Essay IV

Abstract: Differences in economic growth is one of the primary explanations for why welfare state retrenchment has occurred in some countries and not others. Because public spending is only partially indexated to earnings, the welfare state shrinks as the economy prospers. Using growth data from countries with synchronized business cycles as an instrument, I demonstrate that the relationship can be interpreted as a causal effect of growth on both tax ratios and social spending.

Economic growth is deflating the welfare state

Most theories on institutional change assume that changes fill a purpose. The power resources approach analyse welfare outcomes as the result of distributive conflict between socioeconomic classes, typically mediated through class-based parties (Allan and Scruggs 2004; Korpi and Palme 2003). Scholars within the Varieties of Capitalism approach claim that institutions evolve to complement other institutions in a country (Hall and Soskice 2001). Yet others have argued that welfare state development is governed by demographic pressure and inter-generational conflict (Wilensky 1975), or pre-determined by globalization and its economic constraints (Cameron 1978; Schwartz 1994).

Comparative studies of welfare spending typically analyse the welfare state as a distributive conflict between those who benefit from public spending and those who do not (Huber and Stephens 2001; Meltzer and Richard 1981; Moene and Wallerstein 2003). When formalized, the size of the public sector is usually assumed to follow the preferences of the median voter. Identifying the goals and constraints of policy makers or interest groups is often a fruitful approach for understanding reforms, but searching for a purpose when there is none will always prove futile.

In this essay I argue that much of the recent development of taxation and social spending does not reflect voter preferences or the power of organized interests. On the contrary, the different trajectories that welfare states have taken during the last two decades can to a large extent be explained by differences in economic growth. Slow-growing economies have expanded their public sector, while countries with rapid growth have reduced both taxes and social spending.[48] This relationship is illustrated by Figure 4.1, which shows the average annual growth and the accumulated change in tax ratio between 1995 and 2015. We can see a clear pattern where the fastest growing economies have reduced the size of their public sector by around 5 per cent of GDP, while the slow-growing economies have experienced a similar expansion.

[48] I will later argue for the assumed causal direction and for why the tax ratio is a good measure of welfare effort.

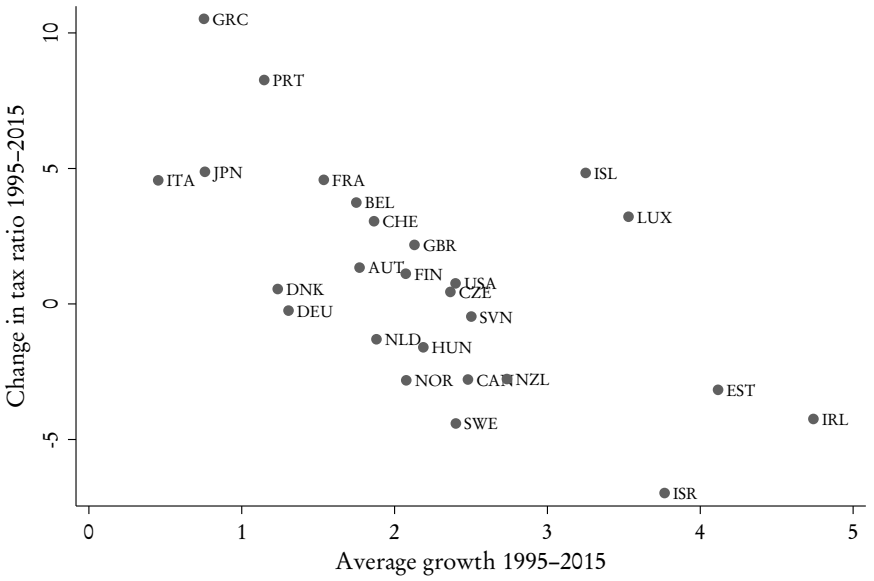

Figure 4.1: Growth and welfare state development 1995–2015

This relation between economic growth and welfare state retrenchment should not be confused with economic determinism – there is no reason why rich countries cannot sustain high taxes and generous welfare systems. Instead, I argue that the relationship reflects an unintended consequence of how the budget process works. When the economy grows, public spending automatically decreases as a share of GDP, because most expenditures are not tied to wages or GDP growth. Unless this erosion of existing programmes is neutralized by government decisions to increase spending, there will be a reduction in the resources available for social insurance and welfare services.

There are several reasons to believe that this deflating effect is not neutralized by discretionary actions. First, when a government faces fiscal space for reforms, it might come naturally to spend it on a mix of tax cuts and spending increases, even if the budget space resulted from a lack of expenditure indexation. It is usually the discretionary actions of a budget that are debated, and by both cutting taxes and increasing expenditure it appears that both sides are being at least partly met. Second, it is well-established that governments can obfuscate cutbacks by letting inflation and wage increases erode the value of transfers instead of taking discretionary actions (Green-Pedersen 2002; Lindbom 2007; Pierson 1994). In addition to obfuscating the cutbacks, rapid growth might also function as a sweetener that makes cutbacks more acceptable. If a government wants to retrench the welfare state without public resistance, we would expect

them to do it during times of economic growth. Third, many countries have introduced medium-term ceilings for aggregate expenditure. When those ceilings are set low enough to be binding, they force governments to implement tax cuts instead of increased spending whenever economic growth has provided fiscal space.

The assumed causal direction in this essay is that economic growth affects the tax ratio and social spending, but the correlation illustrated in Figure 4.1 is of course equally compatible with an effect that goes from tax cuts to economic growth. To avoid the risk of reverse causation, I rely on an instrumental variable approach to identify the causal effect of growth. The GDP growth for each country is instrumented on a weighted average of the growth in countries with synchronized business cycles. While this strategy does not guarantee unbiased identification, I consider it an improvement to common approaches in comparative research, and I expect any remaining bias to be small compared to the estimated effects.

