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Is there a political business cycle in Sweden?

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1.1 Abstract

This paper tests if Sweden experienced a political cycle in fiscal instruments over the period 1982-2019 and the shorter period 1982-1996. The cycle is tested for both the growth rate in transfers received by households and for the growth rate in taxes paid by household. The cycle in both instruments is tested for quarters during election years and the paper is therefore mainly an incursion into opportunistic political business cycle theory. Conditional hypotheses are tested using interactive models to see if performance in a poll affect the cycle.

Conditional hypotheses are also used to test if left-wing governments induce a stronger cycle in transfers and if right-wing governments induce a stronger cycle in taxes. No definitive conclusion is drawn over the period 1982-1996 as the model and short timeframe likely reduce the robustness of the results. For the period 1982-2019 the results indicate a cycle in transfers conditional on performance in the poll and that the effect was different for left-wing governments. Further research is however required to establish if there really has been a political cycle in Sweden.

Keywords: Political business cycle, fiscal instruments, conditional effects, partisan differences

1.2 Introduction

In chapter 2 the paper begins with the theory behind the political business cycle. Focus is centered on the opportunistic cycle and the progression in theory is described. Previous empirical research in the area is also mentioned. At the end of the chapter the hypotheses are presented. In chapter 3 the macroeconomic developments in Sweden during the period are briefly described with emphasis on the fiscal framework. In chapter 4 the empirical analysis starts. First with a description of the model used, followed by a description of the data. In chapter 5 the results are presented. First for transfers and then for taxes. In chapter 6 the results are discussed in conjunction with theory and previous findings by other researches. The paper ends with a brief conclusion.

2.1 The Political Business Cycle

The term a political business cycle can in part be credited to Kalecki (1943) who derived cyclical fluctuations from pressure exerted by “the captains of industry” that influence the political establishment. Downs (1957) discussed the premises of rationality for both voters and incumbent politicians, in doing so he argued against the assumption that politicians are solely interested in maximizing social welfare. “Even if social welfare could be defined,...What reason is there to believe that the men who run the government would be motivated to maximize it” (Downs 1957, p136). It was Nordhaus (1975) that is famed for the presentation of a model in which politicians are the inducers of macroeconomic cycles because of their self-interest to remain in power. Herein lies the origin of the opportunistic theory of political business cycles. PBC theory rests on two pillars, one is the opportunistic and the other is partisan theory. PT is the theory that see preferences of incumbents as heterogeneous and this is what generates a political business cycle. Hibbs (1977) theorized a model on the assumption that right-wing governments would fight inflation to a larger extent and left-wing governments would accept higher inflation to keep unemployment low and this in many ways is the origin of PT theory. Both pillars and their subsequent developments therefore derive a cycle induced by politicians in government but the reasons as to why the cycle exists differs.

Nordhaus (1975) opportunistic model included the assumption that elections are exogenous. Meaning that the timing of elections is fixed in time and given outside the system. Nordhaus also assumed politicians have complete control over fiscal and or monetary policy needed to induce a cycle. Lastly, he assumed voters were myopic, meaning they heavily discount past observations and are blissfully unaware of the macroeconomic connection between inflation and unemployment. Nordhaus envisioned that incumbents would increase spending to boost aggregate demand before an election to lower unemployment. Faced with the resulting higher inflation post-election the economy would be contracted with a disinflationary policy that instead increase unemployment. In principle incumbents exploit the short-term Phillips-curve trade-off between unemployment and inflation Phillips (1958). The latter has lag explained by sticky wages resulting in lower unemployment before the impact of inflation is felt by the electorate. Nordhaus original model has its limitations especially in its crucial assumption of

myopic voters as politicians can and will use the same strategy every time. This leads to a symmetrical PBC in all elections.

For opportunistic PBC theory the advancement of rational expectations in economics meant a revision of previous models. Rogoff and Sibert (1988) and Nordhaus (1989) among others incorporated rational expectations leading to a RPBC model in which voters no longer are myopic and unaware of causal effects. Rogoff and Sibert (1988) modelled how voters albeit rational receive information about the government's performance with a lag. This generates information asymmetry between incumbents and voters. The government signals its competency in macroeconomic performance but as this is observable with a lag by voters there are incentives for distortions prior to elections. Distortions would be boosts in instruments to make voters believe competency is high. The government needs to make up for distortions after the electoral cycle and as macroeconomic performance is dependent on competency level it is beneficial for competent governments to induce a cycle. The cycle is limited to fiscal manipulation in government spending, deficits or money growth, so only in macroeconomic instruments.

More recent developments in opportunistic political business cycles include the moral-hazard model presented by Persson and Tabellini (2000). The moral hazard model builds on the premise of the principal-agent problem. In which there is an incentive for government to take on risk as it does not fully bear the cost of adverse macroeconomic performance. In the moral hazard models' governments have the same incentive structure to manipulate regardless of competence. The key as to why manipulation occurs is that governments have control over instruments which are only observable with delay or hidden from the electorate. The cycle would be observable in budget deficits with excessive borrowing but not in instruments that are clearly observable for the electorate. In the moral-hazard model unlike in the model conceived by Rogoff and Sibert (1988) the political cycle is entirely wasteful because it does not convey information to the voter in regard to the competency of the government.

The opportunistic approach to political business cycles have progressed over time. First with the assumption of non-altruistic politicians that exploit the short-term tradeoff between inflation and unemployment, reduce unemployment before an election today in order to combat inflation tomorrow. Later with the incorporation of rationality to voters reshaping the nature of the cycle to a more limited one in instruments. As voters do not have full information a competent government has an incentive to induce a cycle as it later down the line as it has the capacity to combat the resulting adverse macroeconomic outcome. More recent developments include the moral hazard-model in which all governments have the same incentive structure to pursue a hidden pre electoral expansion. The cycle is only wasteful as it does not help the electorate to distinguish competency.

2.2 Previous empirical research

Research into political business cycles can be divided into primarily theoretical models centered around equilibrium and empirical research. Empirical research is focused primarily on testing of the assumptions predicted by theory and subsequent developments of empirical findings that are not necessarily theorized.

Early empirical research focused on the testing of the symmetrical cycle that Nordhaus (1975) theorized and which he also backed up with his own empirical testing. The results were ambiguous. MacRae (1977) found evidence of an electoral cycle in the US rationalized by vote-loss minimizing behavior by politicians. Under the dynamic relationship between unemployment and inflation, given a myopic electorate, McCallum (1978) conversely found no indication of any political effect in unemployment over US data. Empirical research quickly altered following the progression in theory and the lack of significant findings of an electoral cycle in unemployment and inflation in later research.

Alesina et al (1991) tested both the original repetitive and symmetrical cyclical fluctuations in unemployment and inflation, and the more limited pre and post-electoral cycle that followed from RPBC in a sample of OECD democracies. And as Alesina et al (1991) put it: “The first set of tests (on policy outcomes) in our view, overwhelmingly rejects the political business cycle hypothesis. The second set of tests (on policy instruments) has yielded mixed results” Alesina et al (1991, p5). Almost countless empirical studies into different fiscal instruments exist and have provided many significant results indicating the existence of a cycle in coherence with elections

Alesina et al (1991) reported some significant results in both money growth and fiscal deficits. Schultz (1995) found significant results in transfers, Mink and De Haan (2006) Found significant results of a fiscal cycle in budget deficits for EU. Klomp and De Haan (2013) found that elections had a significant short run effect during elections with increased public spending and reduced budget balances for 70 democratic countries during the period of 1970-2007. Alesina and Paradisi (2017) found significant results of manipulation of flexible real estate taxes in local elections in Italy. Most empirical studies in instruments incorporate additional hypotheses regarding possible factors that facilitate, alter and shape the

nature of the cycle. Statistically significant results imply that the cycle in instruments is indeed conditional on other factors.

Schultz (1995) found that the government increased transfers inversely with its standing in opinion polls. Implying that if the government has a comfortable lead in polls its marginal benefit of excessive spending is significantly reduced. Mink and de Haan (2006) Found that the color of parties in government impact net lending. De Haan and Klomp (2013) results indicated that an electoral cycle is stronger in countries with one party governments than systems characterized by coalitions. They also found that the cycle is stronger in low transparency countries using a proxy for corruption, democratic accountability, bureaucracy and the rule of law. Alesina and Paradisi (2017) showed a significant adverse effect on deficits due to the manipulation in tax rates indicating an intertwined effect. They also found that the political composition in local governments impacted the magnitude of the tax rate. In economics the macroeconomic framework, and its subcomponents the monetary and fiscal frameworks, restrict and impact how macroeconomic instruments function. How this affects the cycle in instruments has also been studied in empiric research.

