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Exchange Rate Regime, Fiscal Foresight and the Effectiveness of Fiscal Policy in a Small Open Economy

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Contents

Exchange Rate Regime, Fiscal Foresight and the Effectiveness of Fiscal Policy in a Small Open Economy

Abstract

This paper studies the effects of discretionary fiscal policy shocks under different exchange rate regimes within a structural vector autoregressive (SVAR) model. We first suggest that by estimating the effects of fiscal policy shocks in two structurally similar small open economies that have opted for different monetary policy regimes (Finland and Sweden), we may control for the economic environment and study the effect of exchange rate regime on fiscal policy transmission. Second, we propose to augment the baseline model with quarterly fiscal forecasts and study fiscal policy shocks under fiscal foresight, i.e., when economic agents may anticipate and respond to fiscal policy measures prior to their implementation. Our findings suggests that discretionary fiscal policy is more effective under a fixed exchange rate regime than under a floating exchange rate regime. This is consistent with the conventional wisdom inherited from the Mundell-Fleming framework and with recent evidence that suggests the effectiveness of fiscal policy depends on the degree of monetary policy accommodation. We also find evidence that unanticipated (as opposed to standard SVAR) fiscal policy shocks have a larger expansionary effect on output than in the baseline.

Key words: Fiscal policy, exchange rate regime, fiscal foresight, small open economy

JEL: E62, E63, F41, H30

Valuuttakurssiregiimi, ennakointivaikutukset ja finanssipolitiikan tehokkuus pienessä avotaloudessa

Tiivistelmä

Tässä paperissa tarkastellaan päätösperäisen finanssipolitiikan shokkeja eri valuttaakurssiregiimeillä rakenteellisessa vektori-autoregressiomallissa. Empiirinen asetelma perustuu päätösperäisten finanssipolitiikan shokkien tarkasteluun kahdessa rakenteellisesti samankaltaisessa pienessä avotaloudessa, jotka ovat valinneet erilaisen rahapoliittisen regiimin (Suomi ja Ruotsi). Paperissa motivoidaan miksi asetelma mahdollistaa talouden rakenteellisten tekijöiden ja suhdannetilanteen kontribuution kontrolloimisen finanssipoliittisten toimien välittymisessä taloudelliseen aktiviteettiin. Lisäksi paperissa tarkastellaan kuinka päätösperäisen finanssipolitiikan vaikutukset muuttuvat, jos talouden toimijat voivat ennakoida ja reagoida tuleviin finanssipoliittisiin toimenpiteisiin ennen niiden toteutusta. Empiiriset tulokset viittaavat siihen, että finanssipolitiikka on tehokkaampaa kiinteällä valuuttakurssilla kuin kelluvalla valuuttakurssilla. Tulokset ovat yhdenmukaisia perinteisen Mundell-Fleming mallin ja viimeaikaisen finanssipolitiikkaa koskevan kirjallisuuden kanssa. Tulokset antavat tukea myös sille, että ennakointivaikutukset vaikuttavat finanssipolitiikan shokkien identifikaatioon ja siten myös finanssipolitiikan estimoituihin kerroinvaikutuksiin.

Asiasanat: Finanssipolitiikka, valuuttakurssiregiimi, ennakointivaikutukset, pieni avotalous

JEL: E62, E63, F41, H30

1 Introduction

The post-2007 economic crisis returned the analysis of discretionary fiscal policy back on the forefront of macroeconomic research (Auerbach 2009). In the beginning of the global crisis, governments worldwide engaged in stimulating the economy through various fiscal policy measures. This ran contrary to the widespread consensus that discretionary fiscal policy would not serve as a useful counter-cyclical macroeconomic stabilization tool, mostly due to the associated long implementation lags and uncertainty related to its effects on the aggregate economic activity (Auerbach 2002, Blinder 2004, Fàtas and Mihov 2003, Feldstein 2009, Taylor 2011). However, as conventional monetary policy became constrained by the zero lower bound, the potency of fiscal policy activism has become widely reconsidered, particularly in circumstances where the degree of monetary policy accommodation is high (Woodford 2011, Cristiano et al. 2011, DeLong and Summers 2012 and Correia et al. 2013). Subsequently, the debate has concerned whether fiscal consolidation measures to curb increasing sovereign debts have turned out self-defeating and whether their contractionary effects on the economy have been underestimated by policy makers and policy making institutions (Blanchard and Leigh 2013).

The debate underlines that there remains a great deal of uncertainty related to the effectiveness of fiscal policy. Recent evidence suggests that there exists no single "fiscal multiplier", but that the effectiveness of fiscal policy depends on time-variant factors, such as the degree of monetary accommodation, trade openness, financial frictions and the amount of liquidity constraints (Favero et al. 2011, Auerbach and Gorodnichenko 2012, Corsetti et al. 2012a and Ilzetzki et al. 2013 among others). This poses a challenge for empirical research as controlling for the economic environment tends to require strict modeling assumptions or extensive data requirements. The current empirical evidence is mostly based on the United States, which could be best described as a large and closed economy. In contrast, the effectiveness of fiscal policy is particularly relevant for small open economies that have given up their monetary policy independence and adopted a fixed exchange rate, such as the euro zone member states. For these countries, discretionary fiscal policy is the only available sovereign macroeconomic stabilization tool against idiosyncratic shocks. Yet, only a few studies to date assess the effects of fiscal policy shocks in small open economies under different exchange rate regimes.

This study contributes to the empirical fiscal policy literature by studying the effects discretionary of fiscal policy shocks in small open economies. In particular, we estimate the effects of fiscal policy shocks in Finland and Sweden, which are structurally similar Nordic countries that have been subject to similar business cycle fluctuations during the past two decades, but have opted for different exchange rate regimes. We first argue that by comparing these two countries, we may convincingly control for the economic environment, which is otherwise likely to affect the effects of fiscal policy shocks over time and across countries. Second, in order to study the effects of exogenous fiscal policy shocks, we follow Blanchard and Perotti (2002) and estimate a structural vector autoregressive (SVAR) model that incorporates institutional information on tax collection and fiscal policy decision making lags to abstract from endogenous variation in

fiscal variables. In addition, we propose to study the effects of fiscal policy under fiscal foresight by augmenting the baseline specification with national quarterly fiscal forecasts. The latter is motivated by recent literature which suggests that economic agents may respond to anticipated as well as implemented fiscal policy shocks (Mertens and Ravn 2012, Ramey 2011a and Leeper et al. 2013).

On the one hand, our empirical results suggest that the effects of fiscal policy shocks depend on the exchange rate regime. In particular, we find evidence that discretionary fiscal policy shocks have a more expansionary effect on economic activity when the exchange rate is fixed. The result is consistent with the conventional Mundell-Fleming model, but also with more recent micro-founded models and recent empirical evidence. On the other hand, we find evidence that anticipation effects may affect the estimated dynamic effects of fiscal policy. Our results indicate that unanticipated fiscal policy shocks tend to have a more expansionary effect on economic activity when compared to standard SVAR fiscal policy shocks that neglect from forward looking behavior.

The rest of this paper is constructed as follows. Section 2 reviews recent theoretical and empirical evidence on the effects of fiscal policy shocks. Section 3 presents our empirical strategy and constructs the baseline SVAR-model. Section 4 presents our dataset and estimates for government expenditure and net tax elasticities. Section 5 shows our baseline results and assesses their sensitivity. Section 6 discusses how the presence of fiscal foresight affects the identification of fiscal policy shocks, constructs a model to account for anticipation effects and shows our estimates of unanticipated and anticipated fiscal policy shocks. Section 7 concludes our findings.

2 Literature

This literature review is a brief overview of the recent theoretical and empirical studies on fiscal policy. We highlight the main findings of the literature in order to understand the transmission mechanism of fiscal policy shocks, the factors contributing to the effectiveness of fiscal policy measures and the challenges related to identifying fiscal policy shocks in empirical analysis.[1] It turns out that the effects of fiscal policy are likely to depend importantly on the economic environment. This is supported by both theoretical and empirical considerations.

2.1 Effects of Fiscal Policy Shocks in Theoretical Models

In their seminal paper, Baxter and King (1993) propose a neoclassical dynamic stochastic general equilibrium (DSGE) model in which wages and prices are perfectly flexible and economic agents are forward looking. They show that, under these assumptions, an unexpected increase in government expenditure increases the discounted value of household tax liabilities and hence

[1] For a comprehensive review of fiscal policy literature and fiscal multipliers we refer the reader to recent papers by Hall (2009), Ramey (2011b), Parker (2011) and Hebous (2010).

generates a negative wealth effect on households. This negative wealth effect decreases private consumption, but it increases labour supply as households increase their working hours to consolidate the decline in their disposable income. In the equilibrium, private consumption and real wages are thus lower, but employment and output are higher than before the government expenditure shock.

New-Keynesian DSGE models introduce monopolistic competition, increasing returns and nominal rigidities to the neoclassical setting (see, f.e., Devereux 1996, Woodford 2011 and Coenen et al. 2012). Again, an unexpected increase in government expenditure induces a negative wealth effect on households. Due to nominal rigidities, however, the government expenditure shock raises labour demand and hence real wages. This suggests that household disposable income may now rise more relative to the baseline neoclassical setting. Yet, if labour supply is not sufficiently elastic, the initial negative wealth effect is likely to dominate and, in the equilibrium, a government expenditure shock induces a decline in private consumption (Devereux 1996).

Standard neoclassical and New-Keynesian models therefore usually predict that an expansion in government expenditure crowds out private consumption, and that the expansionary effect on output is driven by a negative wealth effect on the private sector. The response of private consumption is central as it implies that inter-temporal wealth gains from government expenditure shocks are rather limited. This prediction is, however, in contrast with the frequent empirical finding that private consumption tends to increase in response to expansionary fiscal policy shocks (Blanchard and Perotti 2002, Mountford and Uhlig 2009 and Monacelli and Perotti 2010).

The literature suggests alternative explanations for the rise in private consumption. First, Galí et al. (2007) propose that private consumption may rise in a New Keynesian model if one allows that a proportion of consumers are liquidity constrained, or so-called "rule-of-thumb" consumers who use all of their disposable income each period. This is, in effect, a way of imposing that the households are non-Ricardian. Second, Corsetti et al. (2012b) show that an endogenous reversal in government expenditure, which aims to stabilize the stock of government debt in the medium-term, may account for the rise in private consumption. In particular, an expected future consolidation decreases expectations of future inflation and hence decreases long-term real interest rates which then drives up present private consumption. In addition, for example, Ravn et al. (2012) propose deep-habit formation to account for the rise in consumption and Corsetti et al. (2011) show that in the presence of financial frictions private consumption may increase in response to a government expenditure shock.

Recent theoretical considerations allow the effects of fiscal policy shocks to be non-linear, for example, by allowing the effects to depend on the state of the economy. A substantial amount of research motivates that the effects of fiscal policy shocks depend on the degree of monetary policy accommodation and the potency of monetary policy to lower real interest rates (see, f.e.,

Krugman 1998, Hall 2009, Eggertson and Krugman 2012, Delong and Summers 2012).[2] For example, Woodford (2011) and Christiano et al (2011) show that the government expenditure multiplier can be significantly higher than unity when monetary policy is constrained to a constant real interest rate. In particular, a fiscal expansion in persistent zero-nominal-interest-rate environment lowers the real interest rate due to an increase in inflation expectations and hence creates an expansion in aggregate economic activity. On the other hand, Correia et al. (2013) show that the zero lower bound constraint on monetary policy may be circumvented by innovative design of tax policy measures in a zero lower bound environment, and output may be increased without additional public expenditures.

Motivated by these recent theoretical contributions, many studies assess the effects of fiscal stimulus in estimated empirical DSGE models. For example, Coenen et al. (2012) study the effects of fiscal policy shocks in recent workhorse models as well as in larger empirical New-Keynesian models that are applied in policy analysis in different international institutions. They find that these models, even though differ in their calibration and structure in many respects, imply similar effects from fiscal policy shocks. They show that expansionary fiscal measures stimulate output in the short-run, and that their effectiveness depends on the economic environment and on the fiscal instrument employed. First, they show that temporary government investment and transfers targeted to liquidity constrained households tend to be most effective in stimulating the economy and private economic activity. Second, they show that the stimulating effect is more signifiant when monetary policy in constrained by a zero lower bound. In particular, the fiscal multiplier may significantly exceed unity under a maintained accommodative (zero bound) monetary policy environment. On the other hand, Cwik and Wieland (2011) and Cogan et al. (2010) agree that a contained zero lower bound environment increases the effectiveness of fiscal stimulus, but they still argue that the aggregate effects on private expenditure tend to be negative and the fiscal multiplier significantly below one across a variety specifications. However, the estimates are conditional on the assumptions regarding the period for which the monetary policy is assumed to be accommodative.

On the other hand, literature suggest that the effects of fiscal policy in small open economies depend on the monetary policy regime. The conventional wisdom inherited from the Mundell-Fleming framework emphasizes the importance of the exchange rate regime on the effectiveness of fiscal policy in small open economies that take the international rate of return as given. Under a flexible exchange rate, a fiscal expansion that increases employment and private sector income has a positive effect on output and inflation, but it induces pressure for the inflation targeting central bank to raise interest rates. A rise in the interest rate then appreciates the domestic currency vis-à-vis foreign currencies and reduces net exports, offsetting the expansionary effects of the initial fiscal measure. Under a fixed exchange rate, in contrast, a similar fiscal expansion has a positive effect on output and inflation, but monetary policy is constrained by

[2] As suggested by Krugman (1998), the zero lower bound constrains monetary policy only to the extent that monetary policy cannot control future inflation expectations.

the international rate of return.

Micro-founded models tend to maintain the prediction that the effectiveness of fiscal policy depends on the monetary policy regime. For example, Coutinho (2005) shows within the Obstfeld and Rogoff (1995) open economy model that balanced-budget fiscal policy shocks are more expansionary under a fixed exchange rate regime than under a flexible exchange rate regime in the short-run, but that the effect balances out in the long-run.[3] Importantly, within a fixed exchange rate regime, private consumption moves in the same direction as government expenditure. Corsetti et al. (2011) and Born et al. (2013), on the other hand, reconsider the conventional wisdom within a small open economy New-Keynesian model. They show that government expenditure shocks tend to be more expansionary under a fixed exchange rate regime due to the higher degree of monetary policy accommodation.[4] The difference is more substantial if one introduces liquidity-constrained households to the model. In particular, the initial fiscal multiplier exceeds unity under an exchange rate peg, but it is lower than unity under a float. However, the results on these open economy models are conditional on the assumptions regarding, f.e., Ricardian equivalence, home-bias in consumption and spending reversals.

The zero lower bound has brought new features also to the small open economy setting. Intuitively, a fixed exchange rate regime is analogous to the zero-lower bound in the sense that the monetary policy authority does not counteract the fiscal measures by raising policy rates. However, this analogy changes somewhat in an open economy environment. For example, Farhi and Werning (2012) and Egrec and Lindé (2012, 2013) suggest that while fiscal policy tends to be more effective under a fixed exchange rate regime in normal times, a zero lower bound environment may reverse this conventional wisdom. In particular, when combined with a zero nominal interest rate environment, a fiscal expansion under a under a float has a multiplier effect that exceed the multiplier effect under a conventional fixed exchange rate. This is due to the associated devaluation effect on the domestic currency under a floating exchange rate, which is absent when the exchange rate regime is *credibly* fixed.

Overall, theoretical models provide a large range of estimates for qualitative as well as quantitative effects of fiscal policy shocks. Yet, there seems to emerge a consensus that the effectiveness of fiscal policy depends on time varying factors, such as the state of economy, the degree of monetary policy accommodation, constraints on inter-temporal consumer optimization and openness of the economy. This emphasizes the importance of empirical evidence on the effects of fiscal policy. Also, it suggests that empirical studies should aim to isolate the effect that is due to the economic environment in order to differentiate between alternative theoretical models. Next, we will discuss the strategies to isolate exogenous fiscal policy shocks

[3]Coutinho (2005) shows that these conclusions are dependent on the assumptions regarding Ricardian equivalence and home-bias in government spending among others. If the assumptions are modified home consumption may rise in the short run in response to expansionary fiscal shock even under flexible exchange rates.

[4]Fiscal policy is not necessarily less effective under a flexible exchange rate if they allow for spending reversals to stabilize sovereign debt in the medium-term.

from available data and potential strategies to control for the economic environment.

2.2 Empirical Evidence on the Effects of Fiscal Policy Shocks

In empirical fiscal policy analysis the most challenging task is to identify exogenous fiscal policy shocks from available economic data. In theory, recovering fiscal shocks is a straightforward exercise. When a government implements a new fiscal measure, say a tax cut, government tax revenues decrease and we may recover a fiscal policy shock from tax revenue data respectively. However, the sensitivity of tax revenues to other simultaneous economic shocks hampers this simple identification procedure. For example, when economic activity increases, firms tend to increase their profits, workers earn larger salaries and, as a whole, private sector pays more taxes. Hence, it would be incorrect to interpret the following increase in tax revenues as a (contractionary) tax policy shock. Measurement of fiscal policy shocks becomes even more complex if policies are cyclical, i.e. if governments increase expansionary measures in response to recessions and/or constrain fiscal measures due to deteriorating public finances.

In the literature, two main strategies emerge to identify fiscal policy shocks. First, a so-called narrative approach identifies fiscal policy measures that are exogenous to prevailing economic conditions directly from observed exogenous fiscal policy regime changes (Ramey and Shapiro 1998). Conventionally, these "narrative shocks" consist of large military buildups in the United States, or some significant policy regime changes, that may be considered unrelated to overall economic activity. If the identified shocks are truly exogenous and unanticipated, a reduced-form empirical analysis is sufficient to study the effects of fiscal measures on economic activity. However, the main difficulty then becomes to identify these fiscal policy shocks. In particular, while military spending appears as an appealing instrument to control for endogenous variation in fiscal variables in the United States (see Hall 2009), it does not provide a similar empirical setting for other countries. This follows, among other things, as the role of defense spending in public expenditure is much less significant in other countries, and also as military actions are associated with significant losses of domestic productive capital.

