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**Working Paper**

## Macroeconomic effects of the anticipation and implementation of tax changes in Germany: Evidence from a narrative account

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Désirée I. Christofzik  
Angela Fuest  
Robin Jessen

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# Imprint

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Désirée I. Christofzik, Angela Fuest, and Robin Jessen<sup>1</sup>

# Macroeconomic Effects of the Anticipation and Implementation of Tax Changes in Germany: Evidence from a Narrative Account

## Abstract

*This paper quantifies the dynamic macroeconomic effects of tax changes in Germany, allowing for anticipation effects of preannounced tax reforms. Identification is achieved using a narrative approach which provides information about the timing of tax reforms. For an anticipated tax shock, we find an implied peak tax multiplier after implementation of 1.7. However, this positive effect is accompanied by significantly negative anticipation effects, in particular for output, investment, and hours worked. A substantial positive impact is observed only several quarters after implementation. Our results suggest that tax policy may thus be able to exploit anticipation effects by strategically announcing policy measures.*

*JEL-Code: H20, H30, E32, E62*

*Keywords: Fiscal policy; tax policy; anticipation effects*

*June 2020*

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# 1 Introduction

This paper estimates the dynamic macroeconomic effects of tax shocks in Germany for a sample covering the years 1970 to 2017. We allow for anticipation effects by taking into account the dates of announcements and realizations of changes to tax laws in a VAR model as in Mertens and Ravn (2012). In the spirit of Romer and Romer (2010), we employ a narrative approach to identify exogenous tax shocks. To this end, we use a data set that contains draft, announcement, and implementation dates as well as the magnitude of all relevant tax changes in Germany in the sample period.

The nature of the legislative process of fiscal policy creates lags between the time when economic agents receive news about future policy changes and the date at which these policy changes come into effect. Forward-looking agents react to announcements by adjusting their behaviour before the policy measures are implemented (Hall, 1971; Auerbach, 1989; House and Shapiro, 2006), a phenomenon referred to as fiscal foresight. Thus, analysing the macroeconomic effects of tax changes requires taking into account announcement effects (Yang, 2005). The sluggishness of the policy-making process is a reason why monetary policy might be considered preferable to fiscal policy for stabilization purposes. However, in times of very low interest rates, the zero bound becomes binding and arguably makes monetary policy less effective (see, e.g., Woodford, 2012). This has led to an increased interest in the effectiveness of unconventional tax policies in recent times. Such policy uses distortionary taxes to replicate the effects of negative nominal interest rates. Correia et al. (2013) show that an increasing path for consumption taxes over time coupled with decreasing labor taxes and temporary investment tax credits or capital income tax cuts can circumvent the zero bound problem. Previously, Feldstein (2002) and Hall (2011) have also proposed preannounced increases in VAT to stimulate spending. These policy measures rely on forward-looking agents and impact the prices that matter for intertemporal decisions. Thus, fiscal policy can, in principle, use anticipation effects to its advantage. On the other hand, if anticipation effects are not taken into account by policy makers, negative anticipation effects of presumably expansionary fiscal policies might even prolong an economic downturn (House and Shapiro, 2006; Mertens and Ravn, 2012).

Despite the relevance of fiscal foresight, there is limited empirical evidence on the macroeconomic anticipation effects of tax policy changes. In the empirical fiscal

policy literature, so far, the focus has been mainly on implications of the anticipation of government spending shocks (e.g., Ramey, 2011; Forni and Gambetti, 2016; Ben Zeev and Pappa, 2017). There are some studies on consumer responses to preannounced changes in social security or income tax (Parker, 1999; Souleles, 1999, 2002; Heim, 2007). These papers do not find strong anticipation effects.

In contrast, papers that study anticipation effects to changes in consumption tax rates on households' consumption behavior (D'Acunto et al., 2017, 2018; Crossley et al., 2014) find that the announcement of consumption tax increases leads to sizable increases in goods purchases. Thus, in principle, anticipation effects can be used to stimulate or depress the economy. This finding is of particular importance when dealing with the economic consequences of the COVID-19 pandemic.

Regarding announcement effects on aggregate output, House and Shapiro (2006) construct a dynamic general equilibrium model to assess the impact of time-lags in the implementation of income tax reductions. They show that the time lag depresses output and demonstrate how the slow phase-in of tax changes contributed to the sluggish recovery in the US after 2001. Mertens and Ravn (2012) use the Romer and Romer (2010) data set of narratively identified tax changes in the US and construct a VAR that allows for accounting for anticipation effects of tax changes. They find that anticipated decreases in tax liabilities have a negative impact on output in the years between announcement and implementation and a positive effect afterwards and demonstrate the importance of accounting for anticipation effects. Alesina et al. (2015) use the narrative approach to study the aggregate effects of fiscal consolidation plans for 17 countries including Germany in a set-up that allows for anticipation effects. In stark contrast to our paper, they pool fiscal adjustment plans from different countries in order to obtain sufficient observations. They find that the effects of tax shifts can partially be offset by anticipation of tax shifts in the opposite direction.