In sum empirical research into fiscal instruments have provided results indicating that there seems to be a PBC in different types of instruments and that the effect of the PBC itself is conditional on other factors.

2.3 Hypotheses

In order to test if there were an opportunistic political cycle present in Sweden during the period 1981-2019 a set of hypotheses have been constructed.

H1: There is a political cycle in Sweden.

In the moral hazard theory modelled by Persson and Tabellini (2000), an opportunistic cycle arises due to higher rent-seeking behavior by politicians in election years. The increased risk taking by the government could then show itself as a significant effect in a given instrument in conjunction with elections. Schultz (1995) found increased transfers, Klomp De Haan (2013) increased public spending. Alesina and Paradisi (2017) found an effect in real estate tax. Building on these studies H1 is tested for one measurement of transfers and one of taxes. Research has also shown the same expansion pattern in deficits, for example Alesina Et Al (1991). But as Sweden over the period experienced very volatile changes in public debt a conscious choice was made to not test H1 by looking at deficits.

H2: The political cycle is conditional on how the government performs in polls.

H2 is an extension to H1 which conditions the effect of a potential political business cycle in transfers and taxes on the polling of the government in the election year. H2 is primarily inspired by the findings of Schultz (1995) who concluded that the marginal benefit of fiscal manipulation seems to be inversely related to how well the government polls in the run-up to elections. Fiscal expansionism was lower when the outlook for the government was favorable and vice versa. Assuming rationality for politicians it makes sense to account for the fact that risk-taking would be lower if people support the government. The moral hazard problem of rent-seeking is dependent on governments being in office. What matters is therefore to gain enough votes to acquire a majority as the marginal benefit of more votes would decrease significantly after this point.

H3: Left-wing governments are more inclined to increase transfers and right-wing governments are more inclined to lower taxes during election years.

H3 is an extension to H2 which also conditions on government type.

H3 is not meant to test if the political cycle itself is a result of political heterogeneity as hypothesized in PT theory by Hibbs (1977), but it instead tests if a cycle in transfers and

taxes is magnified when different government types are in office. H3 is as such inspired by Mink and de Haan (2006) who found differences attributed to party affiliation, and Alesina and Paradisi (2017) dependence on the political composition in local governments.

Ever since Nordhaus (1975) the opportunistic political cycle rests on the assumption that politicians have actual power over macroeconomic outcomes. There can be no political cycle unless political agents have power over instruments that can alter the economy. Empirical studies imply that macroeconomic structures can change and alter the political cycle. Clark and Hallberg (2000) showed an impact in policy with a change in exchange rate regime and level of independence of the central bank. Rose (2006) found a reduced effect for balanced budgets and Bastida Et al (2013) that budget rules alter the political cycle. There is therefore reason to believe that the fiscal and monetary developments in Sweden over the period could have reshaped or possibly altered a political cycle. Both because the role of fiscal instruments has changed and because politicians control over them have changed. To also account for this fact H1, H2 and H3 will be tested over both the entire period and an over more limited period before 1997. The reason as to why 1997 has been chosen is due to the developments in the macroeconomic framework in Sweden during the period.

3.1 Background Sweden

Developments in the Swedish macroeconomic framework 1981-2019 is in several regards the result of adverse macroeconomic shocks, starting in the wake of the first and second oil price shocks (OPEC1 and OPEC2). The collapse of the Bretton-woods system in the early seventies facilitated the rise of international capital markets and Sweden turned to international capital markets in order to finance increasing public expenditures. In 1974 a meager 0.1 percent of the national debt consisted of external borrowing but in 1983 this share had increased to 21 percent (Andersson, Jonung, 2019). Minor consolidation of the public finances took place in the mid 1980s, but the state of public finances was still very weak. Deregulation of financial markets in 1985 fueled cheap credit (Andersson, Jonung, 2019) which subsequently lead to a public debt's crisis in 1990.

3.2 Fiscal Framework

As the track record of unsustainability of public finances was apparent a review of the budgetary process was initiated inspired by a study on the EU-level conducted by Jürgen von Hagen (1992), (Andersson, Jonung, 2019 p.9) (Calmfors, Lars, 2015 p.12). The study by Jürgen von Hagen argued that the parliament could easily add on expenditures beyond what was requested by the government (Andersson, Jonung, 2019 p.9). A string of weak governments and a short election period of three years magnified the problem by giving strong incentives for government spending without any restraints on the overall spending (Andersson, Jonung, 2019 p.9). This led to sweeping reform of the fiscal framework process instigated in 1994 (Calmfors, 2015p.11). The revised fiscal framework swung into full effect in 1997 (Molander, Holmquist, 2013). An extension of the numbers of terms in office also led to four-year terms from 1994 and onwards. The rework of the budgetary procedures meant a new top-down approach with votes on total expenditure bounds for expenditure areas, withdrawal of entitlements which previously operated outside the main budget, and the removal of all entitlements without an upper boundary. The dismantling of open-ended appropriations including the social security system was also implemented. Furthermore, a calendar year was introduced, previously the old one started in June (Blöndal, 2001) (Molander, Holmquist, 2013).

A surplus target set at 2 percent of GDP over the business cycle was also announced in 1997 and gradually came into effect until fully implemented in 2001 (Andersson, Jonung, 2019). It has since been revised twice, lowered to 1 percent in 2007 with no general impact on government policy and again to $\frac{1}{3}$ percent of GDP in 2016 (Andersson, Jonung, 2019). The implementation of the new fiscal apparatus meant that spending beyond the amount set for each of the 27 expenditure areas were no longer possible post 1997. Ceilings on total government spending and less interest payments on government debt were also changed to be set in advance on a three-year basis via vote in parliament. The parliament retained power to change the ceilings after the reform, but the track record is that it has refrained from this with the exemption of technical adjustments and elections after which a new government has been installed (Andersson, Jonung, 2019 p.9). This means that new governments are not forced to abide by ceilings set by preceding governments.

The budgetary reforms in the late 90s were the most far reaching reforms in the fiscal area over the time period covered and it has continued to evolve over time. In 2007 the Swedish fiscal policy council was set up tasked with assessing the compliance with the fiscal framework (Andersson, Jonung (2019). The FPC is limited in formal powers but it acts as an authoritative commentator (OECD 2017 sid 11)(Andersson, Jonung (2019). The latest addition to the Swedish fiscal framework is the debt anchor introduced in 2016 and implemented in 2019 limiting Maastricht debt to 35 percent of GDP within a margin of 5 percent (Andersson, Jonung (2019). The evolving fiscal framework has over the period meant more clear rules for government expenditure, budget balance, debt and supervision. However the fiscal framework remains in the hands of politicians as the government can change ceilings and there is no legally binding mechanism for government to follow either surplus goal or debt anchor as long as it has support in parliament (Andersson, Jonung 2019). The top down approach to the budget procedure implemented in 1997 meant that the situation for minority governments significantly improved. This is because allocation of the budget within the 27 expenditure areas is voted on as a package and settled in one vote. In parliamentary practice the budget bill is therefore taken as all or nothing settled in one singular vote (OECD,2017 p.31). In order for a majority opposition to block the proposal by the government it must therefore unite behind one alternative (Molander, Holmquist, 2013 p.14). This happened in 2014 when the incumbent social democrats lost the vote on the budget bill and subsequently governed with the budget of the opposition. However since the reforms of 1997 budgetary amendments have been uncommon in parliament (OECD 2017 p.31).

3.3 Monetary framework

Fiscal reforms were however only one side of far reaching macroeconomic reform that occurred in the covered period. In the 1980s the central bank was not separated from the government and monetary policy objectives were not clearly defined. Rampant inflation in the mid-1980s following an inflation-devaluation cycle put the fight against inflation at the center of gravity. In 1992 Sweden abandoned its fixed currency regime after an interest hike triggered by expectations of a devaluation. In 1993 the central bank adopted an annual inflation target of 2 percent, a target that still remains (Riksbanken.se). The importance of an independent central bank was subsequently underlined in several government commissions (Calmfors, Lars, 2015). In 1999 the central bank was made independent and after this there was a major shift as fiscal policy was reduced in role and scope significantly (Andersson, Jonung 2019).

