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JOACHIM WEHNER
London School of Economics and Political Science

Electoral Budget Cycles in Legislatures

This article examines electoral cycles in legislative budget decisions. Where local structures play an important role in candidate selection and election, legislators' incentives to amend the executive spending proposal can depend on the proximity of elections, leading to more spendthrift behavior in the run-up to popular votes. However, stringent budget institutions can counteract this tendency. Using a unique dataset of executive spending proposals and approved budgets in Sweden, I find strong empirical support for these predictions. Future studies of electoral cycles should pay greater attention to separating the contributions of the legislative and executive stages of the budgetary process and the conditions that foster electoral cyclicality in legislatures.

The idea that politicians may attempt to manipulate economic performance or fiscal policy as they approach elections has been the subject of much empirical work, often with mixed results. This article highlights the role of legislative politics and budget institutions. Most of the existing literature on electoral cycles in fiscal policy does not clearly disentangle the separate contribution of executive and legislative dynamics. I develop a framework for thinking about the effect of elections on legislative spending choices and an empirical strategy to address this gap. In addition, I test whether budget institutions condition the effect of elections on legislative spending decisions, in line with calls for more "context-conditional" work that explores under what circumstances electoral cycles are most likely to occur (Franzese 2002).

My argument builds on the distributive politics approach (Weingast, Shepsle, and Johnsen 1981) and is based on the observation that legislators face tiered career incentives: They first need to be reselected and reelected, which depends on pleasing those who have the power to select and elect candidates; once this has been accomplished, they have incentives to get appointed to positions of influence, which usually depends on pleasing their party's leadership. Where local structures play an important role in candidate selection and election, this can

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create incentives for legislators to internalize a decreasing share of the cost of their actions as elections approach. If these incentives are unchecked, parliamentary changes to the executive budget proposal are likely to vary systematically around the time of elections, with pre-election splurges followed by relative discipline after elections. However, institutional constraints can counteract the temptation for legislators to go on an election-induced spending spree. I examine these hypotheses with a unique dataset of changes to executive spending proposals during their review in the Swedish Riksdag, covering the period since the first fully democratic elections in 1921. To preview, I find systematic evidence of electoral cycles in these changes, conditional on institutional context, even when accounting for the possibility of strategic interaction and a range of other variables. The rigid nature of Sweden's electoral calendar suggests that these effects are at least in principle causal.

The article has three main sections. The first section briefly sketches some relevant trends in the literature and highlight gaps, followed by theoretical background and the hypotheses to be tested. The second section discusses case selection and reviews the relevant variables and data, and the third section contains the empirical investigation. The conclusion examines generalizability and the wider implications of the results.

Electoral Proximity and Legislator Incentives

The literature on electoral budget cycles is extensive and has undergone a series of refinements. Initially, research focused on the manipulation of economic outcomes by politicians (Nordhaus 1975). Empirical work has found rather mixed support. For various reasons, politicians may not be able to directly engineer economic expansions.[1] However, they may have more direct control over the budget and may use it to attempt electoral manipulation (Rogoff and Sibert 1988; Tufte 1978). More recent is a focus on context conditionality (Franzese 2002). Electoral cycles in fiscal policy have been related to the form of government (Persson and Tabellini 2002), democratic maturity (Brender and Drazen 2005), and budget transparency (Alt and Lassen 2006), while Chang (2008) shows that electoral systems condition which types of outlays increase in election years. Using data from the American states, Alt and Rose (2007) find that institutional arrangements that strictly enforce balanced budgets eliminate the fiscal effect of elections (see also Rose 2006). The latter argument is largely untested for parliamentary systems. One exception is the work by Clark and Hallerberg (2000), although their main focus is the interplay between fiscal and monetary policymakers.

A significant shortcoming throughout most of this literature is the lack of a clear distinction between different sets of politicians. Several cross-national studies treat legislative elections in parliamentary systems as equivalent to executive elections in presidential systems (Alt and Lassen 2006, 535; Brender and Drazen 2005; Shi and Svensson 2006, 1370). The assumed irrelevance of legislative elections under separate powers is surprising, given that previous work has found strong congressional effects on budgetary decisions in presidential systems (Davis, Dempster, and Wildavsky 1966; Hallerberg and Marier 2004), including electoral effects on expenditure (Pack 1988). In parliamentary systems, the government is indirectly elected. As a result, the separate contributions to electoral cycles of executive and legislative decisions are more difficult to disentangle with the standard approach in cross-national work, which uses measures of aggregate fiscal performance as dependent variables. One argument might be that only the executive matters in parliamentary systems. However, the budget-formulation process in any democratic country involves two distinct processes: executive formulation of a budget proposal and legislative review and approval (Hallerberg, Strauch, and von Hagen 2009). A priori, both of these could be important for explaining electoral budget cycles.

Rogoff (1990, 34) acknowledges the possible tension between individual legislators seeking reelection and their party's leaders, but the theoretical literature on electoral budget cycles has yet to fully explore this insight. The argument developed here draws on the model of distributive politics developed by Weingast, Shepsle, and Johnsen (1981). With few exceptions (Hallerberg 2004a; Lancaster and Patterson 1990), this work is rarely deployed in studies of parliamentary behavior. My explanation why legislators' spending choices may be affected by the electoral cycle relates to their career ambitions. At election time, a parliamentarian's calculus, assuming a desire to get reelected, must involve pleasing those with the power to nominate and to elect (Strøm 1997). Depending on the design of the electoral system, this can give rise to particularistic incentives that are misaligned with those of the leadership of a candidate's party (Carey and Shugart 1995). This is likely to be the case where decentralized party structures select candidates for particular constituencies or determine the order of candidates on regional party lists (Lancaster 1986). Some issues of importance to a party's central leadership may be less vital for a particular district, where local concerns dominate. These concerns have to be addressed satisfactorily if a parliamentarian wants to achieve reselection by local party structures and reelection by local voters.

