

# Welfare State Retrenchment, Redistribution Strategies, and Credible Commitments

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## Abstract

It is common for political scientists to investigate the degree to which partisanship affects public policy. Less common is consideration of the effect of parties maximising policy preferences *through time*. In this paper, I propose a correction to the “new politics of the welfare state” thesis (Pierson, 1996) in which the mode of redistribution has implications for the degree to which it can be ‘rolled back’ by a future government. Left-wing parties are seen as acting strategically to maximise the present value of a stream of future policy pay-offs. Ultimately, they are influenced by the probability of a future government reversing their policies. Data from 20 OECD countries for the period 1990–2003 supports the claim that left-wing parties spend on cash transfers when they expect to hold veto power in the future, but welfare services otherwise.

## 1 Introduction

It is common for political scientists to investigate the degree to which partisanship affects public policy. Less common is consideration of the effect of parties maximising policy preferences *through time*. In this paper, I propose a correction to the ‘new politics of the welfare state’ thesis Pierson (1996) in which the mode of redistribution has implications for the degree to which it can be ‘rolled back’ by a future government. Based on a distinction between cash transfers and in-kind services in the welfare state, I build and test an argument in which left-wing parties are seen as acting strategically to maximise the present value of a stream of future policy pay-offs. Ultimately, they are influenced by the probability of a future government reversing their policies.

This view of the policy-making process can be seen in the idea of ‘political uncertainty’ proposed by Moe (1990). Horn and Shepsle (1989) offer a similar standpoint in which protection of policy gains is taken to be an inherent part of the political process.

However, these earlier papers were largely concerned with US congressional politics. While Moe (1990, 238-248) provides some theoretical analysis for how the impact of ‘political uncertainty’ will vary from presidential to parliamentary systems, there is generally a dearth of empirically-grounded comparative work that applies the theoretical insight. Furthermore, these authors pay no attention to how their theories will interact with partisanship, and thus they fail to draw conclusions about why parties of similar ideological origin will pursue different policies across countries.

In addition to the effect of ‘political uncertainty’, the division of redistributive expenditure into different modes leads to a contrast between the relative ease with which those modes can be pursued by a government. In this area, I make two core claims.

First, that (means-tested) cash transfers are both far more easily increased and far more easily cut than is the provision of public services. This stems from the differing structure and organisation of interests — particularly with respect to public sector unions — that underlies these two modes of redistribution. Second, I claim that cash transfers are, per unit of expenditure, a more efficient mode of redistribution than public services. The justifications being based both on the simple microeconomic theory of consumption and that it is easier to ensure such expenditure goes predominantly to the poorer sections of society.

Building from these two claims, it follows that a government seeking redistribution will use the prevailing ‘political uncertainty’ that it faces to determine which mode of redistribution will be the most effective. Where governments are more fearful of future (attempted) roll-back of their redistributive policies, they will be more likely to choose public service provision for its in-built political protections. By contrast, where such governments have less fear of future roll-back, they will be free to concentrate on the more effective option of cash transfers.

Analysing the development of the welfare state across OECD countries from 1990 to 2003, this paper seeks to rectify the relative absence of comparative analysis regarding the effects of ‘political uncertainty’. In doing so, it provides a more nuanced understanding of the effect of partisanship, and thus escapes the more simplistic ‘left means more, right means less’ hypotheses.

## **2 The Literature**

### **2.1 Partisanship and the Welfare State**

Building on the seminal work by Hibbs (1977), the broad political science literature is replete with quantitative empirical studies analysing the impact that partisanship has on policy outputs. A common approach for studying effects of partisanship is to model the size of various forms of government expenditure. This has been attempted both within (e.g Blais et al., 1993a; Pickup, 2006; Potrafke, 2006), and across countries (e.g Cameron, 1978; Castles, 1989; Hicks and Swank, 1992; Blais et al., 1993b, 1996; Schmidt, 1996; Cusack, 1997; Huber and Stephens, 2001; Franzese, 2002; Swank, 2002; Allan and Scruggs, 2004).

Despite this voluminous literature regarding partisan effects on policy outputs, until relatively recently, there has been a tendency to ignore the context in which parties operate. Earlier researchers have implicitly assumed that left-wing parties seek the same policies in any political environment. This assumption is exhibited in the plethora of time-series cross-section (TSCS) studies, pooling across OECD countries, that model either welfare expenditure (e.g. Pampel and Williamson, 1988; Hicks and Swank, 1992; Huber and Stephens, 2001; Iversen, 2001; Franzese, 2002; Swank, 2002) or total government expenditure (e.g. Blais et al., 1993b, 1996; Garrett, 1998; Huber and Stephens, 2001).

Furthermore, the political science literature cited above has also had strong tendency to search for the effect of partisanship on the chosen dependent variable, but to give little account to the idea that different types of welfare expenditure can have different redistributive effects — and therefore have different political dynamics associated with them (although see Rosenberry, 1982). To be sure, social policy scholars have been far less remiss in this respect, but this literature, in turn, suffers from a failure to explicitly address and theorise the political underpinnings of its findings. Research in this social policy tradition has tended to employ a kind of policy analysis approach, whereby the effects of different types of social policy are evaluated with respect to criteria such as redistribution or poverty alleviation (e.g. Sainsbury and Morissens, 2002; Hölsch and Kraus, 2004, 2006), rather than explaining how the policies themselves arose. The critique of ‘apoliticism’ can also be applied to research that falls closer to the political science literature in studying (in great detail) welfare state typologies (Esping-Andersen, 1989; Gough et al., 1997, e.g.). Finally, in a recent study, Nelson (2007) does come close to the kind of comparison of the development of different types of social expenditure set out here. However, while he considers how political factors may be relevant in modelling expenditure developments, theoretical explanation of the differing dynamics is not attempted.

Despite frequently ignoring the the theoretical significance of distinctions between types of welfare expenditure, some political scientists have, nonetheless, emphasised the importance of context in mediating political decisions.

Alvarez et al. (1991, 539)<sup>1</sup> provide theory and evidence to suggest that the presence of coordinated and centralised union movements makes it possible for more successful economic outcomes from left-wing government, while the absence of such unions privileges government by the Right in the same sense. However, their analysis of the

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<sup>1</sup>Amended by Beck et al. (1993).

“strategic interaction of governments, labor, and capital” is limited to macroeconomic outcomes.

Garrett (1998, Chapter 4) investigates how and why left-wing and right-wing parties react differently to the onset of globalisation. Across several types of government expenditure measure as dependent variables, he finds positive interactions between “left-labor power” and both capital mobility and trade. His theoretical position, though, is one in which the prevailing politico-economic environment is only seen to temper or enhance partisan differences; not to influence chosen modes of redistribution.

Golder (2004, Chapter 7)<sup>2</sup> emphasises that institutional constraints may be important in the way partisan differences manifest themselves in policy outputs. With changes in government debt as the dependent variable, he tests whether the “centripetal and centrifugal incentives in electoral systems” proposed by Cox (1990) cause partisan effects to grow larger with district magnitude. While the results are directly opposed to those hypothesised regarding the direction of partisan effects, they *do* appear to show that the institutional context provided by the electoral system is an important mediating factor.

In a different line of research, Rueda (2005, 2007) argues that the division between ‘insider’ and ‘outsider’ labour can have important consequences for the kinds of policies pursued by left-wing parties, which, in turn, can have important consequences for inequality. This ‘insider–outsider’ distinction can lead to differing political incentives depending on the nature of the labour market within which each group operates. So, with respect to labour market policies, he argues that left-wing parties have changed the policies that they favour as ‘insiders’ have come to feel less secure in their employment, and so to share policy preferences with ‘outsiders’ (Rueda, 2006).

Beramendi and Rueda (2007) also use institutional context as an intervening factor

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<sup>2</sup>See also Clark et al. (2002).

affecting partisanship. They seek to explain why highly redistributive welfare states are often disproportionately funded by regressive consumption taxes. Their claim is that left-wing parties operating in a “high corporatist” environment have commitments both to welfare expenditure (on the union side) and non-taxation of profits (on the business side). This leaves indirect taxes as the only way to finance the welfare state. By contrast, “low corporatist” environments do not engender these constraining agreements, leaving left-wing governments free *not* to use regressive taxation measures to finance their preferred expenditure. Once again, the evidence suggests that parties do indeed adjust their policies based on the institutional context in which they find themselves.

Finally, Kwon and Pontusson (2008) have recently set out to take time more seriously in the study of partisan effects on welfare states. Their approach is to estimate a series of models for different time “windows”, and then to retrieve the partisanship coefficient estimates and plot them through time. While this yields interesting results, the technique has little power to *explain* the revealed variations in partisanship over the period. As with other authors referenced here, they also test partisanship interaction effects, but these are largely confined to economic variables, and most results are unconvincing at conventional statistical significance levels.<sup>3</sup>

## 2.2 Institutions

Quite separate from the partisanship literature, in the main, another literature focusing on the comparative influence of institutions of a constitutional nature has developed — largely within the economics discipline. The most relevant work focused on how different electoral systems — including the contrast between parliamentary and presidential democracies — act as determinants of varying government expenditure across countries

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<sup>3</sup>They use “economic growth”, “trade openness”, “capital liberalization”, and “union density”.

(Persson et al., 1997, 2000; Lizzeri and Persico, 2001; Persson and Tabellini, 1999, 2002, 2005).<sup>4</sup> Summarising briefly, theory and evidence pointed towards presidential systems having lower levels of government expenditure than parliamentary systems due to the latter having greater incentives for politicians to provide public goods rather than targeted ‘pork’. A similar, although empirically weaker, effect is found in the contrast between proportional and majoritarian electoral systems.

A more recent branch of the economics literature has focused on the political economy implications of commitment mechanisms for the nature and structure of public sectors. Acemoglu and Robinson (2001) provide a model showing that an absence of credible commitment mechanisms will lead governments to adopt policies that increase the likelihood of them winning future elections — with resulting inefficiencies in public policy. The insight is rather similar to that expressed by Moe (1990) and Horn and Shepsle (1989), although this appears to have been unnoticed. Acemoglu (2003) sets out a less formal treatment of similar ideas and Acemoglu et al. (2006) extends them to the (historical) transition from limited to universal suffrage.

Most recently, research has been conducted on the specific ways in which institutions mediate partisan actions. Iversen and Soskice (2006) and Ticchi and Vindigni (2005) propose that proportional electoral systems favour left-wing parties — with the logical conclusions regarding government expenditure. Bawn and Rosenbluth (2006) and Persson et al. (2007) provide theory and evidence as to the effects of intra-coalition bargaining on the size of government. Their conclusions being that more parties lead to higher levels of expenditure.

This paper follows the lead offered by those authors who emphasise the importance of political context on partisanship. However, in contrast to those authors who have

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<sup>4</sup>A textbook treatment is given in Persson and Tabellini (2000).

emphasised policy constraints based on economics (Garrett), institutions in a direct sense (Golder), and interest groups (Beramendi and Rueda), the emphasis here is more explicitly party-political. The very incentives of parties are seen to be conditioned by the political environment — the party system and its associated institutional structures — in which they operate. To make this theoretical case, I draw on the insights of the more recent political economy literature regarding institutions — especially in the area of commitment mechanisms.

### **3 Varying ‘Stickiness’ of Welfare State Expenditure**

Before setting out my core theoretical claims, it is necessary to motivate the distinction between the two types of welfare expenditure that I emphasise. The distinction that I draw is similar to that suggested (almost 30 years ago) by Korpi (1980, 305) with respect to ‘universal’ as opposed to ‘marginal’ welfare policies. However, while Korpi’s dichotomy is useful, part of my claim is that it misses an important political difference between two components of welfare policy that are present, to different degrees, in both of his ideal types. Thus, in this section, I argue that we should draw a distinction between means-tested cash transfers and services in the welfare state. To support the importance of this distinction, I offer two arguments: one based on the relative abundance of labour in the latter, and the other based on how such labour tends to be highly unionised and, therefore, highly organised.

#### **3.1 The Relative Abundance of Labour in Welfare Services**

One of the central claims of the ‘new politics of the welfare state’ espoused by Pierson (1996) is that welfare state expenditure is extremely difficult to roll back. The ‘new politics’ claim is based on the idea that welfare policies are characterised by concen-

trated benefits and dispersed costs. This pattern makes it far more likely that those who benefit will organise to defend the *status quo* than that those who pay will organise to change it.