Traditional OLS regressions and my IV approach both show the same thing: during periods of fast growth, both taxes and public social spending has been reduced as a share of GDP. A permanent increase in the growth rate by one percentage point is expected to reduce the size of the public sector by about 0.5 per cent of GDP.

This essay contributes to several different literatures. First, by providing a new explanation of how welfare states have developed over the last decades, it contributes to the comparative welfare state research. Second, the essay provides a new argument in the debate about whether the size of the public sector reflects the preferences in the electorate, or if there is a bias in any direction (Downs 1960). Because this argument directly relates to institutions created to improve fiscal discipline, the essay also contributes to the public finance literature concerned with fiscal sustainability. Finally, the essay provides a reason why cross-national studies that find a correlation between tax cuts and growth cannot interpret it as a causal effect of taxes.

Indexation and the budget process

The mechanism proposed in this essay rests on two necessary conditions. The first is that public spending automatically decreases as a share of GDP when the economy grows. Because the tax ratio is less affected by economic growth, a growing economy creates a fiscal space which the government can use for tax cuts and spending hikes. Governments can of course create fiscal space by abolishing old programmes or decide on new taxes, but budgeting is still incremental in the sense that major reallocations are rare (Schick 1983, 2009). The second condition is that

governments use a large part of this fiscal space on tax cuts, even if the space was created by a reduction in spending as a share of GDP. Together, these two conditions cause the public sector to shrink as the economy grows. This section deals with one condition at a time, beginning with the reasons why the spending share decreases with economic growth.

Budgets are typically formulated on a nominal basis with discretionary decisions required to adjust expenditures to inflation or increased programme utilization (Marcel 2014). These decisions are made in relation to a baseline estimation, which can be equal to the previous year's expenditure in nominal terms or adjusted for factors like inflation, GDP or population growth. How these baselines are calculated is an informal process in most countries and usually not codified by budget rules (Schick 2009). During times of austerity, the baselines are often manipulated to facilitate fiscal adjustments (Bozeman and Straussman 2015). The calculation of baselines can also vary wildly between different levels of government or between regions in a country (Crain and Crain 1998). The end result is usually that *nominal* expenditures for a programme rise compared to the previous year, but not enough to offset the projected increases in prices and programme utilization (Schick 1986, 2007).

Unlike other expenditures, the indexation of social insurance entitlements usually follows relatively well-defined rules. Most entitlements are tied to prices or wages with either automatic indexation or institutionalized discretionary actions. Unemployment benefits usually have one component that depends on previous earnings and one component that is set in nominal terms or tied to prices or the minimum wage. Family benefits and social allowances are linked to inflation in about half of the OECD countries and not indexated at all in the other half. Price inflation is the most common anchor for the indexation of pensions, but weighted averages of prices and wages are also common, often in combination with stronger indexation for lower pensions. It should also be stated that, in practice, indexation rules have often been over-ruled in times of extreme growth and inflation (Whitehouse 2009).[49]

Because taxes are to a large extent proportional to income, they tend to follow GDP much more closely than public spending. However, the wage increases associated with GDP growth can move people into a higher tax bracket if the thresholds are not tied to wages. This tendency has primarily been debated during times of high inflation and is usually referred to as

[49]There are also other reasons why the spending share will fall over time, like annual reductions in the budget baselines to account for assumed productivity growth and time-limited programmes that will cease to exist unless discretionary actions are taken. However, because they are unrelated to GDP growth they are irrelevant for the case made in this essay.

'bracket creeping'. If the tax system is very progressive, nominal wage increases can have an *increasing* effect on the tax ratio, compared to the *decreasing* effect on public spending as share of GDP.

I will now present three arguments for why we should expect automatic decreases in the spending share of GDP – which provide the government with a fiscal space for reforms – to cause tax cuts. The first reason is that the budget process makes it natural for governments to cut taxes. Most rich countries today use a top-down budgeting approach, where aggregate spending is not determined as the sum of spending requests from the ministries, but centrally decided as a choice between tax cuts and increased spending (or tax hikes and spending cuts). Moreover, inter-governmental discussions and public debate tend to be focused on the discretionary allocation of these resources. Some interest groups will demand lower taxes while other claimants lobby for reforms that increase expenditure. The government cannot reconcile these conflicting interests, but deciding to use the fiscal space on a mix of tax cuts and spending hikes will at least foster the appearance that both interests are being partly met (Schick 1986).

The second reason is that GDP growth provides governments with an opportunity to obfuscate welfare state retrenchment because they can cut back on spending without taking discretionary actions. It is widely accepted that governments use different forms of *blame avoidance* to avoid negative feedback from voters. Pierson (2001) argues that *obfuscation* is the most important of these strategies, and the specific tactic to freeze expenditures within a growing economy is something he refers to as *decrementalism* (Pierson 1994, p. 20). In support of his argument, it has been shown that non-indexed programmes have been more affected by cutbacks than indexed programmes (Green-Pedersen 2002; Lindbom 2007). For the same reasons we should also expect larger spending cuts during periods of fast growth.

Third, many countries have fiscal rules that can prevent governments from meeting unexpected GDP growth with increased spending. During the last decades, many countries have implemented a variety of fiscal rules to improve budget discipline, and different kinds of spending targets can now be found in most rich countries (see Ayuso i Casals [2012] for a survey of the EU member states). They function as a pre-determined ceiling for the government's expenditure, often set a few years in advance, to ensure that governments cannot let temporary revenues cause a permanent increase in spending, and that soaring costs in one area of expenditure must be compensated for with cuts in other areas. Spending targets are usually set in nominal terms, which strengthens their macroeconomic stabilization effect. The point of spending targets is that they should be set

so low that unexpected increases in income cannot be used for increased expenditures. Thus, when the economy grows faster than expected and the spending target is binding, governments must choose between tax cuts and improvements in the budget balance.