Getting elected is, however, not enough for most parliamentarians. Once elected, they have incentives to achieve positions of influence within their party's hierarchy or within the legislature, such as committee chairs or assignments to prestigious committees. If the party of a legislator is part of the government, they might also aspire to be appointed as ministers or junior ministers or to lower-ranking executive positions. Such positions are valuable, as they enhance a politician's public profile and provide a platform to influence policy and service constituency needs. In addition, postelectoral appointments can bring access to patronage resources and increased salaries (Martin 2012). Crucial in this context is that access to these positions, as well as their retention, is likely to require support from the leadership of a legislator's party (Cox and McCubbins 1993; Damgaard 1995; Strøm 1997). As Martin (2012) highlights, where party leaders cartelize control over access to prized post-election positions, or "mega-seats," they have a tool with which to instill discipline and to counteract any centrifugal tendencies that may emanate from the electoral system. Hence, the achievement of a position of influence within the party, legislature, or executive also depends on pleasing party leaders—or at least on not upsetting them excessively.

An implication of this discussion is that a legislator may have different "constituencies" to please at different times in the electoral cycle.[2] As a result, the incentives to internalize the costs of her actions may not be constant. With expenditure that can be targeted at a legislator's district where it produces electorally relevant benefits but costs that are shared equally across all districts, a legislator's optimal project size is an increasing function of the number of districts (Weingast, Shepsle, and Johnsen 1981). If the share of the costs borne by a district is smaller than the share of those voting for the legislator's party, then party discipline reduces the prospending bias. Shortly before elections, parliamentarians are likely to consider the costs for their particular selectorate and electorate: those who are directly involved in their renomination and reelection. As legislators become sensitive to local needs, the power of party leaders to instill discipline is likely to slip. Immediately after elections, by contrast, parliamentarians are more likely to internalize the overall costs to their party, as they depend on its leadership for appointments to positions of influence. This is also the time when the expected value of party, legislative, or executive office is greatest, since the next election is most distant. In some systems, the selectorate can be identical to the leadership of a party, for instance in countries that have proportional representation systems with closed lists and a single nationwide constituency. However, in many instances, the role of local actors to varying degrees weakens the grip of a party's national leadership (Lancaster 1986, 72).

Such fluctuating incentives can affect legislative spending decisions. Immediately after elections, the incentives of legislators tend to be closely aligned with those of their party's leadership. Since the leaders of governing parties are closely involved in the process of drafting a budget proposal, I expect parliamentary changes to the executive budget proposal to be relatively small then. However, approaching elections diminish the incentives to please party leaders, and party discipline may slip. Where legislators internalize a smaller share of the costs than their party leaders as elections draw closer, they favor expenditures that are larger relative to the executive proposal; assuming universalistic logrolls (Shepsle and Weingast 1981), amendments that increase expenditures are more likely. Hence, my hypothesis is that legislative changes to the executive spending proposal are smallest immediately after an election and grow larger as the expiry of the electoral term approaches. However, the possibility of strategic interaction complicates the analysis. For example, the executive may anticipate legislative profligacy in election years and table an austerity budget to accommodate spending increases. Or perhaps the executive responds to a large volume of amendments in one year by anticipating certain changes to the following budget and incorporating these into the draft tabled for parliamentary approval. In the next section, I discuss these possibilities in more detail, including implications for the empirical analysis.

A further caveat is that budget rules and procedures can eliminate the possibility for legislators to increase expenditures. A crucial finding in the work on electoral cycles in the American states (Alt and Rose 2007; Rose 2006) is that budget institutions condition the effect of elections on fiscal policy outcomes. This is a neglected angle in the cross-national literature. Various studies show that constrained legislative authority is associated with better fiscal performance (Alesina et al. 1999; Hallerberg, Strauch, and von Hagen 2009; von Hagen and Harden 1995), but few make an explicit connection between electoral incentives and budget institutions. Looking at Latin America and the Caribbean, Hallerberg and Marier (2004) find that strong executive agenda-setting powers contain the common-pool-resource problem when legislators have incentives to cultivate a personal vote. However, they do not test whether the fiscal effect of elections differs systematically across countries with different budget institutions and electoral systems.[3] Clark and Hallerberg (2000) find that fiscal targets and institutions that centralize budgetary decisions with the finance minister contain debt increases prior to elections in European Union countries. Here, I contribute an empirical test of the interaction between electoral proximity and legislative budget institutions.

Background and Data

The empirical analysis is based on data from the Swedish Parliament, the Riksdag. A major advantage is that Sweden's electoral calendar is unusually rigid. The timing of elections is regulated in the Riksdag Act (chapter one) and the Instrument of Government (chapters three and six). Elections to the Riksdag are currently held on the third Sunday of September every four years. The electoral term was four years until 1970, when it was shortened to three years with the introduction of the unicameral Riksdag. In the mid-1990s, the electoral term was once again extended to four years. It is possible for the government to call an early election, but the resulting mandate is only valid for the remainder of the regular term, which is a strong disincentive (Bergman 2000, 199). In the period covered here, such "extra" elections were held once, in 1958 (Stjernquist 1987, 273–76).[4] In general, incumbent governments in Sweden cannot manipulate the timing of the next regular election, including for reasons associated with fiscal policy or economic outcomes. Endogenous election timing would make it much harder to identify plausibly causal effects (Kayser 2005; Smith 2004).

Sweden's electoral system has been relatively unaltered over the period covered here. Since 1911, elections have been based on proportional representation and universal suffrage first applied in 1921. There have been some reforms over the years, but these were rather minor (Bergman 2004; Stjernquist 1987).[5] Out of currently 349 seats, 310 are distributed across 29 multimember districts. These are congruent with county borders, except in the three largest cities. The remaining adjustment seats ensure proportionality based on the votes obtained by parties nationwide. Local party structures nominate candidates in a process that involves nonbinding primaries. National leaders have little influence on who gets onto regional party lists (Hagevi 2000, 153). In one famous case, the Stockholm branch of the Liberal Party in 1929 refused to return Foreign Minister Eliel Löfgren to his seat in the Second Chamber. Bergman attests "constituency supremacy over nominations," but he goes on: "once elected, MPs see themselves more as party than as constituency representatives" (2004, 216). Yet, the importance of central party structures should not be exaggerated. In a 1994 survey, 76% of Riksdag members reported weekly contacts with their constituency party, but only 22% reported the same frequency for central party structures (Hagevi 2000, 157). Good relations with local party structures remain essential for political survival.