However, there are strong theoretical reasons to suggest that not all welfare expenditure is created equal. Pierson's 'new politics' ignores the fact that the structure of welfare expenditure creates different sorts of winners and losers.<sup>5</sup> Expenditure that redistributes via investment in public *services* will tend to have two distinctive characteristics. First, the provision of services is nearly always very labour intensive. In Baumol's words (Baumol, 1967, 416),

In some cases labor is primarily an instrument — an incidental requisite for the attainment of the final product, while in other fields of endeavor, for all practical purposes the labor is itself the end product.

Services in general, and certainly public welfare services, fall into this latter category. Health care, education, care for the elderly, pre-school, and any number of other welfare services that are provided by the state all embody large amounts of labour. This is true in a way that is not so for simple cash transfers. To be sure, a bureaucracy is required to process cash transfers, but an *equivalent* bureaucracy is needed to administer the provision of public services. Education and health ministries are hardly noted for their small size. The point is that public services require labour *over and above* that for pure administration. I claim, then, that public services will embody a far larger amount of labour than will cash transfer systems.

Recent data for the UK bears out this claim when comparing government expenditure on social security and education. In 2001, social security expenditure was £106.3

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<sup>5</sup>Clayton and Pontusson (1998, 70) discern a similar omission, noting that, "By and large, the retrenchment literature tends to ignore the question of changes in the delivery of social services or, in other words, the question of how the public sector is organized."

billion<sup>6</sup> and expenditure by the Department for Education & Skills (DfES) was £40.9 billion (DfES, 2001, 9). By contrast, the Department of Social Security (DSS) employed a total of 80,000 staff (ONS, 2001, Table A) — including the Benefits, Child Support, and War Pensions agencies — while public sector education employment was estimated at 1.24 million (Hicks, 2005, Table 3). This latter figure includes non-school related employment. However, taking the figure for teachers alone, there were around 450,000 employed in the public sector (DfES, 2001, Table 2.5). The figure is more than five times the size of social security employment, which itself is constituted from expenditure at more than twice the level for education. Only including teachers in the comparison is also a large underestimate as there is a plethora of teaching-related and administrative employees that fall outside of the teaching profession but are directly relevant to the argument advanced here. The evidence, then, supports the view that services embody far larger amounts of labour than do cash transfer systems.

Furthermore, using Baumol’s terminology, the labour administering a cash transfer system is “an incidental requisite for the attainment of the final product”, while that involved in the provision of services “is itself the end product”. Not only does the logic underpinning Baumol’s “cost disease” suggest that productivity improvements will tend to diminish the requirement for labour of the former kind, but the political support that such a group is likely to receive from the general public is minimal. Rightly or wrongly, people campaign for more nurses, not more bureaucrats.

### **3.2 The Organisation of Public Sector Labour**

The first point that services embody a far larger portion of labour than do cash transfers is strengthened by a second consideration. That is, that public sector workers are

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<sup>6</sup>Source: Office for National Statistics, ‘Social security benefit expenditure: by recipient group, 2001/02: Social Trends 33’. N.B. This figure is actually for Great Britain (i.e. excluding Northern Ireland), but this makes little difference to the comparisons here.

particularly likely to be unionised.

Freeman (1986) passed an early commentary on how “unionism comes to the public sector”. He noted the rise of public sector unionism in the USA over the post war era to the early 1980s. Also focusing on the USA, Farber (2005) presents data suggesting that since the mid 1970s, when both public and private sector union density stood at around 25%, the two sectors have diverged dramatically. By 2004, the private sector figure had shrunk to only around 8% while the public sector figure had swollen to around 35%. Draper (2000) presents evidence that this trend is far from particular to the USA. His data shows that across 12 OECD countries, while aggregate union density figures fell in most countries between the 1970s and the 1990s, the proportion of union membership being composed of public sector workers has risen — markedly so in several cases. Taking a snapshot of public and private union density figures for a similar selection of countries at the end of the 1990s, Blanchflower (2007, 6) shows that the difference between the two figures was of the order of 30 percentage points for most countries, and notably more for several. The evidence suggests, then, that while private sector unionism has been on the decline across most of the OECD countries (Ebbinghaus and Visser, 1999; Visser, 2006), public sector unionism has, in both relative and absolute terms, been thriving. Garrett and Way (1999) also note this tendency — going on to draw conclusions as to the potentially pernicious consequences for the macroeconomic health of countries operating corporatist arrangements in which public sector unions come to dominate.

The provision of public services, then, implies the presence of large bodies of highly organised labour — labour that will have a strong interest in maintaining or increasing funding for the services it provides. This is in contrast to those cases where there are large bureaucracies engaged in the distribution of means-tested cash transfers. In the transfer case, the bureaucracy has little to gain from forcing increases in welfare

payments as its level of wages and employment requirements are unlikely to be directly related to this aspect of the policy. Processing payments of £100 will be essentially the same as processing payments of £120.

### 3.3 Implications

Based on the preceding discussion, redistribution of the welfare services sort should, therefore, be protected by the pattern of concentrated and dispersed interests underlying the ‘new politics’ thesis to an even *greater* extent than systems of means-tested cash transfers.

To clarify, the claim is not that the ‘new politics’ thesis does not apply to means-tested cash transfers. They clearly fit the standard description of concentrated benefits and dispersed costs, with taxes collected from a large portion of a population and then (re)distributed to a subset of that population in the form of cash. The claim here is that there is a *difference* in the ‘stickiness’ of this cash and service expenditure. It is this difference that my theoretical claims rely on below.

With that said, however, it is possible to question the relevance at all of the ‘new politics’ thesis to cash transfers. Indeed, Clayton and Pontusson (1998) have produced evidence questioning the empirical accuracy of Pierson’s claims. In a world of redistributive politics in which resources flow from the rich to the poor, there must be some debate as to whether those that fund this redistribution really are a dispersed group. One may suspect that they will actually be a rather well-organised grouping represented by a right-wing party. If this were the case, then roll-back of welfare state expenditure in the form of cash transfers would be considerably more likely than the ‘new politics’ acknowledges. Quite simply, the election of a right-wing government should suffice. Following the same logic as Korpi (1980) regarding the numbers of beneficiaries of policies, we might expect this argument to be even stronger for means-

tested cash transfers as they have a smaller constituency of beneficiaries; and one that is less likely to overlap with right-wing party voters.

By contrast, a service-based form of redistribution is likely to be considerably less exposed to such partisan treatment as portions of the right-wing (rich) constituency — civil servants, teachers, doctors, etc — will themselves be direct beneficiaries of those institutions. Furthermore, in the defence of their position, those same groups will have the ability to engage in industrial action likely to be detrimental to large portions of the rest of the right-wing constituency, thus increasing their leverage.

## 4 Strategic Policy-Seeking

The arguments advanced above allow for the development of a testable hypothesis regarding the ways in which (left-wing) parties might act strategically in pursuing their redistributive goals. To derive this hypothesis, I make several assumptions, each of which is discussed below.

### 4.1 The Future Matters

I first assume that parties care about the future. While they will almost certainly not have a ‘pure’ discount rate of 0 — implying that they value *all* future periods equally<sup>7</sup> — due to pressure for immediate results from the electorate, there is reason to believe that parties do value outcomes in the future. Investment in long term projects such as missile defence, nuclear power and the like are certainly suggestive that the relevant time horizon for political parties may not be as short as might first be thought. Indeed, more relatedly, Jacobs (2008) provides a comparative analysis of pension reforms in the USA and the UK that strongly emphasises long-run interests.

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<sup>7</sup>See Ponthiere (2003) for a short survey of the normative literature on ‘pure’ discounting of time.

## 4.2 Redistributive Effectiveness

One more factor should be relevant to the policy choice of a redistribution-seeking government: how big an effect will it achieve with each of cash or service redistribution? That is, which will give the bigger payoff in the current period? For three reasons, I argue that the payoff from cash redistribution will be larger than that for services (in the current period).

First, basic economic theory shows that the provision of benefits in-kind should be pareto-dominated by provision of cash benefits to the same value. The reason being that cash benefits could be used to purchase the same bundle of services that would have been offered or, *alternatively*, some other consumption bundle instead. In-kind provision is only of equal utility to a consumer if the provider is able to exactly determine the consumer's optimally desired level of service consumption. All other levels entail either an over- or an under-supply of the service. Obtaining sufficient information for a government to determine this optimal level of supply is likely to be extremely difficult; with the implication being that sub-optimal supply is to be expected. Further, while governments are likely to need to supply services of a fairly uniform level, consumers are likely to be heterogeneous in their preferences. Cash benefits would allow each consumer to optimise her consumption bundle, while in-kind benefits do not. One size does not fit all.

Second, the relative inefficiency of services as compared to cash can be justified by thinking about how redistribution through services can be increased. In the first instance, starting a service from scratch will tend to require investment in physical capital — schools, hospitals, etc, must be built. This takes time. More pertinently, this takes longer than simply increasing the value of the cash transfer. However, the argument can be applied even to the incremental build-up of existing services. As labour forms the core of the provision, the quality of labour inevitably has a large

bearing on the quality of the service. Improvement of labour quality should, then, be a logical avenue for making the services more redistributive. While increasing wages is the obvious way to achieve this goal in the long run (c.f. Nickell and Quintini, 2002), the effect is likely to be muted in the short run. There are limits to the number of more highly qualified individuals that can be attracted into, say, the teaching profession in any given year following a wage increase — not least because there will be (implicitly, less qualified) existing teachers in place.

Third, empirically, it appears that public services tend to be used to a large extent by the middle classes (Le Grand, 1982; Goodin and Le Grand, 1987). Although this may be for strategic reasons on the part of the Left (Esping-Andersen, 1985), nonetheless, this will tend to reduce the redistributive capacity of services. Rothstein (1998, Chapter 6) appears to contest this sort of reasoning — which is implicitly one of universal welfare states being less effective at redistribution than the selective variety. First, he shows analytically that universal welfare state policies — in the sense of all citizens receiving the same benefits — *do* entail redistribution as a result of those with higher incomes paying a greater proportion of taxes. Second, he uses Luxembourg Income Study data to show empirically that countries with more universal welfare states tend to be more generate greater redistribution.

However, it is possible to agree with both of his points and yet maintain the assumption that cash transfers are a more effective form of redistribution at a given point in time. It is trivial to use Rothstein's own setup (Rothstein, 1998, Table 6.2) to show that selective redistribution from the rich to the poor produces greater redistributive effect *for a given level of taxation and spending*. The latter condition is key, for Rothstein's second argument is, then, explained by the greater total welfare effort that tends to be found in those countries operating more universal welfare states.

For these reasons — consumption bundle rigidity, the difficulty in improving ser-

vices, and the tendency for services to be ‘consumed’ by the ‘non-poor’ — I assume that cash transfers offer a greater immediate payoff to a redistributively-minded government.

### 4.3 Veto Players

What do these dual distinctions relating to welfare stickiness and redistributive power yield in terms of testable hypotheses? Below, I argue that it is a left-wing party’s expectations about its future capacity to block welfare reforms that leads it to choose redistribution via cash or services.

Veto player analysis of the sort put forward by Tsebelis (2002) highlights the important influence of institutional structure on the possibilities for reform, and Tsebelis and Chang (2004) apply the veto player model to welfare expenditure. Similarly, in analysing welfare expenditure across countries, Swank (2002) emphasises the ways in which institutions preclude some reforms by providing more or less actors with veto powers. However, literature of this sort tends to concentrate on the constraints that institutional context place on the actions of government. The policy preferences of actors are either assumed or estimated, but unlike the position set out in this paper, institutions are not seen as *determinants* of those policy preferences.

As I have argued above, cash transfers are more efficient at redistributing income than welfare services. As such, left-wing parties and, to the extent that they wish to redistribute at all, right-wing parties should prefer to use cash transfers over services. Why might a left-wing party come to prefer services? Clearly, where they expect not to be in a position to block any future right-wing inspired reforms, they will come to fear that possibility more. In that case, the absence of a *legislative* left-wing check against the right makes it more desirable to construct welfare policies that have an *inherent* check against reform. That is, to redistribute through services.

Looked at from the reverse angle, to the extent that left-wing parties actually pursue services (as a defence against right-wing roll-back) in favour of cash transfers, there is notionally a Pareto gain available if both parties could credibly commit to the policy. That is, for a given level of service expenditure, the right-wing party would be better off if the same level of redistributive effect were achieved via cash transfers. Of course, in that case, the left-wing party would be indifferent. This point about the possibility for Pareto improvements in public policy has been made by Alesina (1988). My claim here is that the presence of more institutional checks and balances — veto players in the Tsebelis jargon — actually increases the likelihood of attaining these Pareto gains.