Alternative narrative datasets or instruments are not readily available, but need to be constructed from various sources. Recent narrative approach studies base their account of exogenous fiscal policy shocks on official government documents and projected future fiscal measures (Romer and Romer 2010), real-time news paper resources (Ramey 2011a), average marginal tax rate changes (Barro and Redlick 2011), professional forecast exercises on planned fiscal measures (Auerbach and Gorodnichenko 2012) or fiscal consolidation measures to cut down public debt (Devries et al. 2011). However, these strategies are subject to subjective evaluation on which policy measures are regarded as exogenous, and they are also sensitive to differences in estimating the quantitative size of each fiscal policy measure. Further, fiscal consolidation measures are usually a consequence of drastic economic conditions in the preceding period. Assuming them to be exogenous and unanticipated by economic agents is unwarranted. Another deficiency of narrative records is to identify truly exogenous policy changes that would not

coincide with other policies. For example, in Sweden, there have been reductions in VAT rates (such as food and non-alcoholic beverages) in order to compensate for reduced social transfers (Sørensen 2010). It is likely that this is a relatively common practice in fiscal policy making. As a consequence, fiscal policy analysis has been mostly restricted to alternative identification strategies, at least outside the United States.

The second approach is to construct structural vector autoregressive (SVAR) -models in order to abstract from endogenous variation in fiscal variables. This strategy inspires from Sims's (1980) seminal contribution and proposition to model macroeconomic variables within vector autoregressive (VAR) models. In particular, Blanchard and Perotti (2002) propose to identify fiscal policy shocks in an SVAR-model that incorporates institutional knowledge on tax revenue collection and fiscal policy decision lags. Key to their identification is that governments may not respond to observed output shocks within a quarter due to decision making and legislative lags and, in addition, that the within-a-quarter sensitivity of government expenditure and tax revenues may be consistently estimated from available out-of-sample information. Most subsequent fiscal SVAR studies adopt a similar identification strategy to study the effects of fiscal policy shocks. For example, Perotti (2005) estimates a variant of the model that takes into account monetary policy and inflation dynamics, but which is based on additional identification assumptions. In addition, alternative identification strategies rely on recursive ordering and zero restrictions (Fátas and Mihov 2001 and Ramey 2011a), Bayesian estimation methods, which allow uncertainty to be attached to model parameters (Pereira and Lopes 2010 and Afonso and Sousa 2011), or sign restrictions on impulse responses that rely on economic theory (Mountford anf Uhlig 2009 and Canova and Pappa 2011).

There emerges a considerable heterogeneity in estimated effects of fiscal policy shocks. In particular, contemporaneous empirical estimates tends to depend on the identification strategy. Based on a narrative identification method, Ramey and Shapiro (1998) study the effects of military spending associated with the Korean war, the Vietnam war and Carter-Reagan military buildup and find that output increases, but private consumption, real wages and productivity fall in response to an expansionary fiscal policy shock. Thus, they conclude that their findings are consistent with the standard neoclassical model. Subsequent narrative studies augment the Ramey-Shapiro identification strategy to a VAR framework and tend to find qualitatively similar results (Burnside et al. 2004, Barro and Redlick 2011 and Ramey 2011a). Overall, the fiscal multiplier associated with these "narrative" estimates tends to be below one.

On the other hand, studies based on SVAR methods find that private consumption increases in response to an expansionary fiscal policy shock and that the average fiscal multiplier is close to or above one (Blanchard and Perotti 2002, Fatas and Mihov 2001 and Mountord and Uhlig 2009). More recently, Romer and Romer (2010) find significantly larger output effects once they base their narrative account on post-war tax policy changes rather than defense spending. In particular, they estimate that a one-percentage-point-of-GDP increase in taxes decreases GDP by three percentage points and that private consumption and investment respond negatively

to exogenous tax increases. This evidence is thus not directly compatible with neither the neoclassical nor the New-Keynesian benchmark models.

Subsequent literature discusses the possible sources of the divergence in empirical estimates. Among others, Hall (2009) argues that the estimates based on defense spending are likely to yield a lower bound for the effects of government expenditure shocks as war times are associated with rationing and adverse effect on private consumption and overall economic activity. Barro and Redlick (2011) reach the opposite conclusion by arguing that command-and-control techniques and patriotism associated with war times cause prices and wages to be undervalued, but increase household work effort. Ramey (2011) suggests that the main difference in narrative and SVAR estimates is due to the difference in how these alternative methodologies time fiscal policy shocks. She constructs a narrative record on expected present value of government spending using news resources and finds SVAR results that are qualitatively similar to the Ramey-Shapiro estimates and a fiscal multiplier that is in the range of 0.6-1.2. Favero and Giavazzi (2012) incorporate Romer and Romer (2010) tax policy shocks to a standard fiscal VAR analysis and find significantly smaller output responses that fall in line with the Blanchard-Perotti estimates. Further, Ramey (2011), Mertens and Ravn (2013) and Caldara and Kamps (2012) suggest that the difference could be explained by measurement errors in narrative tax policy data or by differences in implicit net tax elasticities in different specifications. They estimate that the fiscal multiplier is somewhere between the higher estimates of Romer and Romer (2010) and lower estimates of Banchard and Perotti (2002).

While there has been a recent surge in the literature assessing the effects of fiscal policy shocks, there is still much less literature that studies to what extent the effects may vary according to the economic environment. In particular, despite the existing theoretical motivation, most empirical studies assume that the effects are linear and, as a consequence, the estimates only represent the average historical fiscal multiplier, which might not turn out particularly useful for policy analysis. Also, most of the evidence considers the United States, or a few other OECD countries, for which historical series of quarterly fiscal policy data has been available (Perotti 2005). Yet, for example Favero et al. (2011) argue that fiscal policy shocks should be expected to have significantly heterogeneous effects across countries due to differences in country characteristics. However, the present empirical evidence that takes these considerations into account is rather scarce.

First, an emerging strand of literature shows that the effects of fiscal policy may vary considerably according to the state of the economy, i.e. according to the state of the business cycle. Auerbach and Gorodnichenko (2012) augment the Blanchard-Perotti identification strategy to a regime-switching model in which they allow the effects of fiscal policy to vary according to the business cycle. Notably, they find that the fiscal multiplier effects are significantly larger during recession than in expansions while the linear estimate falls in the range of conventional SVAR estimates. Similarly, Baum and Koester (2011) and Baum et al. (2012) construct a threshold VAR model and estimate that the effects of fiscal policy in OECD countries are

more expansionary in recessions than in expansions. Pereira and Lopes (2010) estimate an SVAR-model with time-varying parameters and find some support for more effective fiscal policy during recessions. Using a different approach, Blanchard and Leigh (2013) show that the effects of fiscal policies and consolidation measures were consistently underestimated by policy making institutions during the global financial crisis, while no such results were found for the period prior to the financial crisis. This evidence suggest that in recessions, when the degree of monetary policy accommodation is likely to be higher, financial frictions could be more severe or when there is more liquidity constraints in the economy, fiscal policy shocks tend to have a more expansionary effect on aggregate economic activity.

Second, empirical evidence suggests that the transmission of fiscal policy shocks depends on country's openness to international trade. For example, Cardí and Müller (2011), Beetsma and Giuliodori (2011) and Monacelli and Perotti (2010) estimate that government expenditure shocks have a smaller effect on output in countries in which international trade comprises a larger fraction of GDP. While the evidence is not conclusive on the transmission mechanism, it is consistent with the analogy that more open economies should expect to have a smaller fiscal multiplier due to the potential that a larger fraction of government stimulus is directed to foreign instead of domestic products (i.e. fiscal policy leaks abroad) and that the appreciation of domestic currency may crowd out net exports.

Finally, recent empirical studies suggest that the effects of fiscal policy may depend on the exchange rate regime. However, in order to control for the economic environment, most of this literature adopts identification strategies that differ from the standard SVAR models and none of studies includes tax policy shocks in the empirical analysis. First, Corsetti et al. (2012a) study the effects of fiscal policy shocks in a panel of OECD countries and control explicitly for the state of the economy, public finances and the exchange rate regime. Instead of quarterly data they are restricted to employ annual frequency data. To control for automatic movements in annual fiscal data, they first estimate a fiscal policy rule for each country and then use the unexplained residuals from this rule as a measure for discretionary fiscal policy shocks. Based on this identification strategy, they find that fiscal policy tends to be more effective within a fixed exchange rate regime than within a flexible exchange rate regime. They also find evidence that fiscal multiplier effects tend to be higher during financial crises, but lower when the government is highly indebted.

Second, Ilzetzki et al (2013) study the effects of government expenditure shocks in a large panel of 44 countries using a recursive panel VAR model. They control for the exchange rate regime, country income level, foreign debt and openness of the economy, but are restricted to control for each factor one at a time. They estimate that economies in which the exchange rate has been fixed, the long run fiscal multiplier has been above one, but in economies under a floating exchange rate the multiplier has been close to zero or even negative. In addition, they find that relatively large economies tend to have multipliers close to one, but small open economies tend to have multipliers that are close to zero. Third, Born et al. (2013) estimate

the effect of government expenditure in a recursive panel VAR model that is based on bi-annual data and find that the effects of fiscal policy shocks have been more expansionary under fixed exchange rates.

In addition to the evidence based on international data, an emerging strand of literature has estimated so-called local or regional multipliers as an alternative approach to assess the effects of fiscal policy in a monetary union or under a fixed exchange rate regime. For example, Nakamura and Steinsson (2011) and Acconcia et al. (2011) estimate sub-national government expenditure multipliers from regional differences in military procurement expenditure and infrastructure spending and find that the multiplier effect is in the range from 1.4 to 2.0. Clemens and Miran (2012), on the other hand, find that regional multipliers estimated from pro-cyclical state government spending induced by fiscal institutions tends to be below one.

Although the studies that asses the effects of fiscal policy shocks under different exchange rate regimes do not fully agree on the details of the transmission mechanism (f.e. with regards to the effect on real exchange rate dynamics), and their econometric methodologies differ from each other, the results suggest that the exchange rate regime and the degree of monetary policy accommodation are integral determinants of the effectiveness of fiscal policy shocks.

To conclude, there seems to be sufficient evidence to support the conclusion that the effects of fiscal policy shocks are dependent on the economic environment. Yet, current empirical evidence is not conclusive on the extent that small open economies under alternative monetary policy regimes could rely on fiscal policy as a macroeconomic stabilization tool. The lack of convincing empirical evidence to date is in part due to limited data availability and convincing strategies to control for the economic environment.

3 Empirical Strategy

This section presents our empirical approach to study the effects of fiscal policy shocks in small open economies under different exchange rate regimes. We first motivate our empirical strategy to control for the economic environment and then construct a fiscal SVAR-model similar to Blanchard and Perotti (2002) to study the effects of discretionary fiscal policy shocks.

3.1 Empirical Setting

We propose to control for the underlying economic environment by estimating an SVAR-model for two countries that share a similar economic environment, but have opted for different monetary policy regimes. In particular, we estimate the effects of fiscal policy shocks in Finland, which is part of the euro area, and in Sweden, which has an independent monetary policy authority and a floating exchange rate. This is a compelling empirical setting due to two different observations.

First, as suggested by Jonung and Sjöholm 1999, Korkman and Suvanto 2013 and Gylfason

et al. 2010, among others, Finland and Sweden are structurally similar economies.[5] They are both small open economies that are heavily dependent on international trade. They share a similar industrial structure that relies on manufacturing of investment and durable goods as well as forest-related production. For example, both countries have a rather low degree of product differentiation and, out of selected 12 OECD countries, the manufacturing sector in Finland is closest to that of Sweden (Jonung and Sjöholm 1999). In addition, the two countries share a similar public sector influence on the economy and they both have highly unionized labour markets, which are characteristic to Nordic economies. This is, among other things, due to common historical, geographical and cultural factors. The structural similarities also suggests that Finland and Sweden are subject to similar and synchronized business cycle fluctuations. Indeed, both countries notably endured a deep financial crisis during the early 1990s (Honkapohja and Koskela 1999 and Jonung et al. 2008) and were particularly hard hit during the post-2007 crisis when the volume of international trade and demand for investment and durable consumption goods declined (Gylfason et al. 2010). In particular, these observations suggest that the two countries share most the characteristics that have been emphasized to be the most relevant in terms of fiscal policy effectiveness (Favero et al. 2011, Ilzetsky 2013, Corsetti et al. 2012a).

Second, the difference in monetary policy regimes is mostly due to geopolitical rather than economic considerations (see Gylfason et al. 2010, Chapter 8). This conclusion is supported by the history of recent monetary policy regime changes in the two countries. First, during the European exchange rate mechanism (ERM) crisis in 1992 both Finland and Sweden abandoned the currency peg to the european currency unit (ECU) and adopted a floating exchange rate regime (Kuusterä and Tarkka 2012, pp. 661-686). The float was adopted mostly due to the combination of Bundesbank's tight monetary policy, which raised interest rates pro-cyclically in the two countries that were in the middle of deep financial crisis, and speculative attacks on the currency peg. However, only a few years later, in 14.10.1996, Finland again joined the ERM and fixed it's exchange rate in the run-up to the common currency (Kuusterä and Tarkka 2012, pp. 762-772) and finally joined the monetary union in 1999 (Kuusterä and Tarkka 2012, pp. 776-782).[6] Sweden, on the other hand, decided to opt out from the common currency project, even though it became an EU member state in 1995 and was pre-committed to join also the monetary union, similar to Finland. This divergence in monetary policy strategy emerged despite the fact that both countries faced similar economic trade-offs with regards to the costs and benefits related to the monetary union: the efficiency of having a common currency with main trading partners, stabilization policy against asymmetric macroeconomic shocks and credibility of the monetary policy regime. However, the political motivation to join

[5]For example Gylfason et al. (2010, pp. 167-168) state that due to their differences in monetary policy arrangements "[a] comparison between Sweden and Finland is particularly pertinent, almost a laboratory experiment".

[6]The fixed exchange rate regime was subject to some adjustment during the first quarter of 1997 due to speculative attacks on the regime, but it was credible thereafter (Kuusterä and Tarkka 2012, pp. 762-772)

the monetary union, and thus the core in EU, was higher in Finland than in Sweden (Gylfason 2010 and Jonung and Sjöholm 1999). In particular, Sweden did not share the same geopolitical situation as Finland. This suggest that the divergence in monetary policy regime could be regarded as exogenous to overall economic activity.

Based on these observations, we argue that, since September 1996, the main difference with respect to fiscal policy environment in Finland and Sweden has been the exchange rate regime. We are not the first to take advantage of this type of a setting. It is similar to recent studies by Korkman and Suvanto (2013), Suni and Vihriälä (2013) and Gylfason et al. (2010) who study the macroeconomic performance of the two economies and the effects of exchange rate regime, but who do not attempt to estimate the effects of fiscal policy shocks on aggregate economic activity. On the other hand, previous empirical studies also adopt a similar strategy to control for the economic environment. For example, Cardi and Müller (2011) study the effects of fiscal policy shocks in economies that differ in their openness to international trade. Corsetti et al. (2012a) and Ilzetsky et al. (2013) and Baum et al. (2013), among others, control for the economic environment in a panel setting by placing dummies according to the exchange rate regime, the business cycle regime, government debt or other institutional factors. Compared to these previous studies, our approach does not require choosing possibly arbitrary threshold values to control for the degree of economic slack or other underlying variables. In particular, we may maintain standard (linear) SVAR assumptions, but evaluate the effect of monetary policy regime on fiscal policy transmission.

In order to study the effects of fiscal policy shocks, we follow Blanchard and Perotti (2002) and estimate a structural VAR-model for the two countries. This approach has some advantages over alternative identification strategies. First, an SVAR approach essentially enables a comparison of the two countries during the same time period, which is the key to controlling for the economic environment. For example, a narrative identification approach would not provide us with a similar empirical setting if the estimated discretionary fiscal policy measures were not undertaken in similar cyclical conditions or if the identified shocks did not have a comparable quantitative interpretation (f.e., due to measurement errors). Second, in contrast to most previous studies that consider the effectiveness of fiscal policy under different monetary policy regimes, we may take tax policy measures into account and do not limit ourselves to study government expenditure shocks only. In addition, as the Blanchard-Perotti SVAR approach still constitutes a definitive benchmark to study the effects of fiscal policy shocks (see, f.e., Mertens and Ravn 2013, Favero and Giavazzi 2012 and Auerbach and Gorodnichenko 2012), our results are directly comparable to most previous empirical evidence. Yet, it needs to be noted that this methodology does not account for inflation and monetary policy dynamics *endogenously*. Although this may bias our empirical results, we have have opted to consider a parsimonious model due to the small available quarterly sample. Also, incorporating more variables to the model has the caveat that additional restrictions needs to be imposed on the inter-relationship of all includes variables.

3.2 SVAR Model

Our starting point is a three-variable VAR model that includes government expenditure per capita g_t, net taxes per capita t_t and GDP per capita y_t. We define government expenditure as the sum of general government consumption and investment and net taxes as the sum of direct taxes on business, direct taxes on individuals, indirect taxes and social security contributions net of transfers to households and subsidies to private sector. All variables are in quarterly, real, logarithmic and seasonally adjusted terms. The reduced-form VAR takes the form

$$\mathbf{x}_t = C(L)\mathbf{x}_{t-1} + \mathbf{u}_t, \tag{3.1}$$

where $\mathbf{x}_t \equiv [g_t \quad t_t \quad y_t]^T$ is the vector of endogenous variables, $C(L)$ is an autoregressive lag polynomial and $\mathbf{u}_t \equiv [u_g \quad u_t \quad u_y]$ is a vector of reduced-form residuals with an expected value of zero. Estimating the reduced-form model (3.1) does not help us to study any causal relationship between the endogenous variables as the impulse responses are not unique and the reduced-form errors are cross-correlated, i.e. $\mathbf{u}_t$ has a non-diagonal correlation matrix $\sum_{\mathbf{u}}$ (Lütkepohl 2006). For causal interpretation, we need to identify unique structural errors that are instantaneously uncorrelated.