In this study, we add to the small literature that provides direct evidence on the macroeconomic anticipation effects of tax changes. We estimate the dynamic macroeconomic effects of tax shocks in Germany covering the years 1970 to 2017. Our contribution is two-fold: First, we provide empirical evidence for macroeconomic anticipation effects of tax changes in Germany. To our knowledge, this is the first paper to do so. In the spirit of Romer and Romer (2010), we use the narrative approach to identifying exogenous tax shocks. We use a data set that contains draft, announcement, and implementation dates, the type, and the size of all relevant discretionary tax changes in Germany in the sample period (Hayo and Uhl, 2014; Christofzik and Elstner, 2020).



Following Mertens and Ravn (2012), we allow for anticipation effects by distinguishing between anticipated and unanticipated tax changes and including both lags and leads of anticipated tax changes in our VAR model. Anticipation effects are based on the official announcement date of tax reforms as in Mertens and Ravn (2012) and—in a robustness test—on the date of the draft.

Second, we provide additional evidence for gauging the size of the tax multiplier in Germany. It was analysed in only few studies. Using the narrative approach, but without accounting for anticipation effects, Hayo and Uhl (2014) find a tax multiplier of 2.4, which is in line with estimates using the narrative approach for the US. Other studies apply an SVAR approach to Germany (among other countries) and generally find rather small tax multipliers, in some cases even unexpected signs, see, e.g., Perotti (2005) and studies cited in Hayo and Uhl (2014). Hollmayr and Kuckuck (2018) apply the SVAR approach to quantify the multiplier of fiscal shocks for fiscal policy changes of central, state, and local governments in Germany. They find a multiplier of at most 0.6. Gechert et al. (2020) construct a narrative data set of social security contribution and benefit shocks and estimate a multiplier of 0.4 for contribution cuts. Thus, there is no consensus in the literature on size of the tax multiplier in Germany.

Researchers who want to estimate the size of tax multipliers face the problem that fiscal policy is generally endogenous to the business cycle. The reason is that policy measures often are implemented with the aim to stabilize the economy. Therefore, identification of exogenous tax policy shocks is a critical issue for the estimation of macroeconomic effects of tax law changes. One way to identify exogenous tax shocks is to impose restrictions on the parameters in structural vector autoregressions (see Blanchard and Perotti, 2002; Mountford and Uhlig, 2009). In contrast, the narrative approach, which we apply in this paper, uses explicit information on the motivation of tax law changes and thus assumes that exogenous tax shocks are directly observable. Romer and Romer (2009) identify the motivation for all major post-war tax law changes in the US. Using this data set, Romer and Romer (2010) estimate the impact of exogenous tax changes on real output in the US and find that tax increases by one percent have a substantial negative impact on GDP with a considerable delay after implementation; the strongest effect is observed after two and a half years. Cloyne (2013) applies the narrative approach to the UK, and Guajardo et al. (2014) construct a multi-country data set of deficit driven fiscal policy changes and estimate their impact on output. Ramey (2019) provides an overview of the recent literature on the fiscal multiplier. Studies using the narrative approach typically find tax multipliers—

defined as the largest cumulative multiplier after the implementation of the law—between 2.5 and 3.

We build upon the approach originated by Mertens and Ravn (2012) to identify US tax policy shocks. A notable difference between that study and ours is that our data set contains information on the dates of both the draft and the official announcement of policy reforms. Tax laws are discussed in the media already at the draft stage. While we base our main estimation of anticipation effects based on the lag between announcement and implementation of tax changes, in a further step, we also take into account anticipation effects based on the lag between the draft date and the implementation date.

Similar to Mertens and Ravn (2012), we find that anticipated tax cuts have a negative impact on output before implementation, while a substantial positive impact is achieved only several quarters after implementation. For anticipated tax shocks, we estimate a peak fiscal multiplier of 1.7, defined as the value of the largest increase in GDP above trend post tax cut implementation. In line with Mertens and Ravn (2012), we find large and significant negative anticipation effects on investment.

The paper is structured as follows. Section 2 describes the data set and our measure for legislated tax changes in Germany. In particular, we categorize these changes as unanticipated and anticipated based on their timing. Based on this, we present the empirical specification. In Section 3, we present our main results. We provide a number of robustness checks in Section 4. Section 5 concludes.