In sum Sweden has seen significant changes to its macroeconomic framework in the covered period. Of pivotal importance for fiscal spending were the new fiscal framework in 1997 and the reduction in importance for fiscal spending following the independence of the central bank in 1999. The reforms in 1997 imposed greater scrutiny over public expenditures and meant spending beyond allocation in the 27 spending areas were no longer possible outside of the books. Hence from 1997 onwards politicians ability to exert a hidden effort to manipulate fiscal spending should have been significantly reduced alongside a reduced informational advantage. In 1999 the role of fiscal spending was significantly reduced to only constitute “automatic stabilization” N.G Andersson, Jonung (2019). But for the purpose of testing the hypotheses 1997 was a more significant year as the ability of politicians to exert a hidden effort was reduced without reducing the role of fiscal policy

4.1 Empirical Analysis

4.2 Model Specification

To test the hypotheses a least squares regression model in EViews have been employed. All regression are multivariate and dynamic versions of least squares as a lagged dependent variable is used as an explanatory variable for both transfers and taxes. The complete model takes the form:

$$y_t = \delta_i Gov_t + \delta_i Elect_t + \theta_i Lead_t + \theta_i Lead_t * Elect_t + \theta_i Gov_t * Lead_t + \delta_i Gov_t * Elect_t + \theta_i Gov_t * Lead_t * Elect_t + \alpha + \beta_i y_{t-1} + \beta_i Controls_{t-1} + \beta_i Controls_{t-2} + \beta_i Controls_{t-3} + \varepsilon_t$$

y_t is the dependent variable (either transfers or taxes) at time t , Gov is a dummy for either a left-wing government ($Lgov$) or right-wing government ($Rgov$), $Lead$ is the change in percent between the largest party in governments result in the poll compared to its result in the previous election, $Elect$ is a dummy for an election year. α is a constant, $Controls$ constitute the explanatory variables used as controls and lastly ε is an error term.

The coefficients of: Gov ($Lgov$ or $Rgov$ in the tables), $Elect$ and $Lead$ and the products of these variables (interaction terms) are reported in the regression tables and they are used to answer the hypotheses. As the hypotheses are conditional in nature the conditional effects have also been tabulated. The coefficients for Gov and $Elect$ and the interaction terms containing only Gov and $Elect$ are marked with δ_i this is to indicate that the conditional effect is the same as the coefficient in the output. It does not vary as both dummies will only ever take on the values one and zero. As the hypotheses seek to determine if there is as cycle in an election year $Elect$ will always be one. $Rgov$ and $Lgov$ will always be one, as the cycle is hypothesized to also be stronger for a right-wing and left-wing government. For any interaction term containing $Lead$ the conditional effect varies for the values of $Lead$ and therefore coefficients are marked with θ_i . This means that the coefficients in the output indicate the direction of the conditional effect for $Lead$. If the coefficient for any term consisting $Lead$ is positive it means the conditional effect will increase when $Lead$ does. This is because its hypothesized that the cycle depends on how the government is polling.

To test if there is an political cycle in election years (H1) only two dummies are added to the regression with the dependent variable and the control variables: $y_t = \delta_1 Gov_t + \delta_2 Elect_t + \alpha + \beta_i y_{t-1} + \beta_i Controls_{t-1} + \beta_i Controls_{t-2} + \beta_i Controls_{t-3} + \varepsilon_t$

The coefficient δ_1 represents the effect in the dependent variable for either a right or left-wing government and will serve as an indicator if it is reasonable to assume there is a significant difference for different government types. The coefficient δ_2 will show if there is a significant effect in the dependent variable during election years and hence it answers H1.

To test H2 an interaction term is included with all its constituent terms:

$$y_t = \delta_1 Elect_t + \theta_1 Lead_t + \theta_2 Lead * Elect_t + \alpha + \beta_i y_{t-1} + \beta_i Controls_{t-1} + \beta_i Controls_{t-2} + \beta_i Controls_{t-3} + \varepsilon_t$$

H2 is a conditional hypothesis that says that the political cycle is conditional on the performance of the government in the poll and therefore differentiation is necessary. As shown by Kam and Franzese (2007, p22) this will produce the correct expression for the conditional effect of interest:

$$\frac{\partial y_t}{\partial elect_t} = \delta_1 + \theta_2 * Lead$$

The product of: $\theta_2 * Lead$ constitute the conditional effect for a given values of *Lead*. It is then the sum of $\delta_1 + \theta_2 * Lead$, for different values of *lead* that answers how the effect in the dependent variable varies with *Lead*. The conditional effect will first be tested for significance using a Wald-test which will determine if the conditional effect is statistically different from zero. If the conditional effect is significantly different from zero, the

$$\text{conditional variance: } \sigma^2\left(\frac{\partial y_t}{\partial elect_t}\right)$$

must be calculated to test significance for individual values of *lead* and for the value 1 for *elect*.

To test H3 the complete model including all interactive terms is used:

$$y_t = \delta_1 Gov_t + \delta_1 Elect_t + \theta_i Lead_t + \theta_i Lead * Elect_t + \theta_1 Gov * Lead_t + \delta_2 Gov * Elect_t + \theta_2 Gov * Lead * Elect_t + \alpha + \beta_i y_{t-1} + \beta_i Controls_{t-1} + \beta_i Controls_{t-2} + \beta_i Controls_{t-3} + \varepsilon_t$$

H3 says that the political cycle is conditional on government type in addition to performance in the poll and to answer H3 differentiation is again used: $\frac{\partial y_t}{\partial gov_t} = \delta_1 + \theta_1 * Lead + \delta_2 + \theta_2 * Lead$

A Wald-test is then used to test if conditional effect is significantly different from zero. To answer H3 the significance of the conditional effect for different values of *Lead* is again tested after computation of the conditional variance $\sigma^2(\frac{\partial y_t}{\partial gov_t})$, given that *Gov* and *Elect* are both 1. For this highly interactive relationship between correlated variables there is a very real risk that the standard errors are strained. Inflating them hence making inference harder. This is especially true if the number of observations is small (D Kam, J Franzese Jr, 2007, p41). However, the rule of thumb is not to omit constitutive terms when specifying interaction models (D Kam, J Franzese Jr, 2007). It actually runs the serious risk of both biased and inconsistent estimates of the conditional effects. Therefore, all constitutive terms have been included to test H2 and H3.

Unbiased and consistent estimates is what is desired from the model used. There are some factors however that can induce bias and inconsistency even if all constitutive terms including the products of them are included.

The first is autocorrelation which is serial correlation in the error term, this will lead to bias and inconsistency. This is an implication of using a model with a lagged dependent variable as autocorrelation usually only impact the efficiency (Bailey, A 2020). In order to make sure autocorrelation is not present Breusch-Godfrey serial correlation LM tests have been conducted.

Another problem is simultaneity which will result in biased estimates and it occurs when X causes Y and Y causes X (Bailey A, 2020, 315). As transfers to households are a subset of public expenditures, which are in of itself a part of the GDP identity, there would be potential simultaneous bias when regressing transfers on the current value of GDP. To avoid this source of endogeneity other lags for control variables have been used if there is reason to suspected simultaneity.

Furthermore, the lack of stationarity is a problem that mean a LDV model will generate invalid results (Cryer D, Chan 2008, p16-17). It is therefore important to make sure that all the time series used in the regressions are stationary. If this is not safeguarded against a spurious regression might be the result, implying that a strong causal relationship exists between the dependent and explanatory variables when there in fact is none (Bailey A, 2020). Thus, tests for unit roots in all series have been conducted using Dicky-Fuller tests.

Lastly there are two problems which does not hamper the unbiasedness nor consistency but potentially cause estimates to be imprecise. Firstly, there is the risk of correlation amongst the control variables just as for the interaction terms. Secondly there may be heteroskedastic errors. Tests for heteroscedastic errors have been conducted and when found this has been accounted for by using HAC (Newey-West) errors which adjust for the heteroskedasticity.

4.3 Data

To conduct the hypothesis testing data has been gathered from Swedish quarterly statistics over the period 1981Q1-2019Q4. Quarterly data of transfers, taxes, disposable income, unemployment, and GDP have all been taken from the Swedish bureau of statistics (SCB).