It is worth noting that this claim is in marked contrast to the traditional veto players literature associated with Tsebelis (e.g. Tsebelis, 1995, 1999, 2002; Tsebelis and Chang, 2004). In that body of work, the more veto players and the further apart their preferences are, the less likely is a policy reform.

My claim, then, is that a more nuanced understanding of the impact of veto points is necessary. I focus on how the presence of veto points may come to (partially) determine the *policy* preferences of actors. In this way, I share the sentiment expressed by Ganghof (2003, 10), who notes that, “exogenous shocks may also *change* VPs’ [veto players’] policy preferences by changing their beliefs about the mapping of policies onto outcomes”. Given some (quasi) exogenous shock — such as rising unemployment, greater trade openness, or any other such phenomenon — the presence of more veto players may actually *increase* the likelihood of achieving an optimal policy response. The reason being that more veto players make it more likely that the agreed upon policy will not be subject to subsequent reform in directions unwanted by parties to the original agreement.

It is now possible to analyse the decision-making of an incoming left-wing government. Given their (assumed) concern for the future, what is their optimum policy

choice and what will influence this choice? In an environment in which the Left expects not to be able to block reforms in future with high probability, I claim that they will use their current incumbency to lock in as much redistribution as possible. Despite the lower immediate payoff, that means investing in services as these will be relatively immune from the predations of anticipated future right-wing governments. By contrast, where the Left expects to be able to block unwanted reforms in the future with high probability, they have less to fear from right-wing policy reversals. Consequently, they choose to redistribute more through means-tested cash transfers as the payoff is larger — both in the current period (which is known) and in the future (which is expected).

Another way to think of this is that the decision over whether to spend on cash or services is effectively a decision over which mechanism of checks one wishes to rely upon. Choosing cash expenditure implies a relatively greater reliance on the checks institutionalised by the legislative process. Choosing service expenditure implies the creation of *new* checks in the form of organised public sector labour. Where a left-wing party is satisfied that the future level of legislative checks will be high enough to allow them to protect their preferred welfare policies, cash expenditure becomes more attractive due to its higher payoff. Where legislative checks are expected to be insufficient in the face of future right-wing governments, then recourse to union checks via service expenditure becomes more attractive.

## 5 Empirical Analysis

### 5.1 The Dependent Variables

The decision about how to approach the testing of the theory set out above is not a simple one. On the face of it, the theoretical position taken in this paper is one that makes claims about the *relative* amount of cash and in-kind welfare expenditure. No

explicit predictions about the magnitude of either expenditure are made, only about the way in which they will covary with the political process. As such, the relevant dependent variable to test the theoretical claim would appear to be the ratio of cash to in-kind expenditure.

However, such a measure is actually rather problematic.

An example is the easiest way to show this. Consider a case where a left-wing party has been out of power for a long period. If this period has coincided with a low prevalence of checks/veto-players, then my model suggests that cash transfers, which are more easily rolled-back, would be at a relatively low level. In this situation, we might expect the incoming left-wing party to, at least initially, invest in cash transfers in order to get them up to a reasonable level.<sup>8</sup> However, as set out above, my theoretical claim is that an incoming left-wing party, seeking to secure future redistribution, will choose to invest in services rather than cash transfers. Thus, the logic underlying my theory generates ambiguous predictions when the dependent variable is taken as a ratio of spending.

The same issue is present even when modelling the two expenditure types separately, but there is reason to think it will be more muted. This is because modelling them separately makes it possible to estimate the strategic expenditure shift in one measure — services in the example above — without it being swamped by an expenditure shift in the other. Thus, while the *estimated* strategic partisan expenditure effect will be biased downwards for one category (cash), it will be correctly estimated for the other (services). The direction of the bias is less clear where a ratio is employed — especially where one of the components of the ratio is expected to exhibit higher variance than the other component. Finally, it should be noted that this issue induces bias *against* the theory I seek to test, making any support found from the estimates all the more

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<sup>8</sup>There is an implicit assumption here that cash transfers and services are not *perfect* substitutes.

credible.

A further problem with using a ratio of the two dependent variables arises. As discussed below — in *Section 5.5.1* — it is natural to expect the lag structures of the explanatory variables to be different when modelling cash and service expenditure. The latter will almost certainly require a longer lead time between shifts in explanatory variables and shifts in service expenditure as it is a relatively slow process to negotiate wage changes, employment reductions, and also to hire more employees. Collapsing both the cash and service dependent variables into one would yield a requirement for a complicated lag structure — one that would very likely dramatically reduce the available degrees of freedom as each explanatory variable would require multiple lags.

Given these difficulties, it is apparent that it is necessary to model each expenditure category in levels. This makes it possible to explicitly model the case described above where low levels of cash spending need to be recovered, albeit in an environment in which left-wing parties will tend to prefer services. It also avoids the difficulty of modelling complicated lag structures. For these reasons, I estimate separate models for each of my proxies for cash and service expenditure. Thus, I have two dependent variables,  $ExpCash_{i,t}$  and  $ExpServ_{i,t}$

The two dependent variables are sourced separately. The data used to construct cash spending effort is taken from Nelson (2007), which provides data on means-tested benefits for different types of individual<sup>9</sup> earning an ‘average production worker’s wage’. It covers old-age pensions, unemployment insurance, and sickness insurance. Data is available for representative lone parents, families, and singles, so results are presented below for each of these measures. This data is available for 22 countries<sup>10</sup> for the period

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<sup>9</sup>Specifically, these are a single person, a lone parent, and a two-parent two-child family.

<sup>10</sup>Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Sweden, Switzerland, the United Kingdom, the United States, Spain, Portugal, Greece, and the Czech Republic. I exclude the Czech Republic due to other data constraints and a desire to stay with the more standard sample associated with research into ‘developed democracies’.

1990–2005, although there is some missing data for Portugal.<sup>11</sup> The actual variable employed is the level of means-tested benefits available to each of the three types of recipient as a percentage of gross national income (GNI) per capita (*ExpCash*).<sup>1213</sup> In this way, the relative generosity of each means-tested benefit system can easily be compared across countries. Low ratios indicate instances where means-tested benefits fail to raise the income of recipients to somewhere close to the mean level of income in the country. Such a situation is taken to indicate that those benefits are doing less to counter inequality than if the ratio were higher.

Data for *ExpServ* is taken from the OECD Detailed General Government Accounts. Specifically, I add together three variables capturing the level of expenditure on welfare *wages* in different sectors: ‘social protection’, ‘housing’, and ‘health’.<sup>14</sup> This sum is then divided by total government expenditure, thus giving ‘social wages’ as a percentage of total government expenditure as my proxy for service expenditure, *ExpServ*. This variable was selected for two reasons. First, by focusing specifically on wages, the variable stays closer to the underlying theoretical stance advanced here regarding the importance of large bodies of (organised) public sector labour to the strategic decisions of parties. Second, by dividing by total government expenditure, rather than, say, GDP, the variable remains closer to the theoretical predictions regarding relative expenditure size rather than absolute expenditure size noted above.

At this point, it should be noted that the two proxies for cash and service expenditure are, at one level, not commensurate. The proxy for *ExpCash* effectively captures welfare effort *for a given need*, while the proxy for *ExpServ* captures welfare effort on a

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<sup>11</sup>See *Table 3*, discussed below.

<sup>12</sup>Specifically, I denote the variables  $ExpCash^{LP}$ ,  $ExpCash^{Fa}$ , and  $ExpCash^{Si}$ , corresponding to lone parent, family, and single person benefits, respectively.

<sup>13</sup>The GNI data is taken from the *World Development Indicators* (WDI) produced by *The World Bank*. I use the purchasing power parity (PPP) measures from Nelson (2007) and the WDI.

<sup>14</sup>To be precise, “Total compensation of employees paid by the government: Social protection”, “Total compensation of employees paid by the government: Housing”, and “Total compensation of employees paid by the government: Health”.

more aggregated, less restricted, measure. Despite these differences, each does provide a reasonable proxy for the underlying theoretical concepts at issue — especially given the availability of data. The means-tested benefits data provides exceptionally detailed and precise measures of a variable that maps very closely onto the logic expressed above regarding cash expenditure. The appropriateness of the measure stems from the fact that it reflects *individual level* income — where the individual is a prime example of the type of person that left-wing parties are assumed to want to redistribute to. That there is no equivalent measure for service expenditure actually accords with the theoretical position set forth here. I claim that service expenditure, by its nature, tends to benefit very large sections of society so it is unsurprising that data on the value of individual-level consumption of these services is not available. Thus, I maintain that the two types of dependent variable employed here are good proxies for the conceptual variables implied by my theory.

*Tables 2* and *3* provide summaries of the dependent variables.<sup>15</sup> In particular, *Table 3* shows that there are differing trends and differing levels in both of the variables across countries. Commensurate with the data being for the retrenchment period, cash transfers show signs of decline in several countries — most notably in Canada, Finland, Ireland, and Sweden. The very difference in these cash-retrenching countries suggests that the explanation for such changes must go beyond the ‘liberalisation’ of ‘social democratic welfare states’. Meanwhile, other countries hold cash payments broadly steady. The data for *ExpServ* make plain that the Nordic countries operate notably higher levels of welfare services than do the other countries in the sample. Several countries exhibit increases in *ExpServ* across the period (e.g. Denmark, Greece, and Portugal), while others maintain a fairly consistent level (e.g. France, Germany, and the Netherlands). The data for Austria suggest that service retrenchment there was

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<sup>15</sup>For *Table 2*, the data for *ExpCash<sup>Fa</sup>* is shown, rather than the others as it plays a more prominent role in the ensuing analysis.

the deepest in the sample. Notably, this did not appear to coincide with such deep cuts in cash transfers.

[Table 2 about here.]

[Table 3 about here.]

## 5.2 Core Explanatory Variables

As a partisanship variable, I use the proportion of cabinet seats held by a left-wing party for a given country-year,  $Left_{i,t}$ . This is interacted with a proxy for expectations regarding the future level of veto power in the political system. I denote this variable  $ExpectedChecks_{i,t}$  — where the  $t$  subscript denotes the expectation about the future held at time  $t$ . If the theoretical claim in this paper is correct, we should expect to see this interaction effect work (statistically significant and) opposite directions for the two different dependent variables. In the cash transfer models, the expectation is that the interaction effect is positive, indicating that a higher level of  $ExpectedChecks_{i,t}$  leads left-wing governments to invest more in cash transfers. By contrast, the expectation is of a negative interaction effect in the welfare service models, thus providing evidence that the presence of more checks in a system leads left-wing governments to feel less need to embed their redistributive welfare policies within a structure of organised labour.

Clearly, this setup makes it necessary to derive a proxy for that estimation of future checks ( $ExpectedChecks_{i,t}$ ). I construct this variable from the  $Checks$  variable provided by Keefer and Stasavage (2003). This is a composite index capturing the number of checks on government in a political system, and varies by country-year. The variable takes into account institutional factors, such as bicameralism and presidentialism, as well as partisan factors, such as the distance of governing coalition members from each

other (and non-coalition members) in left-right policy terms.

As a test of reliability, I also construct an alternative to *ExpectedChecks* using the veto player data set provided by Tsebelis. This yields a variable I denote *ExpectedVPs*. However, shorter time series and missing data for some countries reduce the sample size when using this variable by about half, which leads me to place far more weight on the results obtained with the *ExpectedChecks* variable.

With the underlying variables established, the next step is to use them to generate a proxy for *ExpectedChecks*.<sup>16</sup> I provide results that use an ‘adaptive expectations’ approach, discussed below.

### 5.2.1 Adaptive Expectations

The adaptive expectations approach is taken from the work of Cagan (1956) and Friedman (1957, 143). The idea is simply that current expectations are a weighted average of expectations in the previous period and the current observed value of the variable in question. Thus, adaptive expectations for the level of checks within a political system ( $Checks_{i,t}$ ) are formed in the following way:

$$ExpectedChecks_{i,t}^{Adapt,\gamma} = \gamma \cdot Checks_{i,t} + (1 - \gamma) \cdot ExpectedChecks_{i,t-1}^{Adapt,\gamma} . \quad (1)$$

Such an expectations mechanism has been largely discredited in the realms of economics from which it springs. The ‘rational expectations’ revolution initiated by Muth (1961) held that the simple adaptive expectations process assumes that actors are entirely backward-looking when thinking about what will happen in the future. The insight of Muth (1961, 315) was that,

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<sup>16</sup>For brevity, I exclude discussion of *ExpectedVPs* from now until the results section. The construction of the variable is exactly analogous to that for and *ExpectedChecks*.