## 2 Empirical Strategy

### 2.1 Measuring Tax Policy Changes

To study the effects of tax reforms on output already in the run-up to their implementation, we first need to identify discretionary tax changes that are unrelated to other factors influencing output in the near term. This serves to disentangle the effects of tax changes from underlying factors. Secondly, we need detailed information on the timing of tax reforms. For both steps, we resort to the narrative approach, which was introduced by Romer and Romer (2010). This method uses information from the legislative process to identify exogenous tax changes and also includes information on important dates in this process. We make use of the historical account of legislated tax changes by Uhl (2013) and Hayo and Uhl (2014), which was extended by Chris-

tofizik and Elstner (2020) for the period 2010 to 2017. The data set is based on official government documents, in particular annual budget reports of the German Federal Ministry of Finance and draft bills. It comprises 90 tax acts that were introduced or announced between the first quarter of 1970 and the last quarter of 2017. Since each tax act can cover multiple tax liability changes, the total number of individual tax changes amounts to 1,693. The data set also includes estimations by the government on the impact of these tax changes on tax revenues.

Based on the underlying motive of the tax legislation, the tax changes are classified as either endogenous or exogenous.<sup>1</sup> Endogenous tax changes are those changes that are introduced in response to current economic conditions. Hayo and Uhl (2014) define three endogenous categories: countercyclical measures, spending-driven measures, and tax policies in response to macroeconomic shocks. Policies to stabilise the business cycle and policies to finance an increase in government spending are regarded as endogenous reactions. The third category is broader and comprises tax changes induced by policy events such as the introduction of the Euro and changes induced by an endogenous decrease in tax revenues.<sup>2</sup>

In our analysis, we focus on exogenous tax changes. These should be unrelated to contemporaneous macroeconomic conditions. The literature identifies two kinds of exogenous policy measures (e.g., Romer and Romer, 2010; Hayo and Uhl, 2014). The first category comprises tax changes that are implemented in order to consolidate the budget. Inherited budget deficits are the result of past economic conditions and policy decisions and are thus not associated with contemporaneous economic conditions or spending decisions.<sup>3</sup> The second category comprises tax measures that aim at increasing long-run growth by improving structural conditions via promoting investment or consumption. The condition that the tax changes are exogenous allows us to use them in a straightforward way as exogenous regressors in our empirical application.

Our data set contains 1,353 exogenous measures. Figure 1 plots these exogenous tax measures, aggregated to a quarterly series, together with annual real GDP growth. The graph does not indicate any contemporaneous correlation between GDP growth and exogenous tax changes. The correlation coefficient between these two variables is

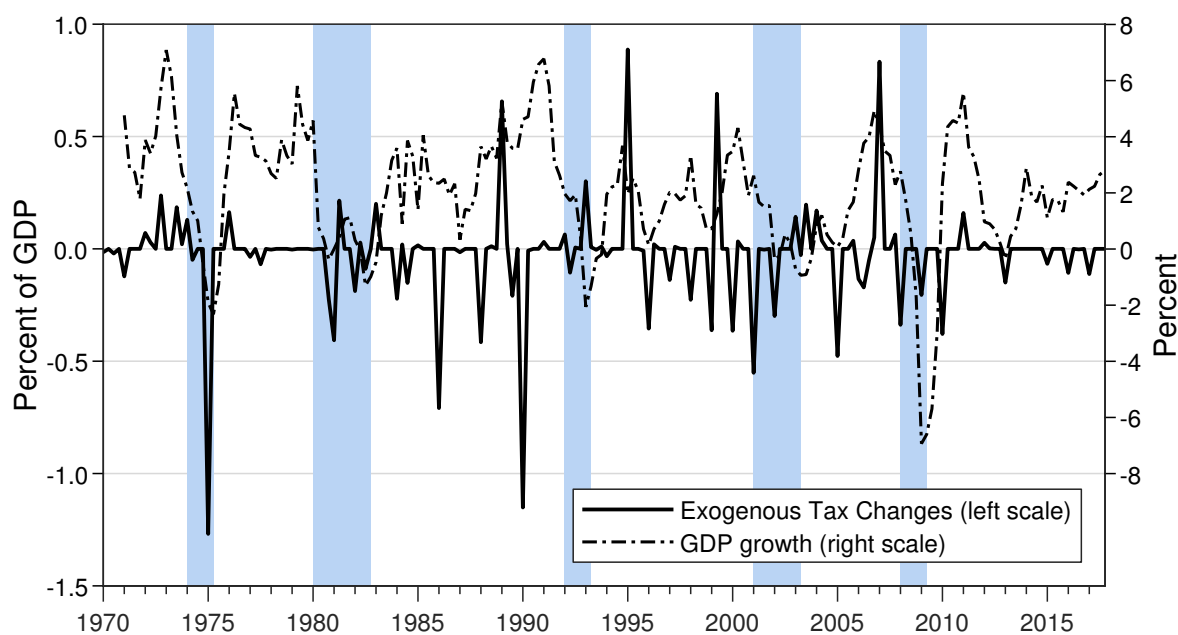
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<sup>1</sup> Explanations and motives for tax changes are primarily drawn from the budget report or government statements (Hayo and Uhl, 2014).