Transfers is the sum of the of public transfers (and transfers from the private sector) received by households, including unemployment benefits. Taxes is the sum of the of taxes paid by households, income taxes paid at all levels (State, Region and municipal) and property taxes are included. Disposable income is the net sum of disposable income of households.

Transfers, taxes and disposable income have all been adjusted using the consumer price index provided by the SCB. Unemployment is the estimated unemployment (random sample) in percent of the population in the ages 16-64 (1981-2000) and 15-74 (2000-2019). GDP is the real GDP.

Data from one monthly opinion poll conducted by SIFO, a major Swedish polling institute, has been used and it asks the question: "If there was an election today, which party would you vote for?". The question has been asked to a random sample of voters in Sweden. The variable *Lead* is the percentage change in support of the major party in government in a quarter to compared to its last result in an election. It has been calculated from the poll. As the raw data is in monthly form it has been adjusted to a quarterly average of support for the major party over a three-month period.

The major political party in government is the Social Democrats for left-wing governments over the entire period 1981Q1-2019Q4, Centerpartiet (1981Q1-1982Q4) and Moderaterna (1982Q4-2019Q4) for right-wing governments. The dummy *Lgov* is equal to 1 whenever the Social Democrats were the largest party in government. The dummy *Rgov* is equal to 1 when Centerpartiet (1981Q1-1982Q4) or Moderaterna (1982Q4-2019Q4) was the largest party in government.

Elect is another dummy which is equal to 1 for all quarters during an election year.

Most of the time series contain unit roots in levels and they have therefore been transformed before being put into the model in order to render the data stationary. Commonly the process is differentiated once from levels but other transformations including logarithmic and percentage changes for the purpose of generating a stationary process (Cryer, Chang 99)(Bailey A, 2020 p 481-482). Most of the time series used in the regressions have been transformed using annualized growth rates (GRT). This is to account for nonstationary and severe quarterly fluctuations that appeared in growth rates that were not annualized. They were subsequently tested again using Dicky-Fuller tests, the results are reported in *table 1* below:

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table 1

Null hypothesis: The series have a unit root

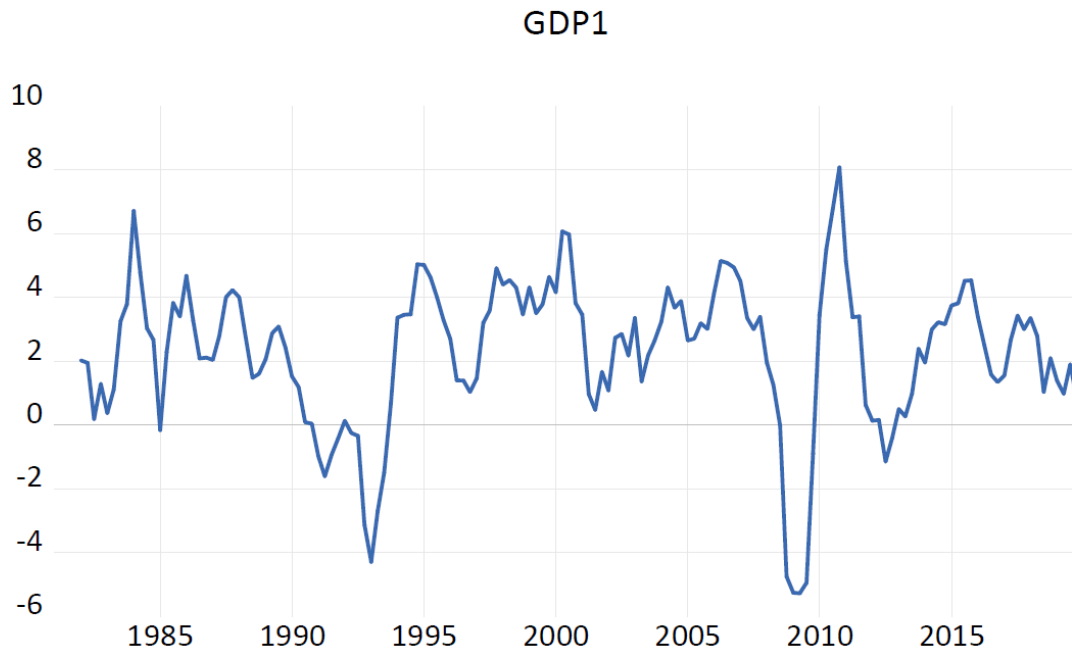
<i>Trans</i> (GRT in Transfers)	P=<0.01 ***
<i>S</i> (GRT in Taxes)	P=0.0140 **
<i>Disp</i> (GRT in disposable income)	P=<0.01***
<i>Un</i> (GRT in unemployment)	P=0.0257**
<i>GDP</i> (one period GRT in GDP)	P=<0.01***
<i>Lead</i> (standing of the largest party)	P=<0.01***

The Augmented Dickey-Fuller test reports the probability (P) that the series have a unit root under the null. Rejection of the null at the 10 percent level= (), 5 percent=(**) and 1 percent =(***) is reported in the table.*

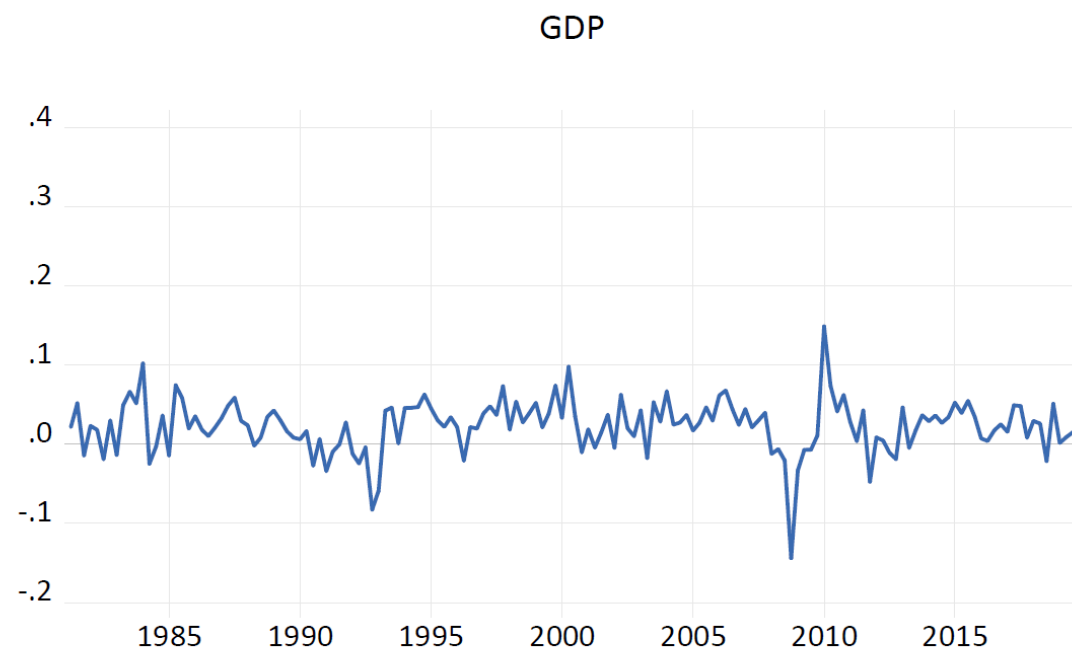
As can be induced from *table 1* the null of a unit root is soundly rejected for most series at the 1% level of significance. For unemployment and taxes, the null is rejected at the 5% level. *Lead* is not transformed to annualized growth rates because it infers with the testing of H2 and H3. The incumbent government might be interested in some form of growth rate in opinion polls as to indicate if it is gaining support. But for the purpose of testing H2 and H3 the government compares its result in the election year with how it performed in the last election. This makes sense as an incumbent government is incumbent because it gained enough votes in the last election. Things might be more complicated than this in a political situation in which many small parties make up the government as *Lead* only indicate how well the main party of the government is performing. Sweden is not a two-party system and over the period several coalition governments have indeed existed. Left-wing governments have however always had the social democrats as the leading party, whenever the party's own performance in election has been low this has predominantly led to post electoral cooperation across party-lines. As coalition building primarily has occurred post-election it makes sense that the social democrats focus on own party performance. For right-wing governments, the situation is a bit more complicated as they have consisted of more parties with more consistent coalitions. The long-term trend has been increased support for one main party on the right (Moderaterna). This development has been reversed only in the two most recent elections.

