Wars, Redistribution and Civilian Federal Expenditures in the US over the 20th Century

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Wars, Redistribution and Civilian Federal Expenditures in the U.S. over the Twentieth Century
Summary:

The size of the U.S. government – like that of many other countries – has steadily grown over the twentieth century. This is in particular the case for the federal government, but also for the total government. While the size of the federal government was less than 3% of GDP around 1900, it has grown to a level of around 20% by the end of the twentieth century. Total government reached about 30% by that time. Especially the Great Depression and World War II seem to have produced permanent effects on the size of the government. While, quite naturally, government grows during major wars or other crises, a priori one should expect it to shrink when those emergencies are over. However, it may not shrink all the way back to its pre-crisis level. This so-called ratchet effect of crises on the size of the government has been studied in detail in Higgs’ seminal book “Crisis and Leviathan”, in which he very carefully documents the emergence of Big Government. Big Government is not only big government as measured by the resources claimed by the government for its own expenditures, but it is Big in terms of its control of the entire economy, including the operation of the private sector. Higgs carefully builds up the argument that the major crises of the twentieth century have had a lasting effect on the size of the government.

This work is more modest in its scope than Higgs’ in that we explore the U.S. government’s growth over the past century in terms of its claim on the economy’s resources, especially as a result of wars and other crises. However, we extend Higgs’ work by looking at sub-categories of government spending, by distinguishing federal and consolidated government and by linking Higgs’ thesis to the political-economy work by Meltzer and Richard (Journal of Political Economy, 1981). They argue that an increase in the franchise leads to more redistribution (more transfers), because the median voter becomes relatively poorer. Empirical tests have not been very supportive of this theory. We link Higgs’ work to the Meltzer and Richard hypothesis by showing that it may have been the interaction of a higher franchise and crisis that has caused the government to permanently increase its size. As far as we are aware, we are the first to put this hypothesis forward and to systematically test it.

For our empirical analysis we use two data sets. The first is the Census, which runs from around 1790 until now, but which is divided only into a very limited number of spending categories. The other data set we use are the National Income and Product Accounts (NIPA), which contain much more detail and seem more reliable generally. However, these series only start in 1929. Visual inspection of the Census data reveals a striking relation between federal defense spending and federal non-defense spending as shares of GDP (excluding veteran benefits and interest spending). While the two series are strongly positively correlated around World War I, they are negatively related around World War II. This finding is also supported by the outcomes of regressions of changes in non-defense spending on changes in defense spending. The Appendix provides a simple model that can rationalize the switch from positive to negative correlation in the two types of spending. The model assumes that defense and non-defense spending on goods and services are complements in social welfare. This complementarity is stronger at low levels of government than at high levels of government. The idea is that effective control of a large war machine requires a minimal level of civilian government. This is especially important in the areas of transportation and communications. The model also features marginal tax distortions that rise with the tax level. Thus at small government, the complementarity argument dominates the tax distortion argument, while the opposite is true at large government.

Next, we turn to the study of the ratchet effect of war on government spending. Here we use the NIPA data and start by looking at transfers. A first regression shows that the share of transfers falls (rises) when defense spending rises (falls). Next, we split movements in defense spending into upward and downward movements in defense spending. It turns out
that the rise in transfer spending associated with a down movement in defense spending is *significantly larger* than the fall in the transfers when defense goes up, indicating a ratchet in transfers associated with a war cycle. On the basis of a counterfactual experiment, we estimate that the movements in defense spending as a result of World War II have led to a permanent increase in transfer spending of 1.9% of GDP. The ratchet effect is robust to the exclusion of veteran transfers and the exclusion of federal transfers to state and local government. The logic behind the exclusion of these components is that they may not really be transfers that result from intensified redistribution policy. To a large extent, transfers to veterans may be the “automatic” consequence of war, while transfers to sub-national governments may not be passed further onto private agents.

We also test for ratchet effects in other components of the budget. Using the NIPA, we find ratchet effects for federal taxes and revenues, while we detect a reverse ratchet effect in civilian government purchases. In other words, this component falls more at the onset of the war than it rises by the end of the war. The question then is whether the aggregate of transfer spending and civilian government purchases exhibits a ratchet effect. We do this by means of investigating a concept that we term “adjusted revenues”. Those are revenues minus military spending minus debt repayments and minus debt interest payments. It measures the amount of resources left to finance transfers and civilian government expenditures, after debt service and defense expenditures have been taken care of. Adjusted revenues exhibit a significant ratchet effect as well, indicating that the ratchet in transfers dominates the reverse ratchet in non-defense government purchases. To see whether the findings are robust against netting out financial flows between different levels of government, we also test whether they survive when we look at the consolidated government. While the ratchet effect in transfers remains, the ratchet in the sum of transfers and civilian expenditures vanishes.

As a final step in our investigations we link the NIPA series on transfers as of 1929 with the Census series on veteran transfers for the period 1900-1928. For this earlier period, virtually all transfers were made up by veteran benefits. With this linked series we can explore the behavior of transfers more thoroughly over a longer period. In particular, we can interact changes in defense spending with the franchise. Voting eligibility increased dramatically during the first three decades of the century. Regressions in which we interact the franchise with up and down movements in defense spending reveal that the ratchet effect in transfers becomes stronger as the franchise increases. This may explain the increase in transfers associated with World War II, but the absence of such an increase around World War I. However, instead interacting defense movements with a dummy that distinguishes the periods before (plus during) and after the Great Depression reveals a similar increase in the ratchet after the Great Depression. Unfortunately, we cannot econometrically discriminate between the view that the World War II ratchet in transfers is due to higher franchise or to the experiences of the Great Depression.

We conclude this paper with a very fundamental, but still open question, namely what has caused the ratchet in transfers associated with World War II? Is the increase in transfers caused by the fact that taxes increased at the start of the war, but then for political reasons did not come down all the way back at the end of the war? In other words, redistributive pressures can only effectively materialize when taxes come down from a high level but not when they have to go up. On this view, movements in *taxes cause movements in transfers*. The other hypothesis is that *transfers cause taxes*. To understand this hypothesis, suppose that someone other than the U.S. government had paid for the U.S. participating in World War II. Would it then still be the case that transfers had gone up after the war, for example because of fundamentally changed views about social justice and distribution during the war?
Wars, Redistribution and Civilian Federal Expenditures in the U.S. over the Twentieth Century

by

Roel Beetsma*, Alex Cukierman** and Massimo Giuliodori***

October 27, 2005

ABSTRACT

We provide empirical evidence on two, major war-related, regularities of U.S. fiscal policy. First, while during and around World War I there is a positive correlation between defense spending and civil non-defense spending, this correlation becomes negative during World War II. This may be explained by a combination of complementarities between defense and civilian spending that decrease with the size of government in conjunction with marginal tax distortions that increase with government’s size.

Second, during and around World War II there are, war-related, ratchets in transfers, veteran spending, taxes and revenues in the following sense. Invariably, the share of taxes and revenues in GDP goes up, and the share of transfers goes down, when the share of defense expenditures goes up. But taxes go down less and transfers go up more per unit change in defense expenditures when those expenditures go down at the war’s conclusion than the amounts by which taxes go up and transfers go down during the buildup in defense expenditures at the beginning of the war effort. There is no

* We thank Henning Bohn for sharing his data with us, Joel Mokyr and seminar participants at the Dutch Central Bank (DNB), Tel-Aviv University, the Kiel Institute of World Economics and the University of Amsterdam for useful reactions on the project in its early stages of formation, and Alan Taylor and participants of the CEPR/CREI conference on “War and the Macroeconomy” (Barcelona, June 29/30, 2005) for helpful comments on an earlier version of the paper. This paper was completed when Beetsma was visiting the Research Department of DNB, which he thanks for its hospitality. We initially became aware of the fact that there might be a substantial ratchet between transfers and defense expenditures during WW-II following preliminary data investigations done by Oded Liviatan and Alex Cukierman in the early nineties.

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evidence of such ratchets during and around World War I. Two, not necessarily mutually
exclusive, explanations for these findings are: 1. The substantially higher franchise
during World War II interacted with the crisis induced by the war to cause a permanent
expansion of the welfare state. 2. The Great Depression permanently changed the norms
of social justice and the interaction of this change with the experience of the War led to a
more generous welfare state.

**Keywords:** World War I and II, ratchet, defense spending, civilian spending transfers,
taxes, revenues, franchise.

**JEL Codes:** E62, E65, N11, N12.
1. Introduction

Wars and the associated expansions and contractions in defense expenditures as a share of GDP led to substantial changes in the magnitude and composition of the U.S. government budget over the twentieth century. Some of those changes vanished after the wars ended, but others persisted. This paper investigates the comovements between the shares of transfer payments, non-defense expenditures, taxes, and veteran compensation of the U.S. government, on one hand, and the share of defense expenditures on the other, over this century.

A central issue addressed by the paper is the extent to which up and down movements in the share of U.S. defense spending during wars are associated with permanent changes in the relative shares of transfers and of other expenditures.¹ Wars lead to permanent changes in the shares of various expenditure and revenue items to the extent that they induce ratchets in those items. The notion that wars create an upward ratchet in the overall size of government is not new. Higgs (1987) for the U.S. and Peacock and Wiseman (1961) for the U.K., among others, argue that the share of government in the economy rises permanently as a result of war. Although the magnitude of governments recedes when wars are over, it does not go back down to its prewar level. Hercowitz and Strawczynski (2004) find an upward ratchet in total government expenditures over the business cycle in OECD countries.

Less attention has been paid to the potential permanent effects of wars on various components of the budget, including in particular transfer payments and the civilian government outlays required to produce the traditional public goods that “normal government” is expected to provide. This paper attempts to fill some of this gap. A main finding of the paper is that there are war-related ratchets in transfers and taxes in the post-1929 period but not during the first thirty years of the century.

¹ In the sequel, “share” is short hand for “share of GDP”. 
In addition to experiencing two world wars and the Great Depression over the twentieth century, the U.S. witnessed a dramatic expansion of the voting franchise, particularly during the first thirty years of the century. Modern theories of redistribution like that of Meltzer and Richard (1981) argue that the substantial increase in the share of transfers in the U.S. since 1900 is due to the expansion of the franchise to income groups with relatively lower levels of income and wealth. However, empirical tests of this theory provide mixed results (Meltzer and Richard (1983), Perotti (1996) and Gouveia and Masia (1998)). We re-examine here the direct impact of the franchise on transfers, as well as its interaction with changes in defense expenditures.

The main finding is that, although a high franchise is not necessarily associated with a high share of transfers by itself, it does have a substantial impact on this share during war times through a war-related ratchet that emerges mainly when the franchise is high. This finding supports Higgs (Op. Cit., ch. 4) claim that government grows mainly during times of crisis and is also consistent with the Meltzer and Richard (1981) theory. But it qualifies and nuances both by showing that the impact of the franchise on transfers operates mainly during crises and that a higher franchise raises the persistence of transfers by creating a war-time ratchet. Those effects are particularly in evidence during WW-II. The Great Depression is found to also be conducive to a war-time ratchet in transfers but we are unable to isolate the relative contributions of the franchise and of the Great Depression to the WW-II ratchet. There is also, in parallel, evidence that a higher franchise is conducive to war-related ratchets in taxes and government revenues.

The paper also reveals the existence of different types of comovements between the share of defense and the share of civilian (non-transfer) expenditures between WW-I and later wars. During and following WW-I the correlation between the change in the share of civilian expenditures and the change in the share of defense expenditures is positive, whereas it is negative during and in the aftermath of WW-II.

For budgetary data the paper utilizes both the National Income and Product Accounts (NIPA) and Census data sets. The first set contains more detailed information but is available only since 1929. The second set covers the entire century but contains less information. For some purposes it is useful to combine series from both data sets.
Section 2 documents the differences in the comovements between defense and civilian expenditures over different time periods and proposes a theory to explain them in the Appendix. Section 3 examines the comovements between transfers and defense expenditures in the post-1929 period and presents evidence on the war ratchet in transfers. Section 4 examines comovements between taxes and other, non-transfer, components of the Federal budget on one hand, and defense expenditures on the other. It presents evidence on war ratchets and examines the robustness of the results by also using data on consolidated (Federal, state and local) rather than only on the Federal government. Section 5 utilizes century long time series from the Census, as well as linked time series, to investigate the impacts of the franchise and of the Great Depression on war ratchets. Broader perspectives, interpretation and remaining research questions triggered by the findings are discussed in Section 6. Concluding remarks appear in Section 7.

2. Comovements between Defense and Non-defense Federal Purchases: WW-I versus Later Wars

This section examines the comovements between Federal defense and non-defense expenditures during different wars over the twentieth century. The main finding is that, while the correlation between defense spending and non-defense spending (excluding interest and transfer payments) is positive during WW-I, it is negative after 1929. This finding can be explained by relatively stronger complementarities between defense and non-defense government expenditures at low levels of government in conjunction with relatively higher tax distortions at higher levels of government.\(^2\)

2.1 Data sources, variables and methodology

We use two main data sources; the National Income and Product Accounts (NIPA) of the US and partially parallel data created by the Bureau of the Census. The NIPA data is based on a modern conception of national income accounting and contains many more series and detail than the Census data. Although more reliable, the NIPA data is available

\(^2\) The Appendix makes those claims more precise by presenting a simple model of a welfare maximizing government that experiences a productivity shock in the provision of national security.
only since 1929 while the Census data goes all the way back to colonial times. In addition, mainly for the pre-1929 period, we use GDP data series starting from the Census linked together from various sources by Johnston and Williamson (2004).