<sup>2</sup> Thus, Hayo and Uhl (2014) identify one more endogenous category of tax changes than Romer and Romer (2010), who acknowledge two endogenous motives: countercyclical and spending-driven.

<sup>3</sup> Past economic conditions impact both current economic conditions and the probability of consolidation tax changes. They are controlled for by including lags in the empirical analysis.

**Figure 1: Exogenous Tax Changes in Germany and GDP growth**



*Notes:* This figure presents the quarterly time series of narratively identified exogenous tax changes in Germany on the left scale. It shows the estimated annual revenue impact of legislated tax measures after they are fully implemented in percent of nominal GDP. This impact is assigned to the quarter in which measures are enacted if implemented before the middle of that quarter and to the next quarter otherwise. The right scale shows annual real GDP growth in percent, seasonally and working day adjusted. Shaded regions denote recessions as dated by the German Council of Economic Experts (2017): 1974Q1-1975Q2, 1980Q1-1982Q4, 1992Q1-1993Q2, 2001Q1-2003Q2, and 2008Q1-2009Q2.

0.09. A hit ratio<sup>4</sup> of 46.34 percent indicates that the exogenous tax changes are roughly randomly distributed. In Section 4.5, we study whether exogenous tax changes can be predicted by past values of the endogenous variables included in the VAR and find no evidence for a systematic relation.

The data set based on the narrative approach also provides detailed information about the timing of the legislative process. This is an advantage over other—more conventional—approaches that, e.g., deduce discretionary tax shocks from changes in the effective tax rate. Three key dates in the legislative process can be discerned: the date of the draft bill, the announcement date, and the implementation date of the law.

<sup>4</sup> Following Hayo and Uhl (2014), we calculate the hit ratio as the number of quarters in which exogenous tax changes and GDP growth have the same sign divided by the number of non-zero quarters of exogenous tax changes.

- i. A draft bill is introduced to be debated in the *Bundestag*, the federal parliament (draft date).
- ii. If, after three readings, the majority of the plenary of the *Bundestag* votes in favour of the bill and the *Bundesrat*, a legislative body representing the federal states, consents, the act will be signed by the Federal President and published in the Federal Law Gazette (announcement date). This publication further declares the date at which the law and its distinct policy measures will come into effect (implementation date).
- iii. The law is implemented.

The different dates are each associated with an expected revenue impact of the reform estimated by the government. This allows for constructing quarterly time series of tax changes at each of the three stages (Christofzik and Elstner, 2020). We follow the convention in the literature and define our measure of tax changes to be included in the analysis as the expected annual revenue impact of the tax measures after they are fully implemented, divided by nominal GDP. Additionally, in line with the literature, we assign the dates of draft, announcement, and implementation to specific quarters as follows. Policy actions that occur in the first half of a quarter are assigned to that quarter, policy actions that occur in the second half are assigned to the following quarter (see e.g., Romer and Romer, 2010). In case a tax change is temporary, this measure is offset in the data set by constructing an equal size change with the opposite sign at the expiration date (*phase-out*). One-time revenue effects are neutralised by a corresponding change with the opposite sign directly in the following quarter.

We construct a quarterly time series of exogenous tax changes for Germany between 1970 and 2017 by adding up the expected revenue effects of all tax policy measures  $\tau_{i,t}$  assigned to a specific quarter so that a tax change at time  $t$  is defined as

$$\tau_t = \sum_{i=1}^{N_t} \tau_{i,t}, \quad (1)$$

where  $i$  denotes a single policy measure, and  $N_t$  is the number of tax measures in period  $t$ . In our baseline estimations, we use the annual revenue impact of the tax measures after they are fully implemented divided by nominal GDP as estimated by the government in the last step of the legislative process.<sup>5</sup> By aggregating the 1,353 exogenous tax measures of our data set, we end up with 84 out of 192 quarters (44%)

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<sup>5</sup> The tax revenue estimated at the draft or announcement stage may differ from the expected revenue impact at implementation. For a robustness check, we assign the revenue estimate at the imple-

**Table 1:** Descriptive statistics of exogenous tax changes

	Total	Draft		Announcement	
		unanticipated	anticipated	unanticipated	anticipated
<i>Tax policy measures</i>					
Observations	1353	58	1295	740	613
Mean (% of GDP)	-0.004	-0.009	-0.003	-0.002	-0.006
Maximum (% of GDP)	0.835	0.102	0.835	0.835	0.822
Minimum (% of GDP)	-1.054	-0.182	-1.054	-1.054	-0.751
Standard Deviation	0.076	0.070	0.082	0.070	0.082
<i>Non-zero quarters</i>					
Observations	84	10	78	44	60
Mean (% of GDP)	-0.051	-0.050	-0.048	-0.026	-0.052
Maximum (% of GDP)	0.887	0.163	0.887	0.690	0.887
Minimum (% of GDP)	-1.269	-0.252	-1.269	-0.692	-1.269
Standard Deviation	0.207	0.034	0.205	0.103	0.179