It is possible that the importance of local constituencies and party structures was stronger in earlier decades. Stjernquist (1987, 233) cites data from 1937 and 1961, which show that 63 and 70% respectively of the members of the First Chamber were also elected officials from county councils or municipalities. This is not surprising since the county councils elected members to the First Chamber. Less expected, perhaps, is that local ties were even stronger in the directly elected Second Chamber. Here, the corresponding figures are 79 and 75%. This meant that legislators had to pay very close attention to local concerns if they wished to be renominated. With the professionalization of local and parliamentary politics, it is now difficult to combine more than one mandate, although many members of the Riksdag are still involved in local politics (Arter 2000, 107). Moreover, the role of parliamentary parties in coordinating the activities of their members has grown (Damgaard 1995; Hagevi 2000; Stjernquist 1987, 250). Given these developments, the dynamics outlined in the previous section may have become less pronounced over time and best capture the early decades of the democratic Riksdag.[6] The empirical analysis includes a formal test of this possibility.

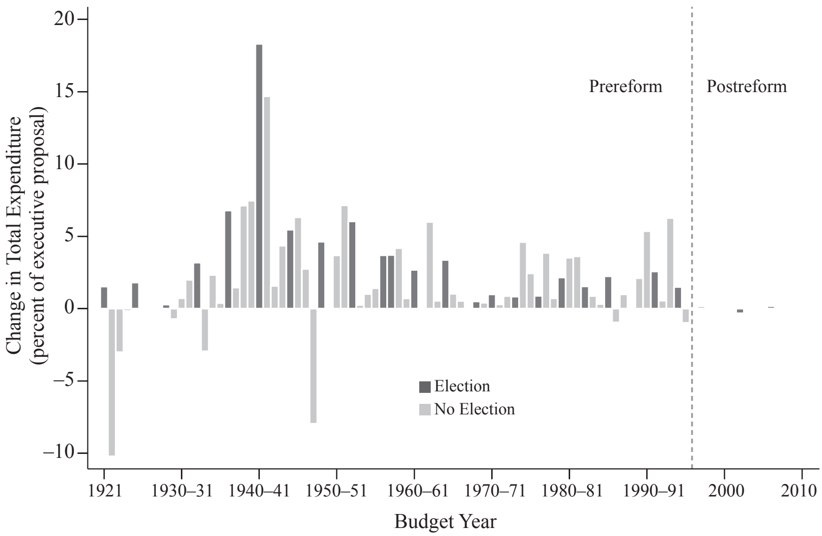
The Riksdag has unfettered constitutional authority to decide the budget. In the bicameral Riksdag, the Committee of Supply was the main appropriations committee. It had different sections that considered the budget proposals from particular ministries and operated with considerable independence. In addition, the Committee on Agriculture had authority to appropriate money in its area of legislative responsibility (Elder 1951; Stjernquist 1987, 246–47). The move to unicameralism in 1970 also coincided with the introduction of subject-specific committees that reviewed spending plans for their particular area of responsibility, such as health, education, and so forth, in a process that lacked central coordination (Mattson 1996). As Crain and Muris (1995) highlight, such a decentralized committee process facilitates a prospending bias, since members of sectoral committees tend to internalize only a share of the costs associated with projects in "their" policy area. These legislative institutions provided members with the formal authority as well as procedural opportunities to exert influence (Strøm 1986).

Institutional reforms in the mid-1990s greatly strengthened the role of the Finance Committee vis-à-vis sectoral committees. These reforms introduced a top-down voting procedure in which total spending and allocations across the different sectors are fixed, based on a report from the Finance Committee, prior to parliamentary deliberations on individual amendments (Blöndal 2001; Hallerberg 2004b). They also reduced the potential for individual members to influence particular spending items, since it is no longer possible to form shifting majorities on individual amendments. Instead, the opposition parties produce alternative proposals that cover total spending, the allocation of expenditure across the different policy areas, and revenue changes, which are voted on as packages (Molander 2001, 36). One initial analysis found that the number of amendments and their net effect on spending was lower after the reforms, compared to the preceding decade (Wehner 2007). This article not only greatly expands the time horizon of these initial studies, but it also considers how these reforms affected electoral budget cycles. The postreform constraints on spending increases make parliamentary impacts on the budget, including those induced by the electoral calendar, less likely.

Data on total expenditure proposed by the government and approved by the Riksdag were coded by hand from the parliamentary record (Riksdagens protokoll). The starting date is 1921, the year of the first fully democratic elections in which all women and men had the right to vote (Metcalf 1987). The dataset covers all 92 budgets for the years 1921 to 2012.[7] My measure of legislative activity is the change in total spending during the parliamentary approval process—that is, the difference in total spending between the original government proposal and the final budget approved by parliament—expressed as a percentage of the government's proposal. I focus on expenditures, in line with the theoretical discussion and "the general consensus in the literature that political budget cycles are stronger in spending than in taxes, deficits, or debt" (Alt and Rose 2007, 847). My approach contrasts with cross-national studies of electoral budget cycles that use measures of aggregate fiscal performance. Only a small number of other studies use the difference between proposed and approved expenditures as a dependent variable (Clarke 1998; Davis, Dempster, and Wildavsky 1966; Mattson 1996; Meyer and Naka 1998), but none of these focus on electoral cycles. Figure 1 summarizes the data.[8]

Next, budgets have to be classified in accordance with their status in the electoral calendar. To convey a first impression, Figure 1 shows which budgets covered a fiscal period in which an election took place; several election-year "spikes" are clearly visible. The theoretical discussion calls for a more nuanced indicator of electoral proximity. My main variable of interest is the number of the years remaining in the electoral term. This variable is set to 0 if the budget covers a fiscal year in which a scheduled election was held, and $t - 1$ in the year after an election, and so on, where t represents the length of the electoral term (either 3 or 4,

FIGURE 1
Changes to Executive Spending Proposals
During the Parliamentary Process in Sweden, 1921 to 2012



Source: Own calculations based on data extracted from Riksdagens protokoll (various years).

depending on the period) and only counting full years. This approach also deals elegantly with the budget in place for the "extra" elections in 1958. The strategic use of amendments requires legislators to be able to anticipate the timing of elections; the variable captures the fact that this may not be possible with elections outside the regular electoral cycle. However, the effect of an additional year may not be constant across the electoral cycle. The empirical part examines alternative specifications.