The comovements between Federal defense and non-defense expenditure are examined by regressing the change in non-defense expenditures net of interest payments and of veteran benefits as a fraction of GDP on the change in defense expenditures as a fraction of GDP controlling for cyclical variations in the rate of growth of the economy and for serial correlation in the regression residuals.

The netting out of interest payments and of veteran expenditures from non-defense spending is motivated by the question we seek to answer, which concerns the effect of changes in defense spending on the net relative size of civilian government. Since both veteran benefits and interest on the public debt are, for the most part, lagged consequences of wars we subtract them from non-defense expenditures in order to isolate the effect of changes in defense spending on the net size of the non-defense (non-transfer) governmental sector.

The share of defense expenditures in GDP is taken to be largely exogenous (or at least predetermined) with respect to other components of government spending. This point of view is based on the presumption that, once a national emergency arises, the funds needed to contain its adverse effects on the nation’s security largely drive all other items of the budget like the required sources of funds (taxes and debt) and remaining expenditure items like non-defense spending and transfer payments, but are not driven by them. A dramatic illustration is the buildup in defense expenditures just prior to and following the bombing of Pearl Harbour in 1942 in which defense spending largely drove the relative sizes of other expenditure and financing items of the federal budget.

### 2.2 Findings

Figure 1 shows the paths of the share of defense expenditures (DEFY_C) in GDP and of non-defense expenditures excluding interest payments and veteran benefits (net civilian expenditures in the sequel) as a share of GDP (NDEFY_EXV_C) over the twentieth century.

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3 Long time series from the Census data set are available for, among others, veteran expenditures at the Federal level, Federal revenues and outlays, defense spending and interest on the public debt.
century based on data from the Bureau of the Census and on the nominal GDP data compiled by Bohn (2005).4,5

Figure 1: Complementarity/Substitutability of Defense and Civilian Spending

A quick look at the figure reveals that the shares of net non-defense spending and of defense spending in GDP are positively related during WW-I and negatively related during WW-II and the Korean War. Unfortunately, the Census data does not break up non-defense expenditures into civilian Federal expenditures and transfers to others than

4 We use two series of nominal GDP data. One is from Bohn (2005) and it refers to fiscal years. This series is used in our work with the Census data, which is also on a fiscal year basis. The other series is the one constructed by Johnston and Williamson. This series refers to calendar years and is used in conjunction with the NIPA data.

5 The relevant series available in the Census data set include Total Federal Outlays (GOVEXP_C), Federal Defense Spending (DEF_C), Net Interest Payments on the Public Debt (NINT_C) and Veteran Expenditures at the Federal level (VET_C). Nominal non-defense expenditures are obtained by substracting DEF_C, NINT_C and VET_C from GOVEXP_C.
veterans. Since these transfers as a share of GDP were relatively small until the mid-thirties, the net non-defense spending from the Census can be taken as a reasonable approximation of the relative size of the civilian Federal government till that time, but hardly thereafter.

This problem can be handled by combining the Census data with direct data on the size of the civilian Federal government from the NIPA data set for the post-1929 period. This is done in the regressions of Table 1, which also provides precise information about the statistical significance of the relations between the shares of the defense and non-defense government sectors. The first two regressions utilize Census data, the last two utilize NIPA data and the third one combines non-defense expenditures from NIPA, for the dependent variable, with defense expenditures from the Census for the main independent variable. The boom and recession variables are crude proxies for the phase of the cycle. The “expansion” (“recession”) variable is equal to the one-year lagged rate of growth of real GDP when it is above (below) average and zero otherwise.

The first two regressions (covering respectively the periods 1900-1929 and 1900-1936) include only WW-I and, respectively, WW-I together with the Great Depression. Both show that the change in the share of defense expenditures has a positive and significant impact on the share of the civilian Federal government. By contrast, when only NIPA data for the period 1931-2003 is used (regression 4) this relation turns negative and significant. But, as suggested by regression 5, the significance of this negative relation vanishes when WW-II is excluded from the sample. Those findings lead to the main conclusion of this section. Loosely stated it is that, whereas defense expenditures had a significant positive impact on civilian Federal expenditures during WW-I, they exerted a significant negative impact on them during WW-II. Here and elsewhere in the paper, when broad interpretations of the results are presented, variables mentioned without any qualifier should be understood as referring to first differences of the shares of those variables in GDP.

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6 When the NIPA and Census data are combined in regression 3, the impact remains negative but becomes insignificant. The conclusion in the text leans more heavily on regression number 4 owing to the better reliability of the NIPA data.
Table 1: Effect of Defense on Non-defense Federal Spending: WW-I versus Later Wars (changes in shares)

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Change in Net Share of Non Defense (Census)</td>
<td>Change in Net Share of Non Defense (Census)</td>
<td>Change in Share of Non Defense (NIPA)</td>
<td>Change in Share of Non Defense (NIPA)</td>
<td>Change in Share of Non Defense (NIPA)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>-0.0014 (-1.45)</td>
<td>-0.0015 (-1.35)</td>
<td>0.00079 (1.90)</td>
<td>0.00053 (1.44)</td>
<td>0.00045 (1.23)</td>
</tr>
<tr>
<td>Change in Share of Defense</td>
<td>0.68 (10.5)</td>
<td>0.65 (9.65)</td>
<td>-0.022 (-1.61)</td>
<td>-0.033 (-2.32)</td>
<td>-0.0080 (-0.16)</td>
</tr>
<tr>
<td>Expansion-λ₁</td>
<td>0.032 (0.84)</td>
<td>0.028 (0.74)</td>
<td>-0.028 (-1.52)</td>
<td>-0.021 (-1.34)</td>
<td>-0.016 (-1.59)</td>
</tr>
<tr>
<td>Recession-λ₂</td>
<td>-0.054 (-1.96)</td>
<td>-0.13 (-4.21)</td>
<td>-0.032 (-1.63)</td>
<td>-0.036 (-1.95)</td>
<td>0.0021 (0.050)</td>
</tr>
<tr>
<td>µ₁</td>
<td>-0.87 (-4.40)</td>
<td>-0.60 (-4.84)</td>
<td>-0.22 (-1.12)</td>
<td>-0.27 (-1.46)</td>
<td>0.028 (0.13)</td>
</tr>
<tr>
<td>µ₂</td>
<td>-0.48 (-2.10)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>R²</td>
<td>0.84</td>
<td>0.75</td>
<td>0.22</td>
<td>0.28</td>
<td>0.030</td>
</tr>
<tr>
<td>DW</td>
<td>2.23</td>
<td>2.23</td>
<td>1.90</td>
<td>1.87</td>
<td>1.95</td>
</tr>
<tr>
<td>H₀: λ₁=λ₂</td>
<td>2.19 p=0.15</td>
<td>12.8 p=0.00</td>
<td>0.01 p=0.91</td>
<td>0.29 p=0.58</td>
<td>0.21 p=0.64</td>
</tr>
</tbody>
</table>

Notes: All estimates are obtained by OLS with a Newey-West correction for heteroskedasticity and a correction for potential serial correlation. Here, µₖ is the AR(k) coefficient of the error term; we include always at least one AR term and, if necessary, more than one. Numbers in parenthesis under the estimated coefficients are t-test statistics. The last row provides F-test statistics and p values for a test of the null hypothesis that the effects of the rate of growth of real GDP do not differ between expansions and recessions. The “Share of Non Defense” from the NIPA is the share of government purchases of non-defense goods and services and corresponds conceptually to the “Net Share of Non-Defense” from the Census.
2.3 Discussion and interpretation

An intriguing question concerns the reason for the change in the sign of the impact of defense spending on non-defense spending between the two World Wars. Our view is that those differences in comovements between defense and non-defense spending are mainly caused by a substantial difference in the prewar size of government between WW-I and WW-II. The Census data suggests that the share of Federal outlays in GDP at the eve of WW-I was about 2 percent of GDP, while it was about 10 percent of GDP at the eve of WW-II. Those dramatic differences in the size of government affect the relation between defense and non-defense spending via two channels. First, when total government is relatively small, it is likely that there are stronger complementarities in production of security between defense and non-defense spending than when it is relatively large. Effective control of a large war machine by civilian authorities requires a certain minimal level for the size of this sector. This is likely to be particularly important in areas like transportation and communications. An example is the Merchant Fleet Corporation that was established to help in merchant shipping during WW-I (Holcombe, 1996, p. 184).

Second, the marginal tax distortions associated with an increase in taxes needed to finance rising defense outlays during wars are larger when the prewar level of government is already high than when it is relatively low. This is due to increasingly adverse incentive effects on work effort and investment at higher levels of government.

We capture these features in a simple model (see Appendix) of a welfare-maximizing government by assuming that, at low levels of government, there are sufficiently strong positive complementarities in the production of security between defense and non-defense outlays and a relatively low marginal tax distortion, whereas at higher levels of government those complementarities are weaker or non-existent and the marginal tax distortion is higher. Broad intuition suggests that, in the face of adverse shocks to national security, those two presumptions will induce a social welfare minded government to raise the share of non-defense expenditures together with the share of defense when government is initially small. Conversely, when the public sector is initially large, such a government will reduce non-defense expenditures when the share
of defense goes up. The reason is that when government is large, the increased losses associated with the higher tax distortion dominate the gains from the complementarities between non-defense spending and the additional defense spending. Using a simple model of a representative individual the analytical discussion in the Appendix confirms this intuition.

3. Ratchets and Co-movements between Transfer Payments and Defense Expenditures in the Post-1929 Period

Time series from the NIPA database show that Federal transfer payments (TRANSY_F) as a share of GDP are generally negatively related to the share of defense expenditures. They go down when the share of defense (G_DEFY_F) goes up and increase when the share of defense goes down. Figure 2 plots the shares of transfers and of defense expenditures between 1929 and 2003. The negative relation between those two shares is quite apparent from the figure during and around WW-II and the Korean War, and to a lesser extent during the Vietnam War. This is confirmed by regression results elaborated later in this section.

When a different impact of defense expenditure on transfers is allowed depending on whether defense expenditures go up or down, the ensuing regression analysis reveals the existence of a significant ratchet in the effect of the share of defense expenditures on the share of Federal transfers over the 1931-2003 period. In particular, the increase in this share per unit decrease in the share of defense expenditures following the war is significantly higher than the reduction in transfers per unit increase in defense expenditures when those expenditures go up. This finding also obtains when Federal transfers excluding veterans’ benefits and grants to state and local governments are considered. It also obtains when the years of the Great Depression are excluded from the sample, but disappears when WW-II is excluded from the sample. Details appear in Section 3.2 below.

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3.1 Data and Methodology

This section utilizes the more reliable data set from the NIPA. Relatively to the Census this data provides a more detailed breakdown of Federal expenditures into various subcomponents like transfers, (non-transfer) civilian government expenditures as well as various subcomponents of each of those items.\(^8\)

Table 2 presents three sets of two regressions each designed to examine the impact of changes in the share of defense expenditures on changes in three alternative measures of the share of transfers in GDP. The first includes all current Federal transfer payments for social benefits, veteran benefits and insurance, unemployment insurance and grants in aid to state and local governments.

\(^8\) For example, federal transfers are further broken down into veteran benefits, social benefits, unemployment insurance and other items.
The second set of regressions takes the change in this concept of transfers excluding veteran benefits as the dependent variable. This is done for the sake of robustness. In particular the reason for also examining this, net of veteran benefits, concept is that, following wars, veteran benefits are expected to naturally grow as an immediate consequence of the war due to increased numbers of eligible veterans and their families even if other transfer payments do not increase. Examination of the impact of changes in the share of defense on changes in the share of transfers net of veteran benefits makes it possible to determine whether wars induce a general tendency of increases in transfers beyond transfers that are a more direct lagged consequence of the war effort.

The third set of regressions additionally excludes from Federal transfers grants in aid to state and local governments. The netting out of such grants is motivated by the observation that at least part of those grants may be used to provide local public goods that differ conceptually from income transfers to individuals. Obviously, a first-best procedure would have been to subtract only the part of such grants that is not subsequently transferred directly by state and local governments to individuals. But data limitations preclude this. The third set of regressions partially addresses this issue by assuming that all grants in aid are used for the provision of local public goods implying that they too should be subtracted in their entirety from transfers. By contrast the second regression set assumes that those grants are used only for transfers to individuals implying that they should not be subtracted. The last two sets of regressions thus provide a broader perspective on the robustness of the results.