Now consider an SVAR model of the form

$$\mathbf{A}\mathbf{x}_t = \mathbf{A}C(L)\mathbf{x}_{t-1} + \mathbf{B}\varepsilon_t \tag{3.2}$$

where $\varepsilon_t \sim (0, \sum)$ is a vector of mutually uncorrelated structural errors and $\mathbf{A}$ and $\mathbf{B}$ are (3×3) matrices. The structural errors ε_t can be estimated from a reduced-form VAR by adding restrictions on matrices $\mathbf{A}$ and $\mathbf{B}$. First, the reduced-form VAR residuals are recovered by multiplying both sides of (3.2) by the matrix $\mathbf{A}^{-1}$ from the left hand side and noting that $\mathbf{u}_t = \mathbf{A}^{-1}\mathbf{B}\varepsilon_t$. This implies that the reduced-form correlation matrix has the form $\sum_u = \mathbf{A}^{-1}\mathbf{B}\mathbf{B}^T\mathbf{A}^{-1T}$ and can hence be consistently estimated from the reduced-form model (3.1) and treat as known. Next, we need to impose the restrictions on the matrices $\mathbf{A}$ and $\mathbf{B}$. For these restrictions we apply economic theory and non-sample information.

Blanchard and Perotti (2002) propose that the reduced-form errors in (3.1) are a weighted average of three different types of shocks. First, taxes and government spending respond automatically to output shocks. This is reflected in increased unemployment related spending and decreased income tax revenues during recessionary time periods, for example. Second, governments may respond systematically to cyclical conditions by increasing subsidies or cutting taxes in recessions. Third, there are random discretionary fiscal measures that are exogenous to output shocks. These latter fiscal measures constitute the structural errors (i.e. fiscal shocks) that we want to recover. Thus, according to Blanchard-Perotti identification, we can express the reduced-form errors as a weighted average of automatic, systematic and structural shocks

$$u_t^g = \alpha_{gy}u_t^y + \beta_{gt}e_t^t + e_t^g \tag{3.3}$$

$$u_t^t = \alpha_{ty} u_t^y + \beta_{tg} e_t^g + e_t^t \tag{3.4}$$

$$u_t^y = \alpha_{yg} u_t^g + \alpha_{yt} u_t^t + e_t^y \tag{3.5}$$

where the subscript t is the time index, the superscript refers to the endogenous variable, the terms multiplied by α_{gy} and α_{ty} capture the automatic and systematic response of fiscal variables to output shocks and e_t^g and e_t^t are the contemporaneously uncorrelated structural shocks. The final equation (3.5) states that observed shocks to output are a combination of net tax shocks, government spending shocks and structural output shocks.

Next, the Blanchard-Perotti identification relies on the assumption that it takes more than a quarter from the government to react to economic shocks (i.e. shocks to real output). This means, quite plausibly, that it takes more than a quarter to decide, to legislate and to implement a new discretionary fiscal measure. Under this assumption, the systematic policy response is unobserved in quarterly frequency data and it is sufficient to estimate only the automatic response of fiscal variables to economic shocks in order to successfully recover the structural shocks above. From the equations (3.3) and (3.4) we can write cyclically-adjusted shocks as

$$u_t^{g,A} \equiv u_t^g - \alpha_{gy} u_t^y = \beta_{gt} e_t^t + e_t^g \tag{3.6}$$

$$u_t^{t,A} \equiv u_t^t - \alpha_{ty} u_t^y = \beta_{tg} e_t^g + e_t^t, \tag{3.7}$$

where the superscript A stands for adjusted and α_{gy} and α_{ty} now correspond to output elasticities of government expenditure and net taxes respectively. Section 2.4 describes the estimation of these elasticities in more detail.

Now, in order to estimate structural errors e_t^g and e_t^t from the equations (3.6) and (3.7) we need to set either β_{gt} or β_{tg} equal to zero, otherwise we would have too many unknowns to solve the system of equations. This corresponds to orthogonalizing government expenditure and net tax shocks to each other. Blanchard and Perotti (2002) argue that there is no theory to choose the order of orthogonalization, but suggest that if the correlation between $u_t^{g,A}$ and $u_t^{t,A}$ is small, the ordering should not affect the identification procedure and dynamic results to any significant degree. Perotti (2005) further argues that any of the chosen rotations is equally valid given that no theoretical argument exists in favor of the other. Following the literature, we choose to set β_{tg} equal to zero implying that net tax measure are made prior to expenditure measures. We may then estimate β_{gt} by standard OLS methods from (3.7). The residual from the OLS regression yields us an estimate of structural net tax shocks e_t^t.

We are now left with the final equation (3.5) to estimate. The problem is that the reduced form errors u_t^g and u_t^t that we have as regressors are correlated with structural shocks to output and are hence endogenous. However, we can use the structural shocks e_t^t and e_t^g, which are orthogonal to other shocks within the model, as instrumental variables for estimation. We may thus estimate the contemporaneous effect of net taxes and government spending to output from (3.5) above by instrumental variables (two-stage least squares) regression. Finally, we may now

write the restriction matrices in (3.2) as

$$\mathbf{A} = \begin{bmatrix} 1 & 0 & -\alpha_{gy} \\ 0 & 1 & -\alpha_{ty} \\ -\alpha_{yg} & -\alpha_{yt} & 1 \end{bmatrix}$$

$$\mathbf{B} = \begin{bmatrix} 1 & \beta_{gt} & 0 \\ \beta_{tg} & 1 & 0 \\ 0 & 0 & 1 \end{bmatrix}.$$

4 Data

This section presents our main fiscal dataset. It consists of two parts. The first subsection describes the quarterly fiscal data that is employed in estimating the underlying VAR model. The second subsection presents our framework to estimate output elasticities of net taxes and government expenditure.

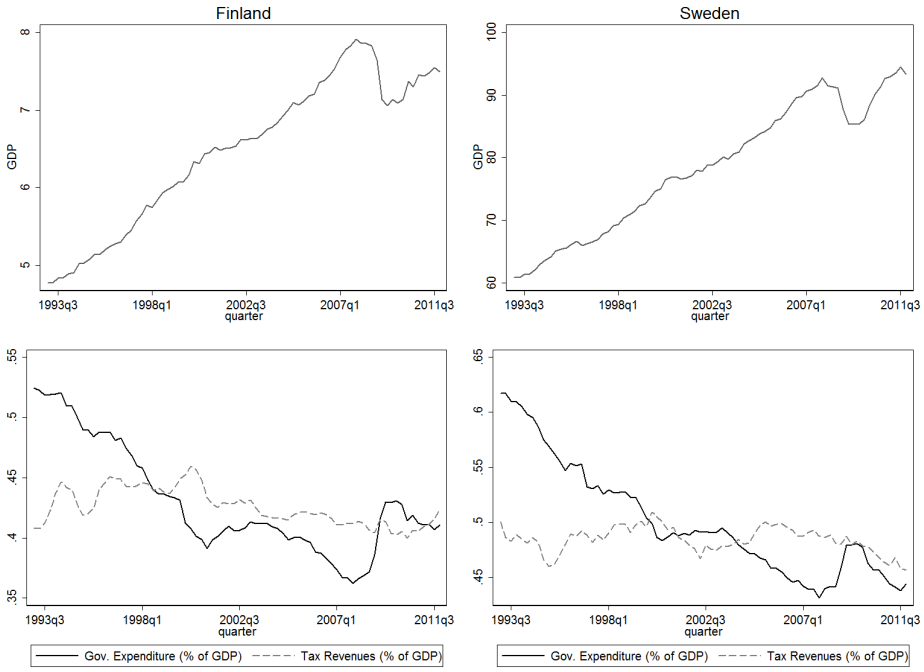
4.1 Descriptive Statistics

Our data set covers quarterly national accounts for Finland (1975:1-2011:4) and Sweden (1993:1-2011:4). To the best of our knowledge, it represents the largest available quarterly sample of fiscal variables for which both the general government revenue and expenditure data is available for the two countries. The Finnish national account data is provided by the Statistics Finland, except for the data on direct taxes on business which is from the Bank of Finland quarterly estimates.[7] The Swedish national account data is provided by the Statistics Sweden. The data consists of quarterly GDP, GDP deflator, general government consumption and investment, tax revenues, transfers to households and subsidies to private sector. Tax revenues consist of direct taxes on business, direct taxes on individuals, social security contributions and indirect taxes. As the quarterly data on tax revenues and transfers is only provided in actual values, which are subject to notable seasonal patterns, we have seasonally adjusted the respective data with the standard TRAMO&SEATS method.

This data set has the advantage that we can study the effects of fiscal policy shocks during the full time period during which the two countries have had a different exchange rate regime. Moreover, we may study how the effects of fiscal policy shocks change over time when Finland was part of different monetary policy regimes (i.e., prior to 1996:4). In addition, the data set provides us the possibility to test for the robustness of our baseline model specification in the presence of fiscal foresight. (We motivate this later in Section 6.)

[7] To the best of our knowledge, Statistics Finland does not provide data for direct taxes on business in quarterly terms. The data Bank of Finland data does not represent true quarterly variation so that it is used only as a proxy for quarterly revenues from direct taxes on business.

Figure 4.1: GDP, Tax Revenues and Government Expenditure (1993:1-2011:4)



Note: Real GDP in logarithmic per capita terms in national currency. Government expenditure and tax revenues as a percentage of GDP.

Figure 4.1 shows government expenditure, tax revenues and real per capita GDP for Finland and Sweden in 1993:1:2011:4. For brevity, we concentrate here on the sample period which is common for the two countries. The first row shows the real output growth path, as measured by logarithm of per capita output in national currency. It illustrates the considerable similarity of the aggregate economic environment in the two countries during the sample period. For example, in 1993:1-2011:4 the average quarterly output growth was 2.3% in both countries and the correlation of quarterly growth was 0.62. The second row shows the similarities in *fiscal policy* environment during the same period and especially the consequences of the early 1990s financial crisis (see Jonung et al. 2008, Honkapohja and Koskela 1999 and Flodén 2013). First, by the year 1993, when both countries had returned to growth path, they were faced with considerable fiscal policy challenges, reflected in the large imbalances between tax revenues and government expenditure. By 2000, the government fiscal position reversed and was in surplus in both countries, mostly due to a combination of strict fiscal consolidation measures, rapid economic growth, external demand and loose monetary policy.[8] Second, the significant fall in economic activity during the post-2007 financial crisis induced similar effects on public finances. It led to an increase in government expenditure due to increases in transfers to households, but

[8]Note that the difference between tax revenues and government expenditure does not equal to the budget balance.

Table 1: Descriptive Statistics (1993:1-2011:4)

		Finland		Sweden	
Variable	Obs	Mean	Std. Dev.	Mean	Std. Dev.
Indirect Taxes	76	0.12	0.005	0.17	0.010
Soc. Sec. Contributions	76	0.09	0.005	0.10	0.012
Direct Taxes on Households	76	0.18	0.013	0.18	0.009
Direct Taxes on Business	76	0.03	0.013	0.03	0.006
Transfers to Households	76	0.19	0.027	0.17	0.014
Gov. Consumption	76	0.21	0.018	0.30	0.034
Gov. Investment	76	0.03	0.003	0.03	0.004

Note: General government expenditure and tax revenues as a percentage of GDP.

also a significant decrease in tax revenues, which are largely proportional to output.

The structural similarities are, on the other hand, illustrated by studying the role of individual expenditure and tax categories. Table 1 describes the disaggregated net taxes and government expenditure for Finland and Sweden. When compared to the OECD average (see, f.e., Sørensen 2010), the distinctive characteristic of these two countries is the relatively large role of direct taxes on individuals (amounting to 18% of GDP in both countries on average) and the high share of transfers to households (amounting to nearly 20% of GDP in both countries). Also, value added taxes (VATs) in Finland and Sweden have been one of the highest in OECD countries and increasing in significance as a source of total tax revenues during the last decade. In our sample, indirect taxes average 12% and 17% of GDP. In contrast, direct taxes on business constitute only a small fraction of tax revenues, amounting to 3% of GDP in both countries. On the expenditure side, government consumption amounts to 21% and 30% of GDP and investment amounts to about 3% of GDP.

Overall, the government expenditure and tax revenue data suggests that the cyclical responsiveness of fiscal policy, or the so-called automatic stabilizers, are of similar magnitude in the two countries. We will pursue to study this further in the following section.

4.2 Net Tax and Government Expenditure Elasticities

The identification of fiscal policy shocks represented in Section 3 relies on the assumption that we may consistently estimate the within-a-quarter automatic response of government expenditure and net taxes to output shocks. As suggested by Blanchard and Perott (2002), we rely on the official OECD estimation method to calculate the output elasticities of fiscal variables (see Girouard and André 2005). However, our estimates differ from the OECD estimates in terms of time-frequency and in terms of the output measure employed. In particular, we estimate the within-a-quarter elasticity of net tax revenues with respect to GDP whereas the OECD calculates the *within-a-year* elasticity of net tax revenues with respect to *the output gap*.

In order to estimate the aggregate output elasticity, we divide net taxes to five categories

that we allow to have different output elasticities: direct taxes on individuals, direct taxes on business, social security contributions, indirect taxes and transfer to households. We assume that unemployment benefits is the only component of transfers that responds to output fluctuations within a quarter and that government expenditure is predetermined within a quarter, implying the associated output elasticity amounts to zero.

Output elasticity of each net tax category consist of i) calculating the elasticity of net tax revenue with respect to its tax base and ii) the elasticity of tax base with respect to output. Overall, the aggregate elasticity of net taxes is then calculated as the weighted average

$$\alpha_{t,y} = \sum_{i=1}^{5} \alpha_{T_i,y} \frac{T_i}{T} = \sum_{i=1}^{5} \varepsilon_{T_i B_i} \varepsilon_{B_i,y} \frac{T_i}{T}, \tag{4.1}$$

where T is the sum of net taxes, ε_{T_i,B_i} is the elasticity of net tax revenue T_i to its tax base B_i, and $\varepsilon_{B_i,y}$ is the elasticity of tax base to GDP. The tax base B_i depends on the net tax category: the tax base for indirect taxes is private consumption; the tax base for transfers to households is unemployment; tax base for direct taxes to business is profits; the tax base for income taxes and social security contributions is the number of wage earners and the wage bill.[9] Thus, for the latter two categories, the corresponding output elasticity effectively consists of three sub-elasticities.[10]

The elasticity of tax revenues to tax base depends on the national tax code. For example, the progressiveness of income taxation, income brackets and household income distribution determine how tax revenues respond to increases in the aggregate wage bill. Overall, estimating these elasticities requires extensive and detailed institutional knowledge on the tax system. Since we prefer estimates that are mutually consistent and directly comparable across the two countries, we obtain tax revenue to tax base elasticities directly from the official OECD estimates (Girouard and André 2005). These estimates are based on a micro-simulation model and incorporate detailed information on national tax code and income distribution.

The elasticity of tax base to output, on the other hand, depends on structural factors such as the labour market, consumer behavior and tax collection practice. For example, the more there are rigidities in the labour market, the lower the within-a-quarter responsiveness of employment and unemployment to output shocks is likely to be. In order to estimate these elasticities, we follow the procedure suggested in the previous fiscal policy literature. In particular, we estimate

[9] We proxy profits by operating surplus.

[10] The output elasticity of direct taxes on households and social security contributions can be decomposed as

$$\alpha_{T_i,y} = \frac{\partial T_i}{\partial y}\frac{y}{T_i} = \frac{\partial\left[(T_i/E)E\right]}{\partial y}\frac{y}{T_i} = \left(\frac{\partial E}{\partial y}\frac{y}{E}\right)\left[\left(\frac{\partial(T_i/E)}{\partial W}\frac{W}{T/E}\right)\left(\frac{\partial W}{\partial E}\frac{E}{W}\right)+1\right] = \varepsilon_{E,y}(\varepsilon_{T_i,W}\varepsilon_{W,E}+1),$$

where E is employment and W is the wage bill. The first term on the right hand side is the elasticity of employment to output and the second term is the elasticity of tax revenues per worker. The latter is composed of the elasticity of tax revenues to wage bill $\varepsilon_{T_i,W}$ (estimated by the OECD) and the elasticity of wage bill to employment $\varepsilon_{W,E}$.

Table 2: Output Elasticities

	Finland		Sweden	
Net tax category	$\alpha_{T_i,y}$	T_i/T	$\alpha_{T_i,y}$	T_i/T
Direct taxes on business	2.17	0.11	2.83	0.09
Direct taxes on individuals	0.14	0.73	0.16	0.55
Indirect taxes	1.00	0.50	1.00	0.52
Social sec. contributions	0.14	0.37	0.14	0.31
Transfers	-0.20	-0.77	-0.14	-0.53
Net taxes	**1.16**	1.00	**0.98**	1.00

Source: author's own calculations and Girouard and André (2005).

tax base to output elasticities from a reduced-form regression of the form

$$b_{i,t} - b_{i,t-1} = a_i + \varepsilon_{B_i,y}(y_t - y_{t-1}) + \epsilon_{i,t}, \tag{4.2}$$

where a_i is a constant term and the coefficient $\varepsilon_{B_i,y}$ represents the elasticity of tax base i to real output y. The tax base $b_{i,t}$ and real output y_t are both expressed in logarithmic form. We include four lagged values in each regression to control for collection lags and possible quarter dependency and estimate the regression in first differences similar to Blanchard and Perotti (2002), Perotti (2005) and Giruard and André (2005).

Table 11 (in the Appendix) shows our estimation results on the elasticity of tax base to output elasticities. We found no evidence that these elasticities would have varied significantly across time so that we decide to take advantage of the full 1975-2011 sample in estimating these elasticities for Finland. The findings can be summarized as follows. First, as expected, we find that profits are highly sensitive to output fluctuations. A one-percentage change in output induces a more than two-percent increase in corporate profits in both countries. Second, we find that the wage bill increases by nearly one percentage point when output increases by one percentage point. However, this occurs only after a one-quarter lag in Finland while it occurs within a quarter in Sweden. Third, we find that employment responses only modesty to output fluctuations within a quarter. Fourth, we find that unemployment decreases by more than one percent within a quarter in response to a one-percent increase in output in both countries.

In addition to these empirical estimates, we assume that consumption responds with a unit elasticity to output shocks within a quarter. This assumption is likely to overestimate the true response of consumption as economic agents may tend to smooth their consumption over time. However, the simplifying assumption is motivated by the observation that estimating the consumption response in a reduced form setting yields possibly ambiguous or biased empirical results (see Girouard and André 2005). We argue that this assumption is not likely to have any significant effect on the identification of fiscal policy shocks, but we will further discuss the uncertainty related to output elasticities below.