To test whether the design of the parliamentary process affected legislative amendment activity, I construct a simple indicator of institutional context that is set equal to 0 for all budgets up to 1995–96 and equal to 1 from 1997. The latter budget was the first that was adopted under the more centralized top-down procedure. To obtain a first visual impression, Figure 1 separates the pre- and postreform periods with a dotted vertical line. The reform appears to have affected the magnitude of changes during the parliamentary stage. The Riksdag approved only nine out of 92 budgets without adjusting the total level of spending, and all of these fall into the period after the procedural reforms. (Some of the changes in the prereform years were very small and hence they are not clearly visible on Figure 1.) The following section analyzes the relationship between these reforms and electoral cyclicality.

Figure 1 reveals no obvious trend, and Dickey-Fuller tests reject the null of a unit root.[9] There are several interesting patterns. First, it is rare for budgets to emerge unscathed from the parliamentary process. Second, the tendency is to approve upward revisions, on average by 1.76% of the executive proposal. Parliamentarians reduced only 16 out of 92 budgets, usually in times of economic crisis such as the 1920s and mid-1990s. Hence, to account for macroeconomic context, I control for GDP growth in the calendar year preceding the relevant budget year. The data are from Edvinsson (2005), supplemented from the International Monetary Fund (2012) for the most recent decade.[10] Third, the series contains large spikes around the time of World War II. This period was associated with a significant expansion of military expenditures in response to German rearmament and the outbreak of the war (Hagelin and Wallensteen 1992, 419). The empirical analysis accounts for this response with an indicator variable set equal to 1 for all budgets approved between 1937 and the end of the war, 0 otherwise.

Until 1970, Sweden had a bicameral parliament, which consisted of the First Chamber (Första kammaren) and the Second Chamber (Andra kammaren). The First Chamber was indirectly elected by county and city councilors under a system where one-eighth of its membership was renewed every year, while the Second Chamber was directly elected (Stjernquist 1987). A system of joint committees facilitated coordination (Elder 1951). Governments needed a majority in each chamber to enact resolutions, but only a majority of the pooled votes of both chambers for budget resolutions. This favored the directly elected Second Chamber, which had 230 members compared to 150 in the First Chamber (Bergman 2000, 201). The bicameral parliament ceased to exist in 1970, when the first elections to the unicameral Riksdag were held. Bicameralism can affect budgetary decisions (Heller 1997). In the empirical analysis, I pool the data from these two periods but control for a possible break in the series.

In addition, it is possible that party political variables affect amendment activity. Apart from the war years, when Sweden was governed by a national coalition of all major parties, many governments were minority administrations (Bergman 2000; Stjernquist 1987). Mattson (1996) explains how, prior to the mid-1990s, minority governments had to negotiate legislative support for their spending plans in the various committees of the Riksdag. The empirical analysis includes an indicator variable set equal to 1 if the budget was submitted by a minority government, 0 otherwise.[11] Moreover, occasional multiparty coalition governments have tended to be less stable (Bergman 2000). To account for the possibility that the latter were less able to contain parliamentary amendments, I include an indicator variable set equal to 1 if a coalition government submitted the budget, 0 otherwise.[12] In addition, I control for the partisan composition of the government, which may affect attitudes towards spending increases (Hibbs 1977). The Social Democrats heavily dominated Swedish postwar politics, until their influence started to wane in the 1990s. The party was in government during the drafting of 66 budgets between 1921 and 2012. The analysis includes an indicator variable set equal to 1 if the Social Democrats were part of the government, 0 otherwise. For these three variables, I use data from the Europa World Yearbook, supplemented with information from the Statesman's Yearbook and Bergman (2000), as well as my own research.

The possibility of strategic interaction complicates the analysis. However, unlike typical cross-national studies that use aggregate fiscal indicators, this dataset can be used to model strategic behavior. For instance, if the executive anticipates legislative profligacy in election years, but cares about containing total expenditure, it could propose an austerity budget so as to leave a wider margin for upward amendments. In this case, the volume of amendments might be larger in election years simply because it contains the extra fiscal space created by the executive. To account for this possibility, I also control for the percentage change proposed by the executive, measured as proposed total spending minus the approved expenditure total in the previous year, expressed as a percentage of the latter. If the executive accommodates legislative profligacy by tabling an austerity budget, then this variable should have a significant negative coefficient, with the magnitude of the latter indicating the degree of accommodation. Second, it is possible that amendments in one year lead the government to anticipate similar changes to the following budget and to adjust its proposals accordingly. In this case, the dependent variable might understate parliamentary impact. The analysis explores this by including a lagged dependent variable.

Finally, the executive in some years tabled amendments to its own budget. Prior to the reforms in the mid-1990s, the main budget had to be tabled in early January. Starting with the 1949–50 budget, the government typically initiated a number of changes after this date, which it later included in a supplementary bill (kompletteringspropositionen). Some of these adjustments were due to updated economic forecasts, while others responded to parliamentary demands. As Mattson (1996, 149) notes, it was uncommon for the executive to negotiate with actors outside of the government prior to tabling the budget in January. Instead, negotiations took place in parliamentary committees whenever there was insufficient

TABLE 1
Mean Spending Changes by Years Left in the Electoral Term

Years Left in Term	Mean	SE	Lower	Upper
0	2.80	0.76	1.29	4.30
1	1.52	0.65	0.24	2.80
2	1.71	0.46	0.80	2.63
3	0.72	1.11	−1.49	2.93
All Years	1.76	0.37	1.03	2.49

Note: Column two gives the mean change in total spending during the parliamentary process as percent of the executive budget proposal for the number of years left in the electoral term indicated in column one. Column three reports the standard error (SE). The final two columns report the upper and lower bounds based on 95% confidence intervals. The sample covers the 1921 to 2012 budgets (N = 92).

support for a proposal. In the analysis, I also control for the percentage increase of the initial budget that is proposed in the supplementary bill. On the one hand, ignoring the supplementary bills might overestimate parliamentary impact by failing to attribute some amendments to changing economic conditions or executive initiatives. On the other hand, at least some of the changes contained in these bills were in response to parliamentary demands, so the inclusion of this variable on the right-hand side most likely leads to an underestimation of parliamentary influence and makes for a particularly tough empirical test.

Empirical Analysis

To take a first look at the data, Table 1 shows the mean percentage changes in expenditures by years left in the electoral cycle. The mean is highest for election-year budgets (2.80), at intermediate levels with one or two years left in the electoral term (1.52 and 1.71, respectively), and smallest immediately after an election (.72). Moreover, the 95% confidence interval excludes 0 when there are two, one, or no years left in the electoral term, but includes 0 when there are three years left in the electoral term. These figures provide initial evidence of an association between elections and changes to the budget during the parliamentary process.