Data and design

This section is divided into three parts. In the first part, I provide an extensive argumentation for why I operationalize welfare effort with tax revenues and public social spending, both measured as shares of GDP. How welfare effort should be measured is a heated subject in the welfare state research, but I believe that most participants in this debate have overlooked the advantages of using tax ratios. In the second part, I describe the instrumental variable approach I use to manage the problem of reverse causality. By using the growth rate in countries with synchronized business cycles, I get an instrument that is strongly correlated with the endogenous growth variable, and which should only be marginally influenced by the country's tax policy. The third part presents the model specifications and the remaining data.

Measuring welfare effort

Esping-Andersen (1990) famously stated that no one 'struggled for spending per se' in his argument for why social spending is an insufficient outcome if we want to understand the expansion of the welfare state. Since then, entire volumes have been written about this dependent variable problem (Clasen and Siegel 2007). Because most authors have been critical about the use of expenditure-based measures – and many of their arguments apply to tax ratios as well – I will briefly comment on their criticism and how it applies to my study. I have divided this discussion into four arguments. In short, I argue that i) measures based on spending (or revenue) data are suited for the causal mechanisms described in the previous section, that ii) previous research has exaggerated the 'denominator problem' of GDP ratios, that iii) while social expenditure is more directly related to money spent on welfare services, other forms of public spending also have a decommodifying effect, and that iv) there are methodological advantages of using the tax ratio as the dependent variable.

My first argument is that aggregate spending (or revenues) is a logical choice for dependent variable given the mechanisms I have described. With his quote, Esping Andersen did not claim that social spending is a bad measure of welfare effort per se. His argument was that we must begin with demands that were promoted by the actors which were important for the welfare state development if we want to test the causal theories that

involve these actors. This argument is sometimes restated to suggest that the choice of dependent variable must follow from the causal argument (Green-Pedersen 2007; Jensen 2011) and that expenditure-based measures can be perfectly justified from a different theoretical perspective (Green-Pedersen 2004). The mechanisms described in this essay are not limited to specific actors or welfare programmes, nor can they be identified as reforms or discretionary actions. On the contrary, they focus on retrenchment that happens because actions are *not* taken to counteract the eroding effect of growth. They should therefore be analysed on the aggregate level and not on single rules or changes in nominal amounts.

My second argument is that the denominator problem of spending shares (or tax ratios) is overstated. Many have argued that the spending share of GDP is problematic as a measure of welfare effort, because it is impossible to separate the amount of money spent by the government and the size of GDP and, similarly, that the ratio can easily divert from nominal or real spending when the economy grows (Clayton and Pontusson 1998; Kühner 2007; Olaskoaga et al. 2013; Scruggs 2006, 2007). There are several problems with this argument. First, it is not the denominator which makes it difficult to separate welfare effort from economic circumstances, but the fact that we are comparing different contexts. The reason for using a denominator is usually that it facilitates comparisons, not because it makes them misleading, and the problem is therefore not isolated to measures that use a denominator. For example, life expectancy is five years longer in Japan than in the United States. Does equal welfare effort still imply equal retirement age? Second, wages tend to follow GDP closely. Thus, constant replacement rates in social insurance schemes as well as maintaining the number of public employees, require that those parts of public spending grow at the same pace as GDP. Using consumer prices instead of GDP as the deflator would therefore lead to misleading conclusions. For example, real social expenditure grew by about 30 per cent under both Reagan and Thatcher, but I doubt that anyone would describe their policies as an expansion of the welfare state.[50] Third, even if productivity gains in the public sector mean that GDP growth will exaggerate the cost development for public spending somewhat, I argue that it is still a good comparison for welfare effort. An effort must always be understood in relation to the resources available. Just as it requires a larger effort to put food on the

[50]Ireland during the period 1980–2000 has become the favourite illustration of how GDP growth distorts spending ratios (Olaskoaga et al. 2013; Scruggs 2006, 2007). However, it was not the GDP growth that caused this anomaly – where spending ratios fell at the same time as replacement rates in social insurance increased – but a combination of an unprecedented reduction in the wage share, falling dependency ratios and reduced means-tested transfers (Timonen [2003] and my own calculations).

table if you are poor, new medical discoveries do not mean we can close hospitals and claim that our welfare effort is the same. It therefore makes sense to measure welfare effort as *the share* of our resources that we spend on welfare programmes.

My third argument is that virtually all public funding strengthens the decommodifying effect of the welfare state. Esping-Andersen stated that social rights should be measured as 'the degree to which they permit people to make their living standards independent of pure market forces' (Esping-Andersen 1990). Hence, social rights have usually been operationalized as the comprehensiveness and generosity of social insurance. The higher the replacement rates in the insurances, the less your consumption will fall if you become sick or unemployed. Measuring welfare effort with the tax ratio is sometimes criticized because government revenues can be used to pay for things usually *not* considered as social rights or welfare services, like road networks, public transportation and recreational facilities. While they might not constitute the core of the welfare state, they might still contribute to its decommodifying effect. When these goods are paid for by tax revenues instead of road tolls, entry fees and bus prices, the consumption of these goods is made independent of one's market income, much like social insurance guarantees a certain living standard for those without a wage. However, if tax revenues are used to pay for non-excludable goods like prisons or the military, there would be no such decommodifying effect. And if the public resources are primarily spent on privileged groups, heavy taxation could – at least in theory – increase people's dependency on their market income. But while many have claimed that wealthy citizens extract at least as much as the poor in terms of many welfare services (Le Grand 1982), tax financing still means that 'services are redistributive in an egalitarian direction, albeit less so than are cash transfers' (Esping-Andersen and Myles 2009). Besides, as shown in Figure 4.2, countries with high shares of social spending also have high taxes. This strong *spatial* correlation suggests that it might be more important to look at the measures' cyclical properties – and, in particular, how it is affected by automatic stabilizers – which makes room for my last argument.