In order to explain fairly simply how expectations are formed, we advance the hypothesis that they are essentially the same as the predictions of the relevant economic theory.

This is very well, but in the political realm, it is far from clear that we have robust enough theories to operate in the rational expectations paradigm. While there are reasonably established accounts of differences across countries, largely stemming from electoral system differences,<sup>17</sup> there is little theoretical consensus regarding how countries may develop through time. The prevalence of checks within a system is undoubtedly related to various institutional features, but these change relatively little through time. When we move to consider what Tsebelis terms “partisan veto players”, the situation becomes even more disputed as there are numerous theoretical accounts for why different parties perform better or worse electorally.

The power resources theory offers a hypothesis that left-wing electoral support will be stronger in the presence of strong unions. Iversen and Soskice (2006) theorise that proportional representation electoral systems will systematically favour left-wing parties, with majoritarian systems favouring right-wing parties. While empirical support for the former is fairly common, there has been little empirical work on the Iversen and Soskice theory. Beyond this, efforts to derive theories regarding the long-term, systematic, electoral strength of the Left relative to the Right have been few and far between. A combination of theories positing stronger economic performance under left- or right-wing governments under particular structural conditions (Alvarez et al., 1991) together with a theory of economic voting (Lewis-Beck and Stegmaier, 2003) offers a possible theoretical logic for relative electoral success. However, such a combination theory has had no direct empirical testing.

Even if we, as political scientists, did have strong theories regarding the relative

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<sup>17</sup>e.g. The ‘consensus’ and ‘majoritarian’ distinction associated with Lijphart (1999).

electoral strength of Left and Right, it would be necessary to confirm that they were indeed shared by political actors in order for them to be valid expectations-forming processes. In the absence of consensus within the academy, then, ‘rational expectations’ (in Muth’s sense) over future electoral strength and, by extension, future checks within a political system, would seem to hold little empirical validity.

If rational expectations are not appropriate, why are adaptive expectations? I use them because they provide for actors to update their beliefs in a fairly simple way based on new information that they receive in each period. The parameter,  $\gamma$ , provides a way of varying the relative weight given to the new information.

In standard economic adaptive expectations models, it is often possible to estimate  $\gamma$  directly within empirical models. This is achieved by substituting the equivalent of (1) into the basic structural model, and applying the Koyck transformation (Gujarati, 1995, 596–597). Unfortunately, in structural models with a lagged dependent variable (as all of mine have), it is not possible to distinguish between the ‘partial adjustment’ process captured by the lagged dependent variable and the adaptive expectations mechanism (Waud, 1966, 1968).

For this reason, I adopt the less elegant empirical strategy of generating adaptive expectation series of the level of *Checks* in a country — denoted as  $ExpectedChecks_{i,t}^{Adapt,\gamma}$ . As can be seen from (1), a starting value is needed to begin the series, so I use the sample mean value of checks for each country ( $\overline{Checks}_i$ ). Of course, this may not be a valid starting point for the value of  $ExpectedChecks_{i,t}^{Adapt,\gamma}$ , potentially leading to biased results. However, my data for  $Checks_{i,t}$  starts in the mid-1970s, while that for my dependent variables start after 1990, so there are 15-20 years for the series to settle down.

Of course, the issue then is what is the appropriate value of  $\gamma$  to use. As my theoretical position is that politicians are making strategic decisions with an eye several

periods into the future, I contend that relatively low values are most appropriate for testing the theory. That is, expectations about future levels of checks adjust fairly slowly to new observations and are not overly sensitive to very short periods of fluctuations, perhaps as a result of cabinet or coalition instability around elections. For that reason, the results presented below are for models estimated with  $\gamma = 0.1$ . Unreported results from estimating alternative specifications with  $\gamma = 0.2$  and  $\gamma = 0.05$  confirm that the findings are robust to plausible alternative levels of  $\gamma$ .

*Table 1* presents a summary of the *ExpectedChecks*<sup>Adapt,0.1</sup> variable for each country at three points in time. It shows that, while the variable is fairly stable in several countries (e.g. Australia, Canada, and Greece), it also shows a fair degree of variation in others. Austria, Denmark, France, Ireland, Italy, Japan, and Portugal all have high values around 10% above their low values. To be sure, this variation is not dramatically large, but the presence of country fixed effects in the models estimated below biases *against* the finding of substantive effects from such a variable — implying that we do not suffer from the problem of being over-confident when drawing inferences from the results.

[Table 1 about here.]

### 5.3 ‘Control’ Variables

In addition to those explanatory variables used to test the theory proposed in this paper, the models also need control variables, of course.

#### 5.3.1 Common Controls

Unemployment ( $Unemp_{i,t}$ ) is an obvious control variable as it has a direct bearing on cash transfers (through unemployment benefits). It has also been suggested that gov-

ernments respond to unemployment by increasing public sector employment, making it directly relevant to the services models, as well.

Following the standard Wagner hypothesis regarding greater public expenditure becoming more desirable at higher levels of GDP, I include (the natural log of) GDP per capita ( $LogGDPPC_{i,t}$ ).

Public debt, measured as a percentage of GDP, is included as it represents a possible measure of budgetary constraint ( $PublicDebt_{i,t}$ ). Higher levels of debt are likely to be met with more austere spending patterns. For the same reason, I also include a measure of the prevailing government budget deficit ( $PublicDeficit_{i,t}$ ).

The 'openness' of the economy, defined as the sum of the value of imports and exports, divided by GDP, is also included ( $Openness_{i,t}$ ). This is in light of the theory and evidence from the likes of Cameron (1984), Katzenstein (1985), Rodrik (1998), and Garrett (1998) regarding the use of the welfare state as a 'compensation' mechanism for the higher risks stemming from globalization.

Following Iversen and Cusack (2000) and Iversen (2001), 'deindustrialization' is controlled for ( $Deind_{i,t}$ ). It is measured as the ratio of agricultural and industrial employment to the total labour force. The logic for its inclusion is that the process of deindustrialization witnessed by most 'developed democracies' is a source of risk for those who have been required to find alternative employment in new sectors of the economy. As they experience higher employment risk, we should expect their demand for social insurance to rise.

Union density is controlled for as it offers a measure of the strength of the union movement ( $UnionDensity_{i,t}$ ). A stronger union movement is likely to be able to lobby for increases in its preferred welfare provisions.<sup>18</sup>

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<sup>18</sup>I recognise the power resource theory associated with the likes of Korpi and Shalev (1979) and Stephens (1979) that claims union strength and left-wing electoral strength are causally related. If this is so, then these two variables would exhibit colinearity, and ensuing expanded standard errors of parameter estimates. For this present paper, I see no easy way out of this difficulty.

### 5.3.2 Cash Controls

Following Roubini and Sachs (1989) and Iversen and Cusack (2000), I also include a control variable for ‘unexpected GDP growth’, defined as, “the difference between actual growth in a given year and average growth in the previous three years” (Iversen and Cusack, 2000, 323). I construct this variable using GDP per capita, yielding ( $UnexpGrowth_{i,t}$ ). The logic being that any unexpected growth will lead cash transfers to fail to grow at the same rate as GDP (per capita), thus reducing my measures of  $Exp_{i,t}^{Cash}$ .

### 5.3.3 Services Controls

For the services models, I include a measure of the so-called ‘dependency ratio’, measured as the ratio of under 16s and over 65s to the working age population ( $DepRatio_{i,t}$ ). Both of the former groups are likely to require higher levels of public services via health and social care.<sup>19</sup>

## 5.4 The Samples

As noted above, the use of Tsebelis’ veto player data severely constrains the sample size. Simply substituting the *VPs* data for the *Checks* data reduces the number of observations from 273 (for 20 countries) to 139 (for 16 countries).<sup>20</sup> As such, the results for the *VPs* models are intended only as indicative robustness tests, rather than fully competing models.

While the *Checks*-derived proxies are preferred here, I do have a concern over the accuracy of the underlying *Checks* variable for France. Excluding France, for the period and country sample in question, the value of *Checks* ranges between 2 and 11,

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<sup>19</sup>And education, but for which I do not have data.

<sup>20</sup>For the *ExpCash* models. For the *ExpServ* models, the sample drops from 221 (for 20 countries) to 121 (for 16 countries).

with a mean of around 4.4. While the French data for the same period falls within this range, in the 5 years immediately prior to the start of the sample (1991), France is a clear outlier with a *Checks* measure reaching as high as 16. So, while the actual period under consideration appears to be less problematic, I nonetheless present results with France excluded to ensure that its inclusion is not driving results. To preempt the findings, this does not appear to be the case.

## 5.5 Modeling Strategies

In modeling the two (types of) dependent variable, I employ error correction models (ECMs). I do so as it allows for greater flexibility in the modeling of short- and long-run effects of the explanatory variables. However, due to their (effective) inclusion of a lagged dependent variable, they are subject of some criticism. Two critiques of the inclusion of lagged dependent variables in models of public expenditure are worthy of mention. Achen (2000, 14) shows that,

In the presence of heavy trending in exogenous variables and disturbances, lagged dependent variables will dominate the regression and destroy the effect of other variables whether they have any true causal power or not.

While this may be true, Achen's preferred modeling approach of excluding lagged dependent variables is also problematic, as he himself acknowledges, because (Achen, 2000, 11),

budgets may be out of equilibrium for certain periods due to startup friction, unforeseen economic or political contingencies, or just routine year-to-year adjustment costs.

This last point gets to the heart of the difficulty in adopting Achen's approach. At the core of the theoretical position set out in this paper lies the idea that budgets are,

albeit with variation by type, difficult items to change. Interest groups will work hard to defend and expand their share of the budget, making the previous year's outlay strongly correlated with the current year's outlay. Consequently, to drop the lagged dependent variable from empirical models is tantamount to jettisoning a core aspect of my theoretical claim. Without an alternative solution to the problem highlighted by Achen, I note that his work implies that models including a lagged dependent variable will be biased *against* finding significant effects from explanatory variables. Thus, if the models that I estimate do appear to show confirmatory results for those explanatory variables of relevance, this is *despite*, not because of problems in the estimation procedure in this respect.

Plümper et al. (2005) offer similar objections to Achen (2000). Their specific objection is that (Plümper et al., 2005, 338),

inclusion of the lagged dependent variable and/or time dummies leaves very little variance for the explanatory variables.

One obvious response to this is that it is far from obvious that a “lack of variance for the explanatory variables” is a bad thing. If the underlying data generating process really is one of partial adjustment and incremental budgeting, then there should not be much variance left to explain. While Plümper et al. (2005), like Achen, recognise that budgets may exhibit a considerable degree of persistence from one period to the next, they go on to assert that (Plümper et al., 2005, 335),

the interpretation of the lagged dependent variable as a measure of time persistency is misleading. [...] [T]he lagged dependent variable not only evaluates the level of persistency. It also models the dynamics of the independent variables [...]. In this regard, the lagged dependent variable implicitly assumes that the dynamics of all independent variables are iden-

tical. Needless to say, this assumption is not very convincing and almost certainly wrong.

They go on to note that the coefficient on the lagged dependent variable will be biased upwards if a researcher fails to correctly model persistent effects from an explanatory variable (Plümper et al., 2005, 339).

For all these reasons, their recommendation is to exclude lagged dependent variables and instead model the serially correlated error process in the error term directly — i.e. to employ a Prais-Winsten estimation procedure, or the like. However, notwithstanding the point that the lagged dependent variable is not *just* a measure for the persistence of budgets from year to year, the fact remains that it will capture *some* of that effect, and thus would still appear to be a better option for an applied researcher testing a theory that explicitly posits such a persistence.

For this reason, I opt for the approach of modeling dynamics through the lagged dependent variable, contra Achen (2000) and Plümper et al. (2005), but following Beck and Katz (1996) and Beck (2001). Again, this approach biases the research *against* finding significant effects from my explanatory variables.

Another modelling issue is whether to treat the two dependent variables as endogenous to each other — as mutually determining in some way. At root, such a model is implied by the theory set out above in that the two types of expenditure are held, partially at least, to be substitutes for each other: a given expenditure is decided upon and then a choice of how to allocate it between the two categories is made. For this reason, it makes theoretical sense to model the two types of expenditure with a system of simultaneous equations.