To summarize, in addition to the basic measure of transfers, we have two additional “net” concepts of transfers to individuals. Transfers net of veteran benefits and transfers net of veteran benefits and grants in aid. All regressions control for expansions and contractions, and for serial correlation of the residuals. Within each set two regressions are presented. The first presents the combined effect (that is without allowance for the possible existence of a ratchet) of changes in the share of defense on the change in the concept of transfer considered. The second allows the impact of defense

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9 This variable is calculated by subtracting veteran benefits and veteran life insurance from the NIPA data from TRANSY_F.
expenditures to differ depending on whether defense expenditures go up or down in order to test for the possible existence of ratchets.\footnote{We do this by defining two new variables. One is equal to the change in the share of defense expenditures when this variable is positive and zero otherwise, and the other is equal to the change in this share when it is negative and zero otherwise. We also experimented with specifications in which the regression constant was allowed to vary depending on whether the share of defense goes up or down. Since the difference between the intercepts was not significant and the effects of other variables remained virtually the same, we do not present those results.} The $F$ test statistic measuring the

| Table 2: Effects of Defense Spending on Alternative Measures of Federal Transfers |
|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|
| Dependent Variable | (1) | (2) | (3) | (4) | (5) | (6) |
| Change in transfers | Change in transfers | Change in transfers net of veteran benefits | Change in transfers net of veteran benefits | Change in transfers net of veteran benefits and grants to state/local government | Change in transfers net of veteran benefits and grants to state/local government |
| C | 0.0024 (2.30) | 0.0026 (2.62) | 0.0028 (3.03) | 0.0028 (3.25) | 0.0017 (2.59) | 0.0018 (2.68) |
| $\alpha_1$ | -0.068 (-1.97) | -0.035 (-1.54) | -0.035 (-1.54) | -0.041 (-2.22) | -0.041 (-2.22) | -0.041 (-2.22) |
| $\alpha_2$ | 0.0033 (0.15) | 0.014 (1.15) | 0.014 (1.15) | -0.001 (-0.076) | -0.001 (-0.076) | -0.001 (-0.076) |
| $\alpha_3$ | -0.110 (-16.1) | -0.063 (-12.4) | -0.063 (-12.4) | -0.0065 (-14.3) | -0.0065 (-14.3) | -0.0065 (-14.3) |
| $\lambda_1$ | -0.026 (-0.73) | -0.057 (-2.01) | -0.033 (-1.52) | -0.054 (-2.95) | -0.0083 (-0.69) | -0.026 (-2.24) |
| $\lambda_2$ | 0.0068 (0.20) | 0.017 (0.48) | 0.016 (0.70) | 0.023 (0.88) | 0.035 (1.86) | 0.041 (1.92) |
| $\mu_1$ | 0.039 (0.32) | 0.028 (0.22) | 0.065 (0.088) | 0.068 (0.77) | 0.057 (0.49) | 0.069 (0.58) |
| $R^2$ | 0.25 | 0.34 | 0.17 | 0.23 | 0.21 | 0.28 |
| DW | 1.90 | 1.89 | 1.97 | 1.96 | 1.97 | 1.96 |
| $H_0: \alpha_2=\alpha_3$ | 27.4 | 42.1 | 42.1 | 51.6 | 51.6 | 51.6 |
| p = 0.00 | p = 0.00 | p = 0.00 | p = 0.00 | p = 0.00 | p = 0.00 |

Notes: All estimates are obtained by OLS with a Newey-West correction for heteroskedasticity. Numbers in parenthesis under the estimated coefficients are $t$ statistics. In relevant cases, the last row provides $F$-test statistics and $p$ values for a test of the null hypothesis that there is no ratchet in the effect of defense expenditures on transfers. Definitions of symbols:

- $c$ – Intercept.
- $\alpha_1$ – Combined effect of change in the share of defense.
- $\alpha_2$ – Effect of the change in the share of defense when this share increases.
- $\alpha_3$ – Effect of the change in the share of defense when this share decreases.
- $\lambda_1$ – Effect of above-average real rate of growth of GDP.
- $\lambda_2$ – Effect of below-average real rate of growth of GDP.
- $\mu_1$ – First-order autocorrelation coefficient of residuals.
significance of the difference between the impacts of up and down movements in the share of defense appears in the last row of the table.

3.2 Findings and robustness

The first columns in all three sets reveal that defense expenditures exert a negative effect on transfer payments and that this effect is statistically significant in two out of the three sets. Broadly speaking, when the share of defense in GDP goes up, the share of transfers in GDP goes down, and when the share of defense in GDP goes down, the share of transfers goes up.

The second columns within each set reveal that this negative effect operates strongly when the share of defense goes down and not at all, when this share goes up. In particular, the impact of defense on transfers is negative and quite significant when the share of defense goes down but insignificant when it goes up. Those findings support the existence of a significant ratchet in the effect of defense on transfers. 11 We also reran the second regressions within each of the first two sets for two different subperiods (not shown). In one case the period of the Great Depression (1929-1936) was omitted. In another, the sample was started in 1948 so as to exclude WW-II and its immediate aftermath. Exclusion of the Great Depression does not change the finding that there is a significant ratchet. However, when WW-II is excluded from the sample, the ratchet disappears supporting the conclusion that the ratchet in transfers is strongly related to this particular war.

Our total transfer series also includes a component labelled “Other current transfers to the rest of the world (net)”. After WW-II this item increased substantially, as it included foreign aid enacted under the Marshall plan. We have redone the previous regressions while excluding this component from our transfer series, and found that the ratchet effect remains highly significant, although its magnitude becomes smaller. At the same time it is not obvious conceptually whether it is appropriate to exclude the Marshall plan expenditures from total transfers for two reasons. First, the payments under the plan started only in 1948, that is, after the bulk of the fall in defense spending related to the

11 The last row of the table shows that the differences between the impacts of “defense up” and of “defense down” are highly significant.
end of the war. Second, the money was largely spent on American products and thus effectively amounted to a redistribution towards the U.S. export sector.

3.3 A counterfactual: the path of transfers had WW-II not occurred

To get a quantitative evaluation of the impact of the WW-II ratchet on the post-war path of transfers we perform a counterfactual experiment aimed at answering roughly the following question: what would have been the evolution of the post-war share of transfers if the war had not occurred? More precisely, we use regression (6) in Table 2 to calculate the post-war evolution of transfers net of veteran benefits under the assumption that the increases and subsequent decreases in the share of defense expenditures associated with the war between 1940 and 1947 did not materialize. This procedure thus neutralizes the effect of the war-related ratchet on the post-1948 share of transfers. Figure 3 shows the actual and the counterfactual paths of the share of transfers excluding veteran benefits. Obviously since we focus on the counterfactual from 1948 and on, the two paths start to diverge only from that year and on.

The main message of the figure is that, in the absence of the ratchet triggered by the increases and subsequent decreases in defense that took place right before, during, and a couple of years following WW-II, the share of transfers net of veteran benefits and of grants to state and local governments after 1948 would have been lower, on average, by 1.9 percent of GDP. In conclusion, the war ratchet contributes significantly to the very substantial increase in the share of transfers in the post war period. But its relative importance decreases as the time since the war gets longer.

---

12 This is done by calculating the predicted values of the share of transfers under the counterfactual assumption that the changes in the share of defense expenditures during those years were all zero.
4. Ratchets and Co-movements between Non-Transfer Components of the Federal Budget and Defense Expenditures

This section examines the comovements between taxes, revenues, civilian expenditures and the sum of those expenditures and transfers on one hand, and defense expenditures on the other in the post 1929 period. All the series on the various components of the Federal budget used in this section are from the NIPA.

4.1 Is there a ratchet between Federal taxes and revenues and defense?

This subsection explores the effects of changes in the share of defense expenditures on the shares of Federal taxes and Federal revenues in GDP. This is done by regressing alternative indicators of the change in Federal receipts as a fraction of GDP on the change
in the share of defense in GDP while controlling for the phase of the cycle and for serial correlation. To examine whether the empirical evidence supports the existence of a ratchet in taxes and revenues, all regressions allow the coefficient on the change in the share of defense to differ depending on whether this share goes up or down.

Since, during wars, the national debt goes up and needs to be repaid after the war it is natural to expect that the share of taxes or revenues will not go down all the way to its prewar level. Thus, a ratchet in taxes or revenues may be caused solely by the need to amortize the debt that has been accumulated during the war. To examine whether wars induce a ratchet beyond this mechanism we also estimate regressions in which the share of taxes (or of revenues) net of debt service and of defense expenditures is the dependent variable. This adjusted share of taxes (or revenues) is defined as total Federal taxes (or revenues) net of interest on the public debt, net of debt repayment, and of defense expenditures as a share of GDP. This adjusted share of taxes or revenues measures, in each year, the amount of resources left to finance transfers and civilian government expenditures, after debt service and defense expenditures have been taken care of.

Table 3 shows the impact of defense spending on Federal taxes, Federal revenues, and on the adjusted values of those two variables. For unadjusted taxes and revenues

---

13 In addition to taxes, Federal revenues include various fees and income from some assets owned by the Federal government.
14 A formalization of this idea is Barro’s (1979) tax smoothing hypothesis. In the extreme case in which a war is a total single surprise it implies that from that point in time and on the tax rate jumps up to a higher new constant level and remains there forever.
15 In symbols, adjusted taxes are defined as \[ \frac{[TAX_F - INTEREST_F + (DEBT_F - DEBT_F(-1)) - G_DEF_F]}{Y} \], where TAX_F is taxes, INTEREST_F is interest payments on federal debt, DEBT_F is debt at the end of the period, G_DEF_F is defense government purchases, all in nominal terms at the federal level and from the NIPA, and Y is nominal GDP from Johnston and Williamson (2004) or NIPA (which are the same for the period under consideration). Similarly, adjusted revenues are defined as \[ \frac{[REV_F - INTEREST_F + (DEBT_F - DEBT_F(-1)) - G_DEF_F]}{Y} \], where REV_F is nominal federal revenues from the NIPA. Except for the debt all data used refers to calendar years, while the debt refers to fiscal years. We converted the fiscal year data on the debt into calendar year data by means of interpolation. Details appear in the next footnote.
16 The construction of this variable requires the combination of flow data from NIPA with data on the stock of debt. We use long unified debt time series from the Bureau of the Public Debt. The NIPA data refers to calendar years, while the federal debt is available for the end of each fiscal year. The fiscal year ends on June 30 during 1929-1952, on December 31 during 1953-1985 and on September 30 between 1986 and 2003. We construct end-of-calendar-year outstanding nominal debt figures for the periods 1929-1952 and 1986-2003 in two steps. First, the rate of growth of the nominal debt between the end of the fiscal year that occurs within calendar year j and the end of the fiscal year that occurs within calendar year j+1 is calculated. Second, an appropriately prorated, value of this growth rate is applied to the debt figure available at the end of the fiscal year that occurs within calendar year j to calculate the debt figure at the end of this calendar year.
Table 3: Effects of Defense Spending on Federal Taxes and on Federal Revenues (Adjusted and Unadjusted)

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>c₁</td>
<td>-0.0027 (-1.89)</td>
<td>-0.0025 (-1.75)</td>
<td>0.0058 (2.17)</td>
<td>0.0059 (2.13)</td>
<td></td>
</tr>
<tr>
<td>α₂</td>
<td>0.19 (4.58)</td>
<td>0.15 (3.55)</td>
<td>0.13 (1.21)</td>
<td>0.087 (0.77)</td>
<td></td>
</tr>
<tr>
<td>α₃</td>
<td>0.048 (5.65)</td>
<td>0.034 (3.21)</td>
<td>-0.18 (-3.52)</td>
<td>-0.21 (-3.61)</td>
<td></td>
</tr>
<tr>
<td>λ₁</td>
<td>0.062 (1.91)</td>
<td>0.10 (2.89)</td>
<td>-0.27 (-2.14)</td>
<td>-0.23 (-1.73)</td>
<td></td>
</tr>
<tr>
<td>λ₂</td>
<td>-0.065 (-2.04)</td>
<td>-0.047 (-1.15)</td>
<td>-0.13 (-2.36)</td>
<td>-0.10 (-1.36)</td>
<td></td>
</tr>
<tr>
<td>µ₁</td>
<td>-0.23 (-1.16)</td>
<td>-0.20 (-1.05)</td>
<td>-0.14 (-1.10)</td>
<td>-0.085 (-0.59)</td>
<td></td>
</tr>
<tr>
<td>R²</td>
<td>0.40</td>
<td>0.36</td>
<td>0.43</td>
<td>0.41</td>
<td></td>
</tr>
<tr>
<td>DW</td>
<td>2.03</td>
<td>2.04</td>
<td>1.99</td>
<td>1.98</td>
<td></td>
</tr>
<tr>
<td>H₀: α₂=α₃</td>
<td>11.3 (p=0.00)</td>
<td>6.69 (p=0.01)</td>
<td>5.81 (0.02)</td>
<td>4.62 (0.04)</td>
<td></td>
</tr>
</tbody>
</table>

Notes: All estimates are obtained by OLS with a Newey-West correction for heteroskedasticity. Numbers in parenthesis under the estimated coefficients are t statistics. The last row provides F-test statistics and p values for a test of the null hypothesis that there is no ratchet in the effect of defense expenditures on Federal taxes and revenues (adjusted and unadjusted). Definitions of symbols:

- \( \alpha₂ \) – Effect of the change in the share of defense when this share increases.
- \( \alpha₃ \) – Effect of the change in the share of defense when this share decreases.
- \( \lambda₁ \) – Effect of above-average real rate of growth of GDP.
- \( \lambda₂ \) – Effect of below-average real rate of growth of GDP.
- \( \mu₁ \) – First-order autocorrelation coefficient of residuals.
- \( \mu₂ \) – Second-order autocorrelation coefficient of residuals.