Finally, we calculate the total output elasticities for each net tax category as well as the

aggregate output elasticity as given in (4.1). Our estimates yield a total output elasticity of 1.16 for Finland and 0.98 for Sweden (see Table 2). Thus, a one percentage point increase in output implies a broadly one percentage point increase in net tax revenues in both countries. This is consistent with the observation that tax revenues tend to follow output movements proportionally (see Figure 4.1). The difference in the elasticity estimates in Finland and Sweden is mostly due to somewhat larger weight on direct taxes on households and transfers to households in Finland within the sample period. In contrast to our estimates, previous studies find that the output elasticity has varied from 0.92 in Germany to 2.08 in the U.S. (Perotti 2005 and Blanchard and Perotti 2002). The lower output elasticity in the Nordic countries than in the U.S. is, in part, due to the lower employment response to output shocks and also due to the much higher corporate profit response in the U.S. (Perotti 2005). Overall, we may conclude that the estimated elasticities, i.e. the automatic response of net taxes to output shocks, is rather similar in Finland and Sweden. This is consistent with our assumption that the public sector institutional factors and the economic structure are similar in the two countries.

5 Results

This section presents our results for the effects of fiscal policy shocks under different exchange rate regimes. The baseline results are estimated for 1996:4-2011:4 for Finland and 1993:1-2011:4 for Sweden. We exploit the full sample for Sweden in order to have the maximum amount of observations. We found no evidence that the inclusion of 1993:1-1996:3 sample would drive our results. However, under the assumption that fiscal multipliers are higher during times of financial crisis, we bias our results toward finding higher multiplier effects for Sweden.

The rest of this section proceeds as follows. First, we estimate the contemporaneous effects of fiscal shocks by instrumental variables regression. Second, we estimate the dynamic effects of fiscal policy shocks in Finland and Sweden when the former was part of the EMU and the latter had an independent central bank and a floating currency. Finally, we consider the robustness of our results in alternative specifications.

5.1 Estimation

We start by estimating a three-variable VAR-model including government expenditure, net taxes and real GDP in log levels with a constant and a linear time trend. To determine an appropriate lag structure, we consider information criteria up to a maximum of four lags, which is the common reference in the previous literature, and test for autocorrelation of residuals by Portmanteau test as well as for the stability of the VAR. In addition, we opt for a parsimonious model due to our limited sample size in the baseline specification. Based on this criteria, we estimate a VAR(3)-model for Finland and a VAR(2)-model for Sweden.

Table 3 shows our estimates of the contemporaneous effects of fiscal policy shocks based

Table 3: Estimated Model Parameters

	Finland			Sweden		
	α_{yg}	α_{yt}	β_{gt}	α_{yg}	α_{yt}	β_{gt}
coefficient	0.07	-1.89*	-0.02	0.05	0.04	-0.05
s.e.	(0.46)	(1.02)	(0.09)	(0.15)	(0.06)	(0.05)
p-value	0.89	0.07	0.84	0.75	0.51	0.36

Note: Standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

on the instrumental variables regression.[11] The left panel shows the results for Finland under a fixed exchange rate regime. We find that a net tax shock has a negative contemporaneous effect on output which is significant at the 10% significance level. The sign of the response is consistent with the assumption that contractionary fiscal policy measures have negative effects on economic activity. In contrast, we find no clear evidence of contemporaneous effect of output to an expenditure shock. The coefficient is nearly zero, but it is associated with a rather large standard error. The right panel shows the results for Sweden under a flexible exchange rate. Now, we find no evidence of either net tax or spending shocks affecting output within the first quarter. The coefficients are close to zero, but the standard errors are also considerably smaller than what was found for Finland.

The contemporaneous output effects that we find are in contrast with, for example, Blanchard and Perotti (2002) who estimate that government expenditure and net tax shocks have symmetric and statistically significant contemporaneous effects on output in the United States. However, we find our estimation results to be qualitatively independent of the variable order, lag structure, sample period and assumptions regarding the underlying trend in time-series. In particular, we find that the contemporaneous negative effect of tax policy shocks in Finland is significant across different specifications. The finding that the standard errors associated with the estimates for Sweden are rather small instead suggests that the contemporaneous effects of fiscal policy shocks might be dampened in small open economies, especially when it is operating under a flexible exchange rate regime.

Finally, the third column of Table 3 shows the estimate of contemporaneous correlation with government expenditure and net taxes. We find that cyclically-adjusted shocks in net taxes have a negligible effect on cyclically-adjusted expenditure shocks in both countries (i.e. the coefficient β_{gt} is not significantly different from zero). This is similar to the findings of Blanchard and Perotti (2002), and it supports the conclusion that the identification procedure is not be driven by the (orthogonalization) ordering of net tax and spending within the VAR. But we will return to the latter later in our analysis.

[11] Note that these shocks do not have a unit per unit interpretation.

5.2 Dynamic Effects of Fiscal Policy Shocks

We now present our baseline results for the dynamic effects of fiscal policy shocks under different exchange rate regimes. We first consider the effects of government expenditure shocks. Figure 5.1 and Table 4 (rows 1-3) show the impulse responses to a positive and exogenous government expenditure shock that equals to one percentage point of GDP. We scale the impulse responses using the average share of net taxes and government spending to GDP.[12] The dashed lines represent bootstrapped one and two standard deviation bands based on 2000 replications respectively.[13]

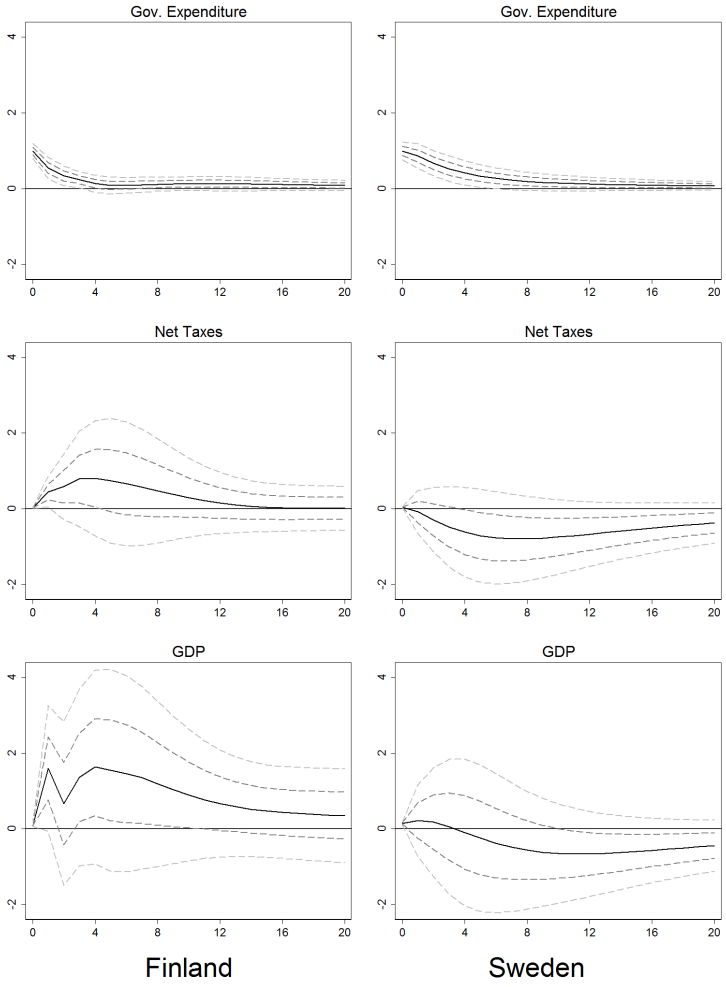
Consider first the left panel that shows the impulse responses for Finland under a fixed exchange rate regime. The first row shows that a 1% of GDP government expenditure shock declines to 0.1 p.p of GDP during the following four quarters after the initial shock. The following rows show the responses of net taxes and output to government expenditure shock. In particular, a government expenditure shocks has a positive effect on GDP at all horizons. The effect peaks at 1.6 p.p. of GDP after four quarters of the initial shock. The peak response follows with some lag similarly as in the previous fiscal SVAR literature considering government expenditure shocks. The initial response is significantly different from zero at the two standard deviation confidence level. Then, except for the second quarter kink, the response is above zero at the one standard deviation confidence level. Though the confidence intervals are wide, the higher-than-unity output response suggests that a government expenditure shock tends to crowd in private activity. Finally, the second row shows that net taxes respond positively to an expenditure shock, consistent with the positive reaction in aggregate economic activity. The response peaks at 0.8 p.p. after four quarters.

In contrast, the right panel of Figure 5.1 shows the impulse responses to a government expenditure shock in Sweden under a floating exchange rate regime. Similarly as above, a government expenditure shock declines gradually after the initial shock, though the shock is now somewhat more persistent. However, an expenditure shock now has distinctly different effects on net taxes and output from those that we found above for Finland. First, output responses rather modestly to an expenditure shock. It peaks at only 0.2 p.p. after two quarters and it becomes negative after four quarters. The output response is essentially insignificant as zero falls clearly between both one and two standard deviation confidence intervals. In addition, net taxes decrease in response to an expansion in government expenditure. The peak response equals to -0.8 p.p. after eight quarters. This is in contrast to the positive net tax response in Finland, but it is consistent with the response in aggregate economic activity. The modest response of output and a negative effect on net taxes suggest that government expenditure crowds out private economic activity, either directly or indirectly by inducing inflation targeting monetary policy to offset the expansionary effects by raising policy rates.

[12] For the baseline sample period these shares amount to 0.24 and 0.23 for Finland and 0.33 and 0.30 for Sweden respectively.

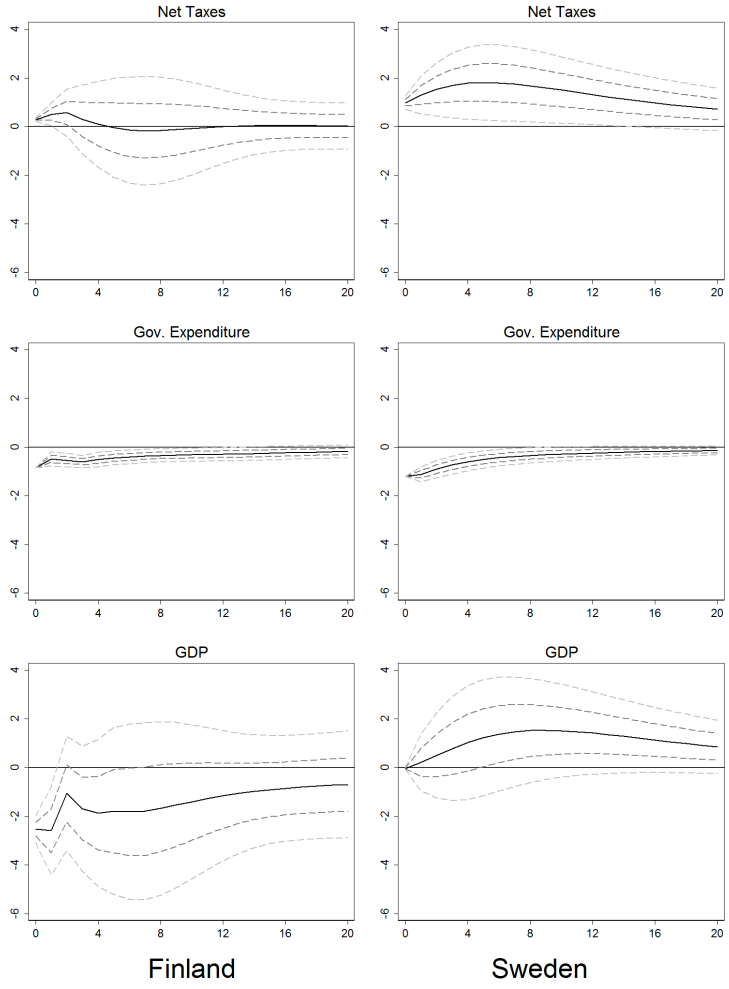
[13] Note that these confidence intervals do not take into account uncertainty related to the model parameter estimation.

Figure 5.1: Impulse Responses to Government Expenditure Shock



Note: Impulse responses to a 1% of GDP government expenditure shock. Finland: impulse responses are based on (1996:4-2011:4) sample and underlying VAR(3) model in log levels with a deterministic time trend. Sweden: impulse responses are based on (1993:1-2011:4) sample and underlying VAR(2) model in log levels with a deterministic time trend. Dashed lines represent bootstrapped one and two standard deviation confidence intervals respectively.

Figure 5.2: Impulse Responses to Net Tax Shock



Note: Impulse responses to a 1% of GDP net tax shock shock. Finland: impulse responses are based on (1996:4-2011:4) sample and underlying VAR(3) model in log levels with a deterministic time trend. Sweden: impulse responses are based on (1993:1-2011:4) sample and underlying VAR(2) model in log levels with a deterministic time trend. Dashed lines represent bootstrapped one and two standard deviation confidence intervals respectively.

Table 4: Impulse Responses to Exogenous Fiscal Policy Shocks

		Finland			Sweden		
Shock	Response	0	4	8	0	4	8
Gov. Exp	Gov. Exp.	1.00**	0.13*	0.12*	1.00**	0.42**	0.19*
		(0.10)	(0.11)	(0.09)	(0.12)	(0.16)	(0.12)
Gov. Exp	Net Taxes	0.03**	0.81*	0.48	0.04**	-0.62*	-0.79*
		(0.00)	(0.76)	(0.69)	(0.01)	(0.60)	(0.56)
Gov. Exp	GDP	**0.09****	**1.64***	**1.19***	**0.15****	**-0.09**	**-0.56**
		(0.01)	(1.30)	(1.09)	(0.02)	(0.97)	(0.78)
Net Taxes	Net Taxes	0.29**	0.10	-0.15	0.99**	1.80**	1.70**
		(0.04)	(0.88)	(1.10)	(0.14)	(0.74)	(0.74)
Net Taxes	Gov. Exp	-0.85**	-0.51**	-0.34**	-1.20**	-0.60**	-0.34**
		(0.00)	(0.15)	(0.13)	(0.00)	(0.18)	(0.16)
Net Taxes	GDP	**-2.53****	**-1.87***	**-1.67**	**-0.03***	**1.03**	**1.52***
		(0.28)	(1.51)	(1.78)	(0.02)	(1.20)	(1.06)

Note. Impulse responses to 1% of GDP fiscal shocks corresponding to quarters 0, 4 and 8. Bootstrapped standard errors in parentheses, ** significant at two standard deviation confidence level, * significant at one standard deviation confidence level. Finland: impulse responses are based on (1996:4-2011:4) sample and underlying VAR(3) model in log levels with a deterministic time trend. Sweden: impulse responses are based on (1993:1-2011:4) sample and underlying VAR(2) model in log levels with a deterministic time trend.

Our findings compare to those of previous empirical fiscal policy studies that have applied a similar SVAR methodology. On the one hand, Blanchard and Perotti (2002) and Perotti (2005) find that discretionary fiscal policy shocks have a near unit effect on total output in the U.S. economy. This is contrasted by our estimates that, conditional on the economic environment, government expenditure shocks in a small open economy may have either much higher (Finland) or lower (Sweden) effects on economic activity than in a large closed economy such as the U.S.. Still, our point estimates are associated with larger confidence intervals, which is most likely to be due to smaller available sample. On the other hand, our results are consistent with Perotti (2005), Monacelli and Perotti (2010) and Cardi and Müller (2011) who find that fiscal policy shocks have been less expansionary, or even contractionary, in small open economies under a floating exchange rate regime, such as Canada and Australia. In addition, our results are in accordance with the findings of Corsetti et al. (2012a), Ilzetzki et al. (2013), Born et al. (2013) and Beetsma et al. (2013) who find, while using different identification strategies, that the effectiveness of discretionary government expenditure is greater within a fixed exchange rate regime than under a floating exchange regime. Similar quantitative estimates emerge also in studies that differentiate the impulse responses according to whether the economy is in recession or expansion regime. For example, Auerbach and Gorodnichenko (2012) estimate using a similar identification strategy that the fiscal multiplier has been negative in economic expansions (-0.33), but highly expansionary in recessions (2.24).

We next turn to study the effects of net tax measures. Figure 5.2 and Table 4 (rows 4-6) show the impulse responses to a positive net tax shock that equals to one percentage point of GDP. The left panel again shows the results for Finland. First, a net tax shock declines during the first four quarters, similar to an expenditure shock above. The shock tends to be hump-shaped and initially below one. This is due to the fact that net tax shocks have a negative contemporaneous effect on output, which then translates to lower collected net tax revenues. Second, a net tax increase has a negative dynamic effect on output, which peaks at -2.6 p.p. during the first quarter following the initial shock. The peak effect is significant at the two standard standard deviation level. Thereafter, except for the second quarter kink following the initial net tax shock, the output effect is negative and significant at one standard deviation level until the eight quarter. In addition, the second row shows that a net tax shock is associated with government expenditure decreases. This is likely to be due to the fact that during the late 1990s the Finnish government was engaged in fiscal consolidation measures to cut down public debt that combined both government expenditure and net tax measures.

The right panel of Figure 5.2 shows the results for Sweden. The effects again differ from the ones we found for Finland. Now, a net tax shock is more persistent when compared to previous cases. Remarkably, we find that a net tax shock tends to have a positive effect on output, i.e., that a net tax increase induces an expansion in aggregate economic activity. The response is small and insignificant on impact, but it increases gradually and peaks at 1.55 p.p. after 9 quarters after the shock. The positive output response to net tax measures is likely to explain the persistency of net tax revenue shock: a moderate or expansionary effect on output induces economic agents to pay more taxes relative to the case in which economic activity decreases. Similar to above, we find that net tax shocks are associated with government expenditure reductions, consistent with consolidation measures that took place in Sweden (Flodén 2013).

Our findings considering the effects of net tax shocks are consistent with the estimates of output responses to government expenditure shocks, and also with previous literature, which finds that the effects depend on the monetary policy regime and the degree of monetary policy accommodation. The moderate and (initially) insignificant response of output to fiscal policy shocks in Sweden suggests that fiscal policy measures crowd our private activity. In particular, our results suggest that expansionary fiscal policy measures induce monetary policy to pursue counteractive measures to maintain price stability (i.e., to raise interest rates) under flexible exchange rates. Higher interest rates may then curb private activity, appreciate the currency and hence reduce net exports. Alternatively, one can interpret this as monetary policy counteracting deflationary pressure associated with fiscal consolidation measures. Under a fixed exchange rate regime, on the other hand, such monetary policy measures are absent.