However, for practical reasons, this is rather difficult. First, estimating a system of equations requires that observations on the two dependent variables coincide: missing values from one variable for a given country-year precludes the use of the other depen-

dent variable for the same case. Unfortunately, the two dependent variable samples do not coincide as much as would be desirable. Estimating the models simultaneously cuts the cash sample by around 100 observations and the service sample by around 30, which is rather a high price to pay given the limited initial sample size. A second practical argument against the simultaneous equation model approach is that it precludes use of ‘panel-corrected standard errors’ which Beck and Katz (1995) show to be an important corrective to traditional estimates. For these reasons, I estimate the two types of models independently of each other.

### 5.5.1 Lag Structures

A note on the lag structures that I use is also in order. This differs across the cash and services models, so I discuss them in turn.

Fundamentally, I make the assumption that the effects of any agents on the level of  $ExpCash_{i,t}$  will be felt without a lag. That is, when a political party is in power in a given year, they are able to determine the level of means-tested cash benefits *in that same year*. For example, when the Labour Party came to power in the UK in May 1997, they were *able* to change the level of means-tested benefits paid out in the year of 1997. This assumption accords with Tsebelis and Chang (2004, 457), who claim that it is correct to “consider the current government responsible for the realization of the budget because, according to the literature, the current government has the means to alter the existing budget”.<sup>21</sup> Any difficulties with this assumption are further ameliorated by the fact that the  $Left_{i,t}$  variable that I employ from Armingeon et al. (2007) is weighted by the proportion of the year each party is in office. So, for the UK Labour Party example above, the  $Left_{i,t}$  variable is adjusted to reflect the fact that Labour did not come to power until May. Unreported results confirm that

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<sup>21</sup>However, Plümper et al. (2005) find that this varies across countries, so there remains a question mark over whether the assumption is truly valid in the pooled sample I use.

the conditional partisan effects presented below do indeed survive in a model with partisanship lagged by one period, albeit with lower statistical significance.

While I assume agency effects to be contemporaneous, I employ an extra lag for most of the control variables used in the model. The aim here is to escape the biases induced by endogeneity. Specifically, it would be reasonable to claim that increases in the level of means-tested cash benefits could have a positive and causal effect on any or all of unemployment levels, the public budget deficit, deindustrialization, and even the openness of the economy as benefits become relatively more attractive to potential recipients. In that way, higher benefits may reduce employment and/or work effort. To avoid this reverse causality, I therefore lag those variable by one period.

For the services models, I make the assumption that political agency effects are manifested with a one period lag. The reason being that the measure I use for  $ExpServ_{i,t}$  is derived from the level of spending on wages (as a proportion of total government expenditure). Spending on wages is a function of two factors: the number of employees and the level of pay for those employees. Certainly, it seems plausible to assert that decisions as to changes in employment levels are likely to take some time to work through for reasons of employment law and notice periods, if nothing else. As to the level of wages for employees, this tends to be bargained between unions and governments in advance, and so again warrants a lagged effect. While this assumption amounts to a rejection of the claim (noted above) made by Tsebelis and Chang (2004) as to contemporaneous effects, it nonetheless appears valid on logical grounds. Again, unreported results confirm that the conditional partisan effects presented below survive in a model with partisanship entering the models contemporaneously (as with the cash models), but again, the findings have lower statistical significance.

That there are reasons to believe that cash and service expenditure levels should be modeled with differing lag structures is of no surprise given the premises of this

paper. Tsebelis and Chang (2004) do not account for this as they do not identify the two expenditure types. Furthermore, the finding by Plümper et al. (2005) of varying lag structures across countries<sup>22</sup> could then be explained by the differing structure of welfare states (in terms of cash and services) across countries.

## 5.6 Estimation Issues

In estimating empirical models below, I employ country fixed effects so as to account for time-invariant differences across countries. This is a standard approach in the comparative political economy literature<sup>23</sup>, but it does introduce a potential problem when combined with the use of a lagged dependent variable (LDV). The so-called ‘Nickell bias’ stems from correlation between the LDV and the error term in the presence of fixed effects.

Nickell (1981) shows that the bias associated with this problem is of the order  $1/T$  and thus of diminishing significance as the number of time periods increases. The data I employ has a maximum  $T$  of 14, with many panels dropping to far smaller numbers of periods. The bias, then, is potentially a significant problem.

Following Wawro (2002), I estimate a series of models utilising the Generalised Method of Moments (GMM) procedure set out by Arellano and Bond (1991). The combination of first-differencing and instrumenting entailed in this method yields unbiased parameter estimates. In related Monte Carlo work, Beck and Katz (2004) provide some evidence that the benefits from more complicated estimation techniques of this sort may be out-weighed by their efficiency costs. Thus, following Bawn and Rosenbluth (2006), I present the results of estimating both GMM and the more usual ordinary least squares (OLS) models with fixed effects and LDV. As is now standard in

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<sup>22</sup>See footnote 21.

<sup>23</sup>Much of which is cited above.

the literature, these latter models employ the panel corrected standard errors (PCSEs) advocated by Beck and Katz (1995) in order to account for panel heteroskedasticity.

## 5.7 Results

The results of estimating the models described above are presented below. A series of OLS/PCSE and GMM models are shown, together with estimates that substitute Tsebelis' veto player variable for DPI's *Checks* as the main explanatory variable of interest. Where not otherwise indicated, models estimated by OLS/PCSE are reported with  $R^2$  values while those estimated by GMM are not. Unreported results of Lagrange Multiplier tests for residual autocorrelation indicate that the estimated models do not suffer from this problem once the lagged dependent variables are included.<sup>24</sup>

### 5.7.1 Cash Models

*Table 4* presents the results from estimating a series of OLS/PCSE models. *Models 1* to *3* are, respectively, for cash transfers to singles, lone parents, and families. *Model 4* re-estimates the families model with France excluded.<sup>25</sup> All models have accurately estimated and negative coefficients on the lagged (level of the) dependent variable; providing reassurance that the specifications do not imply an explosive data generating process.

On the specifics of the results, there is a consistent finding that higher levels of unemployment lead to a long-term decline in cash transfers to each of the three welfare groups. The short-run impact appears to be more discernible for singles and lone parents than for families. There is little evidence that the state of public finances, in the form of public deficits, has any impact on cash generosity.

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<sup>24</sup>The tests were carried out as described by Podestà (2006, Endnote 4) and the results are available from the author.

<sup>25</sup>See discussion in *Section 5.4*.

Unexpected GDP growth, surprisingly, offers little explanatory power and nor does actual GDP growth. Contra Wagner’s Law, higher levels of GDP appear to lead to relatively lower levels of cash transfers. The presence of country fixed effects means that this is a within-country rather than a between country finding. Openness and deindustrialisation both show no sign of a statistically significant effect.

Likewise, union density performs rather poorly as an explanatory variable. Signs change across models and even within models on the basis of short- and long-run effects. The negative result for the long-run effect on lone parent benefits is notable given the weakness of the other results. It could, perhaps, be argued that unions are unlikely to represent lone parents due to a lower probability of them being in employment as compared to singles and members of families. However, it seems wise to place little emphasis on this result.

On the core findings relating to the hypothesised partisanship effect, conditional on the level of *Checks*, the results are very encouraging. All models show an apparent statistically significant positive long-run interaction effect.<sup>26</sup>

The equivalent GMM-estimated models shown in *Table 5* reinforce the OLS/PCSE findings. There is more evidence of a negative effect from public deficits and the inverse-Wagner finding from GDP becomes even more robust. The negative effect from union density for lone parents evaporates. Finally, the partisanship interaction appears to remain fairly robust.

*Table 6* shows the results of re-estimating models with Tsebelis’ *VPs* variable substituted in for the *Checks* variable. This cuts the sample size almost in half so wider confidence intervals around parameter estimates should be expected. Nonetheless, the results are broadly commensurate with those from *Table 4*. Two notable contrasts are a weakening of the inverse-Wagner finding and an apparent negative effect from

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<sup>26</sup>Full conditional effects plots with confidence intervals are provided in *Section 5.8* below.

openness. The positive partisanship interaction with *VPs* largely remains, although the results are now stronger for the short-run than the long-run effect.

[Table 4 about here.]

[Table 5 about here.]

[Table 6 about here.]

### 5.7.2 Services Models

*Table 7* presents the results from estimating a series of models with *ExpServ* as the dependent variable. *Models 13* and *14* are OLS/PCSE and GMM estimations, respectively.<sup>27</sup> *Models 15* and *16* exclude France from the sample. Again, the coefficient on the lagged dependent variable suggests that the models are stable.

The results for unemployment suggest that it is negatively associated with social wages *as a proportion of total government expenditure*. The qualifier is important and, perhaps, explains why the findings for *ExpCash* indicate a negative effect as well. That is, the results together are not inconsistent with higher unemployment levels leading to greater aggregate cash benefits which crowd out social wages expenditure, even while cash benefits become of relatively lower value to each individual. Claimants may rise faster than benefit levels fall.

There is evidence that higher levels of public debt lower government expenditure on social wages, although this appears to disappear in the long-run. The Wagner effect, again, performs poorly with the coefficient on GDP levels largely negative; albeit without statistical significance. A negative short-run effect from openness is suggested by the OLS/PCSE models but this fades under GMM. Likewise, it is absent in the long-run coefficient.

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<sup>27</sup>N.B. The use of GMM is at the expense of Spain, for which data is not available for a long enough time period.

Surprisingly, the dependency ratio shows little sign of affecting *ExpServ*. Deindustrialisation, on the other hand, is a fairly robust predictor of the dependent variable. The results suggest that as jobs are lost in the industrial (and agricultural) sectors, they are replaced with public sector service jobs. It is not clear whether this is a governmental response to higher unemployment levels or a response to greater national income leading to a shift to a more service-oriented economy. The findings noted above for the direct effect of unemployment suggest that the latter interpretation is the more likely. Union density fails to find statistical significance as an explanatory variable. We may speculate that the preferences of different unions over the level of social wages may vary both within and across countries. Private sector unions may well see public sector wage growth as an inflationary threat (c.f. Garrett and Way, 1999).

With respect to the primary explanatory variables of interest here, the partisanship interaction shows a negative effect. Left-wing parties appear to give greater priority to social wages, but this effect diminishes as the level of expected checks in the system gets larger — just as the theoretical account provided above would suggest. There is a suggestion that these results are slightly weaker than those for the cash models, although the true conditional effect cannot be inferred directly from the coefficients and their individual statistical significance. Likewise, it appears that the GMM results are weaker than those from the OLS/PCSE estimation. I address this below.

Turning to *Table 8*, the results from estimations using Tsebelis' *VPs* variable in place of *Checks* are very encouraging. Again, it is worth noting that the change in variable has an effect on the sample size, with the  $N$  dropping by around a half — leading to sample sizes as low as 73. There is a consequent instability in some of the inferences that can be drawn for 'control' variables, but the findings for the interaction effects from partisanship are broadly in line with those from the *Checks* models.

As compared to *Table 7*, several changes are notable. Wagner's Law performs far

better with positive and statistically significant coefficients on the GDP level. The public deficit also finds statistical significance with higher deficits appearing to constrain the proportion of government expenditure that gets channeled to social wages. The evidence in support of higher dependency ratios leading to higher social wages also appears to be far stronger under these specifications. There are also less marked differences across the *Checks* and *VPs* models for public debt and openness.

The results from the partisanship interaction with the *VPs* variable are correctly signed, although it is notable that the GMM results are far stronger. Nonetheless, given the reduction in sample size entailed by the use of *VPs* (as well as the use of GMM), the results do broadly confirm the primary findings from *Table 7*.

[Table 7 about here.]

[Table 8 about here.]

## 5.8 Discussion

While the statistical significance on the partisan interaction terms for the various cash and service models are very encouraging in terms of support of the theoretical position advanced here, it is necessary to determine the true statistical significance of partisanship conditional on the level of *ExpectedChecks*. *Figures 1* and *2* depict these conditional effects for *ExpCash<sup>Fa</sup>* models estimated by OLS/PCSE and GMM, respectively. Likewise, *Figures 3* and *4* plot the conditional effects derived from the two estimation techniques for the *ExpServ* dependent variable. The plots are provided with 95% confidence intervals.

[Figure 1 about here.]

[Figure 2 about here.]

[Figure 3 about here.]

[Figure 4 about here.]

The figures are useful to highlight the very different effect that the level of expected checks has in a political system. As expected checks increase, left-wing parties are seen to have an increasing preference for cash transfers and a decreasing preference for redistributive public services.