I proceed to multiple regression analysis, which allows me to control for other variables that may also affect changes to the budget. The baseline model includes direct electoral and institutional effects. I regress the percentage change in expenditure onto the number of years left in the electoral term and the indicator of budget reform, plus a set of k controls discussed in the previous section. The subscript t indicates the fiscal period to which a budget applies, with approval in the preceding period $t-1$:

$$\begin{aligned} \mathit{Change\ in\ spending}_t = \beta_0 + \beta_1 \mathit{Years\ left\ in\ term}_t + \beta_2 \mathit{Reform}_{t-1} \\ + \sum \beta_k \mathit{Controls}_{t-1} + \varepsilon_t. \end{aligned}$$

Column (1a) in Table 2 reports the results with the basic set of controls. On average, an additional remaining year in the electoral term reduces changes to executive spending proposals during the parliamentary stage by .69 of a percentage point, significant at the 5% level. Moreover, the budget reforms in the mid-1990s reduced parliamentary changes by 1.59 percentage points, significant at the 1% level. The external threat related to World War II has a large and significant positive effect, while GDP growth does not. The switch to unicameralism did not affect amendment activity.

Column (2a) reports an alternative specification of the electoral effect, which relaxes the rigid functional form imposed by the count variable. I use three remaining years in the electoral term as the reference category and estimate separate coefficients for zero, one, and two remaining years. As predicted, relative to the baseline category, all other years in the electoral term have a positive coefficient. This is in line with the hypothesis that upward revisions are more likely as elections approach. Election-year budgets are, on average, revised upward by an additional 2.43% of the executive proposal, relative to budgets at the very start of the electoral period. Similar to the results in Table 1, the equivalent coefficients for the two intermediate years are lower and not too different, and they are estimated with less precision. The difference between the coefficient on one and two years left in the electoral term is not statistically significant with $F(1, 84) = 0.08$ ($p = .78$), while the difference between zero and one years ($F = 2.48$, $p = .12$) as well as zero and two years ($F = 2.14$, $p = .15$) gets very close. The count variable provides an imperfect but reasonable approximation of the electoral effect, and it conserves degrees of freedom. Column (3a) presents results with an alternative electoral measure, which compares regular election years with all other years. On average, election-year budgets are amended upward by an additional 1.52 percentage points. Overall, the result that impending elections tend to lead to upward revisions is not dependent on any particular specification of the electoral effect.

TABLE 2
OLS Estimates of Amendments to Executive Spending Proposal, Direct Effects

	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)
Years Left in Term	−0.69			−0.73	−0.58	−0.56
	(0.27)**			(0.27)***	(0.25)**	(0.26)**
Years Left in Term: 0		2.43				
		(0.91)***				
Years Left in Term: 1		1.12				
		(0.82)				
Years Left in Term: 2		1.33				
		(0.82)				
Election Year			1.52			
			(0.69)**			
Minority Government				−0.30		−0.15
				(1.04)		(0.97)
Coalition Government				1.36		−0.23
				(0.91)		(0.83)
Social Democrats in Government				0.35		−0.59
				(0.56)		(0.66)
Proposed Change from Previous Budget					−0.06	−0.05
					(0.05)	(0.05)
Change in Supplementary Bill					0.84	0.85
					(0.14)***	(0.16)***
Lagged Dependent Variable					0.09	0.11
					(0.19)	(0.21)
Reform	−1.59	−1.54	−1.81	−1.73	−0.47	−0.39
	(0.49)***	(0.50)***	(0.52)***	(0.52)***	(0.54)	(0.69)
GDP Growth	0.03	0.03	0.06	0.07	0.09	0.10
	(0.22)	(0.22)	(0.23)	(0.24)	(0.24)	(0.25)
External Threat	6.43	6.47	6.42	5.06	6.87	7.01
	(1.88)***	(1.90)***	(1.87)***	(1.95)**	(1.64)***	(1.75)***
Unicameral	0.48	0.44	0.73	0.42	0.17	0.18
	(0.81)	(0.80)	(0.88)	(0.82)	(0.70)	(0.78)
Constant	2.06	−0.20	0.53	1.71	1.08	1.49
	(0.93)**	(1.41)	(1.20)	(1.29)	(0.88)	(1.01)
Observations	92	92	92	92	92	92
Adjusted R-squared	0.32	0.32	0.31	0.32	0.48	0.46

Note: Ordinary least squares (OLS) estimates with heteroskedasticity-robust standard errors in parentheses. The dependent variable is the change in total spending approved by parliament as percent of the executive budget proposal, as shown in Figure 1. See the second section for variable descriptions and the appendix for summary statistics.
*Significant at 10%; **significant at 5%; ***significant at 1%.

Column (4a) of Table 2 augments model (1a) with three measures of the party political context of budgetary decisions, indicating minority and coalition government, as well as whether the Social Democrats were in government. None of these variables affect amendment activity. It is also conceivable that these variables condition amendment cycles. However, when model (4a) is augmented with an interaction between the number of years left in the electoral term and, in turn, each of the three political variables, none of the coefficients on these interaction terms are statistically significant. When I substitute the government's seat share for the minority government indicator, the former has no effect and the other two party political variables remain insignificant. Replacing the indicator for coalition government with the number of parties in the government also makes no difference. Overall, these party political variables appear to have no influence on amendment activity.

Column (5a) augments the basic model with a lagged dependent variable, as well as the percentage spending increase proposed by the executive, relative to the expenditure total approved in the previous year, and the percentage change proposed in the supplementary bill. Out of the three additional variables, only the latter is significant. Government-sponsored amendments account for roughly four-fifths of the changes to total spending approved by parliament. The coefficient on the number of years left in the electoral term is slightly smaller than in column (1a), but it remains significant at the 5% level. Note that the coefficient on the reform variable is no longer significant in column (5a), although it retains a negative sign, reflecting that the supplementary bills were discontinued with the reform process. The data reveal electoral cyclicality even when we account for various possible avenues of strategic interaction. Column (6a) shows that the simultaneous inclusion of the three party political variables makes no difference.