*Notes:* Anticipated tax changes are those exogenous tax changes for which the time between draft, respectively announcement, and implementation exceeds 90 days. Unanticipated tax changes are characterized by an implementation lag shorter than or equal to 90 days. The descriptive statistics refer to the estimated annual revenue impact of legislated tax measures after they are fully implemented as estimated by the government in the last step of the legislative process, expressed in percent of nominal GDP. Tax policy measures comprise distinct legislative tax changes implemented between 1970 and 2017 in Germany. Non-zero quarters denote those quarters of the quarterly series of tax changes in which at least one tax policy measure has been implemented. Own calculations.

in which exogenous tax changes were implemented in Germany between 1970 and 2017, see Table 1.

The regulations of the legislative process in Germany allow for a precise identification of draft, announcement, and implementation dates. Therefore, we follow a timing-based approach to distinguish between anticipated and unanticipated tax changes. For the case of the US, Mertens and Ravn (2012) and Poterba (1988) define the anticipation horizon of a legislated tax change as the time between the announcement date, at which the US president signs the law, and the implementation date. In our baseline estimation, we adopt this procedure.<sup>6</sup> In Germany, the majority of tax

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mentation date to the surprise tax changes and the revenue estimate at the announcement date to the anticipated tax changes, see Appendix B. This specification does not change our conclusions from the main section.

<sup>6</sup> In a robustness check, we estimate the effects based on anticipation between draft and implementation, see Figure 6.

changes need either less than a month or more than five months to be implemented after they were announced (right panel of Figure 2). On average, this process takes more than eight months (259 days), while the median is 73 days. Given this long implementation lag, it is reasonable to say that economic agents are able to anticipate tax changes and adjust their behavior accordingly.

The upper panel of Table 1 presents summary statistics of the tax measures. Following Mertens and Ravn (2012), we classify a tax change as anticipated if the time period between announcement (or draft) and implementation exceeds 90 days. Based on this distinction, more than half of the tax changes are categorized as unanticipated. When considering the anticipation horizon between draft and implementation, we identify only 58 unanticipated tax measures in Germany between 1970 and 2017, while 1,295 are anticipated. In some cases, a tax change is announced later than the implementation date. We categorise these retroactive measures as unanticipated.<sup>7</sup>

The lower panel of Table 1 shows summary statistics for quarters in which at least one tax measure (of the specific column type) was implemented. Note that in contrast to the upper panel, the number of total observations does not equal the sum of observations of quarters with at least one unanticipated or at least one anticipated tax measure. The reason is that in some quarters, both anticipated and unanticipated tax measures were implemented. In many cases, several tax measures are implemented within the same quarter. As a result, the standard deviation of the total change in tax revenue relative to GDP per quarter (0.18) is more than twice as large as the standard deviation of tax revenue changes per tax policy measure (0.08).

Table 2 summarizes characteristics of the two series of anticipated tax shocks, in one case defined based on the lag between draft and implementation and in the other case based on the lag between announcement and implementation. Note that the two samples are not identical. The number of anticipated tax changes according to the draft date (1,295 observations) is substantially larger than the number of anticipated tax changes based on the announcement date (613 observations), see Table 1. The median of the implementation lag equals three quarters based on the draft and two quarters based on the announcement. Thus, the anticipation horizon is considerably smaller than in the US—Mertens and Ravn (2012), find a median anticipation

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<sup>7</sup> For instance, on 16 July 2015, the income tax was reduced for the entire year 2015. Overall, there are 21 such retroactive tax changes in the data. In an alternative specification (not reported), we use the announcement date of these tax changes instead of the implementation date. The differences in the results compared to the baseline specification are negligible.