GDP is the only other series which has not been adjusted using the same formula of annualized growth rates. This is because this formula has led to very short and large cycles in the underlying data. GDP has therefore instead been transformed to one-period annualized growth rate which still captures the adverse effect of the main shocks during the period in GDP. Graph 1 show GDP in annualized growth rates, whilst Graph 2 shows the one period annualized growth rate which has been tested for a unit root in *table 1* and which constitute GDP in the regressions presented in the results.

Graph 1



Graph 2



5.1 Results

5.2 Transfers

Table 2 shows the coefficients of the two dummies *Lgov* and *Elect*, the coefficient for *Lead* and the coefficients for the interaction terms of these variables. They are together with the control variables regressed on the annualized growth rate in transfers to households. In column 1-3 the observations after adjustments are limited to the period 1982Q4-1996Q4. In column 4-6 the number of observations after adjustments cover the entire period 1982Q4-2019Q4.

Column 1 tests if there is a political cycle during election years (H1) and if there is an indication that there was a partisan effect, for 1982Q4-1996Q4. It includes two simple dummies which indicate that there was a large and significant effect of a left-wing government and an election year in transfers. The interpretation of the coefficients is that there was a 1.95% increase in the growth rate in transfers whenever a left-wing government was in office during a quarter and there was a 1.86% increase during election quarters. The sum of these coefficients is the increase in transfers attributed to quarters in election years during which the government was left-wing. Column 4 indicates that for 1982Q4-2019Q4 there was no significant effect for either a left-wing government or an election year, the R^2 is also lower. These results indicate that H1 cannot be rejected for the period 1982Q4-1996Q4 but that it is rejected for the period 1982Q4-2019Q4. These results should not be overstated as the number of observations in column 1 are only 57. Nevertheless, it indicates that before the fiscal revisions in 1997 there was political cycle in transfers that was fairly large and that the effect more than doubled when government was left-wing.

Column 2 tests if the cycle is conditional on how the government polls (H2) for 1982Q4-1996Q4. The Wald-test indicates that the conditional effect is significantly different from zero at the 1% level. The conditional effect is the differentiation with regards to *Elect* for transfers in the estimation output. So, the coefficient of *Elect* and the coefficient of *Lead*Elect*. The latter is conditional on the values of *Lead*. And as the coefficient for *Lead*Elect* is negative the effect in transfers was greater the worse the government fared in polls during election quarters. The interpretation is that the government would increase transfer with 0.62% for every 1% decrease in *Lead* in a quarter during an election year. Government would conversely decrease transfer for an increase for *Lead*. This means H2 is

not to be rejected. The conditional effect is calculated for different values of *Lead* in *table 3* as it may or not be significant for the different values.

Column 5 tests H2 for 1982Q4-2019Q4. The Wald-test indicate that the conditional effect is significant at the 5% level. As can be seen in *table 2* the interaction term *Lead*Elect* is now smaller in absolute value but still negative, implying H2 cannot be rejected. The political cycle in election years impact transfers less over the entire period but that there is still a marginal benefit consideration taking place in government. The dummy *Elect* is also negative, which means the conditional effect is lower as a result. The conditional effect is tabulated for the maximum, median and minimum of values for *Lead* in *table 4*. These results taken together indicate that H2 should not be rejected either for 1982Q4-1996Q4 or 1982-2019Q4 but the effect in transfer is stronger in the period prior to 1997.

Column 3 tests if the conditional effect is stronger when the government is left-wing (H3) for 1982Q4-1996Q4. The Wald-test indicate that the conditional effect is not significant meaning H3 is rejected. Column 6 tests H3 for 1982Q4-2019Q4. The Wald-test indicates that the conditional effect is significant at the 5% level. For the full model in column 6 the conditional effect of a left-wing government on transfers is the sum of all coefficients after differentiation with regards to *Lgov* and it is conditional on the values of *Lead*. The terms containing *Lead* include *Lgov*Lead* which is positive and *Lgov*Lead*Elect* which is negative and larger in absolute value. Which means that as *Lead* decreases the growth rate in transfers will increase. The interpretation is that a left-wing government would increase transfer with 0.25% for every 1% decrease in *Lead*, in a quarter during an election year. H3 is therefore not rejected. All coefficients in column 6 are insignificant and most have standard errors that are greater than the coefficients themselves. This is most likely due to the very high degree of collinearity amongst the interactive terms and their constituent parts. The reason the Wald-test still indicates that the conditional effect is significant is because it tests if the terms together are significant. The conditional effect for different values of *Lead* is reported in *table 5*. These results taken together indicate that H3 should be rejected for the period 1982Q4-1996Q4 but not for the period 1982Q4-2019Q4.

<i>Tabel 2</i>	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable (Trans): Growth rate in real transfers to households						
1. Leftwing Government: (Lgov)	1.95* (1.01)	..	1.29 (1.60)	0.56 (0.39)	..	0.90 (0.64)
2. Election Quarter: (Elect)	1.86*** (0.60)	0.64 (0.56)	2.61*** (0.85)	0.07 (0.49)	-0.59 (0.49)	-0.40 (0.73)
3. Government lead in poll: (Lead)	..	0.42*** (0.10)	-0.12 (0.31)	..	0.12** (0.06)	0.11 (0.10)
4. Interaction term: (Lead*Elect)	..	-0.62*** (0.07)	-0.58 (0.45)	..	-0.30*** (0.11)	-0.09 (0.16)
5. Interaction term: (Lgov*Lead)	0.60* (0.32)	0.06 (0.12)
6. Interaction term: (Lgov*Elect)	-2.26** (1.07)	-0.50 (1.02)
7. Interaction term: (Lgov*Lead*Elect)	-0.05 (0.48)	-0.31 (0.19)
8. Wald-test for joint significance Of the conditional effect	..	F=59.83 P=0.00	F=1.61 P=0.19	..	F=4.02 P=0.02	F=2.63 P=0.04
9. Number of observations:	57	57	57	149	149	149
10. R ² :	0.77	0.86	0.87	0.64	0.67	0.68
11. Breusch-Godfrey LM test:	F=0.10 P=0.96	F=0.24 P=0.87	F=0.14 P=0.94	F=0.78 P=0.51	F=0.92 P=0.43	F=1.16 P=0.33

Notes: For all regressions the dependent variable (Trans) is regressed on: Its own lagged value one period back (Trans-1), Growth rate in real GDP(-2) and GDP(-3), Growth rate in disposable income of households DISP(-2) and DISP(-3), Unemployment, UN(-1), UN(-2) and UN(-3), Growth rate in taxes paid by households S(-1) and S(-2). The Wald test reports the F statistic and probability that the conditional effect is zero as this is the restriction given. For (2) this restriction is: Elect=0, Lead*Elect=0. For (3): Lgov=0, Lgov*Lead=0, Lgov*Elect=0, Lgov*Lead*Elect=0. For (5): Elect=0, Lead*Elect=0. For (6): Lgov=0, Lgov*Lead=0, Lgov*Lead*Elect=0. The Breusch-Godfrey LM test reports the F statistic and probability under the null of no autocorrelation for a specified lag length of three for all regressions. Stars indicate significance at either 1 percent=***, 5 percent=** and 10 percent=* levels.

Tabel 3. Conditional effect: $\frac{dtrans}{delect}$

<i>Lead</i>	Conditional Effect
-12.3	8.27*** (0.76)
-12.4	8.33** (0.77)
-6.8	4.86*** (0.53)
1.4	-0.228 (0,62)

Meaningful inference requires computation of the variance of conditional effect: $\sigma^2\left(\frac{dtrans}{delect}\right)$ (D Kam, J Franzese Jr, 2007, p47). This is done for the different values of *Lead* and given that Elect and Lgov are equal to 1. The computation the variance has been conducted according to the formula in the appendix. The root is the standard error of the conditional effect and this is in parenthesis. The conditional effect is then divided by its given standard error and compared to the two-sided critical value of the t-distribution with n-k degrees of freedom. Where (n) is the number of observations minus (k) the total number of regressors including the constant (D Kam, J Franzese Jr, 2007, p48). If the conditional effect for a given value of *lead* is statistically different from zero this is marked in the following manner according to its significance level: 10% (*), 5% (**) and 1% (***).