---

Intercept.
(regressions (1) and (2)) the impact of defense is positive and significant both in the case in which the share of defense goes up, as well as in the case in which it goes down. Strikingly, the coefficient of defense is about four times higher when the share of defense goes up than when the share of defense goes down. The last row of the table confirms that this difference is statistically significant implying that this ratchet is unlikely to be a statistical artifact.

But, as argued above, this ratchet may just reflect the debt service associated with war deficits. Regressions (3) and (4) in which the dependent variables are changes in the adjusted values of the shares of taxes and of revenues, respectively, make it possible to examine whether the ratchet survives when the needs created by debt service and defense expenditures are neutralized.

The impact of the share of defense, although still positive, is no longer significant when this share goes up. Interestingly, the impact of defense is now negative and significant when the share of defense goes down implying that the share of resources available to finance the sum of transfers and civilian Federal expenditures goes up when the share of defense goes down. The last row of the table shows that the difference between the “defense up” and the “defense down” coefficients is statistically significant implying that there is a ratchet in adjusted Federal taxes and revenues as well. The broader meaning of this finding is that a symmetric war cycle in which the share of defense first goes up and then comes back down to the prewar level is associated with an increase in the share of taxes or revenues available to finance non-defense spending and transfers.

4.2 A reverse ratchet between civilian Federal expenditures and defense?

We saw in Section 2 that, once the Federal government became sufficiently large, the impact of the share of defense on the share of civilian Federal expenditures became negative. This subsection examines whether this impact was also subject to a ratchet effect in the post-1931 period covered by the NIPA data set.

Table 4 presents three sets of two regressions each. The three sets differ in the periods covered. The first set covers the entire 1931-2003 period. The second omits the
periods of the Great Depression and the third, by starting in 1948, omits both the Great Depression and WW-II. The first regression in each set does not distinguish between the

Table 4: Effect of Defense Spending on Federal Non-Defense Purchases

<table>
<thead>
<tr>
<th>Period</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>c</td>
<td>0.00053 (1.44)</td>
<td>0.00011 (0.28)</td>
<td>0.00064 (2.03)</td>
<td>0.00047 (1.57)</td>
<td>0.00045 (1.23)</td>
<td>0.00076 (2.00)</td>
</tr>
<tr>
<td>α₁</td>
<td>-0.033 (-2.32)</td>
<td>-0.033 (-4.33)</td>
<td>-0.0080 (-0.16)</td>
<td>-0.0043 (-0.96)</td>
<td>-0.033 (-4.33)</td>
<td>-0.0080 (-0.16)</td>
</tr>
<tr>
<td>α₂</td>
<td>-0.075 (-3.46)</td>
<td>-0.052 (-4.73)</td>
<td>-0.022 (-5.71)</td>
<td>-0.043 (-0.96)</td>
<td>-0.075 (-3.46)</td>
<td>-0.052 (-4.73)</td>
</tr>
<tr>
<td>α₃</td>
<td>-0.0073 (-0.93)</td>
<td>-0.022 (-5.71)</td>
<td>0.11 (1.27)</td>
<td>-0.043 (-0.96)</td>
<td>-0.0073 (-0.93)</td>
<td>-0.022 (-5.71)</td>
</tr>
<tr>
<td>λ₁</td>
<td>-0.021 (-1.34)</td>
<td>0.0049 (0.24)</td>
<td>-0.031 (-3.96)</td>
<td>-0.019 (-1.91)</td>
<td>-0.021 (-1.34)</td>
<td>0.0049 (0.24)</td>
</tr>
<tr>
<td>λ₂</td>
<td>-0.036 (-1.95)</td>
<td>-0.052 (-2.96)</td>
<td>0.0077 (0.67)</td>
<td>-0.0036 (-0.28)</td>
<td>-0.036 (-1.95)</td>
<td>-0.052 (-2.96)</td>
</tr>
<tr>
<td>µ₁</td>
<td>-0.27 (-1.46)</td>
<td>-0.44 (-2.07)</td>
<td>-0.20 (-1.44)</td>
<td>-0.25 (-1.54)</td>
<td>-0.27 (-1.46)</td>
<td>-0.44 (-2.07)</td>
</tr>
<tr>
<td>R²</td>
<td>0.28</td>
<td>0.36</td>
<td>0.42</td>
<td>0.44</td>
<td>0.30</td>
<td>0.070</td>
</tr>
<tr>
<td>DW</td>
<td>1.87</td>
<td>1.94</td>
<td>1.73</td>
<td>1.74</td>
<td>1.95</td>
<td>2.01</td>
</tr>
<tr>
<td>H₀: α₂=α₃</td>
<td>6.25 (p=0.01)</td>
<td>5.09 (p=0.02)</td>
<td>2.51 (p=0.12)</td>
<td>2.51 (p=0.02)</td>
<td>5.09 (p=0.02)</td>
<td>2.51 (p=0.12)</td>
</tr>
</tbody>
</table>

Notes: All estimates are obtained by OLS with a Newey-West correction for heteroskedasticity. Numbers in parenthesis under the estimated coefficients are t statistics. When relevant, the last row provides F-test statistics and p values for a test of the null hypothesis that there is no ratchet in the effect of defense expenditures on civilian Federal expenditures. Definitions of symbols:

c – Intercept.
α₁ – Combined effect of change in the share of defense
α₂ – Effect of the change in the share of defense when this share increases.
α₃ – Effect of the change in the share of defense when this share decreases.
λ₁ – Effect of above-average real rate of growth of GDP.
λ₂ – Effect of below-average real rate of growth of GDP.
µ₁ – First-order autocorrelation coefficient of residuals.
impacts of up and down movements in the share of defense while the second does. All regressions control for the phase of the cycle and for serial correlation of the residuals. The table confirms that the impact of defense in the entire post-1931 period is generally negative. This result obtains also when the Great Depression is omitted. But it vanishes when WW-II too is omitted (the coefficient $\alpha_1$ in regression number (5) is insignificant).

When different coefficients are allowed depending on whether the share of defense goes up or down, the table reveals the existence of a significant reverse ratchet effect for the entire period. That is, the share of civilian Federal expenditures goes down by more when the share of defense rises (per unit change in this share) than the extent to which it goes back up, per unit, when the share of defense falls. The reverse ratchet also obtains when the Great Depression is omitted but it loses significance at the 10% level when WW-II is also omitted from the sample (regressions (4) and (6) respectively).

4.3 Is there a ratchet between total non-defense Federal outlays (civilian expenditures plus transfers) and defense?

The main message of the previous subsection is that (provided WW-II is included in the sample) non-defense Federal expenditures exhibit a significant reverse ratchet. On the other hand, the discussion in Section 2 has shown that there is a regular ratchet between transfers and defense expenditures. This raises an interesting question concerning the dominant ratchet direction, if any, when those two major Federal expenditure items are aggregated. The answer to this question is provided in Table 5, which presents regressions for total non-defense Federal outlays for the period 1931-2003 as well as for various sub-periods.

As expected in view of previous results, the combined effect of the change in the share of defense on the change in the share of total non-defense Federal outlays is negative and significant (regression number (1)). Regression (2), for the entire period, and (3), which excludes the Great Depression, imply that the ratchet effect in transfer payments dominates the reverse ratchet in civilian Federal expenditures. The corresponding test statistics in the last row of the table show that this ratchet is

---

17 That is, per unit and in terms of shares, transfers go up by more when defense goes down than the extent by which they go down when defense goes up.
### Table 5: Effect of Defense on Total Non-Defense Federal Outlays (Civilian Expenditures plus Transfers)

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>c</td>
<td>0.0027 (2.40)</td>
<td>0.0029 (2.67)</td>
<td>0.0031 (2.41)</td>
<td>0.0040 (2.89)</td>
</tr>
<tr>
<td>α₁</td>
<td>-0.10 (-4.46)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>α₂</td>
<td>-0.064 (-2.67)</td>
<td>-0.040 (-1.53)</td>
<td>-0.22 (-1.88)</td>
<td></td>
</tr>
<tr>
<td>α₃</td>
<td>-0.12 (-8.37)</td>
<td>-0.15 (-20.2)</td>
<td>0.15 (0.72)</td>
<td></td>
</tr>
<tr>
<td>λ₁</td>
<td>-0.039 (-1.39)</td>
<td>-0.059 (-2.15)</td>
<td>-0.084 (-2.01)</td>
<td>-0.072 (-1.78)</td>
</tr>
<tr>
<td>λ₂</td>
<td>-0.036 (-1.00)</td>
<td>-0.027 (-0.65)</td>
<td>0.081 (3.47)</td>
<td>0.016 (0.14)</td>
</tr>
<tr>
<td>µ₁</td>
<td>-0.22 (-1.29)</td>
<td>-0.19 (-1.12)</td>
<td>-0.14 (-0.69)</td>
<td>0.050 (0.29)</td>
</tr>
<tr>
<td>R²</td>
<td>0.35</td>
<td>0.36</td>
<td>0.47</td>
<td>0.17</td>
</tr>
<tr>
<td>DW</td>
<td>1.99</td>
<td>1.98</td>
<td>1.66</td>
<td>1.99</td>
</tr>
</tbody>
</table>

Notes: All estimates are obtained by OLS with a Newey-West correction for heteroskedasticity. Numbers in parenthesis under the estimated coefficients are t statistics. The last row provides F-test statistics and p values for a test of the null hypothesis that there is no ratchet in the effect of defense expenditures on total non-defense Federal outlays. Definitions of symbols:

- **c**: Intercept.
- **α₁**: Combined effect of change in the share of defense.
- **α₂**: Effect of the change in the share of defense when this share increases.
- **α₃**: Effect of the change in the share of defense when this share decreases.
- **λ₁**: Effect of above average real rate of growth of GDP.
- **λ₂**: Effect of below average real rate of growth of GDP.
- **µ₁**: First-order autocorrelation coefficient of residuals.

Statistically significant. However, when WW-II is excluded from the sample the ratchet vanishes.

The upshot is that, as long as WW-II is included in the sample, there is a regular ratchet in total non-defense Federal outlays.

#### 4.4 Sensitivity analysis with consolidated government data

Some of the war-related ratchets uncovered so far may be due to a reallocation of various components of expenditures and of revenues between the Federal government on one
hand and the state and local governments, on the other. In order to examine the sensitivity of our results to such a possibility this subsection replicates some of the previous regressions with consolidated government data from the NIPA.

Consolidation means two things. First, a consolidated series aggregates the Federal figures with the corresponding state and local government figures. Second, intergovernmental transfers are netted out. For example, grants in aid from the Federal government to state and local governments are netted out against receipts of such grants by the lower levels of government, so that figures on the consolidated government sector eliminate double counting.\(^\text{18}\)

Table 6 presents replications of the main regressions from this and the previous section with consolidated government data. The table points to four main results. First, the ratchet in transfers remains large and significant implying that this ratchet is robust to consolidation of the three levels of government. Second, although it does not disappear, the ratchet in taxes and revenues weakens. The ratchet in tax revenues of the consolidated government sector remains significant but significance disappears when consolidated total revenues are considered. The weaker ratchet in taxes and revenues found in the consolidated data suggests that wars are associated with a reallocation of taxes away from state and local governments to the Federal government. This confirms the more generally documented finding that wars often lead to centralization of power. Thirdly, the reverse ratchet in civilian (non transfer) expenditures survives consolidation of the government sector. Finally, there is no significant ratchet in total non-defense expenditures (civilian outlays plus transfers) at the consolidated level implying that, at this level, the regular ratchet in consolidated transfers offsets the reverse ratchet in civilian expenditures.

\(^{18}\) Note that consolidation eliminates the need, encountered in Section 3, of having to allocate Federal grants to state and local governments between individual transfers and the production of local public goods.