**Table 2:** Anticipation horizon of anticipated exogenous tax changes in Germany in quarters

	<b>Draft</b>	<b>Announcement</b>
Median	3	2
Mean	4.6	4.9
Minimum	1	1
Maximum	31	30
Standard Deviation	4.96	5.70

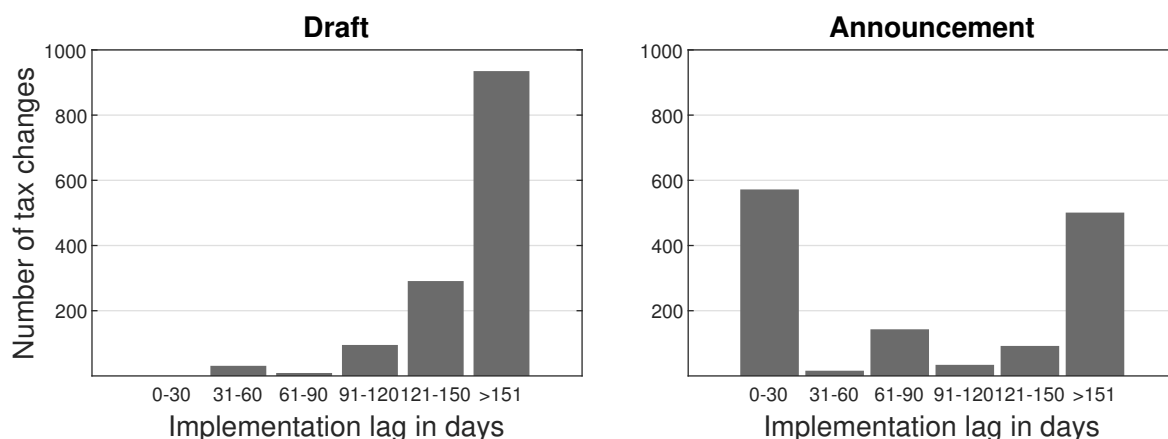
*Notes:* The table shows anticipation horizons for those exogenous tax changes which we classify as anticipated, i.e., tax changes with an implementation lag between draft, respectively announcement, and implementation of more than 90 days. The anticipation horizon is expressed as the number of quarters between draft and implementation and between announcement and implementation, respectively. Own calculations.

lag of six quarters based on an anticipation horizon between announcement and implementation. The largest implementation lags observed in Germany amount to 30 quarters based on the announcement date and 31 based on the draft date, i.e., more than seven years. Allowing for anticipation horizons of such length would lead to a high number of parameters to be estimated in the analysis. Therefore, following Mertens and Ravn (2012), we limit the maximum anticipation horizon in the estimation equation in Section 2.2.

Figure 2 illustrates the distribution of the implementation lags of the identified exogenous tax changes in days. The panel on the left-hand side shows the distribution of implementation lags measured by the lag between draft date and implementation date. The majority of tax changes takes more than five months to come into effect. In the right panel, the implementation lag is the period between announcement and implementation. The twin-peaked distribution of the implementation lags of German tax changes is similar to that of US tax changes reported in Mertens and Ravn (2012). Most tax changes are executed either within one month or after at least five months. However, in contrast to Mertens and Ravn (2012), we find that the majority of tax changes are implemented within 30 days after the announcement date. In the US, the largest share of tax changes is associated with an implementation lag exceeding 151 days.



**Figure 2: Distribution of implementation lags**



*Notes:* The figure shows the distribution of implementation lags for all exogenous tax changes in Germany, expressed in days. In the left panel, the implementation lag is the time between draft and implementation. In the right panel, the implementation lag is the time between announcement and implementation. Own calculations.

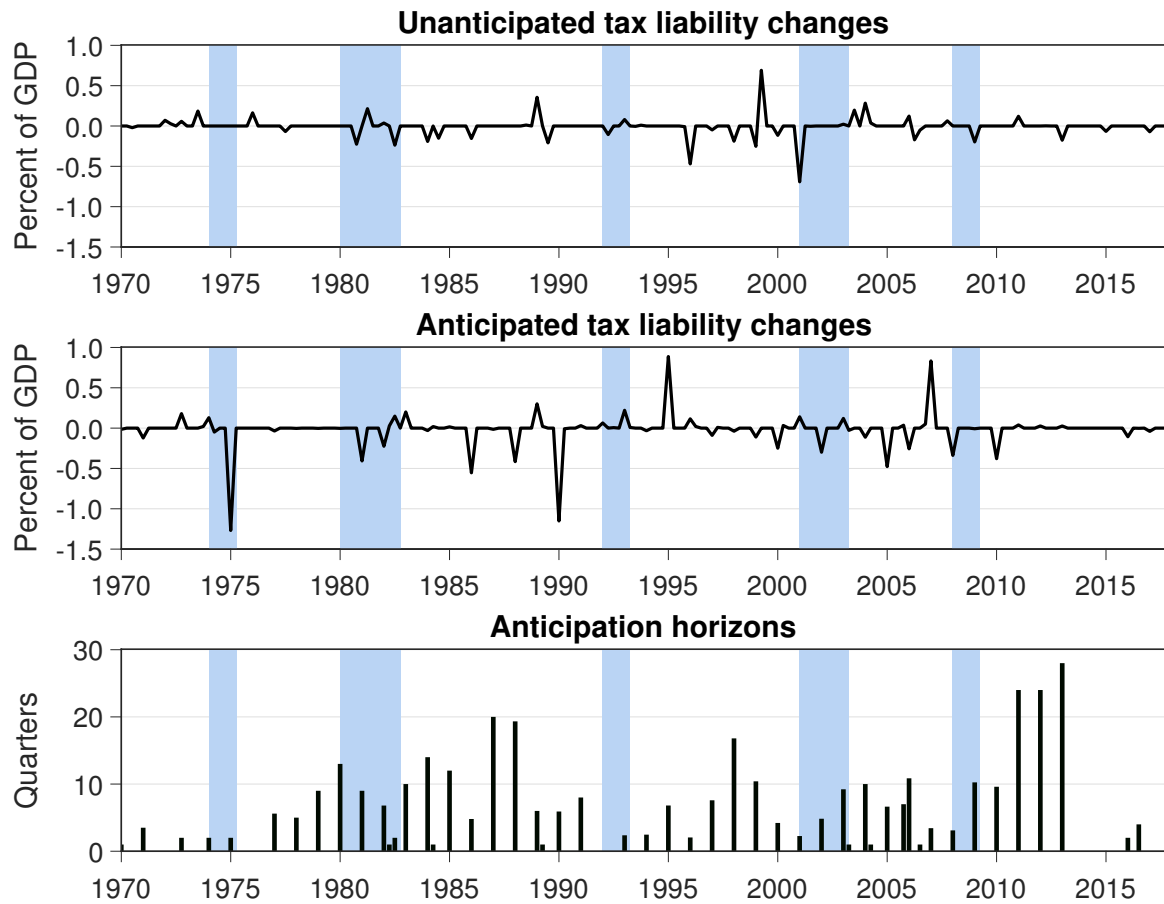
Comprising those tax changes that are known at date  $t$  to be implemented at  $t + i$ , the quarterly series of anticipated tax changes is computed as