Fourth, it is my opinion that the most serious problem with spending data is that government expenditure will always be affected by changes in the dependency of welfare services.[51] We cannot talk about a stable welfare effort if budgets and policies do not adjust to face increasing risks or other forms of changing social circumstances. Economic downturns are the most obvious example, but the demographic structure, changes in sickness

[51] This does not mean that measures based on rules and rights are free of this problem (Hacker 2004).

Figure 4.2: Taxes and social spending year 2000

leave and flows of refugees also give rise to cyclical patterns in expenditure that are unrelated to the rights of citizens. However, the latter should not cause any problems for the empirical analysis in this essay, because they can only affect the results to the extent that these factors are correlated with growth. And, given my instrumental variable approach, only to the extent that they are correlated with economic growth in countries with synchronized business cycles. I will therefore focus my discussion on expenditures related to the business cycle. For social pending, it is obvious that fast growth is correlated with lower needs for unemployment benefits and social assistance. But while automatic stabilizers would bias the effect of growth on expenditure 'upwards' (away from zero), the tax ratio is instead biased 'downwards' (towards zero) and the bias is much smaller. During economic upturns, less people are unemployed, which means that the costs for unemployment benefits and other transfers would decrease during times of economic growth, even if the generosity of the welfare systems remained unchanged. On the other hand, the tax ratio – in absence of discretionary actions – tends to increase during times of rapid growth. Both profits and sales usually increase faster than GDP, and thus also the revenues from corporate and sales taxes. This is the primary reason for why I trust the results for the tax ratio more than those for social spending.

I will conclude with a few additional arguments for why taxation can be a better measure of the long-term commitment to welfare spending

than expenditure-based measures. First, the tax ratio is less susceptible to all kinds of dependency-driven effects – not only the automatic stabilizers discussed above – because deviations in expenditure do not require tax changes, provided that the deviations are temporary. Annually balancing the budget through tax changes would cause the distorting effects of taxes to be larger than necessary (Barro 1979). Second, while *unexpected* growth might lower the spending share of GDP because budgets were based on expected growth (Iversen and Cusack 2000; Roubini and Sachs 1989), noticeable changes in the tax ratio would not happen without discretionary actions. Therefore, they are also less likely to be reverted as soon as the government realizes that the forecast was wrong. Third, I acknowledge that technical differences like taxation of transfers will artificially inflate the tax ratio in some countries compared to others (Adema and Ladaique 2009). However, such differences should be stable over time and can therefore be handled by including country fixed effects in the models.

The instrument

The main methodological challenge in this essay is the obvious risk of reverse causation. A negative correlation between growth and the size of the public sector could also result from an effect of taxes or social spending on growth. To minimize this risk, I rely on an instrumental variable approach to create a plausibly exogenous variation in the growth rate. I exploit the fact that some countries have relatively synchronized business cycles, usually because they have a similar business structure or because they are major trade partners. By using a weighted average of the growth in a set of countries with similar business cycles, I get an instrument that is strongly correlated with a country's growth. While the instrument cannot eliminate the risk of reverse causation, I expect it to remove most of the bias that this problem might cause. In this section I describe the method I use, discuss various threats to the instrument's validity and compare my instrument with other approaches in the literature.

I use recursive all-subsets regression to identify countries with synchronized business cycles.[52] For each combination of country i and year t, I regress the nation's annual growth between 1960 (or the oldest observation available) and $t - 1$ on the growth in the world's N largest economies for which I have GDP data since 1960.[53] All possible subsets of the N largest economies are considered before I choose the set of countries which

[52]Recursive means that I estimate a separate model for each year where only observations before that year are included. The all-subsets regression is performed using the leaps-and-bounds algorithm developed by Furnival and Wilson (1974) and the best subset is defined as the subset of variables which minimizes Mallows's C.

[53]N is set to the number of observations minus five, but is capped at a maximum of 20 countries.

minimizes the information criteria Mallows's C. I then use the regression coefficients from this regression and predict the growth rate in year *t* based on the growth rate this year in the regressor countries. I also record the average change in the tax ratio and social spending for these countries. These variables are later used to control for policy diffusion.

Expressed in less technical terms, the model which provides the best fit for the 1960–1989 period is extrapolated to make a prediction for 1990. I then identify which subset of countries best fits the 1960–1990 data, make a prediction for 1991, and so on. When I have analysed the 1960–2014 period to make a prediction for 2015, I repeat the same procedure for every other country. These predictions then constitute the instrument I will use in the regression analyses.

Fortunately, the instrument turns out to be strongly correlated with the endogenous regressor (Pearson's r is close to 0.7), but there are two problems with its exogeneity. First, the exclusion restriction requires that the instrument only affects the dependent variable through the endogenous regressor. If this restriction is violated, the estimated effect will suffer from bias. There are reasons to suspect minor violations of this restriction. Just as national economies are interdependent, policy can also spread from one country to another (Simmons and Elkins 2004). If the fast growth in neighbouring countries is caused by tax cuts or reduced social spending, it might be their policy decisions which affect policy in the investigated country, and not the rapid growth. To control for such policy diffusion, I use the average change in the tax ratio and social spending, calculated across the countries which were used to instrument the growth rate. In other words, the unbiasedness of this estimator rests on the assumption that – controlling for policy changes – growth in neighbouring countries only affects changes in the tax ratio through its effect on the growth rate.