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>(1) Dependent Variable</th>
<th>(2) Consolidated Tax Revenues</th>
<th>(3) Consolidated Total Revenues</th>
<th>(4) Consolidated Transfers Minus Veteran Benefits</th>
<th>(5) Consolidated Civilian Outlays</th>
<th>(6) Consolidated Total Non-Defense Outlays</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>c</td>
<td>-0.0015 (-0.95)</td>
<td>-0.0011 (-0.69)</td>
<td>0.0022 (2.36)</td>
<td>0.0024 (2.75)</td>
<td>0.0026 (2.67)</td>
</tr>
<tr>
<td></td>
<td>α₂</td>
<td>0.13 (2.87)</td>
<td>0.082 (1.71)</td>
<td>-0.018 (-0.85)</td>
<td>0.0072 (-0.67)</td>
<td>-0.118 (-5.70)</td>
</tr>
<tr>
<td></td>
<td>α₃</td>
<td>0.040 (4.25)</td>
<td>0.022 (1.85)</td>
<td>-0.113 (-15.3)</td>
<td>-0.069 (-13.1)</td>
<td>-0.041 (-5.10)</td>
</tr>
<tr>
<td></td>
<td>λ₁</td>
<td>0.032 (0.92)</td>
<td>0.072 (1.93)</td>
<td>-0.039 (-1.63)</td>
<td>-0.034 (-2.34)</td>
<td>-0.060 (-2.47)</td>
</tr>
<tr>
<td></td>
<td>λ₂</td>
<td>-0.104 (-2.98)</td>
<td>-0.081 (-1.67)</td>
<td>0.016 (0.46)</td>
<td>0.024 (0.86)</td>
<td>-0.058 (-2.38)</td>
</tr>
<tr>
<td></td>
<td>μ₁</td>
<td>-0.22 (-1.07)</td>
<td>-0.14 (-0.68)</td>
<td>0.022 (0.16)</td>
<td>0.12 (1.33)</td>
<td>-0.060 (-0.27)</td>
</tr>
<tr>
<td></td>
<td>R²</td>
<td>0.26</td>
<td>0.19</td>
<td>0.35</td>
<td>0.26</td>
<td>0.45</td>
</tr>
<tr>
<td></td>
<td>DW</td>
<td>2.01</td>
<td>2.01</td>
<td>1.91</td>
<td>1.94</td>
<td>2.09</td>
</tr>
<tr>
<td></td>
<td>H₀: α₂=α₃</td>
<td>3.69 (p=0.59)</td>
<td>1.43 (p=0.24)</td>
<td>18.4 (p=0.00)</td>
<td>30.5 (p=0.00)</td>
<td>11.8 (p=0.01)</td>
</tr>
</tbody>
</table>

Notes: 1. Regression (6): Consolidated Total Non-Defense Outlays = Consolidated Civilian Outlays plus Transfers. 2. All estimates are obtained by OLS with a Newey-West correction for heteroskedasticity. Numbers in parenthesis under the estimated coefficients are t statistics. The last row provides F test statistics and p values for a test of the null hypothesis that there is no ratchet in the effect of defense expenditures on the expenditure or revenue component explained by the regression. Definitions of symbols:

- c – Intercept.
- α₂ – Effect of the change in the share of defense when this share increases.
- α₃ – Effect of the change in the share of defense when this share decreases.
- λ₁ – Effect of above-average real rate of growth of GDP.
- λ₂ – Effect of below-average real rate of growth of GDP.
- μ₁ – First-order autocorrelation coefficient of residuals.
5. Re-estimation with Linked Series and Census Data over the Entire Twentieth Century: Effects of the Franchise and of the Great Depression

Availability of the richer NIPA data only since 1929 precludes in-depth examination of the effects of some variables that strongly changed mainly during the first decades of the century. A case in point is the franchise, which increased dramatically during the first thirty years of the century.

Mainly in order to explore the impact of this variable, we link (subject to feasibility) the transfer and defense series in the NIPA with their counterparts in the longer Census data set to obtain linked time series that cover the entire twentieth century. We then use the linked series to estimate the impact of the franchise in some of the regressions from previous sections.

5.1 The franchise

Figure 4 shows the evolution of the franchise between 1900 and 2003. This measure of the franchise is based on the “voting eligible population” (VEP) as a share of the population that is at or above the minimum voting age. The big jump around 1920 is the result of the 19th Amendment to the Constitution in 1920 that extended female suffrage to the entire nation. However, already before the Amendment, female suffrage was rising as more states were granting voting rights to women in the preceding years. The precise level of the franchise is extremely difficult to measure, because only registered voters are allowed to cast their vote. Registration is done at the state level and records are held at the state level. Registration requirements differ across states, although over time they have become more uniform. In the past, a variety of measures have been used to effectively limit registration. These included restrictions on gender and race, poll taxes, literacy tests and minimum duration of residence. In fact, even if we had complete data on registered voters, it is unclear whether this would provide a conceptually superior measure of the franchise, because many citizens who could register if they had made the effort chose not to do so.
Political economy theories of the size of government like that of Meltzer and Richard (1981) predict that an increase in the franchise should be associated with an increase in taxes and transfers. Existing empirical tests of the theory provide little or no support for this theory (Gouveia and Masia, 1998, and Perotti, 1996). We include the franchise in our investigation in order to re-examine those findings with a different data set and, perhaps more importantly, to examine whether its effects on transfers differ between periods of war and of peace. One may wonder whether an increase in the franchise that results from extending suffrage to women can be used to test the abovementioned theories. After all, most of the voting-age women are married and their resources are determined by the household budget constraint. However, women tend to have lower incomes than men and those that are married are uncertain (e.g., due to possible divorce or death of their husband) about the extent to which they will benefit from their husband’s future income. Hence, most women and especially those with small incomes of their own still may have a preference for more redistribution through government.

In any case since our measure of the franchise may be on the high side, particularly around 1920, we shall check the robustness of our results by also using as an alternative measure the actual number of voters in presidential elections as a share of the population of voting age. One may even argue that this measure is more suitable to test political economy theories of redistribution, because if a larger part of the population casts its vote, this may indicate a stronger pressure for redistribution. A disadvantage is, of course, that this measure could be endogenous to the economic circumstances of the day. On the other hand, an advantage is that it takes into account the lag between legal extensions of the franchise and the active use of the right to vote. Indeed, Lott and Kenny (1999) provide strong evidence of a substantial lag in female voting. Figure 5 depicts the share of actual voters. While there is still a strong upward drift after World War I, the jump associated with the enhancement of the female franchise is more moderate.

19 An excellent description of the expansion of the franchise in the U.S. over the past two centuries appears in Keyssar (2000).
20 However, a more recent test by Husted and Kenny (1997) based on a panel of U.S. states provides evidence that an expansion of the franchise through the elimination of poll taxes and literacy tests has led to an increase in welfare spending.
5.2 Construction of century-wide transfer and defense series by linking different data sets

We link transfers and defense expenditures across the NIPA and Census data sets. Prior to 1929 the bulk of transfers was composed of veteran benefits. Probably because of that the longer and older Census data set does not contain transfers other than veteran benefits. Starting in 1929, the more detailed NIPA provides detailed information about transfers including veteran benefits, as well as for veteran benefits separately. The NIPA shows that, for 1929, veteran benefits make up a substantial part of transfers. In 1929 roughly 80% of Federal transfers, as reported by the NIPA, are taken up by veteran benefits. Based on these observations we create a century-long transfer series by assuming that prior to 1929 transfers were equal to veteran benefits from the Census and from that year and on the NIPA figure on transfers is used. Given that we utilize only changes in the share of transfers in the regressions, any remaining systematic difference between pre-1929 transfers and veteran benefits are unlikely to affect the regression results.

Similarly, both the Census and the NIPA contain data on defense spending. For overlapping years those figures too are not too far apart. Moreover, when looking at the series in differenced form (as a share of GDP) they are close. Based on this observation we construct a linked, century-long, defense outlays series by using the Census series prior to 1929 and the NIPA series thereafter. This linked defense series is used only in conjunction with the linked transfer series in regressions (1) – (3) of Table 7.

For the remaining series and regressions we use the Census data that contains century long information on total Federal outlays, total Federal revenues, defense expenditures and veteran benefits. Subtracting Census defense expenditures from total Federal outlays yields Census total non-defense Federal outlays that correspond conceptually to the sum of civilian Federal expenditures plus transfers from NIPA.

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\[21\] Defense spending for 1929 equals 0.79 billions in the Census data and 0.9 in the NIPA data. However this comparison is blurred by the fact that the Census data refers to fiscal years and the NIPA data to calendar years.
Figure 4: Evolution of the Franchise over the Twentieth Century

Notes: The franchise (FRA) is computed as the eligible number of voters (VEP) divided by the total population at or above the minimum voting age (briefly: “Population Above” or PA). The minimum voting age is 21 years for the period 1900-1970 and 18 years for 1971 and onward. In turn, the VEP is calculated by dividing the actual number of voters in presidential elections (from the Census) by the turnout rate based on eligible voters. Stanley and Niemi (2005) provide the data on the turnout rate. For the period 1900-1944 these data are, in turn, taken from Burnham (1987, Table 5.3), who provides a more detailed description, while for second sub-period they come from McDonald (undated). More details on the latter period are found there and in McDonald and Popkin (2001). In particular, Burnham obtains the eligible population, from which he calculates the turnout rate, by adjusting for race, gender and alien exclusion for the states and periods where this is relevant (as of 1920 suffrage applied to women country-wide and as of 1924 aliens were nation-wide excluded from voting), while McDonald (undated) starts from what is known as the “voting-age population”. He then adjusts this figure by excluding non-citizens and ineligible felons, while including overseas eligible voters. The VEP that we obtain in this way is only available once every four years (2004 being the final year). A simple linear interpolation provides the figures for the other years. However, to take account of the reduction in the voting age in 1971, we compute the numbers for 1969 and 1970 by linearly extrapolating on the basis of the average growth over 1964-1968 and the number for 1971 by linearly (backward) extrapolating on the basis of the average growth over 1972-1976. For the calculation of PA we use the Census, which provides the numbers of people within the various discrete age

In all other regressions in Table 7 the century-long defense series from the Census is used.
groups (0-4, 5-14, 15-24, 25-34...55-64, 65 and older). Hence, it is necessary to allocate the individuals in the 15-24 age group between those above and those below the minimum voting age. This is done by linear interpolation within this age group. In particular, the number of individuals above the minimum age before 1971 is obtained by adding the numbers in all age categories above 25 plus 0.4 multiplied by the number in age category 15-24. From 1971 and on, it is obtained by adding 0.7 multiplied by the number of individuals in age category 15-24 to the total population older than, or at, 25 years of age.

Figure 5: Actual number of voters in presidential elections as a share of total population of voting age

Note: the actual number of voters is obtained by linear interpolation of the number of voters in the presidential elections, as reported by the Census. For the computation of the total population of voting age, see the Notes to Figure 4.
5.3 Preliminary findings and methodology

We start by adding the change in franchise as an additional variable to a transfer regression of the type that appeared in Section 3, but use the linked transfer series as the dependent variable in order to estimate the regression over the entire twentieth century. We also re-estimate the impacts of changes in defense on changes in Federal taxes and revenues (adjusted for debt service and defense) and on civilian Federal expenditures using only Census data over the whole century. In all cases, the effect of changes in the franchise is insignificant (not shown).

Next, we examine whether the interaction between the scope of the franchise and changes in defense expenditures has any impact on changes in the shares of the following Federal variables: transfers, adjusted revenues, total non-defense outlays and veteran benefits. In all cases, the coefficient of the interaction is allowed to have a different impact depending on whether the share of defense goes up or down. Table 7 shows that in all cases the interaction of the franchise with changes in the share of defense is significant.

To understand the meaning of the interaction between the franchise and the up and down movements in the share of defense, note that this formulation allows the impact of defense to depend on the franchise. More precisely, let $\delta_2 = \alpha_2 + \gamma_2 FRA$ ($\delta_3 = \alpha_3 + \gamma_3 FRA$) be the overall impact of a change in the share of defense on the share of some other component of Federal expenditures or revenues when the share of defense goes up (down). By making the overall effect of changes in the share of defense a function of the franchise, this formulation makes it possible to determine whether the overall impact of defense differs depending on whether the franchise is high or low.

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23 To see the connection of those definitions to the interactions between the franchise and changes in defense expenditures, let $SD_u$ be a variable that is equal to the change in the share of defense expenditures when this share goes up and zero otherwise, and let $SD_d$ be a variable that is equal to the change in the share of defense expenditures when this share goes down and zero otherwise. Then $\delta_2 SD_u = (\alpha_2 + \gamma_2 FRA) SD_u = \alpha_2 SD_u + \gamma_2 (FRA)(SD_u)$. The term $(FRA)(SD_u)$ is the interaction between the franchise and positive changes in the share of defense. The interaction between the franchise and negative changes in defense $(SD_d)$ is defined analogously.
Further, since the formulation allows the overall coefficients to vary depending on whether the share of defense goes up or down, it makes it possible to examine how various ratchet effects depend on the franchise.

5.4 Effects of interactions between the franchise and defense on linked transfers

Regression number 1 in Table 7 shows that the coefficient of the interaction of the franchise with downward movements in the share of defense is negative and significant. Although negative too, the interaction of the franchise with upward movements in the share of defense is not statistically significant. Thus the overall impacts of changes in the share of defense are algebraically lower when the franchise is higher. In addition, since the coefficient of the interaction with downward movements in defense is larger (in absolute value) than the coefficient of the interaction with upward movements in defense, ratchets are more likely to appear at high than at low levels of the franchise.