$$\tau_{t,i}^a = \sum_{j=0}^{M-i} s_{t-j}^{a,i+j}, \quad (2)$$

where  $s_{t-j}^{a,i+j}$  denotes anticipated tax measures  $s$  known at date  $t - j$  with an anticipation horizon of  $i + j$ , and  $M$  is the maximum anticipation horizon in the data.<sup>8</sup> Thus  $\tau_{t,i}^a$  denotes the total tax liability change expected at date  $t$  to occur in  $i$  quarters. Figure 3 displays the time series of unanticipated and anticipated tax changes as well as the average implementation lag between announcement and implementation in quarters. Some notable spikes can be discerned. The largest unanticipated tax cut is observed in the second quarter of 1999, in which numerous measures originating from the *Tax Relief Act* came into force. Large anticipated tax cuts occurred for instance in the first quarters of 1975 (*Income Tax Reform Act*) and 1990 (*Tax Reform Act 1990*). Larger tax increases resulted from the *Law on the Implementation of the Federal*

<sup>8</sup> It is not feasible to account for differential effects of tax changes with different anticipation horizons as this would imply a considerable loss of degree of freedom. Since the largest anticipation horizon in the data is 30 quarters, this would lead to a high number of parameters to be estimated. Therefore, we take into account tax liability changes based on their remaining anticipation horizon as in Mertens and Ravn (2012).

**Figure 3: Unanticipated and anticipated tax changes based on the announcement date**



*Notes:* The first two panels show unanticipated and anticipated legislative tax changes in Germany between 1970 and 2017. Anticipated tax changes are those exogenous tax changes for which the time between announcement and implementation exceeds 90 days. Unanticipated tax changes are characterized by an implementation lag shorter than or equal to 90 days. The bars in the bottom panel denote the average anticipation horizon in quarters for the anticipated tax changes. Shaded regions denote recessions as dated by the German Council of Economic Experts (2017): 1974Q1-1975Q2, 1980Q1-1982Q4, 1992Q1-1993Q2, 2001Q1-2003Q2, and 2008Q1-2009Q2.

*Consolidation Programme*, driven in particular by the levy of a solidarity surcharge on personal income and corporate income tax in 1995, and from the *Budget Accompanying Act 2006*, which comprised an increase of the standard VAT rate by three percentage points.

## 2.2 Empirical Specification

Taking into account foresight of policy measures poses challenges to the identification of macroeconomic shocks (see e.g., Ramey, 2016, for an overview). Leeper et al. (2013) show that foresight, i.e., news about future policy changes, can lead to rational expectations equilibria with non-fundamental representations. This majorly impedes the identification of the structural shocks to which private agents respond and thus leads to biased results.

In practice, methods to overcome the challenges of modelling foresight comprise using direct expectations measures, time series restrictions, or theoretical model restrictions. For instance, in the fiscal policy literature, Ramey (2011) constructs a time series of news about future government spending by using information contained in business magazines. Fisher and Peters (2010) construct a time series of news about future government spending by using information from the stock returns of military contractors. Moreover, Poterba (1986) as well as Leeper et al. (2012) exploit the spread between federal and municipal bond yields to derive a gauge of fiscal foresight.