Now, we cannot directly exclude that the "non-Keynesian" effect of net tax shocks in Sweden is i) due to structural changes that widened the tax base, ii) due to confidence effects associated with significant fiscal consolidation efforts that took place in the aftermath of the 1990s financial crisis or ii) due to some omitted variable in our model specification. For exam-

Table 5: Conditional Fiscal Multiplier Effects

	Finland			Sweden		
	0	4	8	0	4	8
Net Taxes	-8.68	-5.50	-13.21	-0.03	0.34	0.56
Gov. Exp.	0.09	2.38	4.12	0.15	0.15	-0.25

Note: Fiscal multipliers are defined as $M_p^g = \frac{\sum_0^p y_p}{\sum_0^p g_p}$ and $M_p^t = \frac{\sum_{p=0}^8 y_p}{\sum_{p=0}^8 t_p}$, where y_p, g_p and t_p are the impulse responses for GDP, government expenditure and net taxes corresponding to quarter p.

ple, Romer and Romer (2010) find some evidence of expansionary tax increases within the U.S. when the motivation for tax changes was to combat deteriorating budget balances. Further, previous studies that consider fiscal consolidation measures in small open economies conclude that fiscal consolidations may have expansionary effects on economic activity (see., f.e., Giavazzi and Pagano 1990 and Alesina and Ardagna 2010 for empirical evidence and Barry and Devereux 2003 for a theoretical consideration). Notably, Giavazzi and Pagano (1995) find that net tax decreases have contractionary effects on domestic demand in Sweden. However, the reduced-form estimation strategy employed in this literature, which relies on cyclically-adjusted fiscal variables and concentration on large and persistent fiscal consolidations, is conceptually different from ours. In particular, this literature does not account for the possibility that the consolidation measures might well have been predicted by macroeconomic data and by the private sector. In addition, the findings of this "expansionary austerity" literature are questioned in recent fiscal policy contributions. For example, Perotti (2011a) argues that in the above cases the main driver of economic expansion was increased external demand which took effect due to exchange rate depreciation and wage moderation rather than fiscal consolidation measures or the associated confidence effects. To conclude, the fact that we do not find similar expansionary net tax increases for Finland, which endured a similar consolidation process, suggests that these expansionary effects are in any case conditional on the economic environment and, especially, dependent on the monetary policy regime.

So far we have studied the effects of fiscal policy shocks through impulse response analysis. This is beneficial as a significant amount of research has presented impulse responses which have a similar interpretation. However, fiscal policy literature in general tends to asses the output effects in terms of fiscal multipliers. In particular, Woodford (2011) suggests that one needs to take into account the persistence of both the fiscal policy shock and the output response. In order to approximate the fiscal multiplier, we divide the cumulative response of output to a fiscal shock by the cumulative impulse of the corresponding fiscal shock. Now, the caveat of this measure is that, as we have shown, the response of net taxes to an exogenous increase in net taxes does not equal to unity, even in the quarter it is implemented, due to contemporaneous feedback from aggregate economic activity to net tax revenues. This implies that calculating the cumulative multiplier underestimates the discretionary fiscal effort underlying net tax measures

and, as a consequence, overestimates the net tax multiplier. This is especially the case in Finland where the contemporaneous output effect is significant and the effect on net revenues becomes negative at longer horizons. On the other hand, government expenditure shocks are not similarly subject to this caveat, at least to the extent that our assumption of zero within-a-quarter response to output is plausible.

Bearing these considerations in mind, Table 5 shows the point estimates for government expenditure and net tax output multipliers. Clearly, one needs to be cautious in interpreting these multipliers as we do not provide confidence intervals for the point estimates. Yet, it is notable that, despite the similar economic environment in the two economies, the estimated (government expenditure) multiplier is considerably higher in Finland than in Sweden. The multiplier falls below one in absolute value in Sweden, but is higher than unity in Finland. Together with the evidence provided in the impulse responses analysis, our estimates are consistent with the hypothesis that there exists a qualitative difference in the effectiveness of fiscal policy in Finland, which is part of the euro zone, and Sweden, which has an independent central bank and a floating currency. In particular, our evidence indicates that expansionary fiscal policy is more effective under a fixed exchange rate regime than under a floating exchange rate regime.

5.3 Robustness Analysis

This section studies the robustness of our baseline results. In particular, we test whether our results are sensitive to ordering of fiscal variables, the assumptions underlying the output elasticity estimates, the selected sample period and the underlying de-trending method. We argue that the main conclusion in the previous section, namely that the effects of fiscal policy are qualitatively different under different exchange rate regimes, is robust to this variety of sensitivity tests. Tables 6, 7 and 8 summarize these results. We present the impulse responses in graphical form in the Appendix.

We first test whether our results are sensitive to ordering of the fiscal variables in the underlying VAR model. In Section 3, we provided evidence that the cyclically-adjusted government expenditure and net tax shocks show no significant contemporaneous correlation. We argued, along with the previous literature, that the ordering of net taxes and government expenditure should not affect our results significantly. However, in the previous section we saw that net tax shocks tend to be associated with contemporaneous government expenditure reductions, possibly due to fiscal consolidation measures and structural reforms that took place during the sample period in the two countries. To be more precise, this is interpreted as a causal link between net tax and government expenditure in our empirical model. This interpretation is unwarranted as we have chosen the ordering between fiscal variables somewhat arbitrarily. Notwithstanding, we find that the qualitative results are largely unaffected once we estimate the model under the assumption that government expenditure measures precede net taxes (see Table 6 row 1). Within this alternative ordering, we find that a net tax shock has a negative

Table 6: Robustness Analysis

		Finland (Peg)			Sweden (Float)		
	Quarter	0	4	8	0	4	8
Alt. Ordering	Net Taxes	-2.45**	-0.47	-0.66	0.14*	0.92*	0.85*
		(0.26)	(1.17)	(1.34)	(0.02)	(0.49)	(0.46)
	Gov. Exp.	6.19**	2.81	2.83	-0.86**	-6.56**	-6.49*
		(0.01)	(3.15)	(3.77)	(0.03)	(3.19)	(3.29)
Alt. Tax Def.	Taxes	-6.35**	-5.64*	-6.19*	0.82**	2.69**	1.83*
		(0.10)	(3.48)	(4.44)	(0.00)	(1.31)	(1.25)
	Gov. Exp.	2.15**	2.34*	2.03*	0.45*	0.83*	0.47
		(0.54)	(1.24)	(1.41)	(0.06)	(0.67)	(0.66)
Stochastic Trend	Net Taxes	-2.38**	0.10	-0.08	-0.06**	0.16	0.01
		(0.23)	(0.27)	(0.10)	(0.01)	(0.21)	(0.04)
	Gov. Exp.	0.02**	0.39*	-0.08	0.21**	-0.07	-0.01
		(0.00)	(0.39)	(0.12)	(0.02)	(0.17)	(0.03)
High Elasticity	Net Taxes	-2.57**	-2.24*	-1.71*	-0.20*	0.88	1.45
		(0.28)	(1.33)	(1.51)	(0.01)	(1.19)	(1.05)
	Gov. Exp.	0.06*	1.59*	1.17*	0.10**	-0.13	-0.58
		(0.01)	(1.24)	(1.03)	(0.01)	(0.98)	(0.79)
Low Elasticity	Net Taxes	-2.12**	-0.93	-1.36	0.13**	1.17*	1.56*
		(0.22)	(1.71)	(1.98)	(0.04)	(1.14)	(1.04)
	Gov. Exp.	0.14**	1.68*	1.21*	0.19*	-0.06	-0.55
		(0.01)	(1.25)	(1.03)	(0.02)	(0.99)	(0.80)

Note: Impulse responses to 1% of GDP fiscal policy shocks in alternative specifications. Bootstrapped standard errors in parentheses, ** significant at two standard deviation confidence level, * significant at one standard deviation confidence level.

effect on output and net taxes under a fixed exchange rate, but a small positive effect under flexible exchange rate. For a government expenditure expansion, output moves to opposite directions.

Second, we test whether our results are sensitive to how we define fiscal variables. By definition, we have included transfers to households in net taxes. This means that net taxes decrease when transfers to households increase. Although tax reductions related income taxation might increase labour supply and hence economic activity, one could argue that increases in transfers to households (such as unemployment benefits and other social benefits) might decrease labour supply and have a negative effect on economic activity. If such a dampening effect is significant, the disincentive effects may be one potential explanation for the non-Keynesian effect of net tax policy measures in Sweden. This possibility might not turn out trivial considering the Nordic welfare state and the extent of public safety net in Sweden. To study whether incentive effects drive our results, we re-define the fiscal variables so that transfers to households are included in government expenditure rather than in net taxes and estimate the impulse responses (see Table 6 row 2).[14] However, we find no clear evidence that disincentive effects would drive our results. For Sweden, we find that a government expenditure shock is now more expansionary than in

[14]Analogously, we have re-estimated the output elasticity of government expenditure and taxes.

the baseline, but a tax shock is still expansionary – even when it does not include transfers to households. In Finland, on the other hand, the impulse responses are similar to those in the baseline, except that the effects on output tend to be higher.

Third, we test whether our results are sensitive to alternative assumptions regarding net tax and government expenditure elasticities. Recall that our estimation methodology relies on reduced-form estimates of tax base to output elasticities and on OECD estimates of tax revenue to tax base elasticities, which are based on tax codes prevailing at a specific point in time. However, Mertens and Ravn (2013) estimate implicit output elasticities from a structural model that incorporates narrative accounts of fiscal measures and find evidence that the OECD tax elasticities underestimate the true underlying tax elasticities in the United States. Although our framework cannot incorporate uncertainty related to model parameters, we may test whether our results are sensitive to alternative elasticity assumptions. In particular, we decide to estimate the impulse responses assuming that net tax elasticities are either 20 percent higher or 20 percent lower. This setting also accounts for the possibility that, despite our estimates, the tax elasticities were actually of equal size in Finland and Sweden. However, we find that these alternative elasticity assumptions have little qualitative significance on estimated effects of fiscal policy (see Table 6 row 3). Similar to Mertens and Ravn (2013) our results suggest that if indeed we underestimated the true elasticities, then our measures of fiscal policy effectiveness would be somewhat underestimated as well. However, this effect is very small. In the case of Finland and Sweden, we argue that it is more likely that tax elasticities have varied across time due to large cyclical fluctuations, rather than that they would be, on average, under or overestimated. This is, in part, due to the fact that we have a relatively short time period, whereas for example Blanchard and Perotti (2002) assume that tax codes are largely unchanged during several decades. On the other hand, we account for time variation due to *composition* of tax revenues in estimating the structural model parameters.[15]

Fourth, we study the possibility that our results are sensitive to assumptions regarding the de-trending method. In particular, we assume that net taxes, government expenditure and GDP follow a stochastic trend and re-estimate the impulse responses (see Table 6 row 4). The impulse responses now have a different outlook, which is due to the fact that we estimate the effect of growth of fiscal policy shock on growth in output and hence lose long-run information from the time series. We find that the impulse responses tend to vary from positive to negative according to the quarter. However, once we estimate cumulative effects of these measures and the fiscal multiplier effect, we find that the results are again consistent with the baseline results. Thus, our results are in accordance with the previous literature, which finds that a linear time trend is sufficient to account for the underlying trend in fiscal time series and that alternative de-trending methods do not affect the results to any significant degree. In addition, one could argue that we should estimate an error correction version of the model and account for the possible co-integration of fiscal variables. While this might affect the estimated dynamic

[15] Addressing for time varying parameters in the VAR model is beyond the scope of our present study.

Table 7: Alternative Sample Periods for Finland

	1975-2011			1975-1992		
	0	4	8	0	4	8
Net Taxes	-1.97**	1.35	1.34	-1.62**	-1.93*	-3.10*
	(0.13)	(1.85)	(2.26)	(0.10)	(1.55)	(1.57)
Gov. Exp.	-0.09**	0.91	1.10	1.16**	4.61*	5.24**
	(0.01)	(1.09)	(1.22)	(0.11)	(2.36)	(2.45)

Note: Impulse responses to 1% of GDP fiscal policy shocks. Bootstrapped standard errors in parentheses, ** significant at two standard deviation confidence level, * significant at one standard deviation confidence level.

effects, it does not affect the identification of fiscal policy shocks, and it is not likely to affect inference as we are primarily interested in the short-run dynamics. Further, we could not find any obvious candidate for a co-integration relationship as, for example, the difference between net taxes and government expenditure is not stationary during our baseline sample period. Moreover, previous literature finds no significant qualitative difference due to inclusion of a co-integration relationship (see Blanchard and Perotti 2002, Perotti 2005).

Finally, we test whether the effects of fiscal policy shocks depend on to chosen sample period.[16] Notably, we have argued that our empirical strategy is particularly appealing as we may control for the underlying economic environment, which is likely to vary over time and over countries. To study whether the economic environment affects the effects of fiscal policy, we estimate the impulse responses for Finland in two alternative specifications: first, based on the full 1975-2011 sample period; and second, based on the period prior to adopting a floating currency 1975-1992. Table 7 shows our estimation results. First, we find that during the full 1975-2011 sample a government expenditure shock increases output on average by about one percentage point. This is in accordance with the range of average estimates of fiscal policy shocks based on the historical data for the United States. However, except for the period following the initial shock, zero now falls between the confidence intervals. Also, the effect of net tax shocks tends to become positive over longer horizons. Second, we find that fiscal policy shocks induced a much higher and statistically significant responses in output before the adoption of a floating currency in 1975-1992. The peak effect is as high as 5.3 p.p. in the seventh quarter for government expenditure and the response is significant at the two standard deviation level. The government expenditure shock tends to be also more persistent than in previous specifications. The response of output to net taxes is somewhat smaller (peaking at 3.3 p.p.), but it is nevertheless statistically significant at various horizons. Further, and in contrast to government expenditure shocks, net tax shocks do not show significant persistency and are not different from zero after five quarters (not shown).

Our results based on these alternative sample periods suggest that the effectiveness of fiscal

[16]The results are also robust for single outliers in fiscal policy shocks. One particularly visible change in fiscal policy is the drop in public expenditure in Sweden in the first quarter of 1997 which coincides with the Government's adoption of a surplus target in budget balance (see, f.e. Flodén 2013 and OECD 1996). However, controlling for this has negligible effects on the estimated impulse responses and are hence not reported.

Table 8: Government Expenditure Multiplier in Alternative Specifications

	Finland (Peg)			Sweden (Float)		
	0	4	8	0	4	8
Alt. Ordering	6.2	3.7	3.6	-0.9	-3.5	-4.8
Alt. Tax Definition	2.5	5.1	7.9	0.5	1.0	1.2
Stochastic Trend	0.0	1.8	1.7	0.2	0.3	0.2
High Elasticity	0.1	2.3	4.0	0.1	0.1	-0.3
Low Elasticity	0.1	2.5	4.2	0.2	0.2	-0.2
1975-2011	-0.1	0.6	0.8			
1975-1992	1.2	2.4	3.1			

Note: Fiscal multipliers are defined as $M_p^g = \frac{\sum_0^p y_p}{\sum_0^p g_p}$, where y_p, g_p and are the impulse responses for GDP, government expenditure corresponding to quarter p.

policy measures has varied over time, likely due to differences in monetary policy regime. First, during the pre-1993 period, the exchange rate in Finland was subject to successive devaluations (see, f.e., Kuusterä and Tarkka 2012). The devaluations were frequent and improved the Finnish price competitiveness in world markets. This suggest that the fiscal policy shocks that we identify tend to be associated with higher than normal output effects, as we do not explicitly control for expansionary monetary policy shocks. Thus, the impulse responses for the 1975-1992 period are likely to represent the upper bounds for the effects of fiscal policy, or, the output effects under a particularly accommodating (or even expansionary) monetary policy regime. Second, the finding that the output effects are smaller and associated with relatively wide confidence intervals within the full sample period is likely to reflect the fact that monetary policy regime and the economic environment have changed considerably over time. For example, Honkapohja and Koskela (1999) show that it was monetary policy that mostly contributed to stabilization policies during the float period. Overall, our estimates based on alternative sample periods are consistent with our assumption that the effects of fiscal policy shocks should be interpreted as conditional on the economic environment. The results are also consistent with the finding that the effects of fiscal policy shocks are larger within a fixed exchange rate regime than under a float.

To conclude our robustness analysis, Table 8 shows government expenditure multiplier effects based on the alternative specifications. In particular, consistent with the baseline specification, it shows that the conditional fiscal multiplier effect has been higher in Finland under a fixed exchange rate regime than in Sweden under a floating exchange rate regime. This suggests that there exists a qualitative difference in the effects of fiscal policy shock that is dependent on the monetary policy regime and that the effect is robust to a variety of sensitivity tests.

6 Fiscal Foresight

Our baseline SVAR model assumes that economic agents only respond to realized fiscal policy shocks. In practice, however, fiscal policy is subject to potential and significant anticipation effects as fiscal policy measures are discussed, debated and announced in public well before their final implementation and, as a consequence, forward-looking economic agents may adjust their inter-temporal behavior in advance. In this section, we first briefly review the implications and evidence of fiscal foresight on fiscal policy analysis. Second, we suggest to augment our baseline model specification with real-time information to account for potential fiscal foresight. Third, we study the effects of unanticipated and anticipated fiscal policy shocks within our empirical model.

6.1 Fiscal Foresight

Fiscal foresight refers to the possibility that economic agents respond to anticipated fiscal policy shocks, f.e., by responding to fiscal policy shocks prior to their implementation. It poses challenges for both theoretical and empirical fiscal policy analysis. In particular, if fiscal foresight is significant, we should expect two kinds of shocks to occur in response to each fiscal policy measure. First, we should expect economic agents to respond to announcements of new fiscal policy measures. This is when forward looking economic agents optimize their inter-temporal behavior as they react to the effects that the announced fiscal measures have on their net wealth. For example, an anticipated tax liability reduction next year should induce an increase in economic agent's consumption today if she assumes this to increase her personal net wealth. Second, we should expect economic agents to respond to implemented fiscal policy measures. This effect may emerge if taxes are distortionary or if there are households that do not respond to permanent income shocks.[17] For example, liquidity constraints or financial frictions may prevent economic agents from responding to shocks on their net wealth by smoothing their consumption over time.