This can be illustrated by using the coefficients of regression number 1 in Table 7 to compare the overall coefficients of upward and downward changes in the share of defense between WW-I and WW-II. Figure 4 shows that the franchise was substantially higher just before and during WW-II than just before and during WW-I. The figure shows that during WW-I the franchise was in the neighborhood of 0.6 and that it was over 0.9 during WW-II. Using those values for the franchise for each of the two World Wars implies that $\delta_2 = 0.027$, $\delta_3 = 0.40$ at the start of the first WW, and $\delta_2 = -0.011$ and $\delta_3 = -0.080$ at the start of the second WW. Hence, the relation between defense and transfers around WW-I is positive, while it is negative around WW-II. Further, the overall coefficients associated with downward movements in defense differ significantly from zero, while those associated with upward movements in defense are not significant. Finally, Figure 6a shows the difference (based on regression number 1 in Table between the overall coefficients together with a 95% confidence interval around this difference. The figure reveals a reverse ratchet around WW-I and a “regular” ratchet around WW-II. The estimates imply that at least part of this switch is due to the relatively more widespread franchise during WW-II and its aftermath.
Table 7: Impact of the Interaction between the Franchise and Changes in the Share of Defense on Transfers, Adjusted Revenues, Total Non-defense Outlays and Veteran Benefits

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>(1) Change in Linked Federal Transfers</th>
<th>(2) Change in Linked Federal Transfers</th>
<th>(3) Change in Linked Federal Transfers</th>
<th>(4) Change in Adjusted Federal Revenues</th>
<th>(5) Change in Total Non-Defense Outlays</th>
<th>(6) Change in Total Non-Defense Outlays (excl. interest &amp; veteran transfers)</th>
<th>(7) Change in Veteran Benefits</th>
</tr>
</thead>
<tbody>
<tr>
<td>Franchise Variable</td>
<td>FRA</td>
<td>Actual voters’ share</td>
<td>Actual voters’ share lagged four years</td>
<td>FRA</td>
<td>FRA</td>
<td>FRA</td>
<td>FRA</td>
</tr>
<tr>
<td>C</td>
<td>0.0018 (2.41)</td>
<td>0.0017 (2.22)</td>
<td>0.0020 (2.57)</td>
<td>0.0022 (1.48)</td>
<td>0.0026 (2.27)</td>
<td>0.0031 (2.85)</td>
<td>-0.00036 (2.04)</td>
</tr>
<tr>
<td>α_2</td>
<td>0.095 (0.76)</td>
<td>0.11 (1.10)</td>
<td>-0.011 (-0.22)</td>
<td>1.35 (2.82)</td>
<td>3.04 (8.66)</td>
<td>2.67 (6.63)</td>
<td>0.18 (2.37)</td>
</tr>
<tr>
<td>α_3</td>
<td>1.26 (7.76)</td>
<td>0.62 (7.70)</td>
<td>0.20 (9.18)</td>
<td>5.81 (7.03)</td>
<td>6.77 (21.0)</td>
<td>6.46 (18.0)</td>
<td>0.69 (11.1)</td>
</tr>
<tr>
<td>γ_2</td>
<td>-0.11 (-0.76)</td>
<td>-0.22 (-1.09)</td>
<td>0.012 (0.13)</td>
<td>-1.26 (-2.33)</td>
<td>-3.17 (-8.12)</td>
<td>-2.77 (-6.31)</td>
<td>-0.21 (-2.29)</td>
</tr>
<tr>
<td>γ_3</td>
<td>-1.41 (-8.43)</td>
<td>-1.40 (-8.74)</td>
<td>-0.55 (-13.7)</td>
<td>-5.76 (-6.36)</td>
<td>-7.02 (-21.3)</td>
<td>-6.61 (-18.1)</td>
<td>-0.78 (-11.9)</td>
</tr>
<tr>
<td>λ_4</td>
<td>-0.036 (-1.93)</td>
<td>-0.035 (-1.97)</td>
<td>-0.041 (-2.17)</td>
<td>-0.045 (-0.90)</td>
<td>-0.076 (-2.52)</td>
<td>-0.072 (-2.28)</td>
<td>-0.0053 (-1.84)</td>
</tr>
<tr>
<td>λ_2</td>
<td>0.012 (0.46)</td>
<td>0.0099 (0.38)</td>
<td>0.012 (0.45)</td>
<td>-0.15 (-2.23)</td>
<td>-0.082 (-2.73)</td>
<td>-0.056 (-2.12)</td>
<td>-0.019 (-3.59)</td>
</tr>
<tr>
<td>µ_1</td>
<td>0.014 (0.12)</td>
<td>0.0045 (0.039)</td>
<td>0.019 (0.16)</td>
<td>-0.25 (-1.24)</td>
<td>-0.21 (-1.90)</td>
<td>-0.26 (-2.33)</td>
<td>-0.34 (-2.03)</td>
</tr>
<tr>
<td>R^2</td>
<td>0.32</td>
<td>0.33</td>
<td>0.33</td>
<td>0.34</td>
<td>0.61</td>
<td>0.58</td>
<td>0.59</td>
</tr>
<tr>
<td>DW</td>
<td>1.98</td>
<td>1.98</td>
<td>2.00</td>
<td>2.11</td>
<td>2.08</td>
<td>2.08</td>
<td>2.05</td>
</tr>
<tr>
<td>H_0: α_2=α_3</td>
<td>23.2 (p=0.00)</td>
<td>11.6 (p=0.00)</td>
<td>11.1 (p=0.00)</td>
<td>23.2 (p=0.00)</td>
<td>68.9 (p=0.00)</td>
<td>61.6 (p=0.00)</td>
<td>18.9 (p=0.00)</td>
</tr>
<tr>
<td>H_0: γ_2=γ_3</td>
<td>24.4 (p=0.00)</td>
<td>15.7 (p=0.00)</td>
<td>24.0 (p=0.00)</td>
<td>29.0 (p=0.00)</td>
<td>62.0 (p=0.00)</td>
<td>55.6 (p=0.00)</td>
<td>19.6 (p=0.00)</td>
</tr>
</tbody>
</table>

Notes: 1. All estimates are obtained by OLS with a Newey-West correction for heteroskedasticity. Numbers in parenthesis under the estimated coefficients are t statistics. 2. The second row indicates which variable we use for the franchise. In columns 1 and 4 – 7 we use the franchise as depicted and computed in Figure 4. In columns 1 and 2 we use the actual number of voters as share of PA (see Figure 5). 3. The penultimate row provides F-test statistics and p values for a test of the null hypothesis that there is no ratchet in the effect of defense expenditures on the expenditure or revenue item explained by the regression, holding the franchise constant. 4. The last row provides F-test statistics and p values for a test of the null hypothesis that there is no difference in the effect of the interaction between the franchise and the change in defense expenditures depending on whether the share of defense goes up or down. 5. The dependent variable, as well as the defense series used for the independent variables, in columns 1 – 3 are obtained by appending the relevant series from the Census for 1900-1928 to the relevant series from the NIPA for 1929 and on. In the regressions reported in columns 4 – 7, the dependent variable and the defense series used for the independent variables are always and entirely based on Census data. In other words, in all instances,
both the dependent and independent variables are jointly based on the fiscal year (Census) or on the calendar year (NIPA). Definitions of symbols:

c – Intercept.
\( \alpha_2 \) – Effect of the change in the share of defense when this share increases.  
\( \alpha_3 \) – Effect of the change in the share of defense when this share decreases.  
\( \gamma_2 \) – Effect of the interaction between the franchise and the change in the share of defense when this share increases.  
\( \gamma_3 \) – Effect of the interaction between the franchise and the change in the share of defense when this share decreases.  
\( \lambda_1 \) – Effect of above-average real rate of growth of GDP.  
\( \lambda_2 \) – Effect of below-average real rate of growth of GDP.  
\( \mu_1 \) – First-order autocorrelation coefficient of residuals.

For reasons of robustness discussed earlier column 2 of Table 7 repeats the regression in column 1 by replacing FRA with the actual number of voters as a share of the total population of voting age. The significance or insignificance of the parameters is unaffected and, again, an increase in this alternative measure of the franchise pushes the overall effect of a change in the share of defense spending on transfers downward. Figure 6b depicts the difference between the overall up and down coefficients of defense (\( \delta_2 - \delta_3 \)). As before, there is a reverse ratchet during the first two decades of the sample and a regular ratchet during the remainder of the century. However, the absolute size of the former has shrunk, while the size of the regular ratchet has increased.

It cannot be excluded a priori that our alternative “franchise” measure may, to some extent, be endogenous, since turnout could be affected by recent policy steps (such as changes in transfers) taken by government. To check whether the results remain at least qualitatively unaffected, column 3 in Table 7 repeats the regression in column 2 with the actual voters share lagged four years instead of the current value of this variable. It turns out that the ratchet effect described above survives but becomes smaller, while the reverse ratchet effect at the beginning of the sample becomes insignificant. For brevity, we do not present the corresponding figure.
Fig. 6: Difference in Overall Impact up minus down Movements in Defense on Transfers

(a) Based on FRA (column 1, Table 7)

(b) Based on actual voters over PA (column 2, Table 7)
5.5 Effects of interactions between the franchise and defense on other components of Federal expenditures and revenues

5.5.1 Adjusted Federal revenues

Regression number 4 in Table 7 shows that the coefficients of the interactions of the franchise with movements in the shares of defense are negative and significant. For the range of variation of the franchise experienced over the twentieth century the overall coefficients, $\delta_2$ and $\delta_3$, are positive implying that the share of adjusted Federal revenues goes up and down in conjunction with the share of defense. This, together with the fact that the interaction coefficients are negative, implies that the franchise generally reduces the overall coefficients. Inserting the values of the franchise into the definitions of $\delta_2$ and $\delta_3$, reveals that both coefficients are always larger than zero. Both $\delta_2$ and $\delta_3$ are highly significant around World War I, but only marginally significant (at the 5% level) or insignificant around World War II. Further, $\delta_3$ is larger than $\delta_2$, during and around both wars. The difference between these two overall coefficients is significantly negative at the 5% level until 1944, after which it becomes insignificant. At the start of the century this difference is large in absolute terms, but it continually shrinks over the subsequent four decades. For example, for 1917, $\delta_2 = 0.58$ and $\delta_3 = 2.30$, while for 1942, $\delta_2 = 0.15$ and $\delta_3 = 0.34$.

In conclusion, there is a reverse ratchet in adjusted Federal revenues around WW-I, when the franchise is relatively low. By the end of WW-II, when the franchise is already high, this reverse ratchet is non existent. More generally, those findings imply that while the share of resources available for transfers and civilian purchases shrinks after the war when the franchise is low, it is not affected by the war when the franchise is sufficiently high.

5.5.2 Total non-defense outlays

We look at two regressions for total non-defense outlays. The first one includes veteran benefits and interest payments on the public debt (column 5 of Table 7). The other
excludes these items, recognizing that they may be largely caused by war efforts (column 6 of Table 7). For the first measure, the full coefficient associated with upward movements in defense spending, $\delta_2$, is positive and significant for almost all the first four decades of the twentieth century, while the one associated with down movements in defense spending remains significantly positive until almost the end of World War II. After this period, $\delta_2$ remains insignificantly different from zero until the end of the eighties, while $\delta_1$ is significantly negative until the end of the eighties (except briefly during the end of the sixties). The difference between the two coefficients, $\delta_2 - \delta_1$, is significantly negative until the start of World War II supporting the existence of a reverse ratchet in total non-defense outlays. During this period, the difference continually shrinks, while after this period it is insignificant until the mid-eighties. This general pattern indicates that an increase in the franchise is associated with elimination of the reverse ratchet in total non-defense outlays.

For the second measure – the one that excludes interest payments and veteran benefits – the pattern is similar. The coefficient associated with upward movements in defense spending, $\delta_2$, is significantly positive during roughly the first four decades of the twentieth century, after which it becomes insignificant for the remainder of the period until the mid-nineties. The other coefficient, $\delta_1$, is significantly positive until the end of World War II. After that, it is sometimes significant and sometimes insignificant, but it remains much smaller in absolute magnitude. The difference between the two coefficients, $\delta_2 - \delta_1$, is significantly negative until the end of the forties. During this period, it continually shrinks and after this period, it remains close to zero and insignificant until the mid-eighties (except briefly at the end of the sixties). The broad conclusion from column 6 is similar to that from column 5: an increase in the franchise reduces $\delta_2 - \delta_1$, thereby reducing or eliminating the, war-related, reverse ratchet in total non-defense expenditures net of interest payments and of veteran benefits.

5.5.3 Veteran benefits
Veteran benefits are, to some extent, qualitatively different from other types of transfer payments since they can be viewed as a delayed payment for military services performed,
and/or insurance payments to families of war casualties and to those that were injured or incapacitated while on active duty. However, setting the precise magnitude of those payments inevitably involves an element of redistributive politics. Holcombe (1999) notes that, during the fifty years following the Civil War, veterans formed a well-organized interest group with a strong political lobby. It is therefore conceivable that the same political forces underlying some of the ratchet in other transfers also operate on veteran benefits.

The last regression in Table 7 provides some evidence on this issue. We find that the overall coefficient associated with upward movements in defense spending, $\delta_2$, is significantly positive for the first two decades of the twentieth century, after which it becomes (and remains) negative and insignificant. Similarly, $\delta_3$ is significantly positive for the first two decades of the twentieth century, after which it becomes significantly negative for the remainder of the century. The difference $\delta_2 - \delta_3$ is significantly negative during the first two decades. It then turns positive, but remains insignificant for most of the twenties. After that, it becomes significantly positive and remains so until the end of the sample period. The upshot is that there is clear support of a ratchet effect in veteran benefits in the latter period, when the franchise is high, and no support for it in the early period, when the franchise is low.

5.6 Effects of the Great Depression on the share of transfers
This subsection examines the effects of the collective trauma experienced during the Great Depression on the subsequent evolution of the share of transfers. For this purpose we define a Great Depression dummy (DGD) that assumes a value of one after the Great Depression (from 1937 onwards) and zero otherwise. This dummy is used to expand the linked transfers regression in two ways. First, we use it to examine the possibility that the intercept changes after the Great Depression. Second, we explore the possibility that the impacts of upward and of downward changes in the shares of defense on the share of transfers change after the Great Depression. We do this by adding two interaction terms to the basic linked transfers regression. The first interacts upward movements in the share

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24 Since the difference between the intercepts in the post Great-Depression period and in the previous period is insignificant, Table 8 only presents results with a uniform constant.
of defense with DGD and the second interacts downward movements in the share of defense with DGD.