With level of  $ExpectedChecks^{Adapt,0.1}$  varying between 2.5 (Portugal, 1997) and 8 (France, 1994) within the sample, the confidence bounds indicate that the partisanship effect at each extreme of  $ExpectedChecks^{Adapt,0.1}$  are different from each other at the 95% confidence level.<sup>28</sup> The figures provide further support for the theory set out above. Higher levels of expected checks in a political system lead to an increase in cash-based redistribution. This is in sharp contradiction to the predictions from the standard Tsebelis veto players model.

Substantively, the estimated partisanship effects are rather large. The presence of a 100% left-wing government in an environment with a low level of expected checks, for example Sweden with a value of around 3.5, leads to a reduction in the level of cash benefits to families of about 13 percentage points.<sup>29</sup> The same left-wing government coming to power with expected checks at a relatively high level, for example Ireland with a value of around 7, would increase cash benefits to families by about 14 percentage points.<sup>30</sup> These differential effects are remarkably large and caution must be exercised in interpreting them as there remains uncertainty in the estimated effects.

The findings for the service models are somewhat more muted. Nonetheless, they are policy-relevant. A 100% left-wing government with approximately Swedish levels of expected checks would raise the proportion of government expenditure on social wages

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<sup>28</sup>This drops to 90% for the *ExpServ* results from GMM estimation shown in *Figure 4*.

<sup>29</sup>See *Figure 1*:  $100 \times (-0.13) = -13$ .

<sup>30</sup>See *Figure 1*:  $100 \times 0.14 = 14$ .

by around 0.7 percentage points. The same government with Irish levels would lower social wages by about 1 percentage points.

One feature that emerges from the figures plotting conditional partisanship effects is that low (high) levels of expected checks actually leads to a negative partisanship effect on cash transfers (services). On the face of it, this may appear surprising. However, it is actually broadly in accordance with the theoretical argument advanced here. Taking the estimated conditional partisanship effects for both cash and services together, it becomes clear that the negative effects for one dependent variable correspond with positive effects for the other. Effectively, then, the conditional effects plots support the contention that the two types of expenditure are (at least partially) substitutes and that governments face a budget constraint when choosing what to spend on. Higher service expenditure will often come at the cost of lower cash transfers, and vice versa.

Other aspects of the results are also of interest. The findings for the cash models vary somewhat across the three different welfare groups for which we have data. There is some evidence that partisanship effects — conditional or averaged — are smaller for singles than they are for lone parents and families. Coefficients from both the OLS/PCSE and the GMM models suggest that the substantive size of the effect and the accuracy with which it is estimated is smaller for singles. This fits with a view of welfare whereby single adults are deemed to be less in need of state support as they should have fewer impediments to gainful employment and fewer ‘innocent’ victims — in the form of dependent children — of any perceived reticence to work.

## 6 Conclusions

The theory presented here has suggested that it is necessary to split welfare effort into two distinct components: cash transfers and in-kind services. The two have differing

redistributive effects and differing political consequences. Furthermore, it has been argued that left-wing parties are rational actors and choose an optimal bundle of cash and service redistribution so as to maximise their expected utility into future periods. Expectations of being able to veto changes in policy in the future make it less likely that right-wing parties will be able to (further) retrench cash-based redistribution. Consequently, left-wing parties will prefer this more efficient form of redistribution where veto points are numerous. By contrast, a low likelihood of future left-wing veto power will lead to a preference for service-based redistribution which is inherently more difficult to roll back. Left-wing parties effectively buy insurance at the cost of reduced efficiency.

The empirical evidence presented above provide support for this theory. Results are robust across different estimation techniques and alternative measures of expected checks in a political system. Further, the estimated effects are found to be substantively large.

The findings here challenge the prevailing ‘new politics’ view in which the welfare state is perceived as largely homogeneous. A look at the raw data presented in *Table 3* indicates that this view is overly simplistic. The patterns revealed there are largely of divergent trends across the two spending types. The results should also lead us to be sceptical of the recent theoretical work in the historical institutionalism school which argues that ‘history’ and ‘time’ are of great importance in explaining policy change, but that there is an inherent randomness to this process that means explanation by political scientists can only, at best, be post hoc (c.f. Pierson, 2004). In the alternative view presented here, time is again taken to be of crucial importance, but it is argued that we can posit rational, strategic, actors as operating within the sorts of processes outlined by the historical institutionalists. Those rational actors are political parties in the argument advanced here. In recent work by Jacobs (2008), they are interest

groups. It would appear likely that there are other such actors that we could usefully apply rational foresight to, as well as other policy areas.

As well as advancing our understanding of the dynamics of welfare states in the retrenchment period, the argument advanced here points towards the benefits of a research agenda that seeks to marry some of the empirical insights of the historical institutionalist school with the analytic insights of the rational choice institutionalist school. Such an agenda holds great promise to help explain many areas of public policy, not least the initial development of welfare states across countries.

Country	$ExpectedChecks^{Adapt,0.1}$		
	1990	1995	2000
Australia	4.20	4.12	4.26
Austria	4.34	4.61	4.67
Belgium	5.27	5.44	5.10
Canada	4.04	4.03	4.01
Denmark	6.34	6.17	6.51
Finland	4.56	4.74	4.23
France	7.56	7.47	6.46
Germany	4.92	4.64	4.60
Greece	3.01	3.09	3.05
Ireland	5.54	5.86	6.33
Italy	4.61	4.49	4.22
Japan	3.49	4.21	4.19
Netherlands	4.14	4.28	4.79
New Zealand	3.02	2.84	3.01
Norway	4.08	4.08	4.18
Portugal	2.74	2.44	2.57
Spain	3.59	3.95	4.04
Sweden	3.32	3.92	3.73
Switzerland	3.03	3.02	3.32
UK	4.05	4.03	3.75
USA	4.80	4.71	4.83

Table 1: Summary of the key ‘expected checks’ ( $ExpectedChecks^{Adapt,0.1}$ ) for each country in 1990, 1995, and 2000.

Table 4: Determinants of means-tested income support (as a percentage of GDP per capita) — OLS utilising  $Checks$  variable.

	(1)	(2)	(3)	(4)
	$\Delta ExpCash_{i,t}$	$\Delta ExpCash_{i,t}$	$\Delta ExpCash_{i,t}$	$\Delta ExpCash_{i,t}$
$ExpCash_{i,t-1}^{Si}$	-0.212*** (-3.44)			
$ExpCash_{i,t-1}^{LP}$		-0.186*** (-3.82)		
$ExpCash_{i,t-1}^{Fa}$			-0.166***	-0.166***

			(-2.88)	(-2.89)
$\Delta Unemp_{i,t-1}$	-0.326***	-0.345*	-0.368	-0.331
	(-2.67)	(-1.75)	(-1.52)	(-1.32)
$Unemp_{i,t-2}$	-0.278***	-0.434***	-0.541***	-0.561***
	(-3.89)	(-3.38)	(-3.30)	(-3.28)
$\Delta PublicDeficit_{i,t-1}$	-0.0946	-0.143	-0.231	-0.223
	(-1.24)	(-1.33)	(-1.50)	(-1.44)
$PublicDeficit_{i,t-2}$	-0.0202	-0.0157	-0.0325	-0.0411
	(-0.38)	(-0.20)	(-0.28)	(-0.35)
$UnexpGrowth_{i,t}$	-9.693	-15.42	-19.36	-19.98
	(-1.20)	(-1.19)	(-1.22)	(-1.26)
$\Delta LogGDPPC_{i,t}$	-8.791	-13.30	-14.06	-13.72
	(-0.97)	(-0.89)	(-0.78)	(-0.75)
$LogGDPPC_{i,t-1}$	-7.173**	-12.70***	-11.65*	-11.70*
	(-2.28)	(-2.59)	(-1.83)	(-1.85)
$\Delta Openness_{i,t-1}$	0.0106	-0.00220	-0.0250	-0.0234
	(0.48)	(-0.07)	(-0.61)	(-0.57)
$Openness_{i,t-2}$	-0.0198	-0.0245	-0.0423	-0.0343
	(-1.20)	(-1.06)	(-1.41)	(-1.10)
$\Delta Deind_{i,t-1}$	7.624	6.110	-3.862	-8.236
	(0.63)	(0.31)	(-0.16)	(-0.35)
$Deind_{i,t-2}$	4.974	7.791	3.996	3.999
	(0.89)	(0.70)	(0.33)	(0.29)
$\Delta UnionDensity_{i,t-1}$	0.0471	-0.0302	-0.000140	0.00694
	(0.50)	(-0.19)	(-0.00)	(0.03)
$UnionDensity_{i,t-2}$	0.0186	-0.0754*	-0.0272	-0.0240
	(0.70)	(-1.93)	(-0.45)	(-0.37)
$\Delta Left_{i,t}$	0.00296	-0.0363*	-0.0179	-0.0148
	(0.23)	(-1.82)	(-0.67)	(-0.42)
$Left_{i,t-1}$	-0.0203***	-0.0523***	-0.0596***	-0.0766***
	(-3.23)	(-4.87)	(-4.73)	(-4.90)

$\Delta ExpectedChecks_{i,t-1}^{Adapt,0.1}$	-1.174 (-1.18)	-2.426* (-1.72)	-4.067** (-2.17)	-3.457* (-1.66)
$ExpectedChecks_{i,t-1}^{Adapt,0.1}$	-0.308 (-0.97)	-0.537 (-1.18)	-0.480 (-0.82)	-1.068 (-0.96)
$\Delta(Left \cdot ExpectedChecks_{i,t}^{Adapt,0.1})$	-0.00105 (-0.48)	0.00547 (1.56)	0.00312 (0.69)	0.00216 (0.30)
$(Left \cdot ExpectedChecks_{i,t-1}^{Adapt,0.1})$	0.00358*** (3.03)	0.00925*** (4.47)	0.0115*** (4.98)	0.0155*** (4.28)
Fixed Effects	Yes	Yes	Yes	Yes
$R^2$	0.475	0.465	0.488	0.494
N	262	262	262	248
Countries	20	20	20	19

Notes: OLS/PCSE estimation. Z-statistics in parentheses. \*, \*\*, and \*\*\* denote p-values of 0.1, 0.05, and 0.01, respectively.

Table 5: Determinants of means-tested income support (as a percentage of GDP per capita) — GMM utilising *Checks* variable.

	(5)	(6)	(7)	(8)
	$\Delta ExpCash_{i,t}$	$\Delta ExpCash_{i,t}$	$\Delta ExpCash_{i,t}$	$\Delta ExpCash_{i,t}$
$ExpCash_{i,t-1}^{Si}$	-0.642*** (-5.69)			
$ExpCash_{i,t-1}^{LP}$		-0.497*** (-6.23)		
$ExpCash_{i,t-1}^{Fa}$			-0.413*** (-8.29)	-0.407*** (-9.15)
$\Delta Unemp_{i,t-1}$	-0.439 (-1.53)	-0.442 (-1.18)	-0.497 (-1.16)	-0.371 (-0.95)
$Unemp_{i,t-2}$	-0.535***	-0.625**	-0.994***	-1.004***

	(-3.17)	(-2.53)	(-2.85)	(-2.90)
$\Delta PublicDeficit_{i,t-1}$	-0.134***	-0.140	-0.265**	-0.246**
	(-2.93)	(-1.43)	(-2.39)	(-2.13)
$PublicDeficit_{i,t-2}$	-0.134***	-0.119	-0.130	-0.118
	(-2.77)	(-1.02)	(-1.57)	(-1.51)
$UnexpGrowth_{i,t}$	-2.521	-15.82	-9.337	-8.586
	(-0.24)	(-0.83)	(-0.54)	(-0.49)
$\Delta LogGDP_{i,t}$	-19.02**	-20.23**	-27.78***	-29.25***
	(-2.39)	(-2.00)	(-3.47)	(-3.81)
$LogGDP_{i,t-1}$	-24.72***	-34.63***	-38.45***	-40.16***
	(-3.93)	(-5.34)	(-5.47)	(-5.88)
$\Delta Openness_{i,t-1}$	0.0259	0.0344	0.0370	0.0392
	(0.95)	(0.74)	(0.65)	(0.69)
$Openness_{i,t-2}$	0.0100	0.0255	0.0268	0.0456
	(0.45)	(0.72)	(0.72)	(1.22)
$\Delta Deind_{i,t-1}$	17.10	7.545	10.16	3.512
	(1.01)	(0.26)	(0.35)	(0.12)
$Deind_{i,t-2}$	19.63	11.40	30.88	26.35
	(1.02)	(0.38)	(1.01)	(0.85)
$\Delta UnionDensity_{i,t-1}$	0.128	0.102	0.125	0.0718
	(0.89)	(0.41)	(0.44)	(0.24)
$UnionDensity_{i,t-2}$	0.0929	-0.0738	0.0493	0.00365
	(1.30)	(-0.68)	(0.32)	(0.02)
$\Delta Left_{i,t}$	-0.00810	-0.0499**	-0.0270	-0.0436
	(-0.48)	(-2.00)	(-0.64)	(-0.84)
$Left_{i,t-1}$	-0.0338**	-0.0846***	-0.0909***	-0.114***
	(-2.12)	(-3.39)	(-2.75)	(-2.88)
$\Delta ExpectedChecks_{i,t-1}^{Adapt,0.1}$	-0.250	-1.602	-1.448	-1.283
	(-0.36)	(-1.30)	(-1.05)	(-0.83)
$ExpectedChecks_{i,t-1}^{Adapt,0.1}$	-0.0832	0.706	1.007	1.370
	(-0.10)	(0.53)	(0.74)	(0.76)

$\Delta(\text{Left} \cdot \text{ExpectedChecks}^{\text{Adapt},0.1})_{i,t}$	0.000854 (0.29)	0.00760 (1.59)	0.00536 (0.75)	0.00967 (1.06)
$(\text{Left} \cdot \text{ExpectedChecks}^{\text{Adapt},0.1})_{i,t-1}$	0.00513* (1.92)	0.0141*** (3.35)	0.0163*** (2.63)	0.0221*** (2.83)
N	223	223	223	211
Countries	20	20	20	19

Notes: GMM estimation. Z-statistics in parentheses. \*, \*\*, and \*\*\* denote p-values of 0.1, 0.05, and 0.01, respectively.