The conditional effects in *table 3* indicate that the effect in transfers is large when the government is far behind its last result in an election for the period 1982Q4-1996Q4. If the government is -12.3% below its result from the last election in the poll during a quarter in an election year, the growth rate in transfers for that quarter is 8.27%. The magnitude of the conditional effect diminishes when the outlook for the government is improved and vice versa, implying there is a marginal benefit consideration conducted by the government. Fiscal expansion is magnified if the outlook is bad and it's reduced if the outlook is good. Again, the number of observations is limited, and the negative values of *Lead* are rather large.

Table 4 Conditional effect =: $\frac{dtrans}{delect}$

<i>Lead</i>	Conditional effect
7.2	-2.75*** (1.04)
-3.3	0.4 (0.49)
-16.7	4.42*** (1.66)

The values for *Lead* report values over a sample range ranging from the maximum through the median and lastly the minimum for the variable *lead* something which makes sense given the data and which is also recommended by D Kam, J Franzese Jr, 2007, p24). If the conditional effect for a given value of *lead* is statistically different from zero this is marked in the following manner according to its significance level: 10% (*), 5% (**) and 1% (***).

The conditional effects for 1982Q4-2019Q4 in *table 4* indicate that the increase in transfers is smaller than for 1982Q4-1996Q4. At the minimum value of *Lead* (-16.7) the conditional effect is equal to 4.42. The interpretation is that the government that fared the worst in the sample period 1982Q4-2019Q4 would increase transfers by 4.42% given that it performed this poorly in a quarter during an election year. Compared to *table 3* in which a government at -6.8 for *Lead* would increase transfers by 4.86% it is clear the effect in transfers is lower. At the maximum value 7.2 for *Lead* the effect in transfers is actually negative and fairly large in absolute numbers. This means that a government 7.2% ahead in the poll in a quarter during an election year would decrease transfers with 2.75%. At the median value of *Lead* the effect

in transfer is small and insignificant implying that there is an effect in transfers but only when the government is performing either very poorly or very well in the poll.

Table 5 Conditional effect: $\frac{dtrans}{lgov}$

<i>Lead</i>	Conditional effect
7.2	-1.4 (1.37)
-3.3	1.22** (0.60)
-16.7	4.57** (1.81)

The conditional variance is now given by: $\sigma^2 \left(\frac{\partial y}{\partial Lgov} \right)$. It is calculated according to the formula in the appendix for the different values of *Lead* and again given that *Lgov* and *Elect* are equal to 1. If the conditional effect for a given value of *lead* is statistically different from zero this is marked in the following manner according to its significance level: 10% (*), 5% (**) and 1% (***).

The level of significance is lower in *table 5* than for previous conditional effects. At no value for *Lead* is the conditional effects significance higher than five percent. But now interestingly enough the conditional value is significant at the median value for *lead*. The interpretation when *Lead* is -3.3 is that a left-wing government which is -3.3% behind in the poll compared to the last election would increase transfers by 1.22%. If it's in a quarter during an election year. Compared to *table 4* the conditional effect is larger. When the left-wing government is performing very poorly at -16.7 the effect is very close to the effect in *table 4*. It cannot be said however that left-wing actually lowers transfer even when performing very well as the conditional effect is not significant at the max of *Lead*. This implies that there is a partisan effect in election years. Left-wing governments increase transfers even at the median of *Lead* and whilst government type does not matter if the outlook in the poll is very poor left-wing governments seem more reluctant to lower transfers even when its going very well.

In sum the results in transfers indicate there is a political cycle in transfers during election years both for the period 1982Q4-1996Q4 and for the period 1982Q4-2019Q4. But in the latter the political cycle is conditional on the performance of the government during the election year. And the effect is only significant if the government is performing very poorly

or very well in the poll. A partisan effect was also present in the period 1982Q4-2019Q4 as left-wing governments were inclined to increase transfers even at a moderately low performance in the poll. Left-wing governments were also more reluctant to reduce transfers even when performing very well in the poll.

5.3 Taxes

Table 6 shows the coefficients of the two dummies *Rgov* and *Elect*, the coefficient for *Lead*, and the coefficients for the interaction terms of these variables. They are together with the control variables regressed on the annualized growth rate in taxes paid by households. In column 7-9 the observations after adjustments are limited to the period 1982Q4-1996Q4. In column 10-12 the number of observations after adjustments cover the entire period 1982Q4-2019Q4.

Column 7 tests H1 for 1982Q4-1996Q4. It includes two simple dummies which indicate that there was a very large positive and significant effect of a right-wing government in the growth rate of taxes paid by households. There was also a large negative effect during election quarters. This effect in election quarters is however insignificant and H1 is therefore rejected. The interpretation of the coefficients is that there was a 6.95% increase in the growth rate in taxes in a quarter when a right-wing government were in office and there was a 2.31% decrease during quarters in an election year. The sum of these coefficients is the effect in taxes during quarters in election years in which the government was right-wing. It is positive. This is contrary to what was anticipated but it might be explained by the fact that during the period there was only one right-wing government in office. The liberal-conservative government under Carl Bildt that took office in autumn 1991. It took office one year after the eruption of the finance crash in 1990. Therefore, it is likely that the coefficient for *Rgov* is anomalous insofar as it primarily indicates that that taxes paid by households stabilized from low levels after the initial steep drop, brought forth by the crisis. *Graph 3* lend support to this notion. Column 10 test H1 for the entire period and again the effect in election quarters is negative but insignificant and H1 is rejected. This time the effect on taxes is negative for a right-wing government albeit very small and insignificant. With such large standard error, the coefficient could just as well be positive. However, it does seem to be the case that the coefficient in Column 7 was an anomaly induced by too few observations for the dummy *Rgov*. These results taken together indicate H1 is rejected both for the period 1982Q4-1996Q4 and for 1982Q4-2019Q4.

Column 8 tests H2 for the period 1982Q4-1996Q4. The Wald-test indicate that the conditional effect is significant at the 1% level. The coefficient for $Lead*Elect$ is positive which means H2 is not rejected as the government then decreases taxes when its performing bad in the poll. The interpretation of the coefficient is that the government lowers the growth rate in taxes by 0.42 % for every 1% decrease in $Lead$ if it is a quarter in an election year. Or conversely it increases taxes when it gains support. The conditional effect is again tabulated for the max, median and min of $Lead$, in *table 7*. Column 11 tests H2 for the entire period. The Wald-Test is far from significant and it cannot be said that the conditional effect is different from zero. The results taken together and with the findings in *table 7* mean H2 is rejected both for the period 1982Q4-1996Q4 and 1982Q4-2019Q4.

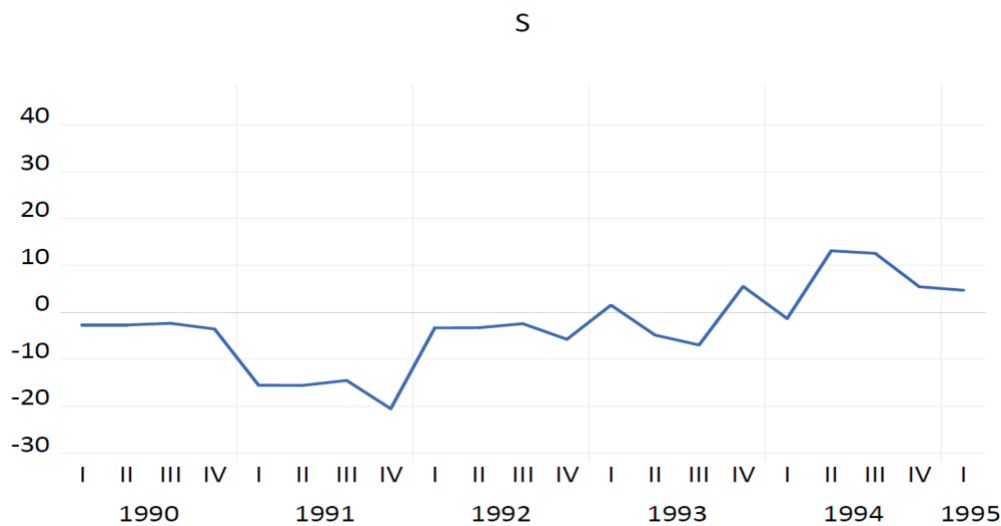
Column 9 tests H3 for the period 1982Q4-1996Q4. The Wald-Test indicate that the conditional effect is significant at the 5% level. However, there is serious reason to doubt that this is due to anything else than the same anomalous findings reported earlier. For the full model in column 9 the conditional effect of a right-wing government on taxes is the sum of all coefficients after differentiation with regards to $Rgov$ and its conditional on the values of $Lead$. The coefficient for $Rgov*Lead$ is positive but $Rgov*Lead*Elect$ is negative and four times as large, which means that taxes increase when the government is right-wing and by alot if it performs poorly in a quarter during an election year. Conversely it lowers taxes when it performs well. It's interesting that the conditional effect in taxes is opposite to what was hypothesized, but it is not safe to draw any conclusion as the number of observations is severely constrained. The sheer magnitude of the conditional effect implies something is wrong. For larger values of $Lead$ in absolute values the results are almost ludicrous. All terms containing $Rgov$ have very large coefficients and again the sheer size of the positive coefficient for $Rgov$ suggest something is wrong. The number of observations is as previously mentioned too limited as the conditional effect is constrained both to a short period and a period in which there only was one incumbent right-wing government. The conditional effect is therefore very likely seriously inflated and it is not appropriate to draw any conclusions. Column 12 tests H3 for 1982Q4-2019Q4. The Wald-Test is only barely significant at the 10% level, but the coefficients are this time more reasonable. The term $Rgov*Lead$ is positive whilst $Rgov*Lead*Elect$ is negative. The latter is also larger in absolute values, again, the conditional effect is tabulated in *table 8*. The results taken together indicate that H3 is rejected both for the period 1982Q4-1996Q4 and for the period 1982Q4-2019Q4.