The second problem is that we cannot rule out a reverse effect of tax policy on the instrument, because the instrument is not entirely exogenous to the endogenous regressor. A share of the covariation between the regressor and its instrument will be caused by economic growth in the investigated country affecting economic activity in the countries used for the instrument. If policy affects growth, and the growth rate in turn affects growth in the countries used to create the instrument, the estimated effects of growth on policy would suffer from bias caused by reverse causation. Fortunately, this reverse effect will be small compared to how much the other countries affect growth in the investigated country, because the aggregated size of the former economies tends to be much larger. On average, the total GDP of the countries used to create the instrument is six times larger than that of the investigated country. Besides, a large part of the correlation in growth rates originates from common shocks

that are exogenous to the policy process in both the investigated country and the countries used for the instrument. If most of the covariation between the endogenous growth variable and its instrument consists of common shocks and the influence from other countries, it is safe to assume that the bias caused by a causal effect of the endogenous regressor on the instrument will be small compared to the size of the effect. I investigate this further in the robustness section.

The most common way of instrumenting growth is to use lagged values of growth (Barro 2000) or growth predictors (Acemoglu et al. 2008). However, the exclusion restriction would be violated as soon as tax changes are anticipated and growth reacts to expectations of future taxes. Apparently, this would also be a weaker instrument than the one I use. Others have used the quality of institutions (Dollar and Kraay 2002) or weather variations (Miguel et al. 2004), but institutions cannot explain short-term variation in growth, and weather is only relevant for agricultural economies.

An approach similar to mine has been used by Acemoglu et al. (2008) and Brueckner et al. (2015), but we differ in how the weights are assigned. They use trade shares as weights, but bilateral trade is only one reason behind business cycle synchronicity. Other reasons include similarities in industrial structure and factor intensity, the degree of industrialization, currency unions, financial integration, similarities in export and import baskets and the openness of the economy (Baxter and Kouparitsas 2005; De Haan et al. 2008). By restricting countries with syncronized business cycles to countries with much bilateral trade, the instrument becomes much weaker.[54] This restriction could be motivated if bilateral trade were less susceptible to exogeneity problems than business cycle synchronicity caused by other factors, but I find that unlikely. On the contrary, I would argue that both the problems described above are of larger concern for countries that trade a lot than, for example, countries where business cycles are synchronized because their import and export baskets are similar. Trade partners are likely candidates for policy diffusion and their business cycles will be correlated because of trade, not because of common exogenous shocks.

To sum up, I recognize that the exogeneity of my instrument is not perfect, but compared with OLS it should drastically reduce any bias caused by reverse causation. The strength of the instrument also means that minor violations of the exclusion restriction will only marginally affect the estimated effects (Murray 2006). I will now proceed to the model specifications.

[54] When the variables have been recalculated to growth instead of levels, the correlation between the instrument and the regressor is 0.16 in Acemoglu et al. (2008).

Empirical specification

The OLS regressions follow Eqation 4.1, where the tax ratio ($T_{i,t}$) is regressed on its lagged value ($T_{i,t-1}$), the growth rate (g_i^t) and a vector of control variables ($\boldsymbol{\chi_{i,t}}$). These are country dummies, the annual inflation, the output gap, the cyclically adjusted budget balance, the unemployment rate, the size of the working age population, a dummy for election years, a dummy for right-wing governments and a linear time trend. The choice of a time trend over year dummies is discussed in the results section. When the tax ratio is substituted by different measures of social spending, I only substitute the dependent variable and its lagged value.

$$T_{i,t} = a_1 + \beta_1 \times T_{i,t-1} + \beta_2 \times g_{i,t} + \boldsymbol{\delta_1} \times \boldsymbol{\chi_{i,t}} + e_{i,t} \tag{4.1}$$

In the IV approach, the growth rate is replaced by the predicted growth rate ($\hat{g}_{i,t}$), which is estimated in the first stage equation. To control for policy diffusion effects, I also include the average change in the tax ratio in the countries which were used to create the predicted growth rate ($\Delta T^*_{i,t}$). The equation for the predicted growth rate includes the same variables as the second-stage equation, in addition to an instrument based on growth rates in countries with synchronized business cycles ($g^*_{i,t}$). The two equations are estimated using 2SLS.

$$\begin{aligned} T_{i,t} &= a_3 + \beta_6 \times T_{i,t-1} + \beta_7 \times \Delta T^*_{i,t} + \beta_8 \times \hat{g}_{i,t} + \boldsymbol{\delta_3} \times \boldsymbol{\chi_{i,t}} + u_{i,t} \\ g_{i,t} &= a_2 + \beta_3 \times T_{i,t-1} + \beta_4 \times \Delta T^*_{i,t} + \beta_5 \times g^*_{i,t} + \boldsymbol{\delta_2} \times \boldsymbol{\chi_{i,t}} + e_{i,t} \end{aligned} \tag{4.2}$$

In this essay, the tax ratio is defined as the sum of taxes on production and imports (indirect taxes), current taxes on incomes and wealth (direct taxes), social security contributions, and other current receipts, expressed as a percentage of GDP. The measure of public social spending is a gross measure; i.e., it has not been adjusted for cross-national differences in the taxation of transfers. However, because such adjustments are quite stable over time, it should not affect the results in this essay.

Most of the data come from the OECD. The tax ratio, growth, unemployment rate and inflation are gathered from OECD Economic Outlook 98 (December 2015). The share of youths and elderly people is collected from the publication Country Statistical Profiles, data on public social expenditure are from the Social Expenditure Database (SOCX) and both election years and the ideological position of the government come from the Database of Political Institutions. To extend the growth series back to 1960, I add data from Economic Outlook 87 (May 2010). I also use this version of Economic Outlook together with data from the World Bank to

replace missing values in the inflation variable. Imputation and linking procedures are described in the supplementary information.

Results

The regression results are presented in three tables. Table 4.1 presents different specifications with the tax ratio as the dependent variable. One of these specifications is then used when the tax ratio is substituted with different kinds of public social expenditure. These results are presented in Table 4.2. The section ends with an examination of how robust the results are to alternative specifications. Table 4.3 presents the results from the robustness checks.