Notwithstanding that the presence of fiscal foresight is widely acknowledged and economic agents are assumed forward looking in standard DSGE models, most theoretical and empirical fiscal policy analysis implicitly assumes that implemented fiscal policy measures are, in effect, unanticipated by economic agents. Recent theoretical literature suggest that the implications of fiscal foresight for fiscal policy might not turn out trivial and that the potential effects may depend on the fiscal instrument employed. For example, Yang (2005) shows within a neoclassical DSGE model that anticipated labour and capital tax shocks move private economic activity to opposite directions on the period when the measures are announced, but to the same direction once they are implemented. Leeper, Richter and Walker (2012) study the effects of fiscal foresight in an estimated DSGE model and show that fiscal foresight substantially affects

[17] As Perotti (2012) suggests, if taxes were lump-sum non-distortionary taxes, tax regime changes would, ceteris paribus, only change the present value of household tax liabilities and households would react only at the time of the announcement.

the effects of government expenditure. They find evidence that taking anticipation effects into account yields higher estimated fiscal multipliers, but that the aggregate effects depend on the degree of foresight. In addition, Leeper et al. (2010) show that anticipated government investment shocks, as opposed to unexpected shocks, may have only small or even negative effects on economic activity in the short run. In particular, private investment may fall and economic agents lower their labour supply due to the positive wealth effect associated with the government investment stimulus. However, theoretical studies that estimate the effects of fiscal foresight need to make specific assumptions regarding the correct anticipation horizon as well as economic agents' responsiveness to anticipated shocks.

Fiscal foresight also carries adverse implications for empirical fiscal policy studies that base their inference on time-series analysis. Fiscal foresight could imply that fiscal policy shocks identified in conventional SVAR-models do not represent true structural fiscal policy shocks observed by economic agents. This is driven by the differences in econometrician's and economic agent's available information set. While economic agents may respond to both realized and anticipated future shocks, the econometrician bases her inference on realized shocks only. More specifically, the economic agents' inter-temporal optimization behavior creates an equilibrium time-series with a non-fundamental moving average representation (see Hansen and Sargent 1980, 1991 and Leeper et al. 2013). This hampers the use of conventional (causal) fiscal policy VAR models that do not explicitly incorporate information on anticipated fiscal policy measures. For example, Yang (2005) shows that only a single quarter of fiscal foresight is sufficient to substantially distort the estimated effects of fiscal policy shocks.

Nonetheless, there is still little literature that assesses empirically the quantitative significance of fiscal foresight. For example, Perotti (2005) argues that anticipation effects are not likely to affect the results from Blanchard-Perotti type identification strategy as the identified SVAR fiscal policy shocks are not predicted by OECD semi-annual government expenditure forecasts. In contrast to our baseline model, however, Perotti's (2005) SVAR model incorporates "forward looking" variables (i.e., long-term interest rates and inflation), which may mitigate problems related to fiscal foresight through the effects that anticipated fiscal policy shocks have on long-term interest rates and hence on economic activity. Also, a large body of microeconomic evidence suggests that private consumption is not responsive to anticipated reimbursements while implemented reimbursements have a positive effect on private consumption (see Poterba 1988, Souleles 1999, Parker 1999 and Parker et al. 2013). Yet, more recently, Ramey (2011) studies federal fiscal forecasts and real-time newspaper resources and shows that fiscal shocks identified from these sources predict the shocks identified in a typical fiscal SVAR model. She suggests that the SVAR studies miss the timing of the fiscal policy shocks and, as a result, the empirical estimates based on the SVAR-literature are distorted. In addition, for example, Hsieh (2003) shows that consumers respond to anticipated reimbursements in case the income changes are sufficiently large and predictable.

There are few empirical studies that incorporate fiscal foresight to VAR analysis and esti-

mate the quantitative or dynamic effects of anticipated and unanticipated fiscal policy shocks on aggregate economic activity. In their original paper, Blanchard and Perotti (2002) estimate the effects of fiscal policy shocks under fiscal foresight, but assuming that economic agents have a *perfect* one-quarter-ahead foresight horizon and, in addition, that governments may not respond to output shocks within two quarters. They argue that allowing for fiscal foresight does not affect their estimates on the effects of fiscal policy shocks significantly. Yet, their the point estimate for government expenditure shock under (one-quarter) fiscal foresight equals to about almost 2 p.p. of GDP while in the baseline it is about 1 p.p. of GDP (they do not provide confidence intervals for the estimates). For tax policy shocks the difference between the baseline specification is smaller. However, a perfect foresight seems unlikely as even observing the real-time government expenditure is subject to significant uncertainty due to data revisions.

Recent studies that allow for a flexible foresight horizon show that fiscal foresight could have empirically significant effects. First, Mertens and Ravn (2012) identify anticipation effects by differentiating between Romer and Romer's (2010) tax policy shocks that i) were pre-announced well in advance and those that ii) were implemented within three months of the announcement. They find that pre-announced tax cuts induce an initial contraction in output, investment and real wages, but that they have a negligent effect on private consumption. Implemented tax measures, whether anticipated or not, increase output, consumption, investment, worked hours and real wages. Leeper et al. (2013), on the other hand, identify unanticipated and anticipated tax policy shocks by taking advantage of the different tax treatment of municipal and treasury bonds in the United States.[18] In particular, they construct an implicit tax rate as a weighted average of discounted future tax liability changes and augment the Blanchard-Perotti SVAR with the implicit tax rate data. They estimate that anticipated tax policy shocks induce a contraction in output, but, in contrast, unanticipated tax policy shocks tend to be more expansionary than in the baseline no-foresight Blanchard-Perotti specification. In addition, recent studies by Auerbach and Gorodnichenko (2012) and Born et al. (2013) incorporate government expenditure forecasts to fiscal policy SVAR models to account for anticipation effects. Auerbach and Gorodnichenko (2002) find evidence that the absolute size of fiscal multiplier tends to increase when the shocks are unanticipated while Born et al. (2013) do not find qualitatively significant effects.

6.2 SVAR with Fiscal Foresight

As emphasized above, our baseline Blanchard-Perotti approach incorporates observed fiscal variables. It neglects from any information that economic agents might have in real time and before fiscal policy measures are implemented. In addition, the model does not contain variables that could possibly mitigate the information problem, such as long-tern interest rates. Therefore, we next aim to study whether the baseline results are sensitive to fiscal foresight.

[18] In particular, municipal bonds are free from federal taxes in the United States.

To account for fiscal foresight, we augment the Blanchard-Perotti model with quarterly fiscal forecasts, i.e. forecasts on future fiscal outcomes. This strategy is similar to the empirical fiscal policy models proposed recently by Leeper et al. (2012, 2013), Auerbach and Gorodnichenko (2012) and Born et al. (2013). It is consistent with the analysis of Leeper et al. (2013) and Giannone and Reichling (2006) who suggest that one should augment VAR models with variables that help to predict (or are Granger causally prior to) future outcomes of the fiscal variables. This is a strategy to augment the econometricians information set, and hence to avoid the underlying "non-fundamentallness" representation problem. In particular, we add information to the model that is observed by economic agents in real time, but unobserved in ex-post data. The advantage of using fiscal forecasts is that we may add real-time information to the model that is not restricted to country specific indicators of fiscal foresight.

Our starting point is a four variable VAR model that includes real GDP y_t, government expenditure g_t, net taxes t_t and government expenditure forecast errors f_t as endogenous variables. We use forecast errors instead of fiscal forecasts to control for the endogeneity to movements in fiscal variables. In order to identify shocks that have a structural interpretation, we set restrictions for the reduced-form VAR errors as

$$u_t^g = \alpha_{gy} u_t^y + \alpha_{gf} u_t^f + \beta_{gt} e_t^t + e_t^t \tag{6.1}$$

$$u_t^t = \alpha_{ty} u_t^y + \alpha_{tf} u_t^f + \beta_{tg} e_t^t + e_t^g \tag{6.2}$$

$$u_t^y = \alpha_{yg} u_t^g + \alpha_{yt} u_t^t + \alpha_{yf} u_t^f + e_t^y \tag{6.3}$$

$$u_t^f = \alpha_{fg} u_t^g + \alpha_{ft} u_t^t + \alpha_{fy} u_t^y + e_t^f \tag{6.4}$$

where the equations (6.1)-(6.3) correspond to equations (3.3)-(3.5) in the benchmark model and are based on the Blanchard-Perotti assumptions. Next, we assume that government expenditure forecast errors do not contemporaneously affect government expenditure, net taxes and output.[19] This is similar to Leeper et al. (2013) who assume that implicit future tax rate shocks do not affect fiscal variables nor output within a quarter. This assumption implies that the coefficients α_{gf}, α_{tf} and α_{yf} above are all equal to zero. The final equation (6.4) states that forecasts errors are a combination of shocks to government expenditure, net taxes, real GDP and structural forecast errors. Thus, our approach accounts for the possibility that forecast errors are correlated with within-a-quarter output shocks as well as within-a-quarter movements in fiscal variables that are unobserved by fiscal forecasters in real-time.[20] The structural forecast errors e_t^f now reflect real-time anticipation effects that are unobserved in net tax and government expenditure ex-post quarterly data, but observed by the economic agents. Under these assumptions, we may estimate the effects of both anticipated and unanticipated fiscal

[19] Forecast error as defined as the difference between period t projection for $t+p$ and the ex-post figure for $t+p$.

[20] Fiscal forecasters only observe preliminary estimates of quarterly output and fiscal variables which are subject to significant revisions over time. In effect, data revisions explain a part of observed forecast errors.

shocks on economic activity.

6.3 Measuring Fiscal Foresight

We rely on quarterly fiscal forecasts made by national economic research institutes in order to measure fiscal foresight. We rely on a single source for each country to guarantee mutual consistency in all forecast exercises over time. In particular, we obtain quarterly forecasts for general government consumption and investment for Finland from the Research Institute of the Finnish Economy (ETLA) and for Sweden from the National Institute for Economic Research (NIER). The data is available in quarterly frequency for the period 1987:1-2005:4 and 2000:1-2011:4 for Finland and Sweden respectively.

We construct quarterly forecast errors in a two step procedure. First, we combine consumption and investment forecasts to a single government expenditure forecast by calculating their weighted average g_i. Now, each forecast exercise issued in quarter t includes forecasts for the present (g_i) and for the following year (g_{i+1}). This implies that the information content of these forecasts depends on the quarter in which they are published. Thus, in the second step, we follow Gerlach (2007) and extract the quarterly information in these consecutive forecasts as

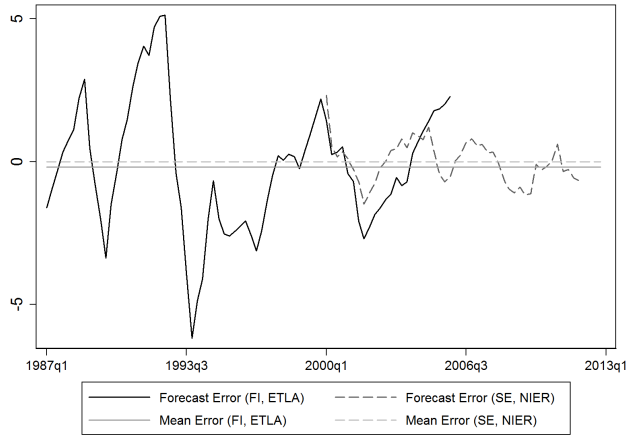
$$f_t = E_t[\gamma_q g_i + (1-\gamma_q) g_{i+1}] - [\gamma_q g_i + (1-\gamma_q) g_{i+1}], \tag{6.5}$$

where f_t is a weighted forecast error of the present and the following year forecast made in quarter t and γ_q is a quarterly weighting factor that takes a value $\gamma_q \in \{\frac{3}{4}, \frac{2}{4}, \frac{1}{4}, 0\}$ corresponding to quarter $q \in \{1, 2, 3, 4\}$ respectively. The weighting factor thus broadly controls for the time-varying forecast horizon. Overall, this yields a measure of anticipated government expenditure shocks that are about to take place during the present and the following year. Although it does not contain information on the specific quarter in which fiscal measures are about to be implemented, it reflects the quarterly information flow that economic agents may observe.

Figure 6.1 plots the government expenditure forecast errors. Positive values of forecast errors are associated with lower-than-expected growth of government expenditure and negative values are associated with higher-than-expected government expenditures. First, one can see that the (two-year-horizon) forecast errors are somewhat large and seem to be correlated with large cyclical turning points, which is consistent with the observation that cyclical turning points are unpredictable in two-year horizon. In addition, while only a small part of the sample overlaps, we may observe that the forecast errors are correlated in the two countries. This suggests that there is significant noise in the forecast error that is likely to be explained by common cyclical conditions. Yet, the expected value of forecast errors is close to zero and statistically insignificant for both countries which suggest that the forecasts have not been systematically biased.

Our strategy to control for fiscal foresight comes with some appealing features compared to the previous literature. First, our measure assumes no specific predetermined foresight

Figure 6.1: Fiscal Forecast Errors



Source: The Research Institute of the Finnish Economy (ETLA), National Institute for Economic Research (NIER) and author's own calculations.

Table 9: Predictability of SVAR Shocks

	Finland	Sweden
Null Hypothesis	p-value	
Fiscal forecasts do not predict Gov. exp. shocks	(0.033)	(0.001)
Gov. exp does not predict fiscal forecast errors	(0.892)	(0.542)

Note: Grager causality test based on the underlying four variable VAR model.

horizon, but it allows economic agents to anticipate fiscal policy measures a maximum of two years ahead. This is in accordance with the observation that the foresight horizon could vary considerably over time (Leeper, Richter and Walker 2012 and Mertens and Ravn 2012). In contrast, most studies that consider fiscal foresight allow for a foresight horizon of exactly one quarter (Blanchard and Perotti 2002 and Auerbach and Gorodnichenko 2012) or a single predetermined multiple-quarter horizon at a time (Perotti 2005, Ramey 2011a and Born et al. 2013). Second, our strategy allows anticipated fiscal policy shocks to be discounted by the private sector according to when the measure are about to be implemented. This accounts for the possibility that news about fiscal measures that are planned for the more distant future include more uncertainty than measures that are about to take effect in the near future. In addition, our measure takes into account anticipated, but not realized fiscal measures. For example, most studies considering fiscal foresight restrict their analysis only to anticipated measures that were also eventually implemented. This neglects from the fact that many fiscal measures that are discussed in public may be postponed, reversed or even dismissed.

We have argued that fiscal forecasts increase the information content in the underlying VAR model. Of course, this has little justification if they do not, in effect, include information on future fiscal policy measures. Next, we aim to assess whether this is indeed the case. If

fiscal forecasts include information on future fiscal policy actions that are unobserved in ex-post fiscal data, we would expect fiscal forecasts to predict the baseline fiscal VAR variables and the identified fiscal policy shocks. We study this by testing whether fiscal forecast errors Granger cause the identified SVAR fiscal policy shocks as suggested by Perotti (2005) and Ramey (2011).[21] Given the assumption that government expenditure is not contemporaneously affected by GDP, net taxes or forecast errors, we can perform a Granger causality test based on the underlying VAR-model. Table 9 shows our results. We find that the fiscal forecast errors are Granger causally prior to government expenditure shocks. We reject the hypothesis that forecast errors do not predict SVAR shocks at the 5% significance level.[22] Further, we find no evidence that government expenditure shocks would predict fiscal forecast errors. This result is consistent with Ramey's (2011) finding that forecast errors based on Survey of Professional Forecasters data predict future government expenditure shocks. On the whole, our results suggests that fiscal forecasts include information on future fiscal measures that could be used to address the lack of information in observed ex-post fiscal data.

Now, before we continue to the analysis of dynamic effects, we want to underline some limitations to our strategy to incorporate fiscal foresight. First, due to data availability, we are forced to study a different sample period than in the baseline specification we presented in Sections 3 and 5. This different sample period implies that the economic environment with regards to the business cycle and the exchange rate regime are different than in the baseline. Second, our measure of fiscal foresight is restricted to government expenditure only and we cannot address anticipated tax policy changes.[23] In theory, this implies that government expenditure shocks may be anticipated, but that tax policy shocks are not anticipated. In the following, we thus restrict our analysis primarily to government expenditure shocks. This could also imply that the underlying VAR model is still subject to the non-fundamentalness problem described above. However, to the extent that our measure adds to the information set of the econometrician, it should not be inferior to the baseline specification.

6.4 Dynamic Effects of Anticipated and Unanticipated Shocks

This section considers to what extent the baseline model might misinterpret the dynamic effects of fiscal policy shocks due to fiscal foresight. In particular, we estimate both the baseline model and the fiscal foresight augmented model for Finland (1987q1-2005q4) and for Sweden

[21] More specifically, we test whether the coefficients a_i in the equation

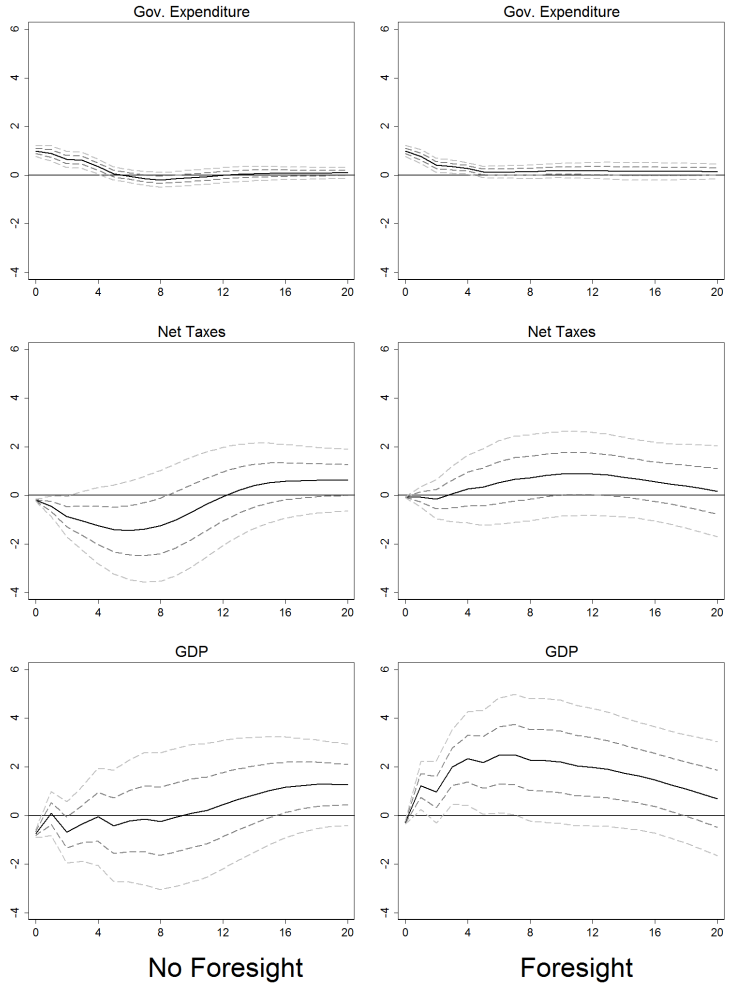
$$e_t^g = \sum_{i=1}^{p} a_i f_{t-i} \tag{6.6}$$

are all equal to zero.