<i>Tabel 6</i>	(7)	(8)	(9)	(10)	(11)	(12)
Dependent variable (S): Growth rate in taxes paid by households						
1. Rightwing Government: (Rgov)	6.95** (2.61)	..	10.9*** (4.01)	-0.09 (0.69)	..	-0.78 (1.32)
2. Election Quarter: (Elect)	-2.31 (1.37)	-1.41 (1.78)	-0,30 (1.52)	-1.09 (0.86)	-0.94 (0.90)	-1.12 (0.88)
3. Government lead in poll: (Lead)	..	0.48* (0.26)	0.42 (0.27)	..	0.01 (0.09)	0.01 (0.10)
4. Interaction term: (Lead*Elect)	..	0.42 (0.40)	0.43 (0.35)	..	0.08 (0.17)	0.19 (0.22)
5. Interaction term: (Rgov*Lead)	1.23 (1.35)	0.07 (0.28)
6. Interaction term: (Rgov*Elect)	-7.09** (3.93)	0.83 (2.47)
7. Interaction term: (Rgov*Lead*Elect)	-4.81 (3.08)	-0.62 (0.49)
8. Wald-test for joint significance Of the conditional effect	..	F=5.50 P=0.01	F=2.83 P=0.04	..	F=0.80 P=0.45	F=2.05 P=0.09
9. Number of observations:	57	57	57	149	149	149
10. R^2:	0.74	0.76	0.81	0.64	0.65	0.66

11. Breusch-Godfrey LM test:	F=1.62 P=0.2	F=1.70 P=0.18	F=1.70 P=0.18	F=1.93 P=0.13	F=2.00 P=0.12	F=1.70 P=0.17
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Notes: For all regressions the dependent variable is the annualized growth rate in taxes paid by households (S) it is regressed on: Its own lagged value one period back S(-1), one period annualized Growth rate in real GDP(-2) and GDP(-3). Annualized Growth rate in disposable income of households DISP(-2) and DISP(-3), Unemployment, UN(-1), UN(-2) and UN(-3), annualized growth rate in transfers received by households Trans(-1) and Trans(-2). The Wald test reports the F statistic and probability that the conditional effect is zero as this is the restriction given. For (8) this restriction is: Elect=0, Lead*Elect=0. For (9): Rgov=0, Rgov*Lead=0, Rgov*Elect=0, Rgov*Lead*Elect=0. For (11): Elect=0, Lead*Elect=0. For (12): Rgov=0, Rgov*Lead=0, Rgov*Elect=0, Rgov*Lead*Elect=0. The Breusch-Godfrey LM test reports the F statistic and probability under the null of no autocorrelation for a specified lag length of three for all regressions. Stars indicate significance at either 1 percent=***, 5 percent=** and 10 percent=* levels.

Graph 3



Graph 3 shows that the tenure of the only right-wing government in the period 1982Q4-1996Q4 is correlated with an upwards growth in taxes (S) following after an initial drop following the crisis in 1990. It's clear that the growth rate in taxes stabilized significantly starting in Q4 1991 just as the government took office.

Table 7

Conditional effect: $\frac{dS}{delect}$

<i>Lead</i>	Conditional Effect
7.2	1.61 (4.45)
-3.3	-2.80*** (0.97)
-16.7	-8.42 (5.2)

If the conditional effect for a given value of *lead* is statistically different from zero this is marked in the following manner according to its significance level: 10% (*), 5% (**) and 1% (***). Conditional variances have been calculated in the same manner as for transfers , again according to the formula in the appendix.

For the period 1984Q4-1996Q4 there is some indication of an electoral cycle in taxes but as mentioned previously the amount of observations is seriously constrained. Even though the conditional effect for the modest low number of *lead* is very significant the standard errors increase a lot as *Lead* goes in either direction. And for other values close to -3.3 the conditional effect is not significant. So, at most there is only a conditional political cycle in taxes around the median of *Lead* in the period 1984Q4-1996Q4. It can also not be said that there is an increase in taxes even if the government performs very well in the poll.

Table 8

$$\text{Conditional effect} = \frac{dS}{R_{gov}}$$

<i>Lead</i>	Conditional Effect
7.2	-3.91 (3.65)
5.03	-2.72 (3.00)
-3.3	1.86* (1.11)
-7.15	3.98** (1.55)
-16.7	9.235** (4.27)

If the conditional effect for a given value of *lead* is statistically different from zero this is marked in the following manner according to its significance level: 10% (*), 5% (**), and 1% (***).

For the period 1982Q4-2019Q4 in *table 8* some additional values of *lead* have been added to better reflect the underlying data. During the entire period 1982Q4-2019Q4 there were only 2 different right-wing governments in office with a total tenure of 11 years. During this time the lowest performance of the right-wing governments were at the value -7.5 for *Lead*. The highest value of *Lead* was 5.03. Contrary to what was hypothesized a right-wing government seems to increase the growth rate in taxes when it is performing poorly. The conditional effect is also not significant when the right-wing government is doing well in the poll and H3 therefore needs to be soundly rejected. The conditional effect is still significant and the largest so far for the value -16.7 of *Lead*, but this should be taken with a grain of salt as no right-wing government performed as poorly in the period.

5.4 Summation

In sum the results indicate there might be a political cycle in taxes during the period 1982Q4-1996Q4 and 1982Q4-2019Q4. For the first period there is some indication of a cycle conditional on the performance of the government during the election year. It indicates that a government would lower taxes if it performs moderately low in the poll. But it's only significant over a small span of values for the conditional effect. For the latter there is some indication of a cycle contrary to what was hypothesized, that is, a right-wing government which is performing poorly during an election year would actually increase taxes. It cannot be said that the right-wing government lowers taxes when it performs well.

6.1 Discussion

The results indicate the presence of a fiscal cycle in the growth rate of transfers to households in Sweden which was stronger 1982-1996 but nevertheless present over the entire period 1982-2019. The presence of fiscal cycles has been reported by Alesina Et Al (1992), Mind and de Haan (2006) and De Haan and Klomp (2013). And the results are in line with these previous findings. As the cycle was stronger 1982-1996 there is some indication that the great revision to the fiscal framework in 1997 limited the cycle. In the moral-hazard setting exemplified by Torsten and Tabellini (2000) cycles occur in instruments which are hidden from the public and viewed in this light the revisions in 1997 may have been pivotal as governments experienced a significantly reduced ability to hide any fiscal expansionism in off the grid boosts in transfers. The period is however too limited to draw far reaching conclusions. As the period 1982-2019 saw several additional developments it's more reasonable to assume all other developments contributed to a smaller effect during election years, as researches including Clark and Hallberg (2002) have shown that the political cycle is indeed constrained by more macroeconomic factors.