The results could be distorted by the inclusion of the war years and the associated temporary departure from established political dynamics. The core results in Table 2 hold even when I exclude the years in the run-up and during World War II from the regressions, but there are some differences. Excluding the 1937–38 to 1945–46 budgets and rerunning model (2a), all three of the separate dummies for the number of years left in the electoral term become statistically significant at the 10% level or higher. In model (4a), the coefficient on coalition becomes statistically significant at the 10% level, and the same applies to the coefficient on budget process reform in model (5a). The latter also gets closer to statistical significance in model (6a). Overall, the election effects reported in Table 2 are robust to the exclusion of conflict years, which tends to strengthen the precision of some estimates.

Next, I allow the effect of elections on budgetary changes to differ before and after the procedural reforms in the mid-1990s. This involves augmenting the regression reported in column (1a) with an interaction between the number of years left in the electoral term and the budget reform indicator. If the reforms dampened the electoral cycle, the coefficient on the number of years left in the electoral term should increase in magnitude, and we should obtain a positive and significant coefficient on the interaction term. Column (1b) in Table 3 reports the results. The coefficient on the number of years left in the electoral term now refers to the prereform period (Kam and Franzese 2007), and it is larger in absolute terms than the corresponding coefficient in column (1a) for the entire sample period. Moreover, the coefficient on the interaction term has the expected positive sign, is of similar magnitude, and significant at the 1% level. As a result, the postreform coefficient on the number of years left in the electoral term is $-.86 + .87 = .01$, with a standard error of .26. The remaining columns (2b) to (6b) in Table 3 augment the corresponding models (2a) to (6a) in Table 2 with the relevant interaction. In each model, the magnitude of the coefficient(s) on the electoral measure(s) increases, compared with the corresponding coefficient(s) in Table 2. At the same time, the coefficients on the interaction terms wipe out any electoral effect. These results confirm that electoral cyclicality in amendments was absent after the institutional reforms.

The results in Table 3 support the hypothesis that the reform of the budget process eliminated electoral cyclicality. The electoral effects reported in Table 2 average differential effects across two distinct periods. However, it is possible that the electoral effect weakened already prior to the mid-1990s, and perhaps more gradually. Building on Chow (1960), the Quandt likelihood ratio (QLR) test for coefficient stability can provide an additional check that theory and historical knowledge have indeed helped to pinpoint the most likely point in time at which the break occurs (Quandt 1960). It can also detect the slow evolution of the regression function, for instance due to weakening local ties or the increasing authority of party caucuses. The basic idea behind the QLR test is that, if there is a distinct break in the regression function, the date at which the largest Chow statistic occurs is an estimator of the break date. The test implemented here is based on the regression model reported in column (1a) of Table 2, but excluding the two variables which assume precise break dates, that is, the unicameralism and budget reform indicators. The results are reported in Figure 2. Testing for a shift in the constant and the coefficient on the years left in the electoral term, the F-statistic increases sharply and surpasses the relevant critical value just

TABLE 3
OLS Estimates of Amendments to Executive Spending Proposals, Conditional Effects

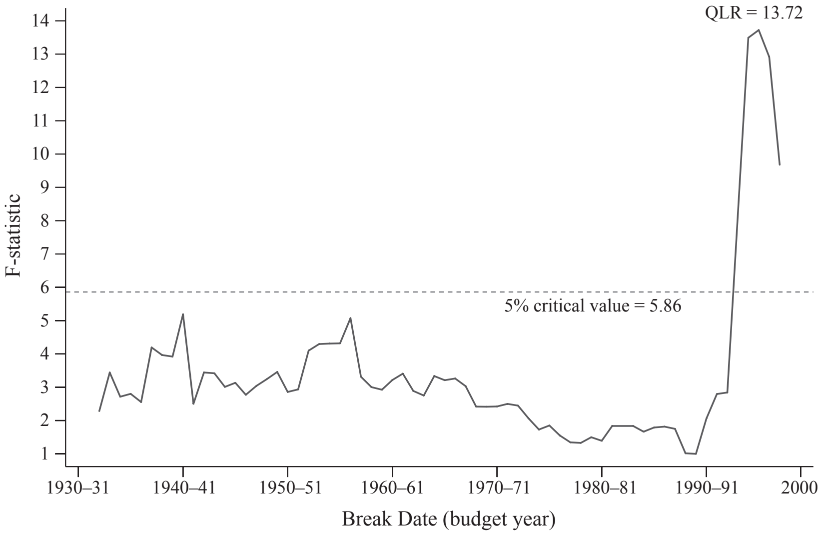
	(1b)	(2b)	(3b)	(4b)	(5b)	(6b)
Years Left in Term	−0.86			−0.89	−0.69	−0.67
	(0.31)***			(0.30)***	(0.27)**	(0.28)**
Years Left in Term × Reform	0.87			0.77	0.55	0.50
	(0.33)***			(0.38)**	(0.35)	(0.38)
Years Left in Term: 0		3.11				
		(1.06)***				
Years Left in Term: 0		−3.18				
× Reform		(1.08)***				
Years Left in Term: 1		1.56				
		(1.01)				
Years Left in Term: 1		−1.53				
× Reform		(0.97)				
Years Left in Term: 2		1.81				
		(1.03)*				
Years Left in Term: 2		−1.82				
× Reform		(1.00)*				
Election Year			1.81			
			(0.78)**			
Election Year × Reform			−1.73			
			(0.92)*			
Reform	−2.79	0.25	−1.34	−2.79	−1.19	−1.06
	(0.50)***	(0.92)	(0.64)**	(0.55)***	(0.57)**	(0.67)
Years Left in Term |	0.01			−0.11	−0.14	−0.17
Reform = 1	(0.26)			(0.35)	(0.34)	(0.38)
Controls as in Table 2	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)
Observations	92	92	92	92	92	92
Adjusted R-squared	0.32	0.31	0.31	0.32	0.48	0.46

Note: Ordinary least squares (OLS) estimates with heteroskedasticity-robust standard errors in parentheses. The dependent variable is the change in total spending approved by parliament as percent of the executive budget proposal, as shown in Figure 1. All models include the controls from the equivalent model reported in Table 2, plus a constant. See the second section for variable descriptions and the appendix for summary statistics.
*Significant at 10%; **significant at 5%; ***significant at 1%.

prior to the institutional reforms. The interactive specifications in Table 3 are based on an appropriate break date.