Table 6: Determinants of means-tested income support (as a percentage of GDP per capita) — OLS and GMM utilising  $VPs$  variable.

	(9)	(10)	(11)	(12)
	$\Delta \text{ExpCash}_{i,t}$	$\Delta \text{ExpCash}_{i,t}$	$\Delta \text{ExpCash}_{i,t}$	$\Delta \text{ExpCash}_{i,t}$
$\text{ExpCash}_{i,t-1}^{\text{Si}}$	-0.390*** (-2.70)			
$\text{ExpCash}_{i,t-1}^{\text{LP}}$		-0.303*** (-2.68)		
$\text{ExpCash}_{i,t-1}^{\text{Fa}}$			-0.230** (-2.50)	-0.423*** (-3.57)
$\Delta \text{Unemp}_{i,t-1}$	-0.523*** (-2.77)	-0.411 (-1.11)	-0.444 (-1.31)	-0.350 (-0.73)
$\text{Unemp}_{i,t-2}$	-0.385*** (-3.00)	-0.534*** (-2.61)	-0.644*** (-3.23)	-1.257*** (-2.92)
$\Delta \text{PublicDeficit}_{i,t-1}$	-0.0509 (-0.55)	-0.197 (-1.11)	-0.142 (-0.83)	-0.473** (-2.48)
$\text{PublicDeficit}_{i,t-2}$	-0.0563 (-0.86)	-0.0309 (-0.29)	-0.0588 (-0.50)	-0.384** (-2.56)

$UnexpGrowth_{i,t}$	2.877 (0.48)	5.680 (0.50)	-5.079 (-0.40)	19.47 (0.83)
$\Delta LogGDP_{i,t}$	-11.79* (-1.73)	-17.15 (-1.41)	-4.705 (-0.37)	-32.35 (-1.26)
$LogGDP_{i,t-1}$	-10.40** (-2.08)	-11.73 (-1.59)	-2.418 (-0.33)	-30.26 (-1.43)
$\Delta Openness_{i,t-1}$	-0.0956* (-1.86)	-0.184** (-2.27)	-0.209** (-2.46)	0.000813 (0.01)
$Openness_{i,t-2}$	-0.141*** (-3.12)	-0.246*** (-3.52)	-0.286*** (-3.94)	-0.163* (-1.69)
$\Delta Deind_{i,t-1}$	17.13 (0.78)	-10.26 (-0.24)	-20.06 (-0.45)	23.32 (0.51)
$Deind_{i,t-2}$	14.03 (0.92)	12.11 (0.42)	-3.321 (-0.10)	37.13 (0.74)
$\Delta UnionDensity_{i,t-1}$	0.262** (2.05)	0.191 (0.81)	0.458** (2.12)	0.0115 (0.04)
$UnionDensity_{i,t-2}$	0.00239 (0.03)	-0.131 (-1.02)	-0.0127 (-0.12)	-0.000211 (-0.00)
$\Delta Left_{i,t}$	-0.0264*** (-3.02)	-0.0672*** (-3.39)	-0.0666*** (-3.47)	-0.0674* (-1.82)
$Left_{i,t-1}$	-0.0128 (-1.61)	-0.0250* (-1.79)	-0.0234** (-2.20)	-0.0567 (-1.33)
$\Delta ExpectedVPS_{i,t-1}^{Adapt,0.1}$	-0.582 (-0.28)	-3.078 (-0.96)	-0.935 (-0.38)	-5.424 (-0.89)
$ExpectedVPS_{i,t-1}^{Adapt,0.1}$	-0.225 (-0.39)	1.258 (1.00)	-1.772 (-1.36)	-4.947* (-1.89)
$\Delta (Left \cdot ExpectedVPS_{i,t-1}^{Adapt,0.1})_{i,t}$	0.0122** (2.55)	0.0322*** (3.39)	0.0341*** (3.56)	0.0341* (1.77)
$(Left \cdot ExpectedVPS_{i,t-1}^{Adapt,0.1})_{i,t-1}$	0.00297 (0.63)	0.00508 (0.58)	0.00971 (1.42)	0.0326 (1.59)
Fixed Effects	Yes	Yes	Yes	

$R^2$	0.672	0.654	0.695	
N	139	139	139	108
Countries	16	16	16	16

Notes: OLS/PCSE and GMM estimation. Z-statistics in parentheses. \*, \*\*, and \*\*\* denote p-values of 0.1, 0.05, and 0.01, respectively.

Table 7: Determinants of expenditure on wages for the provision of social welfare (as a percentage of government expenditure) — OLS and GMM utilising *Checks* variable.

	(13)	(14)	(15)	(16)
	$\Delta ExpServ_{i,t}$	$\Delta ExpServ_{i,t}$	$\Delta ExpServ_{i,t}$	$\Delta ExpServ_{i,t}$
$ExpServ_{i,t-1}$	-0.382*** (-2.72)	-0.563*** (-3.96)	-0.421*** (-3.03)	-0.604*** (-4.15)
$\Delta Unemp_{i,t-1}$	-0.0658* (-1.79)	-0.0785** (-2.16)	-0.0761** (-2.01)	-0.0771* (-1.85)
$Unemp_{i,t-2}$	-0.0507* (-1.74)	-0.0282 (-0.59)	-0.0509 (-1.62)	-0.0266 (-0.62)
$\Delta PublicDebt_{i,t-1}$	-0.0181*** (-2.72)	-0.0236* (-1.74)	-0.0200*** (-3.02)	-0.0249* (-1.87)
$PublicDebt_{i,t-2}$	-0.00480** (-2.18)	-0.00588 (-1.59)	-0.00578*** (-2.65)	-0.00548* (-1.71)
$\Delta PublicDeficit_{i,t-1}$	-0.00267 (-0.16)	-0.00288 (-0.26)	-0.00121 (-0.07)	0.000142 (0.01)
$PublicDeficit_{i,t-2}$	-0.00146 (-0.14)	-0.0114 (-0.39)	-0.00130 (-0.12)	-0.00868 (-0.31)
$\Delta LogGDP_{i,t-1}$	-1.620 (-1.55)	-4.380* (-1.82)	-1.660 (-1.53)	-4.757* (-1.96)

$LogGDPPC_{i,t-2}$	-1.171*	-1.949	-1.063	-1.877
	(-1.71)	(-1.28)	(-1.53)	(-1.28)
$\Delta Openness_{i,t-1}$	-0.00440	0.00111	-0.00568	-0.000103
	(-0.86)	(0.21)	(-1.02)	(-0.02)
$Openness_{i,t-2}$	-0.00645	0.00315	-0.00813*	0.00206
	(-1.40)	(0.45)	(-1.73)	(0.30)
$\Delta DepRatio_{i,t-1}$	25.38**	10.16	26.55**	8.808
	(2.11)	(0.45)	(2.24)	(0.36)
$DepRatio_{i,t-2}$	2.967	-0.236	6.426	1.141
	(0.75)	(-0.03)	(1.51)	(0.16)
$\Delta Deind_{i,t-1}$	3.624	5.131	4.580	6.005
	(1.06)	(1.54)	(1.37)	(1.51)
$Deind_{i,t-2}$	4.226	5.151	4.169	4.831
	(1.19)	(1.04)	(1.16)	(1.18)
$\Delta UnionDensity_{i,t-1}$	0.00791	0.0282	0.0187	0.0234
	(0.28)	(1.03)	(0.66)	(0.92)
$UnionDensity_{i,t-2}$	-0.0185	-0.0329	-0.0124	-0.0269
	(-1.07)	(-1.33)	(-0.70)	(-1.04)
$\Delta Left_{i,t-1}$	0.00985**	0.00850	0.0133***	0.0103
	(2.18)	(1.07)	(2.58)	(1.04)
$Left_{i,t-2}$	0.00807*	0.00923*	0.0148***	0.0165**
	(1.94)	(1.76)	(3.11)	(2.30)
$\Delta ExpectedChecks_{i,t-1}^{Adapt,0.1}$	0.211	0.347	0.358***	0.507
	(1.52)	(1.26)	(2.71)	(1.63)
$ExpectedChecks_{i,t-2}^{Adapt,0.1}$	0.176**	0.288*	0.353***	0.536**
	(2.21)	(1.65)	(3.56)	(2.22)
$\Delta (Left \cdot ExpectedChecks^{Adapt,0.1})_{i,t-1}$	-0.00194**	-0.00173	-0.00275**	-0.00213
	(-2.16)	(-1.11)	(-2.57)	(-1.06)
$(Left \cdot ExpectedChecks^{Adapt,0.1})_{i,t-2}$	-0.00166**	-0.00179*	-0.00321***	-0.00344**
	(-2.02)	(-1.65)	(-3.27)	(-2.21)
Fixed Effects	Yes		Yes	

$R^2$	0.385		0.412	
N	189	155	181	149
Countries	18	17	17	16

Notes: OLS/PCSE and GMM estimation. Z-statistics in parentheses. \*, \*\*, and \*\*\* denote p-values of 0.1, 0.05, and 0.01, respectively. Coefficients for country dummies not reported.

Table 8: Determinants of expenditure on wages for the provision of social welfare (as a percentage of government expenditure) — OLS and GMM utilising *VPs* variable.

