and as implicitly recognized by Bartels (2016: 57-8), one alternative explanation of my findings is a purported shift in the parties' ideologies since the late 1970s. Although I am partial to arguments which lend importance to the ideas political actors hold, I am not convinced by a *purely* ideological argument in this specific case (see Jacobs & Myers (2014) and Jacobs & Dirlam (2016)). It strains credibility to assume that Democrats, after decades of producing strong economic growth and redistribution of wealth, suddenly decided to start doing the opposite. In other words, why change a winning team into a losing team – and stick with it? Even though I do not doubt Bill Clinton's sincerity when he described himself as an 'Eisenhower Republican' intent on pursuing lower deficits, free trade, and a strong bond market (Bartels 2016: 55), I doubt any explanation of this shift in self-perception which does not provide any causal importance to the changing international order under which current Democrats have to develop and implement their policies.

Beyond this particular thesis, political scientists usually face a problem when it comes to establishing causal relationships. This is in large part due to the fact that we rarely get to design experiments in order to test our hypotheses. Yet in some rare cases, nature (i.e. chance) does the job for us. In my case however, I may have to

thank the American voters (or more accurately, the American electors). In the last campaign video released by Donald Trump on the eve of the 2016 election, the narrator – Trump himself – lambasted big money on Wall Street for moving jobs abroad and destroying factories, factories which until recently employed hard working Americans. The profiteers from such un-American economic activity was a set of 'global special interests' (read over pictures of investor George Soros and Federal Reserve Chair Janet Yellen). Trump identified 'a global power structure' as the main orchestrators behind this assault on the middle and working classes, which over the years had 'robbed the working class', 'stripped our country of its wealth' and 'put that money into the pockets of a handful of large corporations' (Team Trump, 2016).

'Moving jobs abroad' is just another way of saying that owners of capital have exercised international mobility to reap higher returns on their investments – it is in fact capital which moves, not jobs. In other words, at least Trump seems to buy into the international capital mobility hypothesis. Donald Trump's planned policy response, beyond talks of punishing corporations for investing abroad (in effect imposing a limitation on capital mobility), is to increase domestic spending on infrastructure: 'to fix our inner cities and rebuild our highways, bridges, tunnels, airports, schools, hospitals' (Trump, cited in *The New York Times* 2016).[50] According to the findings above, this promise of expansionary spending, coupled with tax cuts, should lead to an unwelcome response from the owners of capital. When the bond market closed on election day, the yield on a 10 year U.S. government bond stood below 1.9 percent annually. At the time of writing, the yield has climbed to 2.4 percent, reaching its highest level in over a year.[51] However, whether this response is sufficient to dissuade Trump from embarking on an interventionist economic policy remains to be seen. If Trump is able to push through his agenda, without prior endogenous or exogenous shocks to the level of international capital mobility, it would serve as strong evidence that the limitations on policymaking space hypothesized in this thesis have been exaggerated.

[50] Walls were conspicuously absent from this long list of crucial infrastructure investments.
[51] Data from CNBC, 'US 10-YR', available at: http://data.cnbc.com/quotes/US10Y/tab/2.

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Appendix A

Table A:
Parameter Estimates from Bartels' Statistical Analysis of Income Growth, 1949-2014

Annual real pre-tax income growth (percent) for families at various points in the income distribution. Parameter estimates from seemingly unrelated regression (standard error in parenthesis, * = p<0.05 ** = p<0.01). Significance levels are my calculations, Bartels does not provide them in his original table. Partisan control is lagged by one year.

	20th percentile	*40th percentile*	*60th percentile*	*80th percentile*	*95th percentile*
Democratic president	2.08** (.72)	1.37** (.51)	1.29** (.48)	1.07* (.47)	.49 (.57)
Oil prices (lagged)	-.0275 (.0175)	-.0311* (.0125)	-.0353** (.0119)	-.0274* (.0115)	-.0317* (.0140)
Labor force (percentage change)	4.47** (1.29)	4.32** (.93)	2.97** (.88)	2.73** (.85)	3.24** (1.03)
Lagged dependent	-.226** (.082)	-.268** (.069)	-.304** (.071)	-.374** (.084)	-.007 (.110)
Lagged 95th growth	.455** (.146)	.305* (.108)	.263* (.102)	.298** (.105)	–
Linear trend	-15.10** (5.71)	-16.69** (4.12)	-12.15** (3.88)	-9.35* (3.73)	-6.54 (4.55)
Quadratic trend	12.68* (5.77)	13.87** (4.15)	8.79* (3.91)	6.80 (3.77)	5.42 (4.60)
Intercept	2.76 (1.22)	3.97 (.88)	3.87 (.84)	3.56 (.80)	2.92 (.97)
S.E.	2.82	1.99	1.89	1.85	2.23
Adjusted R^2	.39	.51	.44	.35	.28
N	66	66	66	66	66

Source: Bartels (2016: 44, Table 2.2).