In the future, this aggregate analysis could be complemented with a more fine-grained look at individual amendments and how they affected the various constituencies. Unfortunately, any records of the proceedings in committees and debates held in party groups are not public documents. The information contained in formal committee

FIGURE 2
Quandt Likelihood Ratio Test for Coefficient Instability



Note: At a given break date (budget year), the F-statistic tests for a break in the coefficient on the years left in the electoral term or the intercept in the following regression equation: *Change in spending*$_t = \beta_0 + \beta_1$*Years left in term*$_t + \gamma_1[D_t(\tau) \times$ *Years left in term*$_t] + \gamma_2 D_t(\tau) + \beta_2$*GDP growth*$_{t-1}$ + β_3*External threat*$_{t-1} + \varepsilon_t$. The subscript t refers to the fiscal period to which a budget applies, with approval in the preceding period $t - 1$. Using the conventional 15 percent trimming, τ denotes a break date (budget year) between 1932–33 and 1998. $D_t(\tau)$ is a binary variable that equals 0 before the break date and 1 after. Under the null hypothesis of no break, $\gamma_1 = \gamma_2 = 0$. The 10, 5, and 1 percent critical values are 5.00, 5.86, and 7.78, respectively (Stock and Watson 2007, 568).

reports on budgetary justifications and debates is also limited. Nonetheless, some anecdotal evidence from the parliamentary records is consistent with the quantitative analysis. Parliamentary debates in the early years often highlighted neglected needs, and the government faced accusations of "ruthless thrift" (hänsynslös sparsamhet).[13] Cost of living adjustments to salaries attracted particular attention in the 1920s, with government defeats on this matter so regular that one finance minister lamented it had become "constitutional practice" (konstitutionell praxis).[14] A closer look at which departments benefited from upward amendments reveals some patterns in line with the theory. The Ministry of Civil Affairs (Civildepartementet), mainly responsible for local government affairs, was a beneficiary of parliamentary review in several years. For instance, in the election-year budget for 1960–61, its allocation was amended upward by 45%. Yet, the exact items of expenditure that bring electoral benefits to parliamentarians are likely to differ across constituencies and over time. Moreover, the changing classification of budgets, the introduction of new programs and the demise of others, as well as reorganizations of ministerial structures and responsibilities make it extremely difficult to consistently trace the evolution of particular spending items over a longer period.

Conclusions

This article contributes a new framework to study legislative behavior across the electoral cycle and the implications for public spending choices. It highlights that legislators' incentives to amend the executive spending proposal can depend on the proximity of elections, leading to more spendthrift behavior in the run-up to popular votes. Data from the Swedish Riksdag reveal highly robust and systematic differences in parliamentary changes to executive spending proposals, which are larger prior to an election than immediately after. These effects are plausibly causal, given the rigid nature of Sweden's electoral calendar, where incumbent governments cannot manipulate the timing of the upcoming scheduled election. The election-induced cycle disappeared with the adoption of procedural reforms that make it more difficult to increase spending.

How generalizable are these results? On the one hand, the extended time series used here covers a range of political contexts, including majority and minority governments, as well as coalition and single-party governments. These variables do not appear to impose scope conditions. On the other hand, some parliaments are constrained by constitutional restrictions on amendments and the possibility for governments to push budgets through on a vote of confidence (Huber 1996). Yet, a substantial number of parliaments have unfettered amendment authority, for instance in the other Nordic countries (Denmark, Finland, Iceland, and Norway) and elsewhere in Europe (Germany, Switzerland), and limited powers might nonetheless be used to extract concessions (Hallerberg, Strauch, and von Hagen 2009; Wehner 2010). My approach should also travel well to presidential systems, where the fiscal effects of legislative budget institutions have already been established, but without considering their conditioning role for electoral budget cycles (Alesina et al. 1999; Hallerberg and Marier 2004).[15] Moreover, the cycles described here arise from legislators' incentives to please local constituencies as elections approach, which depend on the design of the electoral system. Comparative work shows that this potential exists, to varying degrees, across a wide range of countries and systems (Carey and Shugart 1995;

Hallerberg and Marier 2004; Lancaster 1986). How such tendencies can be counterbalanced by party leaders' control over legislative careers is a topic that awaits detailed comparative analysis (Martin 2012). A possible limit on generalizability is that the cyclicality described here may not be observable in some parliamentary systems where governments have extensive discretion to call an early election (Kayser 2005). Yet, on balance there are good reasons not to rule out the relevance of the findings for a potentially much wider group of cases. Ultimately, this is a topic for further empirical analysis, for which this study establishes a comparative benchmark.

The analysis raises several implications. Most fundamentally, it shows that the literature on electoral budget cycles can benefit from a more nuanced focus that distinguishes different groups of politicians and their incentives. An extension of the distributive politics approach beyond the United States offers promise as a theoretical basis for systematic comparative analysis along these lines (Lancaster 1986). Empirically, the focus on legislative dynamics in this article was underpinned by the use of budgetary variables that help to distinguish executive and legislative effects much more clearly than is possible with standard measures of aggregate fiscal performance. My approach requires going beyond off-the-shelf datasets, but the results suggest that the payoffs of further work in dusty archives are potentially large. A second implication for comparative scholars is that executive dominance over the parliamentary arena should not be too hastily asserted (Esaiasson and Heidar 2000). The Swedish data show that, in most years, the spending plans proposed by the government changed significantly during the parliamentary approval process. The amounts involved are often small relative to the size of the total budget, but not always. Moreover, the cumulative effect of many small changes can be substantial. The finding that the executive does not accommodate upward amendments by proposing lower growth in spending suggests that the legislative dynamics picked up here also have aggregate fiscal consequences. This aspect deserves further investigation. For instance, cross-national studies should explore whether electoral cycles in fiscal policy are conditional on legislative budget institutions.

In terms of policy implications, the findings contribute to a body of literature that highlights the importance of budget institutions in managing fiscal policy. The global financial and economic crisis that started in 2007 has left many countries with unprecedented debt burdens. Governments across the world face a daunting challenge of fiscal adjustment and consolidation, which can only be achieved with supportive institutional arrangements (International Monetary Fund

2009, 41). In these circumstances, fiscal indiscipline during the legislative process can all too easily derail any carefully prepared fiscal strategies and undermine economic stabilization. My analysis suggests that the types of reforms implemented in Sweden in the mid-1990s can play an important role in containing spendthrift tendencies in legislative bodies. Similar arrangements can support efforts elsewhere to regain control over deficits and debt.