[22] This is independent of VAR lag order selection and ordering of VAR variables.

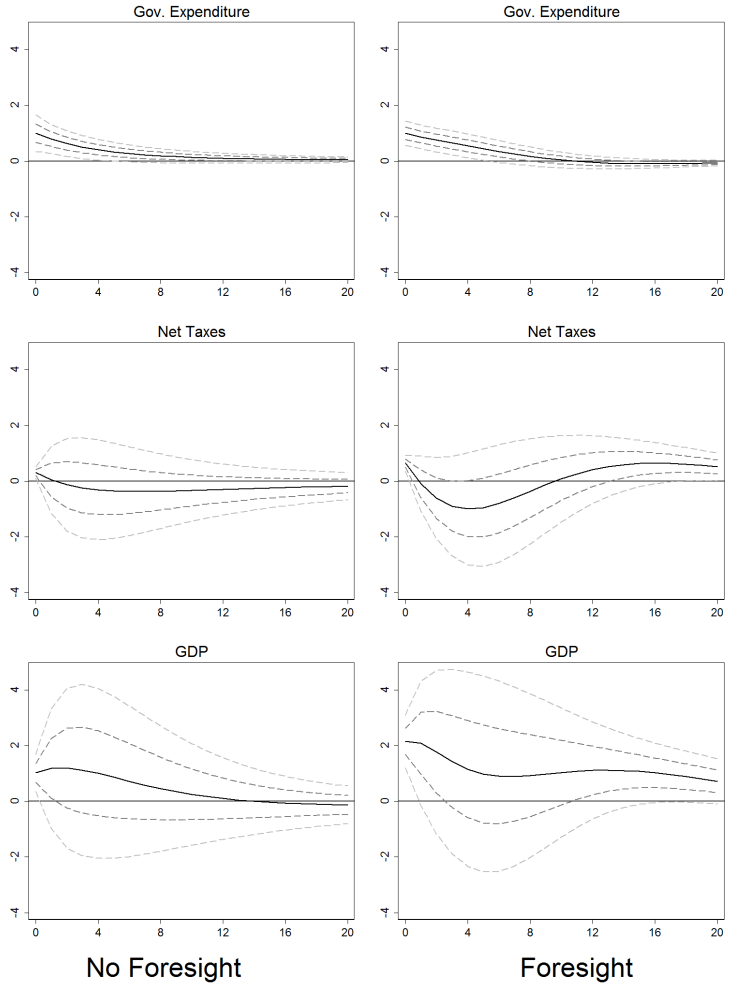
[23] The forecast exercises do not include the necessary estimates for government revenue categories. In addition, we have not come across alternative strategies to control simultaneously for anticipated government expenditure and tax measures in an SVAR model framework.

Figure 6.2: Impulse Responses to Government Expenditure Shock in Finland (1987-2005)



Note: Impulse responses to a 1% of GDP government expenditure shock. Finland: impulse responses are based on (1987:1-2005:4) sample and underlying VAR(4) model in log levels with a deterministic time trend. Sweden: impulse responses are based on (2000:1-2011:4) sample and underlying VAR(1) model in log levels with a deterministic time trend. Dashed lines represent bootstrapped one and two standard deviation confidence intervals respectively.

Figure 6.3: Impulse Responses to Government Expenditure Shock in Sweden (2000-2011)



Note: Impulse responses to a 1% of GDP government expenditure shock. Finland: impulse responses are based on (1987:1-2005:4) sample and underlying VAR(4) model in log levels with a deterministic time trend. Sweden: impulse responses are based on (2000:1-2011:4) sample and underlying VAR(1) model in log levels with a deterministic time trend. Dashed lines represent bootstrapped one and two standard deviation confidence intervals respectively.

Table 10: Output Responses to Fiscal Policy Shocks

		No Foresight			Foresight		
Country	Shock	0	4	8	0	4	8
Finland	Gov. Exp.	-0.75**	-0.05	-0.24	-0.29**	2.34**	2.28*
		(0.08)	(1.00)	(1.41)	(0.03)	(0.97)	(1.28)
	Ant. Gov. Exp.				0.00	-0.08	0.89*
					(0.00)	(0.42)	(0.59)
	Net Taxes	-1.43**	-0.95	-2.36*	-0.14**	-2.70*	-3.46*
		(0.06)	(1.28)	(1.74)	(0.06)	(1.42)	(2.02)
Sweden	Gov. Exp.	1.02**	1.00	0.46	2.15**	1.15	0.92
		(0.33)	(1.53)	(1.13)	(0.46)	(1.73)	(1.46)
	Ant. Gov. Exp.				0.00	2.10**	1.65**
					(0.00)	(0.85)	(0.75)
	Net Taxes	-0.08	0.14	0.44	-1.67**	-0.88	-1.03
		(0.15)	(1.20)	(0.93)	(0.23)	(2.02)	(1.68)

Note: Impulse responses to a 1% of GDP fiscal shocks. Finland: impulse responses are based on (1987:1-2005:4) sample and underlying VAR(4) model in log levels with a deterministic time trend. Sweden: impulse responses are based on (2000:1-2011:4) sample and underlying VAR(1) model in log levels with a deterministic time trend. Bootstrapped standard errors in parentheses, ** significant at two standard deviation confidence level, * significant at one standard deviation confidence level.

(2000q1-2011q4) using identical sample size and the same underlying lag structure.[24] In order to control for the monetary policy regime change in Finland, we introduce a shift for the float period (1992q4-1996q3). This is arguably a relatively rough approach to control for the monetary policy regime change. A more sensitive approach would be to introduce monetary policy variables to the model, but this strategy would require a larger underlying VAR model and additional assumptions for identification. However, our qualitative results regarding fiscal foresight are not dependent on the inclusion of the shift.

We first study the dynamic effects of implemented discretionary fiscal policy shocks under the two different model specifications. Figures 6.2 and 6.3 and Table 10 show our results. Note that the results differ from the baseline results due to the different sample period and differences in the economic environment. The left panel shows the results for the baseline model, in which we assume that economic agents have no fiscal foresight (henceforth "no foresight"). Thus, these impulse responses are based on a combination of possibly anticipated and unanticipated fiscal policy shocks. The right panel shows the results for the augmented model, in which we take the possibility of fiscal foresight into account (henceforth "foresight"). These impulse responses are based only on unanticipated fiscal policy shocks.

Under the assumption of no fiscal foresight, a government expenditure shock has moderate or even negative effect on output in both countries. In Finland, the output response is 0.08p.p.

[24] We estimate the impulse responses based on VAR(4) model for Finland and VAR(1) model for Sweden in both specifications. The latter is due to very limited sample size, but the qualitative results are unchanged if we estimate the impulse responses assuming an underlying VAR(2)-model.

after a quarter, but it turns negative in the following quarters. In Sweden, the output response peaks at 1.2 p.p. after two quarters. Yet, the output response is insignificant in both countries already after two quarters. In addition, the output responses to net taxes are broadly in line with the previous evidence from the baseline specification, namely that the output response is insignificant but moderately positive in Sweden but negative in Finland.

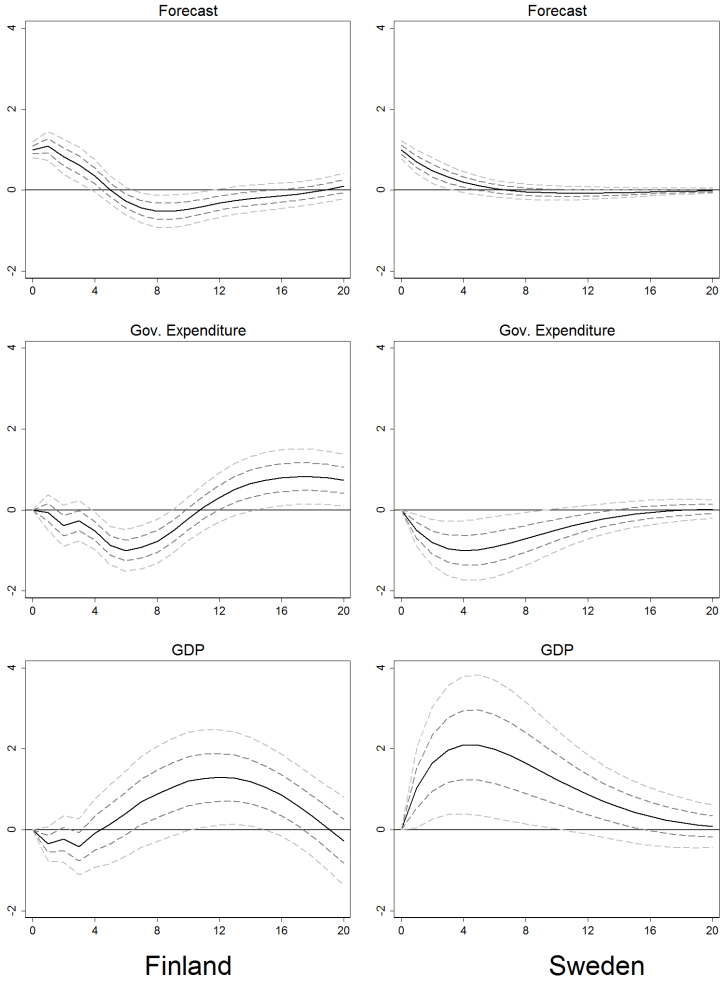
Once we take fiscal foresight into account, the response of output to a government expenditure shock is positive and higher than in the baseline specification in both countries. Strikingly, in response to a government expenditure shock that is unanticipated by economic agents, output peaks at +2.5 p.p. and +2.15 p.p. in Finland and Sweden respectively. In particular, the output effect in Finland is much more persistent than in the baseline and significant at two standard deviation level. Also, the standard errors associated with the impulse responses are smaller despite a larger underlying model VAR model. The output effect in Sweden, on the other hand, is not significantly different from the baseline, except for the first two quarters. Interestingly, output responses to net tax measures now also have higher point estimates (in absolute value), and the initial response is significant in both countries.

Our results thus suggest that, conditional on the economic environment, unanticipated fiscal policy shocks yield a more expansionary effect on economic activity than fiscal policy shocks identified in the baseline SVAR-model. In particular, including real-time information to the SVAR model brings about differences in the impulse responses to government expenditure shocks. This finding is in accordance with recent literature that studies the effects of unanticipated fiscal policy shocks (Mertens and Ravn 2012, Leeper et al. 2012, 2013 and Auerbach and Gorodnichenko 2012). In particular, Leeper et al. (2012, 2013), who apply a similar identification strategy to control for anticipation effects, show that the estimated fiscal multiplier associated with unanticipated fiscal policy shocks is higher than in the baseline Blanchard-Perotti model. Thus, our results suggest that not incorporating real-time information to the model may distort the estimates fiscal policy effectiveness.

Having said that, we find no evidence that the presence of fiscal foresight would contradict the conclusion made in the previous chapter. Now, while the estimated impulse responses are based on a different sample than those presented in Section 5, the results support the conclusion that fiscal policy is more effective in stimulating economic activity under fixed exchange rate regime. First, the output response to unanticipated government expenditure shock is statistically significant and more persistent in Finland, which had a fixed exchange rate regime during most of the sample period. Also, the point estimate is considerably higher for Finland than for Sweden. This holds for both government expenditure as well as for net tax measures. In addition, unanticipated government expenditure shocks have a small positive effect on net taxes in Finland, but a negative effect on net taxes in Sweden, which suggests that government expansion crowds out private activity in Sweden. In particular, if more output is produced by the public sector directly, it is likely to generate less tax revenues than private activity it potentially crowds out. Thus, we argue that the results based on the augmented model are not

in conflict with the baseline results when it comes to exchange rate regime and the effectiveness of fiscal policy.

Figure 6.4: Impulse Responses to Anticipated Government Expenditure Shock



Note: Impulse responses to an anticipated 1%-of-GDP government expenditure shock. Finland: impulse responses are based on (1987:1-2005:4) sample and underlying VAR(4) model in log levels with a deterministic time trend. Sweden: impulse responses are based on (2000:1-2011:4) sample and underlying VAR(1) model in log levels with a deterministic time trend.

Next, we may study the response of output to anticipated government expenditure shocks. Figure 6.4 and Table 10 show the impulse responses to an anticipated government expenditure shock of one percentage point of GDP. Now, as the government expenditure forecasts are available only in first differences, but other variables are expressed in levels, there is no direct quantitative interpretation to anticipation shocks. In line with Leeper et al. (2012, 2013), we scale the impulse responses by the peak response of government expenditure to an anticipated government expenditure shock. The left panel shows the results for Finland and the right panel

shows the results for Sweden. We find that anticipated shocks have different effects on output in the two cases. In Finland, output declines during the first four quarters – the peak negative response comes after three quarters. The effect becomes positive after five quarters, which coincides (on average) with the implementation of the fiscal measure. The output response then peaks twelve quarters after the shock. In Sweden, on the other hand, output increases in response to an anticipated government expenditure shock. The effect is both significant and persistent up until 12th quarter following the shock.

On the one hand, the output response in Finland suggests that there might be inter-temporal substitution effects that are driving the result on economic activity. In particular, the finding that output responses positively to implemented measures suggests that permanent income channel is outweighed by other effects. This interpretation is consistent with Leeper et al. (2013), Mertens and Ravn (2012) and Romer and Romer (2010) who find that an anticipated tax decrease has a negative initial effect on real output. For example, Mertens and Ravn (2011, 2012) argue that the contraction in output is driven by supply side effects. They show that an announced tax decrease induces a contraction in investment and hours worked while private consumption does not respond significantly. Yet, also anticipated measures have a positive effect on output once the measures are implemented. They argue that this is evidence of inter-temporal substitution in which economic agents substitute today's work effort with the work effort in the future. In our setting, this would suggest that economic agents are willing to postpone their investments or consumption today in case they anticipate subsidies for investment and consumption in the future.

On the other hand, the positive response of output to anticipated fiscal policy shock in Sweden is not easily compatible with a similar interpretation for supply side effects, at least if we assume that the anticipated government expenditure measures were, on average, similar in both countries. Yet, the positive response is consistent with the empirical findings of Mountford and Uhlig (2009) and Barro and Redlick (2011) who conclude that an anticipated government expenditure shock induces an increase in aggregate economic activity. The positive output response suggests that economic agents either smooth their consumption over time (signal of a positive net wealth effect) or increase their labour supply (signal of a negative net wealth effect).

Differences in economic environment could also account for the differences in output responses to anticipated government expenditure shocks. This would be consistent with the considerations that the fiscal policy environment matters. This interpretation is also supported by the observation that liquidity constraints and sustainability concerns were likely to be more significant in Finland during the period 1987-2005, when it experienced a deep financial crisis, than they were in Sweden during 2000-2011. For example, according to AMECO database the average unemployment rate was 9.7% in Finland and 6.9% in Sweden during the respective time periods.[25] If a significant proportion of economic agents are liquidity constrained and may

[25]http://ec.europa.eu/economy_finance/db_indicators/ameco/

not increase their present consumption despite the positive effect on future income, the output effect would be positive only once the measures were implemented, as was found for Finland. The output response in Sweden, on the other hand, suggest that liquidity constraints were not as significant and were outweighed by opposing substitution or confidence effects. Further, in case anticipation effects indeed depend on the economic environment, it is also likely account for the larger disparity in impulse responses in Finland under the no-foresight and the foresight specification than was found for Sweden (see Figures 6.2 and 6.3).

However, our empirical framework does not allow to a study the responses of GDP components or hours worked to anticipated government expenditure shocks. Hence, we may not single out the factors underlying the output responses. Also, the small sample of observation for Sweden suggests that the results could be driven by single observations or possible outliers. As a consequence, we leave a more conclusive analysis of the transmission mechanism of anticipation shocks for future research.

There are also possible caveats to our identification of anticipated fiscal policy shocks. In particular, if fiscal forecasts do not represent the full information set of economic agents with regards to government expenditure measures in real-time, our estimates of unanticipated and anticipated shocks might not reflect true structural shocks that economic agents perceive. This could arise, for example, if the foresight horizon is longer than two years, if the anticipation effects depend on the exact quarter in which fiscal measures are implemented or if fiscal forecasts do not contain real information on future fiscal policy shocks. For example, the strategy of Leeper et al. (2013) to incorporate information on anticipated tax policy changes from implicit tax rate indicator capture anticipated tax policy shocks that reach farther into the future than our measure. However, given the absence of a similar instrument for Finland nor for Sweden, this type of strategy is beyond our present analysis.

In addition, Perotti (2011b) suggests that fiscal forecast errors contain information "for the wrong reason". He argues that forecast errors predict future fiscal outcomes only because the actual forecast figures only contain uninformative noise. The difference between noise and ex-post data then has approximately the information content of ex-post data. This is mostly driven by the fact that forecasts contain a significant error due to data revisions that are unobserved in real-time. We did not pursue an exhaustive study of of this claim, but we have allowed the forecast errors to be correlated with realized quarterly fiscal policy shocks that may have been unobserved by the forecasters. Hence, our strategy controls for the forecast errors that are predictable by past data. In addition, we have incorporated national fiscal forecasts that potentially include more detailed information on national fiscal policy making than the fiscal forecasts from international organizations. Finally, as suggested by Perotti (2011b), if fiscal forecasts only contain noise or if anticipation effects do not matter, the foresight and no-foresight specification should, in principle, yield identical impulse responses. However, we find evidence that this is not the case. Overall, it needs to be noted that no single indicator is likely to contain information on all expected fiscal policy measures and empirical analysis is

hence restricted to imperfect indicators of anticipation effects.