The first column of Table 4.1 shows the results from when the tax ratio is regressed on the GDP growth and the main set of control variables. An increase in GDP growth by one percentage point is estimated to immediately reduce the tax ratio by 0.08 percentage points. To put the size of this coefficient into perspective, for a country with an average sized public sector (40 per cent of GDP), where 50 per cent of public spending is tied to economic growth, each additional percentage point of growth will reduce public expenditure by 0.2 per cent of GDP. If this fiscal space is equally divided between taxes and expenditure, the tax ratio would fall by 0.1 per cent of GDP, which can be compared to the coefficient of 0.08. We can also calculate the long-term change in the tax ratio, which would follow from a permanent change in the growth speed. In this case, a permanent increase in the growth speed by one percentage point would reduce the tax ratio by 0.3 per cent of GDP ($-0.08/(1 - 0.73)$).

In this specification, it is assumed that the effect of all independent variables decay geometrically at the same rate. This assumption would be false if governments compensate their previous mistakes as soon as they realized that the tax ratio had fallen unintentionally. To allow for a temporary effect of growth, the model presented in the second column includes two lags of the GDP growth. However, both the lags have coefficients close to zero, indicating that changes in the tax ratio caused by growth are just as persistent as other factors that have affected the tax ratio. The growth lags are therefore excluded from subsequent models.

If changes in the tax ratio affect the GDP growth, the OLS estimates in the first column are biased. To address this problem of reverse causation, columns 3–4 present the results from models where the growth is instrumented on the growth in other countries. As shown by the difference between column 1 and column 3, the IV approach actually leads to larger effects. The immediate effect of a change in growth by one percentage point is now a reduction in the tax ratio by 0.13 per cent of GDP. The first-stage F statistic is 14.24, which confirms the strength of the instru-

Table 4.1: The effect of growth on the tax ratio

	(1)	(2)	(3)	(4)	(5)
Tax ratio (t−1)	0.73***	0.73***	0.74***	0.79***	0.77***
	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)
Growth	−0.08***	−0.08***	−0.13*	−0.37**	−0.12***
	(0.03)	(0.03)	(0.07)	(0.16)	(0.04)
Year	−0.03**	−0.03*	−0.03**		
	(0.01)	(0.01)	(0.01)		
Inflation	−0.04	−0.04	−0.02	−0.14	−0.06*
	(0.03)	(0.03)	(0.07)	(0.10)	(0.03)
Output gap	0.02	0.01	0.07	0.20*	0.04
	(0.03)	(0.04)	(0.07)	(0.12)	(0.04)
Adj. net lending	0.07**	0.06**	0.08***	0.09***	0.08**
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Unemployment	0.07**	0.07**	0.10**	0.08	0.01
	(0.03)	(0.03)	(0.04)	(0.06)	(0.03)
Working age population	−0.18*	−0.18*	−0.25***	−0.13	−0.13*
	(0.09)	(0.09)	(0.09)	(0.13)	(0.07)
Election	−0.08	−0.07	−0.06	−0.02	−0.09
	(0.07)	(0.07)	(0.07)	(0.09)	(0.07)
Right-wing	−0.13	−0.14	−0.19	−0.20	−0.15
	(0.19)	(0.19)	(0.18)	(0.15)	(0.17)
Growth (t−1)		0.01			
		(0.03)			
Growth (t−2)		0.01			
		(0.02)			
ΔTax, instr. countries			0.30***	−0.14	
			(0.08)	(0.15)	
Constant	81.44***	77.39**	88.17***	19.34**	21.51***
	(28.27)	(30.74)	(25.38)	(9.68)	(5.55)
Estimator	OLS	OLS	2SLS	2SLS	OLS
Country dummies	Yes	Yes	Yes	Yes	Yes
Year dummies	No	No	No	Yes	Yes
Observations	379	377	352	352	379
First stage F statistic	-	-	14.24	3.80	-

Dependent variable: tax revenues as a percentage of GDP. Country-clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

ment (Sovey and Green 2011). However, instrumenting growth on the growth in other countries reduces precision, and the coefficient is only significant at the 90 per cent level. The large standard errors mean that the difference between the OLS and IV estimates can be coincidental, but if the OLS estimates are actually biased towards zero, one explanation could be that tax cuts are associated with decreased spending and that the fiscal multipliers for government expenditure are larger than those for taxes (IMF 2012). Another possibility is that 'imported' growth is more difficult to predict and that the policy response differs between predicted and unpredicted growth.

The model shown in the fourth column includes year dummies. This is usually standard procedure, but the dummy variables cause problems when they are included in a model where the growth is instrumented on the growth in other countries. The reason for this is that the instrument becomes much weaker when the year dummies effectively control for world growth, which in turn leads to inflated standard errors and a smaller F statistic. The size of the estimated coefficient (−0.37) is much larger than in previous models and also larger than what I would expect from theory. While I find it reassuring that year dummies do not reduce the effect, I interpret this estimate as a result of bad precision.

To improve the precision, the fifth column presents the results from when year dummies are added to the model presented in the first column. The estimated coefficient is now larger than when only a linear time trend was included, confirming that the omitted variable bias caused by excluding the time dummies at least do not make the effects appear larger than they actually are.

Taken together, these results support the argument that rapid economic growth reduces the size of the public sector. However, it is not possible to obtain precise estimates while both including year dummies and using the IV approach, which makes it difficult to know exactly how large this effect is. Because I regard reverse causation as my main identification problem, I will use the IV model without year dummies as I proceed to analyse the change in social spending.