Over the period 1982-2019 the results indicated that the fiscal cycle in transfers was only significant when the effect was conditioned on performance in the poll. The results mimic what was found by Schultz (1995). There seems to be a link between how well governments performed in polls with an impact in transfers. The conditional effects indicate that the government does not induce an expansion in transfers or a contraction in transfers unless it is performing very poorly or very well. This should be noted as it indicates that a fiscal cycle in transfers is not always present in election years, it might in fact only be present in a few. How well the government polls could serve as a signal similar to how a competency is signaled in

macroeconomic performance to voters as theorized by Rogoff and Sibert (1989). The difference being that opinion polls would be observed by government in the present and signal voter satisfaction directly. As governments probably follow opinion polls closely in election years it's likely that conditioning the political cycle in instruments on polls can help detect a cycle which is not necessarily present in all election years.

There also seems to be partisan effect in transfers. Left-wing governments in the period 1982-2019 increased transfers even when performance was moderately low. As the left-wing governments were dominated by one major party for the entire period the results indicate what was shown by De Haan and Klomp (2013), namely that one-party governments seem to induce a stronger political cycle in comparison to coalitions. It also appears that left-wing governments are more reluctant to actually lower transfers even if it is going well. In the PT theory presented by Hobbs (1977) parties policies differ because the preferences of voters are heterogenous. Future research could be assisted by adjusting for the fact that voters may have heterogenous preferences in policy more so than in outcomes. It's probably safe to assume all voters desire low unemployment but that their preferences differ in how to achieve this outcome. One government might exert an effort to raise unemployment benefits in an election year whilst another might reduce income taxes. Both appealing to voters who desire low unemployment, but the effect would show up in transfers and taxes respectively.

The results also indicate that there may be a fiscal cycle during election years in Sweden for the growth rate of taxes paid by households. The cycle for 1982-1996 appears to indicate that governments lower taxes conditional on performance in the poll. The effect is however only highly significant for a very limited span of values in the poll. It does not seem that the same marginal benefit consideration occurs in government as indicated in transfers. The effect in taxes is not significant when it is going very poorly for the government at which point the marginal benefit of lowering taxes should be the highest. For the period 1982-2019 there is indication of a cycle when the government is right-wing but contrary to what was expected the results suggest that the right-wing governments actually increased taxes when performing very poorly. But they did not lower taxes even when enjoying very strong support. This is surprising as the effect occurred in election years. The findings by Alesina and Matteo (2017) suggested the opposite is true and right-wing governments indeed lower taxes in election years. There is serious reason to question the results that indicate the contrary. The results do not make sense intuitively and might purely be due to the fact that the results are conditioned on too few right-wing governments in the sample.

By conditioning on election years, which in Sweden occurred every four years after 1994, the observations are limited. When also conditioning on left- and right-wing governments there is the very real risk that tenure is correlated with an adverse shock or some other exogenous factor besides a political cycle. This indeed what seems to be the case in taxes. For taxes there was a sharp decrease in the growth rate prior to an upwards spike after stabilization in the first year of tenure for the only right-wing government prior to 1997. Any model that conditions effects on some criteria therefore need to include more observations otherwise the results might be flawed. Therefore, all results presented for all periods should be considered with this in mind. It might be the case the data is constrained too much. Future research of this kind would likely benefit from a larger span of observations and as such the results should not be overstated. Furthermore, it is important to note that when a potential political cycle is investigated in different instruments the macroeconomic shocks might cause different effects. Many counter cyclical social benefits are included in transfers to households. During an adverse shock transfers would increase as more people become unemployed. This direct correlation was likely stronger in the period before 1997 as fiscal stabilization had a more predominant role and the cycle found was also stronger in this period. The growth rate in taxes paid by households go down before the shock is accommodated, whilst transfers go up. How different instruments used to investigate any political business cycle behave in response to shocks could be elaborated on in future research.

Conclusion 6.2

In conclusion the results lend support to the hypothesis of a political cycle in transfers during election years for the latter period 1982-2019 but only for governments that were about to lose or comfortably win the upcoming election. Left-wing governments seems to be an exemption as transfers increased even when the outlooks in the poll was less dire. Left-wing governments were also more reluctant to reduce transfers. Before 1997 the cycle in transfers was stronger and not dependent on performance in the poll. No partisan effect could however be found. The magnitude in transfer might be explained by the revised fiscal framework, but more macroeconomic changes post 1997 could also be attributed to reducing the cycle. The period is however too short to safely conclude that the results in transfers are robust enough. For taxes the results primarily underline the limitations imposed by the limited period and application of conditional effects. It cannot be concluded without major uncertainty that there was any cycle in taxes. The results in transfers for the period 1982-2019 should also primarily be regarded as an indication that there is reason to further investigate an opportunistic political cycle in Sweden.

7.1 Appendix

Table 1: Marginal Effects and Variances for Various Multiplicative Interaction Models (Double and Triple Interaction Terms)

Case	Equation	Marginal Effect	Variance
1a	$\hat{Y} = \beta_0 + \beta_1 X + \beta_2 Z + \beta_3 XZ$	$\frac{\partial Y}{\partial X} = \beta_1 + \beta_3 Z$	$\hat{\sigma}_{\frac{\partial Y}{\partial X}}^2 = \text{var}(\hat{\beta}_1) + Z^2 \text{var}(\hat{\beta}_3) + 2Z \text{cov}(\hat{\beta}_1, \hat{\beta}_3)$
1b	$\hat{Y} = \beta_0 + \beta_1 X + \beta_2 Z + \beta_3 XZ$	$\frac{\partial Y}{\partial Z} = \beta_2 + \beta_3 X$	$\hat{\sigma}_{\frac{\partial Y}{\partial Z}}^2 = \text{var}(\hat{\beta}_2) + X^2 \text{var}(\hat{\beta}_3) + 2X \text{cov}(\hat{\beta}_2, \hat{\beta}_3)$
2	$\hat{Y} = \beta_0 + \beta_1 X + \beta_2 Z + \beta_3 W + \beta_4 XZ + \beta_6 ZW$	$\frac{\partial Y}{\partial X} = \beta_1 + \beta_4 Z$	$\hat{\sigma}_{\frac{\partial Y}{\partial X}}^2 = \text{var}(\hat{\beta}_1) + Z^2 \text{var}(\hat{\beta}_4) + 2Z \text{cov}(\hat{\beta}_1, \hat{\beta}_4)$
3	$\hat{Y} = \beta_0 + \beta_1 X + \beta_2 Z + \beta_3 W + \beta_4 XZ + \beta_5 XW + \beta_6 ZW$	$\frac{\partial Y}{\partial X} = \beta_1 + \beta_4 Z + \beta_5 W$	$\hat{\sigma}_{\frac{\partial Y}{\partial X}}^2 = \text{var}(\hat{\beta}_1) + Z^2 \text{var}(\hat{\beta}_4) + W^2 \text{var}(\hat{\beta}_5) + 2Z \text{cov}(\hat{\beta}_1, \hat{\beta}_4) + 2W \text{cov}(\hat{\beta}_1, \hat{\beta}_5) + 2ZW \text{cov}(\hat{\beta}_4, \hat{\beta}_5)$
4	$\hat{Y} = \beta_0 + \beta_1 X + \beta_2 Z + \beta_3 W + \beta_4 XZ + \beta_5 XW + \beta_6 ZW + \beta_7 XZW$	$\frac{\partial Y}{\partial X} = \beta_1 + \beta_4 Z + \beta_5 W + \beta_7 ZW$	$\hat{\sigma}_{\frac{\partial Y}{\partial X}}^2 = \text{var}(\hat{\beta}_1) + Z^2 \text{var}(\hat{\beta}_4) + W^2 \text{var}(\hat{\beta}_5) + Z^2 W^2 \text{var}(\hat{\beta}_7) + 2Z \text{cov}(\hat{\beta}_1, \hat{\beta}_4) + 2W \text{cov}(\hat{\beta}_1, \hat{\beta}_5) + 2ZW \text{cov}(\hat{\beta}_1, \hat{\beta}_7) + 2ZW \text{cov}(\hat{\beta}_4, \hat{\beta}_5) + 2WZ^2 \text{cov}(\hat{\beta}_4, \hat{\beta}_7) + 2ZW^2 \text{cov}(\hat{\beta}_5, \hat{\beta}_7)$

The formula of the marginal variance (variance of the conditional effect) is taken from the book: *Multiple Regression: Testing and Interpreting Interactions*.

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