	(17)	(18)	(19)	(20)
	$\Delta ExpServ_{i,t}$	$\Delta ExpServ_{i,t}$	$\Delta ExpServ_{i,t}$	$\Delta ExpServ_{i,t}$
$ExpServ_{i,t-1}$	-0.642*** (-3.24)	-0.876*** (-12.36)	-0.671*** (-3.40)	-0.955*** (-15.35)
$\Delta Unemp_{i,t-1}$	-0.139*** (-3.58)	-0.243** (-2.46)	-0.147*** (-3.78)	-0.238** (-2.37)
$Unemp_{i,t-2}$	-0.0605 (-1.39)	-0.0967 (-1.35)	-0.0656 (-1.52)	-0.113 (-1.50)
$\Delta PublicDebt_{i,t-1}$	-0.0226*** (-4.19)	-0.0214 (-1.30)	-0.0202*** (-3.65)	-0.0172 (-1.07)
$PublicDebt_{i,t-2}$	-0.00596 (-1.08)	-0.00377 (-0.56)	-0.00150 (-0.24)	0.00441 (0.69)
$\Delta PublicDeficit_{i,t-1}$	-0.00428 (-0.16)	0.00971 (0.25)	-0.00887 (-0.31)	0.00491 (0.13)
$PublicDeficit_{i,t-2}$	-0.0684*** (-2.97)	-0.102*** (-4.07)	-0.0735*** (-3.02)	-0.108*** (-5.68)
$\Delta LogGDP_{i,t-1}$	-0.228 (-0.19)	-5.301 (-1.00)	-0.0214 (-0.02)	-4.669 (-0.84)

$LogGDPPC_{i,t-2}$	0.503 (0.56)	0.534 (0.36)	0.893 (0.93)	1.217 (0.95)
$\Delta Openness_{i,t-1}$	-0.00181 (-0.36)	-0.00123 (-0.15)	-0.00126 (-0.24)	0.000784 (0.11)
$Openness_{i,t-2}$	-0.0124** (-2.28)	-0.00715 (-0.88)	-0.0112** (-2.09)	-0.00607 (-0.84)
$\Delta DepRatio_{i,t-1}$	18.08 (1.18)	10.57 (0.44)	30.61* (1.77)	28.96 (1.37)
$DepRatio_{i,t-2}$	16.87** (2.06)	29.88*** (3.96)	22.62** (2.40)	39.01*** (5.26)
$\Delta Deind_{i,t-1}$	2.924 (0.56)	10.09 (1.48)	2.266 (0.42)	8.702 (1.13)
$Deind_{i,t-2}$	-5.327 (-0.91)	-5.374 (-1.45)	-7.333 (-1.17)	-8.398** (-2.48)
$\Delta UnionDensity_{i,t-1}$	0.0283 (0.85)	0.0210 (0.51)	0.0301 (0.95)	0.0240 (0.62)
$UnionDensity_{i,t-2}$	-0.0339 (-1.33)	-0.0283 (-1.13)	-0.0376 (-1.43)	-0.0248 (-1.05)
$\Delta Left_{i,t-1}$	0.00127 (0.19)	0.00321 (0.58)	0.000917 (0.14)	0.00395 (0.58)
$Left_{i,t-2}$	0.00788* (1.65)	0.0118*** (4.01)	0.00787* (1.66)	0.0121*** (3.73)
$\Delta ExpectedVPS_{i,t-1}^{Adapt,0.1}$	-0.368 (-0.76)	-0.535 (-0.61)	-0.894 (-1.57)	-1.262** (-1.97)
$ExpectedVPS_{i,t-2}^{Adapt,0.1}$	-0.0436 (-0.29)	-0.156 (-0.77)	-0.217 (-1.07)	-0.523*** (-3.11)
$\Delta (Left \cdot ExpectedVPS_{i,t-1}^{Adapt,0.1})$	0.000466 (0.16)	-0.000325 (-0.19)	0.000435 (0.15)	-0.000467 (-0.21)
$(Left \cdot ExpectedVPS_{i,t-2}^{Adapt,0.1})$	-0.00391** (-2.30)	-0.00572*** (-8.58)	-0.00426*** (-2.60)	-0.00620*** (-9.70)
Fixed Effects	Yes		Yes	

$R^2$	0.585		0.602	
N	105	77	99	73
Countries	14	14	13	13

Notes: OLS/PCSE and GMM estimation. Z-statistics in parentheses. \*, \*\*, and \*\*\* denote p-values of 0.1, 0.05, and 0.01, respectively.

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	Mean	Standard Deviation
$ExpCash^{Fa}$	63.7	16.9
$ExpCash^{LP}$	52.3	14.0
$ExpCash^{Si}$	29.7	9.1
$ExpServ$	3.25	2.31

Table 2: Summary statistics for the four dependent variables used in the analysis.

Country	$ExpCash^{Fa}$			$ExpServ$		
	1990	1995	2000	1990	1995	2000
Australia	61.1	61.7	54.5			2.65
Austria	50.9	50.8	47.0		2.91	2.13
Belgium	57.1	52.3	45.5	0.80	0.86	0.88
Canada	81.4	77.2	51.7			
Denmark	87.9	85.6	82.3	7.96	7.99	8.23
Finland	88.2	94.3	71.9	6.22	6.68	5.66
France	47.6	46.9	45.2		3.33	3.30
Germany	76.7	78.2	73.5		1.50	1.39
Greece				2.20	3.71	4.09
Ireland	109.8	92.1	58.7		2.87	2.72
Italy	66.0	62.8	67.3	2.85	2.60	2.67
Japan	66.2	66.4	68.0	0.61	0.66	0.70
Netherlands	62.3	59.8	50.3		1.73	1.62
New Zealand	81.3	77.6	74.4			
Norway	60.5	63.8	68.2	5.26	6.09	5.66
Portugal			42.1	2.55	3.18	3.60
Spain	52.6	39.2	31.4			3.05
Sweden	85.3	90.6	67.1		7.66	7.30
Switzerland	70.5	80.8	72.0			
UK	65.3	67.4	65.7	3.67	4.01	3.79
USA	43.3	39.1	32.2	1.38	1.36	1.16

Table 3: Summary of dependent variables for cash transfer ( $ExpCash^{Fa}$ ) and welfare services ( $ExpServ$ ) for each country in 1990, 1995, and 2000.

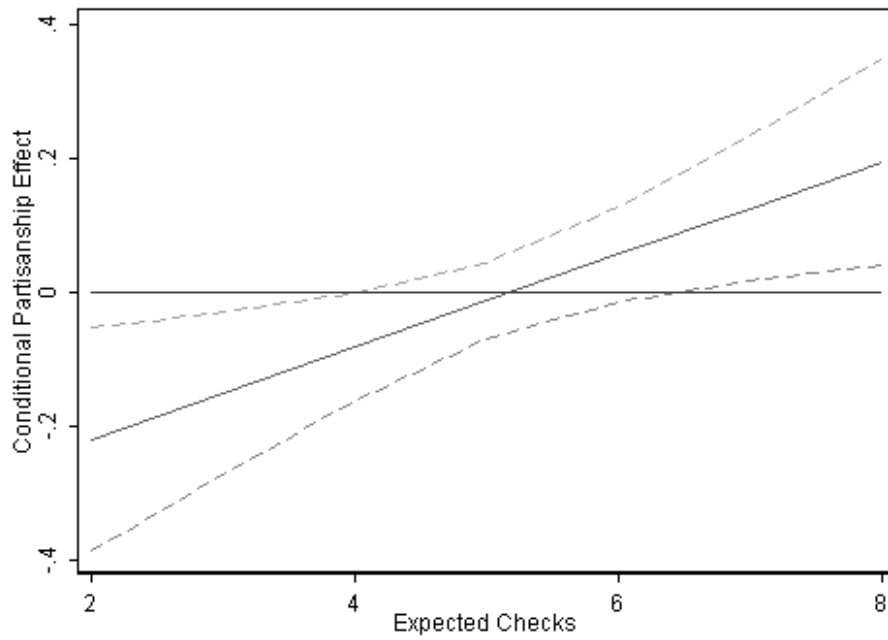


Figure 1: Partisanship effect on  $ExpCash^{Fa}$ , conditional on  $ExpectedChecks^{Adapt,0.1}$ , estimated from Model (3) [OLS/PCSE]. 95% confidence intervals shown.

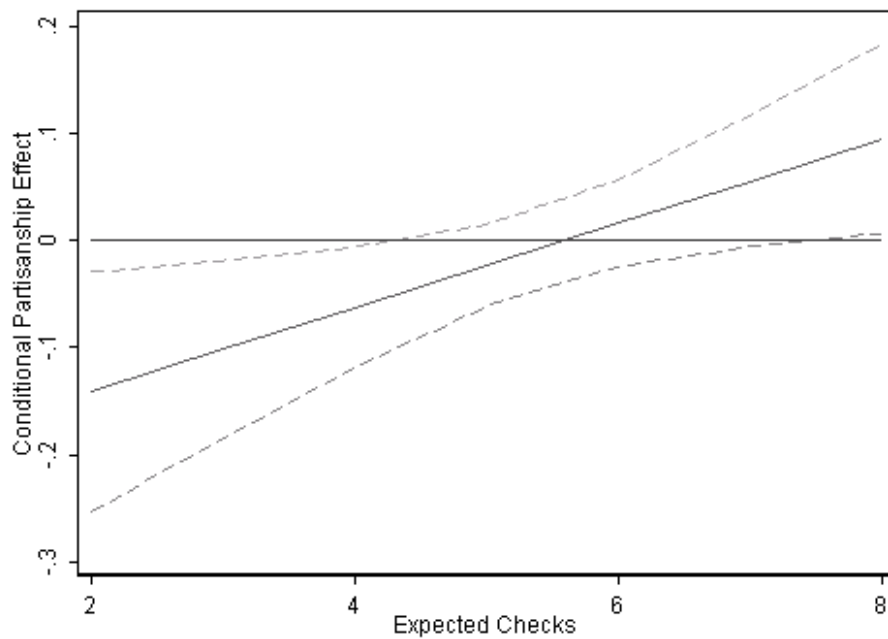


Figure 2: Partisanship effect on  $ExpCash^{Fa}$ , conditional on  $ExpectedChecks^{Adapt,0.1}$ , estimated from Model (7) [GMM]. 95% confidence intervals shown.

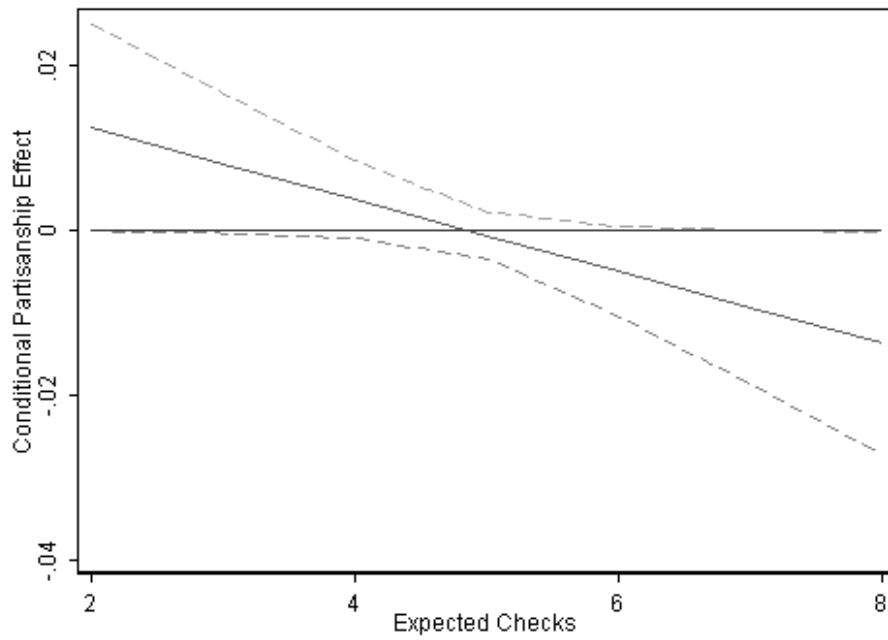


Figure 3: Partisanship effect on  $ExpServ$ , conditional on  $ExpectedChecks^{Adapt,0.1}$ , estimated from Model (13) [OLS/PCSE]. 95% confidence intervals shown.

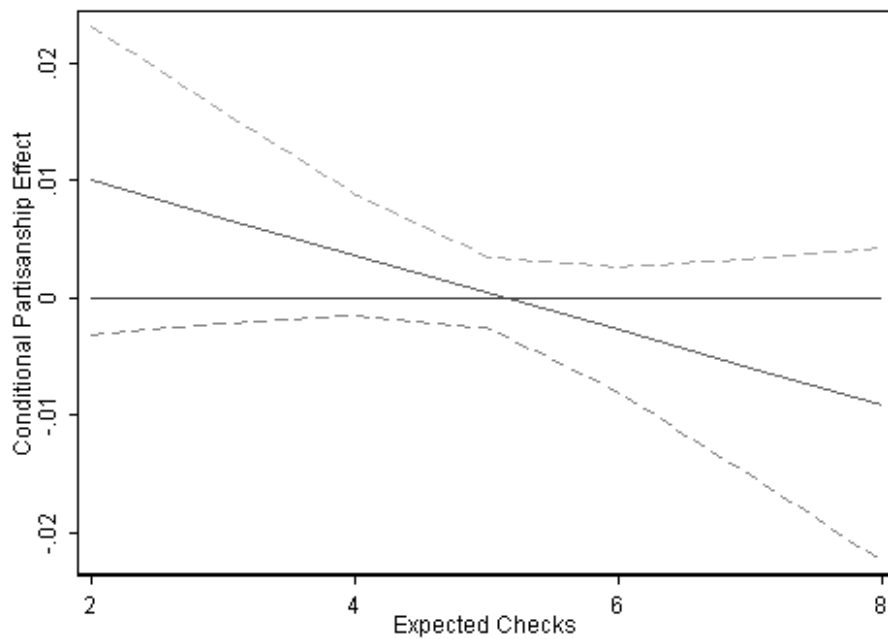


Figure 4: Partisanship effect on  $ExpServ$ , conditional on  $ExpectedChecks^{Adapt,0.1}$ , estimated from Model (14) [GMM]. 95% confidence intervals shown.

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