There are methodological arguments for why I believe that the tax ratio is better than spending data for measuring welfare effort. These are discussed in detail in the next section. However, it is still interesting to see which kinds of expenditure are most affected by growth, and how the effect differs compared to when the tax ratio is used as the dependent variable. All regressions on spending data follow the same specification as the model presented in the third column in Table 4.1.

The results for these regressions are presented in Table 4.2. The first column shows how growth affects total public social expenditure, as a share of GDP. As expected, the estimated effect is much larger than for taxes. The second and third column have the effect decomposed into cash benefits and benefits in kind, which indicates that the aggregate effect is driven by cash benefits. Column 4–6 show how the effects differ between expenditure related to unemployment (unemployment benefits and active labour market policy), health (spending related to health and incapacity) and other sources (family related, pensions, housing benefits, etc.). The three areas of expenditure show similar effects, but spending related to unemployment amounts to a much smaller share of GDP than the other areas of expenditure and is therefore – relatively speaking – more strongly

Table 4.2: The effect of growth on social expenditure

	(1)	(2)	(3)	(4)	(5)	(6)
Growth	−0.37***	−0.30***	−0.05**	−0.16***	−0.09**	−0.16***
	(0.07)	(0.05)	(0.02)	(0.05)	(0.04)	(0.05)
Lagged dependent	0.89***	0.94***	0.92***	0.96***	0.84***	0.87***
	(0.04)	(0.04)	(0.04)	(0.08)	(0.05)	(0.05)
ΔDV, instr. countries	−0.04	−0.13	0.31***	−0.40*	0.05	−0.02
	(0.09)	(0.12)	(0.11)	(0.22)	(0.11)	(0.09)
Inflation	−0.15***	−0.11***	0.01	−0.04*	−0.06**	−0.08***
	(0.03)	(0.03)	(0.04)	(0.03)	(0.02)	(0.03)
Output gap	0.03	0.07	−0.02	0.05	0.00	0.00
	(0.07)	(0.06)	(0.02)	(0.04)	(0.04)	(0.04)
Adj. net lending	−0.05	−0.03	−0.02	−0.01	−0.02	−0.04***
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Unemployment	−0.08*	−0.01	−0.07***	0.01	−0.06**	−0.05*
	(0.05)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
Working age population	−0.05	−0.01	−0.09	0.05	−0.01	0.05
	(0.08)	(0.05)	(0.06)	(0.04)	(0.04)	(0.06)
Election	0.17***	0.10*	0.04	0.06	0.03	0.09**
	(0.06)	(0.06)	(0.04)	(0.04)	(0.03)	(0.04)
Right-wing	0.00	−0.05	0.11	−0.00	0.07	−0.02
	(0.07)	(0.05)	(0.08)	(0.03)	(0.06)	(0.05)
Year	−0.03***	−0.03***	0.00	−0.02***	−0.01	−0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Constant	66.79***	54.04***	3.80	39.75***	13.95	22.08*
	(19.43)	(13.86)	(25.00)	(11.03)	(13.19)	(12.92)
Dep. variable	Total	Cash	In kind	Unempl.	Health	Other
Estimator	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Country dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	No	No	No	No	No	No
Observations	350	350	350	302	303	292
First stage F statistic	9.44	10.17	11.31	4.42	5.72	6.94

Dependent variable: Public social expenditure as a percentage of GDP. From left to right: total expenditure, cash benefits, benefits in kind, expenditure related to unemployment, expenditure related to health and expenditure related to other areas. Country-clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

affected by growth. The large effects on cash benefits and expenditure related to unemployment, as well as the large effects on social spending compared to taxes, probably reflect the fact that automatic stabilizers reduce spending when the economy grows. It is also worth noting that I only find policy diffusion on benefits in kind. This would be expected if the variation in cash benefits primarily consisted of variation in the number of beneficiaries, and not policy decisions. The smaller sample used for the last three regressions resulted in a weaker instrument, as illustrated by the small F statistics. The results should therefore be interpreted carefully.

Robustness

In this section I address two specific concerns. The first concern is whether the endogenous growth variable affects the instrument. If tax policy (or social spending) affects growth, and the growth rate in turn affects growth in the countries used to create the instrument, the estimated effects of growth on tax policy would suffer from bias caused by reverse causation. Because most economies are much smaller than the sum of the economies used to create the instrument, this effect is probably small compared to the effect that the instrument exercises over the endogenous variable. On average, the total GDP of the countries used to create the instrument is six times larger than that of the country for the endogenous variable. Besides, a large part of the correlation between the countries consists of common shocks which are exogenous to implemented policy.

However, for the largest countries in the sample, the size of their economy often equals the size of the economies used to create the instrument. In these cases, the bias caused by reverse causation (tax policy affects growth which in turn affects the growth in the countries used for the instrument) could be substantial. To analyse whether this bias has had any major impact on the results, I create a variable which is defined as a country's GDP over the GDP in the countries used to create the instrument. I then allow for an interaction-effect between this measure of relative GDP and the growth rate. If there is a bias caused by reverse causation, and this bias is larger for large economies, the interaction effect will have the same sign as the bias. The effect of the growth variable can then be interpreted as the estimated effect when the bias is small (if the bias is linear to relative GDP, it can be interpreted as the effect in the absence of bias).