Table 8 presents two linked transfers regressions both of which include the above Great Depression variables. The difference between the two regressions is that the first one does not include interactions between the franchise and up and down movements in defense in the group of control variables, while the other does. The main new result is that (in the absence of interactions of the franchise with “defense up” and “defense down”) the coefficient of the interaction between DGD and the change in the share of defense when this share goes down is negative and significant.\(^{25}\)

This formulation allows the impact of defense to depend on the period under consideration (before and including the Great Depression versus after the Great Depression). Let \( \theta_2 \equiv \alpha_2 + \varepsilon_2 DGD \) (\( \theta_3 \equiv \alpha_3 + \varepsilon_3 DGD \)) be the overall impact of a change in the share of defense on the share of transfers when the share of defense goes up (down).\(^{26}\) We find that \( \theta_2 \) is positive and insignificant over the entire sample period, while \( \theta_3 \) is positive and insignificant before 1937 and negative and significant from 1937 and on. Finally, the difference between the two coefficients, \( \theta_2 - \theta_3 \), is negative and insignificant before 1937 and positive and significant for the period since 1937 and on. This supports the view that the memories and lessons of the Great Depression are at least partially responsible for a ratchet in the effect of defense on transfers.

Finally, the second regression in the table shows that, when interactions of “defense down” and “defense up” with both the franchise and the Great Depression dummy are entered as regressors, all these interactions become insignificant. The findings here and in Subsection 5.4 above imply that each interaction, taken separately, tends to produce a ratchet. The last regression in Table 8 suggests, however, that it is hard to econometrically discriminate between the view that the WW-II ratchet in transfers is due to a higher franchise or to the experiences of the Great Depression.

\(^{25}\) However this interaction is not significant in the range in which the share of defense goes up.

\(^{26}\) Essentially, this formulation allows the coefficients of up and down movements in the share of defense to differ between the period prior to (and during) the Great Depression from the post Great Depression period.
Table 8: Effects of the Great Depression on Linked Transfers

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>period</td>
<td>1902-2003</td>
<td>1902-2002</td>
</tr>
<tr>
<td>c</td>
<td>0.0018</td>
<td>0.0018</td>
</tr>
<tr>
<td></td>
<td>(2.44)</td>
<td>(2.27)</td>
</tr>
<tr>
<td>$\alpha_2$</td>
<td>0.010</td>
<td>0.29</td>
</tr>
<tr>
<td></td>
<td>(0.81)</td>
<td>(0.27)</td>
</tr>
<tr>
<td>$\alpha_3$</td>
<td>0.014</td>
<td>-1.27</td>
</tr>
<tr>
<td></td>
<td>(1.39)</td>
<td>(-0.58)</td>
</tr>
<tr>
<td>$\gamma_2$</td>
<td>-0.38</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-0.26)</td>
<td></td>
</tr>
<tr>
<td>$\gamma_3$</td>
<td>1.45</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.59)</td>
<td></td>
</tr>
<tr>
<td>$\epsilon_1$</td>
<td>-0.019</td>
<td>0.061</td>
</tr>
<tr>
<td></td>
<td>(-0.91)</td>
<td>(0.20)</td>
</tr>
<tr>
<td>$\epsilon_2$</td>
<td>-0.126</td>
<td>-0.25</td>
</tr>
<tr>
<td></td>
<td>(-12.9)</td>
<td>(-1.20)</td>
</tr>
<tr>
<td>$\lambda_1$</td>
<td>-0.037</td>
<td>-0.037</td>
</tr>
<tr>
<td></td>
<td>(-2.06)</td>
<td>(-2.03)</td>
</tr>
<tr>
<td>$\lambda_2$</td>
<td>0.011</td>
<td>0.0094</td>
</tr>
<tr>
<td></td>
<td>(0.42)</td>
<td>(0.36)</td>
</tr>
<tr>
<td>$\mu_1$</td>
<td>0.019</td>
<td>0.016</td>
</tr>
<tr>
<td></td>
<td>(0.16)</td>
<td>(0.14)</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.33</td>
<td>0.33</td>
</tr>
<tr>
<td>DW</td>
<td>1.99</td>
<td>1.99</td>
</tr>
</tbody>
</table>

All estimates are obtained by OLS with a Newey-West correction for heteroskedasticity. Numbers in parenthesis under the estimated coefficients are t statistics. Definitions of symbols:

- **c**: Uniform intercept
- **$\alpha_2$**: Effect of the change in the share of defense when this share increases
- **$\alpha_3$**: Effect of the change in the share of defense when this share decreases
- **$\gamma_2$**: Effect of the interaction between the franchise and the change in the share of defense when this share increases
- **$\gamma_3$**: Effect of the interaction between the franchise and the change in the share of defense when this share decreases
- **$\epsilon_1$**: Interaction of Great Depression dummy (DGD) with the change in the share of defense when this share increases
- **$\epsilon_2$**: Interaction of Great Depression dummy (DGD) with the change in the share of defense when this share decreases
- **$\lambda_1$**: Effect of above-average real rate of growth of GDP
- **$\lambda_2$**: Effect of below-average real rate of growth of GDP
- **$\mu_1$**: First-order autocorrelation coefficient of residuals
6. Broader Perspectives on the Findings

This section takes a broader look at the implications of various regularities uncovered in the previous sections, examines how they might fit into existing conceptual frameworks concerning the budget and discusses a couple of research questions triggered by the findings.

6.1 Relationships between war ratchets of different budgetary items

The main findings on war-related ratchets between defense and other expenditure items may be quickly summarized as follows. The share of transfers is negatively related to the share of defense, and the shares of taxes and revenues are generally positively related to the share of defense. Mainly, during and following WW-II, there are ratchets in the relation between the share of defense, on the one hand, and the shares of transfers and of taxes and revenues on the other.

More precisely, the (positive) impact of a decrease in the share of defense after the war on the share of transfers is higher than the absolute value of the (negative) impact of an increase in the share of defense on transfers during the early phases of the war. In parallel, the (positive) impact of defense on taxes and revenues when the share of defense goes up is larger, in absolute value, than the (negative) impact of a decrease in defense on taxes and revenues after the war. This implies that a “symmetric” war cycle in which the share of defense goes back, after the war, to its prewar level is associated with higher post-war shares of transfers, taxes and government revenues than prior to the war.

During and in the aftermath of WW-II there is also a reverse ratchet in the share of civilian government expenditures in the sense that they decrease more per unit of change in the share of defense when this share goes up than they increase per such unit when the share of defense goes down after the war. In summary, during and following WW-II there are “regular” ratchets in transfers and taxes and a reverse ratchet in the share of civilian (non-transfer) expenditures.

We can shed some light on the relations between those three ratchets by looking at the following intra-period budget constraint of the government.

\[
TR = - ND + [R + B - (1 + i)B_{-1} - D]
\]
Here, TR is the share of transfer payments in GDP, D is the share of defense expenditures, ND is the share of non-defense expenditures, R is the share of government revenues, B is outstanding debt as a fraction of GDP and i is the nominal interest rate. Note that the last group of terms in square brackets is equal to our earlier measure of Federal revenues adjusted for debt service and defense expenditures. Thus, the findings on ratchets in conjunction with the budget constraint above imply that, following WW-II, the ratchet in transfer payments was “financed” by a ratchet in adjusted Federal revenues and by a reverse ratchet in civilian Federal expenditures.

6.2 What is the direction of causality between revenues and transfers?

The relationships among the ratchets described above constitute a reduced-form description of relations between various components of government expenditures. They obviously do not provide an explanation for the deeper social, political and economic forces that combined to produce those regularities. Nor do they, as of themselves, provide clues about the direction of causality between the non-defense components of the budget. A more articulated conceptual framework is needed to address those issues in a systematic manner and to discriminate between alternative explanations that are consistent with the findings above. In particular, we focus here on possible causal links between the share of transfers and the share of taxes (or revenues).

There are at least two distinct hypotheses that are consistent with the existence of war-related ratchets in transfers and taxes. The first relies on the observation that, following the war, it is possible to raise the share of transfers and at the same time reduce the share of taxes by utilizing the post-war decrease in the share of defense to do a little bit of both. The opportunity to raise social benefits while reducing the tax burden is obviously a politician’s dream, particularly if the public evaluates elected officials by comparing its post-war welfare with its welfare during the war. This makes it easier to raise transfer payments immediately after the war because taxes are already high so that the higher benefits do not necessitate tax raising legislative actions. As a matter of fact

\footnote{Recall that the last column of Table 3 implies that there is a ratchet also in adjusted Federal revenues.}
the share of transfers can be raised while reducing the share of taxes by just not reducing it all the way to its pre-war level.

From the vantage point of the end of the war, politically controversial high tax rates are already a fact of life since they had been raised at the beginning of the war in the face of impending national security threats. Presumably, at that point it is easier to mobilize the political consensus required to pass high tax rates since national security is a public good that benefits (almost) everybody while a higher share of transfers benefits some and imposes costs on others. On this view, the post-war high share of taxes induces a higher share of transfer payments in the post-war period implying that causality runs from a, war-induced, high share of taxes to a higher post-war share of transfers.

The second hypothesis is that, at the end of the war transfers were expected to go up in order to compensate the less fortunate for the havoc of war, like casualties, the need to help survivors and the need to help soldiers reintegrate in the civilian labor force. It is likely that following wars stronger feelings of solidarity with those that served their nation, as well as with those at the lower end of the income distribution also played a role. Such feeling might have facilitated the expansion of redistributional schemes by creating new transfer programs or expanding existing ones. This hypothesis implies that, following the war, the share of taxes was not reduced to its prewar level in anticipation of the need to finance a higher share of transfer payments. On this view, causality runs from anticipated higher transfer payments to higher taxes and revenues rather than being due to the high tax rates inherited from the war. Obviously, these two hypotheses need not be mutually exclusive.

To summarize, under the first hypothesis, a high share of taxes inherited from the war induces a higher share of transfers. Under the second, a higher share of transfers expected to materialize in the future prevents the share of taxes from going down all the way to its prewar level. Thus, under the first hypothesis taxes drive transfers while, under the second, expected transfers drive taxes. Sharp discrimination between these hypotheses would enrich our understanding of the dynamics of budgetary decisions but we do not currently possess a clear cut discriminatory test of those hypotheses. Note that a standard Granger causality test cannot discriminate between them, since both imply that Granger causality should run from taxes to transfers. This is obvious in the first case and,
in the second case, is a consequence of at least some forecasting ability with respect to transfers on the part of legislators.

Another potential way to discriminate between the two hypotheses is to look at the budgetary importance of new programs created following wars in comparison to the budgetary importance of existing programs. It would appear, at first blush, that creation of new programs lends some support to the view that the existing high share of government revenues, by facilitating financing, led to their creation. Conversely, continuation of only existing programs, particularly with prewar benefit rates, lends support to the view that the post-war ratchet in the share of taxes is due to the expectation that the benefits mandated by those pre-existing programs will have to be financed. Thus, the GI bill program created after WW-II may perhaps lend some credence to a story in which causality runs from high taxes to high transfers, while the Social Security system created prior to the war is more in tune with causality running in the opposite direction.

However, creation of new programs can also be consistent with causality running from expected transfers to taxes. This will be the case, for example, if the new programs are created only because of increased feelings of solidarity in the aftermath of the war. The upshot is that, although suggestive, creation of new transfer programs versus continuation of old ones does not provide a clear-cut discriminatory criterion with regard to the direction of causality. Nonetheless, our gut feeling is that, during the post WW-II period, causality between taxes and transfers is likely to have been operating in both directions. In fact, President Truman’s Fair Deal program, which was presented immediately after the War, led to both higher benefits in existing programs (such as higher minimum wages) and the creation of new programs (such as a housing program). While these developments are consistent with higher revenues causing an expansion of the welfare state, they also are consistent with the other hypothesis, as the program was motivated by Truman’s belief in the federal government guaranteeing economic opportunity and social stability.

A final, indirect way of discriminating between the two hypotheses would be to examine how defense spending affects transfers provided by state and local governments.

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28 The obvious political-economy consideration here is that, following wars, the new programs can be implemented even as the share of taxes goes down.
The idea is that, because defense is a federal expenditure item financed by federal revenues, revenues of state and local governments should not go up at the start of a war and would, therefore, not induce more transfers due to a “peace dividend” when the war is over. A look at the sum of total local and state revenues (these include grants from the federal government) as a share of GDP actually reveals a monotonic fall from 1938 until 1944, after which it starts rising again. Only in 1948 is it back at its 1942 level. Thus, at least for WW-II, the presumption that the share of state and local government revenues does not go up during the war is amply validated.

In view of this, if higher post war solidarity is the main source of the ratchet in transfers we should observe an increase in the share of state and local government transfers at the end of the war in spite of the fact that their revenues did not increase during the war. However, if it is the war induced rise in revenues that induces transfers, then a war ratchet in transfers of state and local governments should be absent.