Appendix B

Table B:
Statistical Analysis of Income Growth, Including Macroeconomic Conditions, 1949-2014

Annual real pre-tax income growth (percent) for families at various points in the income distribution. Controlling for relevant macroeconomic outcomes. Parameter estimates from two separate seemingly unrelated regressions (standard errors in parenthesis, * = p<0.05 ** = p<0.01). Partisan control lagged by one year.

	20th percentile	*40th percentile*	*60th percentile*	*80th percentile*	*95th percentile*
Democratic president (1948-81)	.351 (.771)	-.070 (.603)	-.474 (.536)	-.671 (.815)	-.310 (.936)
Democratic president (1981-14)	.869 (.526)	.885* (.403)	1.084** (.313)	1.302** (.361)	1.261* (.607)
GDP growth (1948-81)	1.124** (.145)	.646** (.114)	.598** (.099)	.306 (.177)	.110 (.165)
GDP growth (1981-14)	.219 (.153)	.311** (.119)	.280** (.092)	.222* (.105)	.139 (.178)
Unemployment (1948-81)	-.719 (.408)	-.921** (.313)	-.952** (.279)	-.996** (.386)	-.358 (.486)
Unemployment (1981-14)	-.125 (.352)	-.803** (.266)	-1.078** (.204)	-.912** (.240)	-.348 (.381)
Inflation (1948-81)	.598** (.192)	-.104 (.149)	-.320* (.131)	-.460** (.156)	-.650** (.230)
Inflation (1981-14)	-.526* (.242)	-.721** (.183)	-.959** (.142)	-.979** (.164)	-.904** (.275)
Intercept (1948-81)	-.63 (2.74)	5.34 (2.16)	9.35 (2.08)	6.36 (2.29)	3.96 (3.36)
Intercept (1981-14)	7.72 (16.42)	37.47 (12.53)	53.84 (9.66)	55.34 (11.42)	37.01 (18.62)
S.E. (1948-81)	1.65	1.29	1.16	1.40	2.02
S.E. (1981-14)	1.23	.97	.75	.86	1.45
Adjusted R^2 (1948-81)	.86	.84	.83	.68	.53
Adjusted R^2 (1981-14)	.72	.77	.83	.75	.52
N (1948-81)	33	33	33	33	33
N (1981-14)	34	34	34	34	34

Sources: Bureau of Labor Statistic; Bureau of Economic Analysis; Census Bureau.

Appendix C

Table C:
Statistical Analysis of Income Inequality, Including Macroeconomic Conditions, 1949-2014

Annual percentage change in four income ratios. Control variables omitted. Parameter estimates from two separate seemingly unrelated regressions (standard errors in parenthesis, * = p<0.05 ** = p<0.01). Partisan control lagged by one year.

	80/20 income ratio	*95/60 income ratio*	*99/60 income ratio*	*99.99/60 income ratio*
Democratic president (1948-74)	.341 (.829)	.046* (.018)	-.615 (2.163)	-1.764 (6.172)
Democratic president (1974-14)	-.347 (.546)	-.012 (.009)	.197 (1.450)	3.537 (5.845)
GDP growth (1948-74)	-1.024** (.178)	-.010* (.004)	1.551* (.533)	2.277 (1.366)
GDP growth (1974-14)	.072 (.167)	-.003 (.003)	1.495** (.429)	5.033** (1.726)
Unemployment (1948-74)	-.168 (.403)	.033** (.009)	2.974** (1.035)	3.999 (3.017)
Unemployment (1974-14)	.597* (.244)	.001 (.004)	1.048 (.613)	4.915* (2.480)
Inflation (1948-74)	-1.090** (.191)	-.009* (.004)	.216 (.544)	.575 (1.465)
Inflation (1974-14)	.125 (.183)	-.007* (.003)	.553 (.487)	3.559 (1.974)
Intercept (1948-74)	9.13 (2.59)	1.17 (.361)	-22.08 (7.56)	-40.73 (20.07)
Intercept (1974-14)	-10.90 (8.54)	1.16 (.36)	-49.99 (22.69)	-220.68 (92.02)
S.E. (1948-74)	1.41	.03	3.94	10.95
S.E. (1974-14)	1.46	.03	3.91	15.74
Adjusted R^2 (1948-74)	.80	.70	.39	.25
Adjusted R^2 (1974-14)	.23	.98	.33	.37
N (1948-74)	26	26	26	26
N (1974-14)	41	41	41	41

Sources: Bureau of Labor Statistic; Bureau of Economic Analysis; Census Bureau; and Piketty & Saez.