The results from these regressions are presented in the first two columns of Table 4.3. The first column uses the tax ratio as the dependent variable, while the second column uses public social expenditure as a share of GDP. Fortunately, the coefficient for the interaction term is close to zero. In fact, it is slightly positive, indicating that the bias from reverse causation might make the effects appear smaller than they actually are. Similarly, if I, instead of including the interaction-term, excluded the largest economies – measured as GDP relative to GDP in the countries used for the instrument – the estimated effect would become larger. The more observations I removed, the larger the effect would become. In other words, both these methods show that the estimated effects are larger for countries where the endogenous growth variable can be expected to exert little influence over its instrument. Similar to when we went from OLS regressions to the IV approach in Table 4.1, there is nothing to suggest that reverse causation induces a *negative* bias in the estimates of how growth

Table 4.3: Robustness checks

	(1)	(2)	(3)	(4)
Growth	−0.14*	−0.32***	−0.12***	−0.22***
	(0.08)	(0.10)	(0.03)	(0.05)
Tax ratio (t−1)	0.74***		0.73***	
	(0.04)		(0.05)	
Social spending (t−1)		0.88***		0.86***
		(0.05)		(0.03)
ΔDV, instr. countries	0.29***	0.16	0.27***	0.33***
	(0.08)	(0.20)	(0.07)	(0.08)
Growth × Rel. GDP	0.09	0.18		
	(0.36)	(0.15)		
Relative GDP	−0.12	−0.48		
	(0.79)	(0.35)		
Year	−0.03**	−0.02**	−0.03**	−0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Inflation	−0.02	−0.13***	0.01	−0.07***
	(0.07)	(0.04)	(0.05)	(0.02)
Output gap	0.07	0.00	0.04	−0.03
	(0.06)	(0.07)	(0.04)	(0.02)
Adj. net lending	0.08***	−0.06*	0.09***	−0.06***
	(0.02)	(0.03)	(0.02)	(0.02)
Unemployment	0.10**	−0.08**	0.12***	−0.06**
	(0.04)	(0.04)	(0.03)	(0.02)
Working age population	−0.25***	−0.05	−0.28***	−0.14**
	(0.09)	(0.07)	(0.08)	(0.05)
Election	−0.06	0.16**	−0.02	0.14***
	(0.08)	(0.06)	(0.07)	(0.05)
Right-wing	−0.19	−0.00	−0.19	0.01
	(0.18)	(0.06)	(0.19)	(0.07)
Constant	87.75***	53.06***	94.94***	38.71*
	(24.78)	(19.98)	(29.69)	(20.53)
Dep. variable	TR	SS	TR	SS
Estimator	2SLS	2SLS	2SLS	2SLS
Country dummies	Yes	Yes	Yes	Yes
Year dummies	No	No	No	No
Observations	352	352	341	351

Dependent variable: Tax revenues (column 1 and 3) and public social expenditure (column 2 and 4) as per cent of GDP. Country-clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

affects the tax ratio.

The second concern is the Nickell (1981) bias which is introduced when a model combines fixed effects with a lag of the dependent variable. Most regressions in this essay average 17–18 observations per country, which means that OLS should perform relatively well (Katz and Beck 2009). I have therefore used OLS (and 2SLS) in my main specifications. To examine whether the results are robust to alternative specifications, the last two columns of Table 4.3 present the results from the popular Arellano-Bond estimator. The estimated coefficients are similar to before.

Additionally, when the lags of all the other variables are included as instruments, the precision of the growth effect increases and the effects in both column 3 and 4 are now statistically significant at the 99 per cent level.

We have now looked at a wide range of regression models, and it appears that the results are quite robust to different specifications. In short, economic growth reduces the size of the public sector, regardless of whether it is measured as tax revenues or public social spending.

Conclusion

The need for improved public finances is larger now than ever before. Different kinds of deindexation have been proposed as a 'less painful way of generating fiscal space' (Marcel 2014). This argument is probably correct. Obfuscation has strong support in the 'new politics' literature (Pierson 1996; Weaver 1986) and in the first essay in this thesis I find that voters only punish governments for fiscal consolidations when the growth rate is not fast enough to hide the spending cuts.

However, institutionalizing the obfuscation of spending cuts can be questioned from a democratic point of view. Not only does it hurt the input legitimacy, but this essay provides evidence that it also affects political outcomes in a direction unrelated to the actions and wishes of the people. Here I provide a new argument to an old discussion about how the size of the public sector relates to the preferences in the electorate (Downs 1960).

Institutions that strengthen fiscal discipline through constraints on the policy process are typically defended on the basis that they move policy closer to the electorate's actual preferences. However, that argument is only true when fiscal discipline is achieved through actions on both the spending and the revenue side. Institutions that 'bias' the size of the public sector can also be criticised for their distorting effects on policy outcomes. To solve this problem, one should consider whether the effects outlined in this essay can be counter-acted by medium term targets for the tax ratio or other kinds of fiscal institutions. Another possibility is to increase fiscal transparency with regard to how expenditures are deflated through wage increases and inflation. This would be preferable in terms of input legitimacy, but making cutbacks more transparent could also reduce the electoral incentives for governments to consolidate public finances. This, in turn, could entail worse outcomes compared to medium term targets for the tax ratio.

In addition to these dilemmas between input and output legitimacy, this essay also contributes to two other areas of political science. First,

many of the explanations for the *expansion* of the welfare state have proven to be less relevant if we want to understand how expanded welfare states develop. By showing that differences in economic growth to a large extent can account for the cross-national trends in how the size of the public sector has developed, this essay also makes an important contribution to the welfare state research. To expand on this insight, future research should attempt to calculate how responsive expenditures are to growth and inflation, and how this responsiveness differs between countries.

Second, the causes and consequences of economic growth lie at the heart of many debates in political science. In some specific contexts, it has been possible to create valid instruments for economic growth. For example, Miguel et al. (2004) present a natural experiment where rainfall is used as an instrument for economic growth in African countries. However, it is difficult to find similar instruments for advanced economies. In this essay I have suggested a 'middle-ground' between the naive OLS regressions and natural experiments. For any relatively open economy, we can construct a strong instrument by regressing the economic growth on the growth rate in other countries. By controlling for the change in the tax ratio in the countries used to create the instrument, and ensuring that the results are not driven by large economies that are likely to influence the growth rate in other countries, the instrument allows for relatively strong causal claims. There are many fields of research which could benefit from the use of this or similar instruments.

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