A regression of state and local governments social benefits as a share of GDP on up and down movements in the share of defense spending shows a reverse ratchet (not reported). To eliminate the possibility that this reverse ratchet is caused by the reduction in the revenues of state and local governments over the war cycle we run the same regression (not reported) using instead the share of state and local government social benefits in their total budgetary spending as a dependent variable. The reverse ratchet survives but only at a 10% significance level. These findings are consistent with the hypothesis that it is revenues that cause transfers but they do not exclude other potential explanations.29

6.3 Other regularities of war-time finance

6.3.1 The share of taxes during wars and tax smoothing
A striking aspect of the data is that the share of taxes goes up at the beginning of wars and only part of the way back down after wars. This is particularly evident during the two World Wars. Note that this pattern is inconsistent with a simple tax smoothing story

29 For example it is possible that, following WW-II, social assistance became to be viewed more than before as a task for the federal government.
(Barro, 1979) in which the war and its impact on the future path of government expenditures are taken to be a one-time surprise. For, in such a case the tax-smoothing hypothesis implies that the share of taxes should take a one-time jump upward and remain at this level until the next major surprise with respect to the future path of government spending. Such a pattern is predicted to arise under the tax-smoothing hypothesis in order to service the additional debt accumulated during the war.

One probably could rescue the tax-smoothing hypothesis by maintaining that towards the end of wars there were one-time big surprises downward with respect to the future path of government spending. But, even if we accept this possibility other pieces of evidence put a question mark on the view that, during wars, the share of taxes went up mainly in order to service future debt. The reason is that most of the post-war decreases in the debt to GDP ratio following wars were due to growth in real GDP and inflation. This is particularly striking following WW-II. In the aftermath of this war there were only two years in which there was some net repayment of nominal debt. At the end of WW-II the nominal debt in current dollars was about 240 billions. It went down by about ten percent over 1946 and 1947 and increased or remained flat in all subsequent years. Thus, at least with the benefit of hindsight, there clearly was no need to maintain a high share of taxes in the immediate post WW-II period in order to repay the debt accumulated during this war.\footnote{Obviously, a fuller analysis should also factor in interest payments.}

6.3.2 Some evidence on the effect of wars on the structure of taxation

Aggregate time series on the share of taxes obviously give only an indirect picture of the direct decisions of legislators with respect to tax rates and burdens. This subsection reports three regularities concerning the evolution of the structure of taxation during and in the aftermath of wars over the twentieth century.

First, top marginal income tax rates increase dramatically as the war builds up and remain at the high war time levels for several years after the war. This pattern is already in evidence during and after WW-I, but is particularly dramatic during and after WW-II and the Korean War. Thus, the top marginal income tax rate rose from 7\% in 1915 to a peak of 77\% in 1918 and remained at a slightly lower level of 73\% over 1919-1921
before coming down to 56% in 1922 and 25% in 1925. Prior to WW-II the top marginal income tax rate was relatively high to start with because of the Great Depression. But it increased substantially as the war unfolded. The rate was 79% in 1936. It increased to 81% in 1940, to 88% in 1942 and reached a peak of 94% during the last two years of WW-II. It decreased to a bit above eighty percent in the immediate aftermath of the war, was raised again to 91% during the Korean War and remained at this level till 1963. Second, the twentieth century is characterized by a steady trend of increase in the share of direct taxes and a parallel decrease in the share of indirect taxes within total tax revenues. This trend has usually been accentuated by wars. Finally, wars tend to raise the portion of tax collections done at the Federal level at the expense of tax collections by state and local governments.

6.4 War ratchets, the franchise and the Great Depression

The evidence presented in previous sections as well as additional regressions that were not presented support the view that the war ratchet in transfers is mainly associated with WW-II. Figure 6 in Section 5 showed that there was no ratchet or even a reverse ratchet on (linked) transfers in the aftermath of WW-I. This raises an important question about the origin of the difference in war ratchets between the two World Wars. The econometric analysis of Section 5 identifies the difference in the level of the franchise as one potentially important reason for this difference in the behavior of war ratchets. In particular, a higher franchise appears to be associated with a war ratchet in transfers.

Political economy models like those of Meltzer and Richard (1981) predict that a rise in the franchise should be associated with more redistribution. Existing empirical tests of this theory provide little or no support for this theory (Gouveia and Masia, 1998, and Perotti, 1996). When the franchise itself is added as a separate explanatory variable to the regressions of Section 5 we also found that it had no significant impact on the share of transfers.

However, we did find that the franchise changes the impacts of up and down movements in the share of defense in a way that creates a war ratchet. The wider implication of this finding is that, although a high franchise does not necessarily raise the

31 These figures come from the table “Historical Top Tax Rate” in the Tax Facts Database: http://www.taxpolicycenter.org/TaxFacts/Tfdb/TFTemplate.cfm?DocID=213&Topic2id=20&Topic3id=22
share of transfers during peace times, it significantly contributes to the expansion of this share over a war cycle by interacting with the dramatic changes in the shares of defense induced by the war buildup and its aftermath. This mechanism is particularly in evidence during and after WW-II.

The evidence in Section 5 is also consistent with the view that the Great Depression significantly contributed to the WW-II ratchet in transfers by changing the impact of up and down coefficients of defense on the share of transfers. However, it is not possible to discriminate econometrically between the contribution of the franchise and the contribution of the Great Depression to the WW-II ratchet in transfers. When proxies for both are used in the same regression, the separate effects of the franchise and of the Great Depression on the impacts of defense on the share of transfers become individually insignificant.

7. Concluding Reflections

The paper’s main findings can be divided into two groups. One concerns the relation between defense and civilian (non-transfer) government expenditures in different periods over the century. The other concerns war-related ratchets.

Relation between defense and civilian spending: The main finding here is that, while the correlation between the share of defense and the share of civilian government expenditures is positive during and around WW-I, it is negative during later wars. This is consistent with the view that: (i) there are stronger complementarities between military and civilian expenditures at low than at large government, and (ii) the marginal tax distortion is an increasing function of the relative size of government.

War-related ratchets: 1. There is a war-related ratchet in the share of transfer payments after 1929. This ratchet is robust to the use of consolidated government sector data rather than Federal data and to the exclusion of veteran benefits from transfers. But it disappears when WW-II is excluded from the sample. 2. There is a war related ratchet in the share of Federal taxes and revenues after 1929. This ratchet is robust to the exclusion of debt service and defense expenditures from taxes and revenues. 3. There is an inverse war-related ratchet in the share of civilian Federal expenditures after 1929. 4. There is a war-related ratchet in the share of veteran benefits after 1929. 5. Century long time series,
when available, show that the war ratchets above are more likely to develop after, than prior to, the Great Depression and when the franchise is relatively high. This provides political-economy underpinnings to the fact that there were no war-related ratchets during and around WW-I, whereas such ratchets were strongly in evidence during and after WW-II.

The finding about ratchets in the shares of taxes and of transfers following WW-II raises an intriguing question about the direction(s) of causality between those two shares. Did the war raise the demand for transfers preventing the share of taxes from fully decreasing when the share of defense went back down or was it the availability of a large tax share, conveniently created by the war, that induced the Administration and Congress to expand existing transfer programs and to create new ones? Or, perhaps it was a bit of both? Although the paper does not provide a definitive answer to this question, it does suggest possible ways to board it.

Whatever the answer to the causality issue, the findings in the paper show that differences in the franchise and the occurrence of the Great Depression go a long way towards providing an explanation for the fact that there were significant ratchets following WW-II and practically no ratchets following WW-I.\(^{32}\) This supports, combines and qualifies the Meltzer and Richard (1981) rational theory of the size of government and Higgs (1987) “Crisis and Leviathan” thesis. The first thesis implies that the share of transfers should increase with the franchise and the second that government should permanently grow during and following wars.

The paper shows that most of the impact of a high franchise on transfers occurs following wars, thereby stressing the importance of the interaction between wars and the franchise rather than the franchise alone. In particular, it produces systematic evidence showing that the share of transfers in GDP permanently grew following WW-II when, by far, most of the adult population was already affranchised, but did not following WW-I, when a much smaller fraction of the population was allowed to vote.\(^{33}\)

\(^{32}\) This is particularly remarkable in view of the fact that most of the war agencies created during WW-II were quickly dismantled after the war (Rockoff, 1999).

\(^{33}\) Similar but less dramatic differences between the aftermaths of the two wars arise with respect to the shares of taxes and of government revenues.
References


Appendix

This appendix presents an illustrative model of a welfare maximizing social planner in which defense spending and civilian public spending are positively correlated at low levels of government and negatively correlated at high levels of government. The basic assumptions leading to this conclusion are: 1. When a war shock occurs, the marginal utility of defense expenditures goes up; 2. At low levels of government defense and civil expenditures complement each other in the sense that the marginal utility of each type of expenditure goes up when the quantity of the other rises. By contrast, at high levels of government, the marginal utility of each type of expenditures is independent of the quantity of the other type; 3. The rate at which marginal tax distortions increase with taxes is positive only above a certain threshold size of government. Below this threshold the marginal tax distortion is constant.

We normalize the population size to unity. Let utility from (per capita or total) defense spending, $m$, and civilian public spending, $g$, be given by,

$$u(\alpha m, g),$$

(1)

where $\alpha$ is a shock affecting the marginal utility from defense spending (the outbreak of a war would classify as an increase in $\alpha$). We assume that $u_m > 0, u_g > 0, u_{mm} < 0, u_{gg} < 0$ and that $u_{gm} \geq 0$. Here, subscript $m$ ($g$) denotes the partial derivative with respect to the first (second) argument. We ignore transfers. Hence, civilian spending is the non-defense part of what the standard macroeconomics textbook calls “government purchases”. Defense spending and civilian public spending are financed by taxes $t$. Assuming that the government balances the budget, the government budget constraint reads:

$$g + m = t.$$  

(2)
Taxes lead to losses that are described by a distortion function

$$c(t),$$

with a positive and (weakly) increasing marginal distortion rate; i.e, \(c' > 0\) and \(c'' \geq 0\).

The government’s objective is to maximize \(u(am, g) - c(t)\) subject to the budget constraint (2). Substituting the constraint (2) into (1), the government’s problem is to choose \(m\) and \(t\) so as to maximize:

$$V(m, t) \equiv u(am, t - m) - c(t)$$

The first-order conditions for an internal maximum are:

$$V_m(m, t) \equiv a_u m (am, t - m) - u_g (am, t - m) = 0$$
$$V_t(m, t) \equiv u_t (am, t - m) - c'(t) = 0.$$

The second-order conditions for an internal maximum are

$$V_{mm}(m, t) \equiv a^2u_{mm} - 2au_{mg} + u_{gg} < 0$$

$$V \equiv V_{mm}V_{tt} - (V_{mt})^2 > 0$$

where \(V_{tt}(m, t) = u_{gg} - c''\) and \(V_{mt}(m, t) = au_{mg} - u_{gg}\). We now formulate the assumptions presented at the outset more precisely.

**Assumption 1:** The marginal utility of defense is positively affected by the shock \(a\) or more formally

$$\frac{d(au_m)}{da} = u_m + amu_{mm} > 0.$$
**Assumption 2:** There are sufficiently strong positive complementarities in utility between defense and civilian expenditures at relatively low government \((g + m)\) is small), and no utility interactions between them at relatively large government \((g + m)\) is large). Formally,

\[
\begin{align*}
  u_{gm} &> 0 \text{ at large government,} \\
  u_{gm} &= 0 \text{ at small government.}
\end{align*}
\]

**Assumption 3:** When the share of taxes in GDP is lower than a certain positive threshold, the marginal tax distortion is constant. Formally,

\[c'' = 0.\]

Next, we evaluate the effect of the war shock on \(m\) and \(g\) by performing a comparative statics experiment with respect to \(a\) and using the three assumptions above. Differentiating the first-order conditions totally with respect to \(a\) we obtain after some algebra

\[
\begin{align*}
  \frac{dm}{da} &= \frac{1}{V} [mu_{gm}(au_{gm} - c'') + (c'' - u_{gg})(u_m + amu_{mm})], \\
  \frac{dg}{da} &= \frac{1}{V} [mu_{gm}(c'' - a^2u_{mm}) + (au_{gm} - c'')(u_m + amu_{mm})].
\end{align*}
\]

When government is large the second part of Assumption 2 implies that those equations reduce to
\[
\frac{dm}{da} = \frac{1}{V} (c'' - u_{gg})(u_m + amu_{mm}) > 0, \quad (10)
\]
\[
\frac{dg}{da} = -\frac{1}{V} c'' (u_m + amu_{mm}) < 0, \quad (11)
\]

where the inequalities follow from Assumption 1 and the fact that \( V > 0 \) by the second order condition. Consequently, at relatively large levels of government, defense and civilian expenditures are negatively correlated, as is the case during WW-II when government was relatively large.

When government is sufficiently small equations (8) and (9) reduce to

\[
\frac{dm}{da} = \frac{1}{V} [amu_{gm}^2 - u_{gg} (u_m + amu_{mm})] > 0
\]
\[
\frac{dg}{da} = \frac{1}{V} [-a^2 u_{mm} u_{gm} + au_{gm} (u_m + amu_{mm})] > 0.
\]

The inequalities are implied by the three Assumptions, the fact that \( V > 0 \), and the fact that the marginal utilities of civilian, and of defense, expenditures are both decreasing in the shares of those variables. Those arguments provide a proof to the following summary proposition.

**Proposition 1** Given Assumptions 1-3 above, when the size of government is sufficiently small, both defense spending and civilian public spending are increasing in the war shock \( a \) and are, consequently, positively related. When the size of government is sufficiently large, civilian public spending is decreasing in the war shock \( a \), while defense spending is still increasing in it, implying that civilian public spending and defense spending are negatively related.
Financial acceleration of booms and busts