Joachim Wehner <j.h.wehner@lse.ac.uk> *is Associate Professor of Public Policy at the London School of Economics and Political Science, Houghton Street, London WC2A 2AE, United Kingdom.*

DATA APPENDIX

Change in spending: The difference between total expenditure approved by parliament and total expenditure in the executive budget proposal, as percent of total expenditure in the executive budget proposal. *Source*: Riksdagens protokoll (various years).

Change in supplementary bill: The difference between total expenditure in the supplementary bill (kompletteringspropositionen) and total expenditure in the executive budget proposal, as percent of total expenditure in the executive budget proposal. *Source*: Riksdagens protokoll (various years).

Coalition government: Indicator variable set equal to 1 if a coalition government submitted the budget, 0 otherwise. *Sources*: Europa World Yearbook; Statesman's Yearbook; Bergman (2000); author's research.

Election year: Indicator variable set equal to 1 if *Years left in term* is equal to 0, 0 otherwise.

External threat: Indicator variable set equal to 1 for all budgets approved between 1937 and 1945, 0 otherwise.

GDP growth: GDP growth, annual percentage. *Sources*: Edvinsson (2005); International Monetary Fund (2012).

Minority government: Indicator variable set equal to 1 if the budget was submitted by a minority government, 0 otherwise. *Sources*: Europa World Yearbook; Statesman's Yearbook; Bergman (2000); author's own research.

Proposed change from previous budget: The difference between total expenditure proposed by the executive and total expenditure approved by parliament in the previous budget, as percent of total expenditure approved by parliament in the previous budget. *Source*: Riksdagens protokoll (various years).

Reform: Indicator variable set equal to 1 for all budgets adopted for 1997 onwards under a reformed parliamentary procedure, 0 otherwise.

Social Democrats in government: Indicator variable set equal to 1 if the budget was submitted when the Social Democrats were in government, 0 otherwise. *Sources*: Europa World Yearbook; Statesman's Yearbook; Bergman (2000); author's own research.

Unicameral: Indicator variable set equal to 1 if the budget was approved by the unicameral Riksdag established in 1971, 0 otherwise.

Years left in term: The number of the years remaining in the electoral term. This variable is set to 0 if the budget covers a fiscal year in which a scheduled election was held, $t - 1$ in the year after an election, and so on, where t represents the length of the electoral term (either 3 or 4, depending on the period) and only counting full years. *Source*: http://en.wikipedia.org/wiki/Elections_in_Sweden

TABLE A1
Summary Statistics

Variable	Obs	Mean	SD	Min	Max
Change in Spending	92	1.76	3.52	–10.22	18.31
Change in Supplementary Bill	92	0.96	1.75	–0.64	8.63
Coalition Government	92	0.32	0.47	0	1
Election Year	92	0.27	0.45	0	1
External Threat	92	0.10	0.30	0	1
GDP Growth	92	2.98	3.21	–9.30	11.30
Minority Government	92	0.66	0.48	0	1
Proposed Change from Previous Budget	92	7.35	6.90	–6.25	29.46
Reform	92	0.17	0.38	0	1
Social Democrats in Government	92	0.72	0.45	0	1
Unicameral	92	0.45	0.50	0	1
Years Left in Term	92	1.39	1.09	0	3

Note: SD = standard deviation.

NOTES

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1. For instance, Canes-Wrone and Park (2012) analyze how election-induced policy uncertainty leads private actors to postpone investment decisions.

2. Lancaster (1986, 76) points out that the underlying dynamics of distributive politics are not limited to political systems with territorially defined constituencies.

3. One of their regressions assesses whether elections have a direct effect on fiscal policy outcomes (Hallerberg and Marier 2004, 582). They find no significant effect.

4. In addition, the first elections to the unicameral Riksdag in 1970 took place two years prior to the expiry of the electoral term.

5. In 1952, the d'Hondt method for the Second Chamber was replaced by the Sainte-Laguë method. The change to a unicameral Riksdag in 1970 saw a move to complete proportionality with a 4% threshold, initially using 40 out of 350 (349 as of 1976) seats to adjust representation. A "positive" preference vote was introduced in 1998.

6. Due to a system of evenly staggered indirect elections, amendment incentives in the First Chamber are not obviously linked to the electoral cycle at the national level. However, strong local linkages may have contributed to the overall *level* of amendment activity and the tendency towards augmentation.

7. Sweden adjusted its fiscal year during the sample period, abandoning the "broken" budget year for the calendar-year model that is most common in Europe and elsewhere (Tarschys 2002). The budget year initially covered July 1 to the following June 30. The 1995–96 budget was extended to cover an 18-month period, so that from 1997 onwards, the budget year coincided with the calendar year.

8. Some budgets also include nonappropriated lines, which have varied over the years. For instance, a line for "net additional spending needs" (beräknat tillkommande utgiftsbehov, netto) appears in some budgets, indicating the cost of further initiatives that the government was planning to propose at a later stage. We net out such lines so that we analyze appropriated expenditures only. When we include any nonappropriated amounts, this does not affect the pattern of results reported below.

9. The test statistics (1% critical values) are −6.23 (−3.52) without and −6.29 (−4.06) with a linear time trend.

10. I also collected data from Angus Maddison (http://www.ggdc.net/Maddison/) and Thorsten Persson (http://people.su.se/~tpers/sweddat.htm). None of the sources provide GDP growth data for the entire sample period. The reported results do not depend on any specific data source.

11. For the bicameral period up to 1970, this measure is based on the pooled seats across both the First Chamber and the Second Chamber, as a majority of these pooled votes was necessary to pass budgetary measures (Bergman 2000, 201). When I consider only seats in the directly elected Second Chamber, this has no substantive effect on the results. The same applies when I use the seat share of the government instead of the minority government indicator.

12. I also experimented with the number of parties in government. This did not affect the reported results.

13. Carl Gustav Ekman cited from the parliamentary yearbook for the 1924–25 session (FK 1924:3, 9). I thank Ingvar Mattson for supplying the quotes in this paragraph.

14. The quote is from a speech by Jakob Beskow in the First Chamber (FK 1924:37, 17).

15. One possible limit of the approach is in systems where the budget originates in the legislature without an executive proposal. However, in practice, the executive budget proposal is "the authoritative metric for measuring legislative action" (Schick 2002, 21) in most countries.

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