Despite the potential caveats to our identification, we find our results supportive of the claim that anticipation effects matter in identifying fiscal policy shocks in SVAR models. In particular, by increasing the information content within the underlying VAR model, we find significantly different effects on output than in the baseline identification. Future fiscal policy research should aim to elaborate the transmission mechanism of anticipation shocks to economic activity and to develop alternative indicators of real-time information.

7 Conclusions

This paper studies the aggregate effects of discretionary fiscal policy in small open economies. First, we suggest that a comparison of two structurally similar Nordic economies (Finland and Sweden), which have opted for different monetary policy regimes, provides a compelling setting to test the effects of fiscal policy shocks under different exchange rate regimes. Second, we propose to study the effects of anticipated and unanticipated fiscal policy shocks by augmenting the baseline model with fiscal forecasts.

On the one hand, our results suggest that the effects of discretionary fiscal measures depend on the exchange rate regime. In particular, we find evidence that fiscal measures tend to be more expansionary under a fixed exchange rate. Our results are consistent with the conventional wisdom inherited from the Mundell-Fleming model, but also in accordance with the recent literature that emphasizes the importance of monetary policy accommodation and the economic environment in the effectiveness of fiscal policy shocks. The quantitative results should be interpreted with caution as the confidence intervals associated with our estimates are, similar to previous studies, rather large and as the quantitative estimates of the effects of government expenditure and net tax measures vary according to the model specification. Nonetheless, we provide evidence that the qualitative result is robust across various specifications.

On the other hand, our analysis suggest that anticipation effects are likely to matter both in terms of quantitative effects of fiscal policy and in terms of identifying structural shocks within SVAR-models. We find evidence that unanticipated fiscal policy shocks are more expansionary than fiscal policy shocks identified in contemporary SVAR-models that do not allow for fiscal foresight. This is consistent with recent empirical literature that has studied anticipation effects in fiscal policy, and it suggests that economic agents tend to respond to anticipated fiscal policy measures. Further, we also find some tentative evidence that anticipation shocks could have different effects on economic activity depending on the economic environment.

We emphasize that our empirical setting could be extended to understand the effects of fiscal shocks also on other macroeconomic variables. Needless to say, our results are not conclusive with respect to the effects of fiscal policy shocks in small open economies. In order to understand the transmission mechanism, one should further study the effects of fiscal policy shocks on GDP components, external balance, net exports and real exchange rate movements, for example.

Recent studies have considered the transmission mechanism (for example, Monacelli and Perotti 2010, Ravn et al. 2012 and Born et al. 2013), yet they have not restricted their analysis on countries as small as Finland and Sweden or aimed to control explicitly for the economic environment. Also, it is likely that the comparison of the two countries constitutes an intriguing empirical setting also in the future when more data becomes available.

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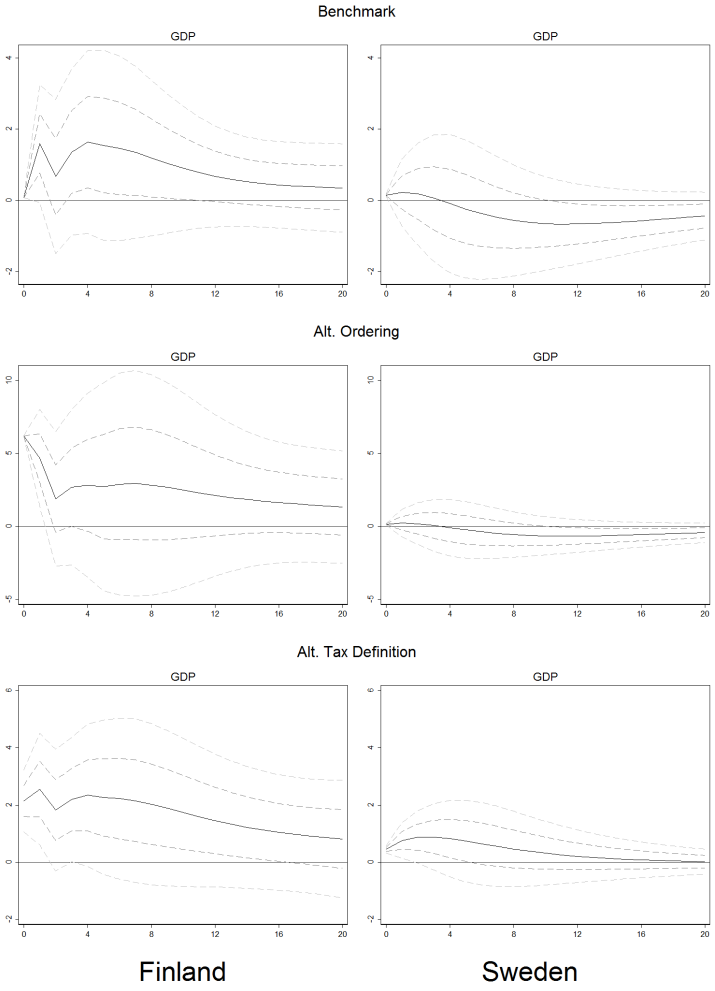
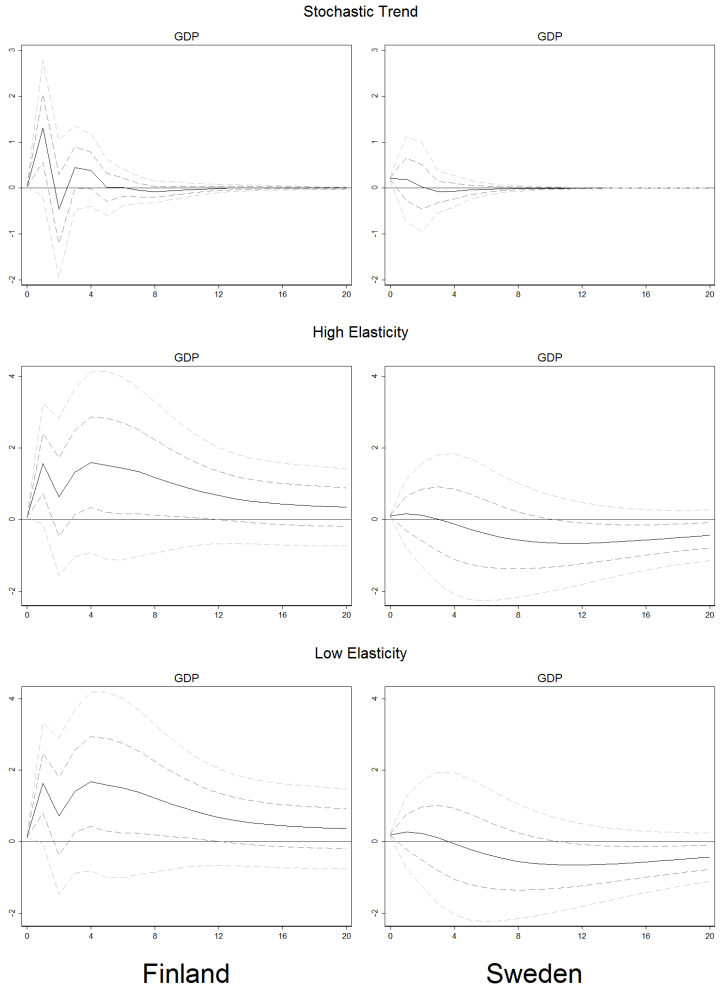
Appendix

Figure 7.1: GDP Responses to a Government Expenditure Shock: Robustness Analysis I



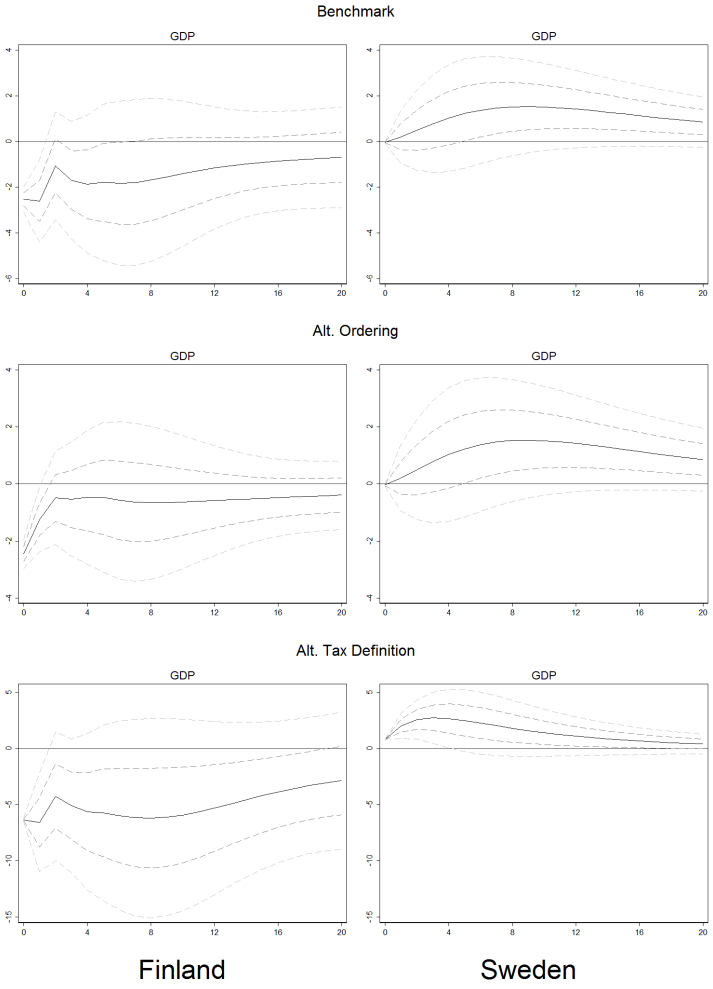
Note: Impulse responses to a 1% of GDP government expenditure shock. Finland: impulse responses are based on (1996:4-2011:4) sample and underlying VAR(3) model. Sweden: impulse responses are based on (1993:1-2011:4) sample and underlying VAR(2) model. Dashed lines represent bootstrapped one and two standard deviation confidence intervals respectively.

Figure 7.2: GDP Responses to a Government Expenditure Shock: Robustness Analysis II



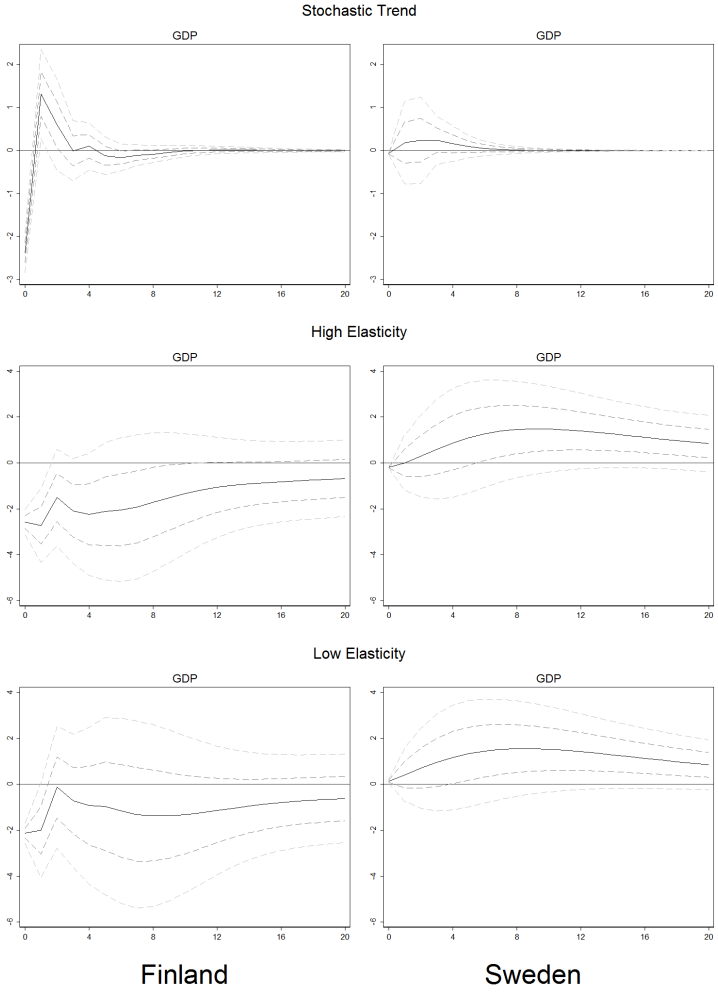
Note: Impulse responses to a 1% of GDP government expenditure shock. Finland: impulse responses are based on (1996:4-2011:4) sample and underlying VAR(3) model. Sweden: impulse responses are based on (1993:1-2011:4) sample and underlying VAR(2) model. Dashed lines represent bootstrapped one and two standard deviation confidence intervals respectively.

Figure 7.3: GDP Responses to a Net Tax Shock: Robustness Analysis I



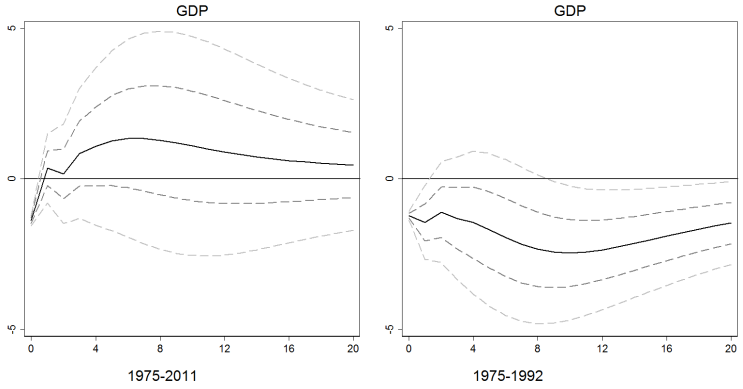
Note: Impulse responses to a 1% of GDP net tax shock. Finland: impulse responses are based on (1996:4-2011:4) sample and underlying VAR(3) model. Sweden: impulse responses are based on (1993:1-2011:4) sample and underlying VAR(2) model. Dashed lines represent bootstrapped one and two standard deviation confidence intervals respectively.

Figure 7.4: GDP Responses to a Net Tax Shock: Robustness Analysis II



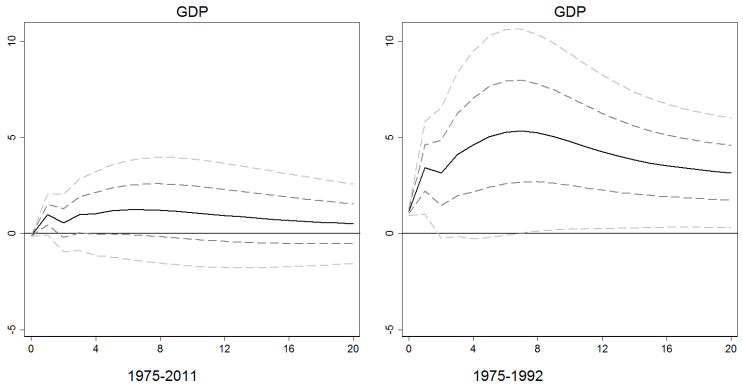
Note: Impulse responses to a 1% of GDP net shock shock. Finland: impulse responses are based on (1996:4-2011:4) sample and underlying VAR(3) model. Sweden: impulse responses are based on (1993:1-2011:4) sample and underlying VAR(2) model. Dashed lines represent bootstrapped one and two standard deviation confidence intervals respectively.

Figure 7.5: GDP Responses to a Net Tax Shock: Alternative Sample Periods for Finland



Note: Impulse responses to a 1% of GDP net tax shock. Impulse responses are based on the underlying VAR(3) model. Dashed lines represent bootstrapped one and two standard deviation confidence intervals respectively.

Figure 7.6: GDP Responses to a Government Expenditure Shock: Alternative Sample Periods for Finland



Note: Impulse responses to a 1% of GDP government expenditure shock. Impulse responses are based on the underlying VAR(3) model. Dashed lines represent bootstrapped one and two standard deviation confidence intervals respectively.

Table 11: Tax Base to GDP Elasticity Estimates

	Sweden				Finland			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
lag	$\hat{\varepsilon}_{profit,y}$	$\hat{\varepsilon}_{wage,employment}$	$\hat{\varepsilon}_{employment,y}$	$\hat{\varepsilon}_{unemployment,y}$	$\hat{\varepsilon}_{profit,y}$	$\hat{\varepsilon}_{wage,employment}$	$\hat{\varepsilon}_{employment,y}$	$\hat{\varepsilon}_{unemployment,y}$
0	2.832***	0.940***	0.0717	-1.242**	2.168***	0.00563	0.141***	-1.365***
	(0.973)	(0.279)	(0.0444)	(0.512)	(0.236)	(0.240)	(0.0357)	(0.512)
1	0.178	-0.210	0.190***	-1.597***	-0.133	0.919***	0.158***	-1.563***
	(1.209)	(0.386)	(0.0400)	(0.400)	(0.223)	(0.254)	(0.0361)	(0.537)
2	0.637	0.0712	0.103**	-1.130***	-0.00487	0.388	0.209***	-1.200**
	(1.153)	(0.309)	(0.0437)	(0.355)	(0.194)	(0.260)	(0.0451)	(0.535)
3	0.477	-0.205	0.0955*	0.0148	-0.388***	-0.157	0.0761	-0.712
	(0.797)	(0.380)	(0.0514)	(0.587)	(0.132)	(0.255)	(0.0471)	(0.451)
4	-0.387	0.332	0.00986	-0.633	-0.344***	0.314	0.101*	-0.552
	(0.894)	(0.447)	(0.0523)	(0.404)	(0.114)	(0.239)	(0.0574)	(0.410)
Observations	74	74	74	74	143	145	143	95
R-squared	0.175	0.117	0.512	0.481	0.649	0.266	0.511	0.703

Robust standard errors in parentheses:*** p<0.01, ** p<0.05, * p<0.1

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