A different approach is taken by Mertens and Ravn (2012). They include leads of anticipated discretionary tax changes in their regression equation. According to their classification, tax liability changes are anticipated if the period between the announcement date and the implementation date exceeds 90 days.

We follow this approach, and accordingly base our analysis on regression equation (3). Mertens and Ravn (2011) show that this equation can be derived as an approximation of the observables in a DSGE model that incorporates stochastic shocks to tax rates. The specification is given by:

$$X_t = Av_t + Bt + C(L)X_{t-1} + D(L)\tau_t^u + F(L)\tau_{t,0}^a + \sum_{i=1}^K G_i\tau_{t,i}^a + e_t, \quad (3)$$

where  $X_t$  is a vector that contains the logarithms of real per capita GDP, real per capita investment, real per capita private consumption, the logarithm of hours worked per capita, and the logarithm of real wages per employee.  $t$  is a linear trend,  $C(L)$ ,  $D(L)$  and  $F(L)$  are lag polynomials and  $e_t$  is an i.i.d. error term.<sup>9</sup> To account for the financial crisis we further add a dummy variable, setting the last quarter of 2008 and the first quarter of 2009 to one.  $v_t$  contains this dummy in addition to a constant.

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<sup>9</sup> Using a quadratic trend in the spirit of Ramey (2011) changes the results only marginally.

Unanticipated tax changes are denoted by  $\tau_t^u$ . We distinguish between contemporaneous tax changes that have been anticipated ( $\tau_{t,0}^a$ ) and anticipated tax changes that will be implemented in  $t + i$  ( $\tau_{t,i}^a$ ). The construction of these tax series is explained in more detail above in Section 2.1. Allowing for differential coefficients for the lags of anticipated and unanticipated tax changes in the VAR model makes our results directly comparable to Mertens and Ravn (2012).

As in Mertens and Ravn (2012), in our main specification, we set the order of the lag polynomials  $D(L)$  and  $F(L)$  to 12, the number of lags of the endogenous variables ( $C(L)$ ) to 1, and we choose a maximum anticipation horizon of  $K = 6$ . In Section 4, we show that the results are robust to varying these parameters.

To facilitate comparison with studies that do not take anticipation effects into account, we additionally run regressions that do not distinguish between anticipated and unanticipated tax shocks (see Section 4.4). In this specification, all exogenous tax changes enter with the same coefficient:

$$X_t = Av_t + Bt + C(L)X_{t-1} + G(L)\tau_t + e_t. \quad (4)$$

Parameters and variables are unchanged compared to the baseline regression.

In practice,  $\tau_{t,i}$  cannot be interpreted as actual changes in taxes because the forecasted revenue effects of tax law changes reported in the drafts and bills contain forecast errors. Assuming classical measurement error, this leads to a bias of the associated coefficients towards zero. In principle, an alternative approach would be to use our measures of tax shocks as instruments for the total change in the actual tax revenue, see Mertens and Ravn (2013), who formulate this approach as a proxy VAR. However, accounting for fiscal foresight is more straightforward in our set-up. Furthermore, the proxy VAR approach might be susceptible to weak instruments (see Hebous and Zimmermann, 2018). Moreover, the coefficient associated with the forecasted impact of tax changes on tax revenue instead of the impact of actual tax changes might be the more relevant policy parameter as policy-makers can only use the former. It should also be relevant for individuals when expecting tax changes.

## 3 Empirical Results

### 3.1 Baseline Results

The left column of Figure 4 records our benchmark results. It shows the responses to a one percent decrease in tax revenue relative to GDP, based on the model specified in Equation (3). The anticipation horizon is defined with respect to the announcement date of reforms. Throughout the paper, error bands are constructed by a residual-based bootstrap procedure as applied, e.g., by Mertens and Ravn (2011). To demonstrate the importance of allowing for anticipation effects, we contrast these results with the responses of the same variables for all types of exogenous shocks, without distinction between anticipated and unanticipated tax shocks, see Equation (4). The corresponding impulse response function is displayed on the right-hand side of Figure 4.

Anticipated tax cuts are associated with a significant decline in output before implementation. These negative anticipation effects of tax cuts are not revealed when considering effects only after their implementation. Output continues to stay below its trend for some quarters after the implementation. At first glance, it might seem surprising that the impulse response function is still negative after tax cuts are realized. However, the increase in output occurs gradually. Therefore, output rises significantly above its trend not until two years after implementation. This also resembles findings by Mertens and Ravn (2012) for the US. The implied peak tax multiplier of an anticipated tax cut for Germany based on the point estimate is 1.7.

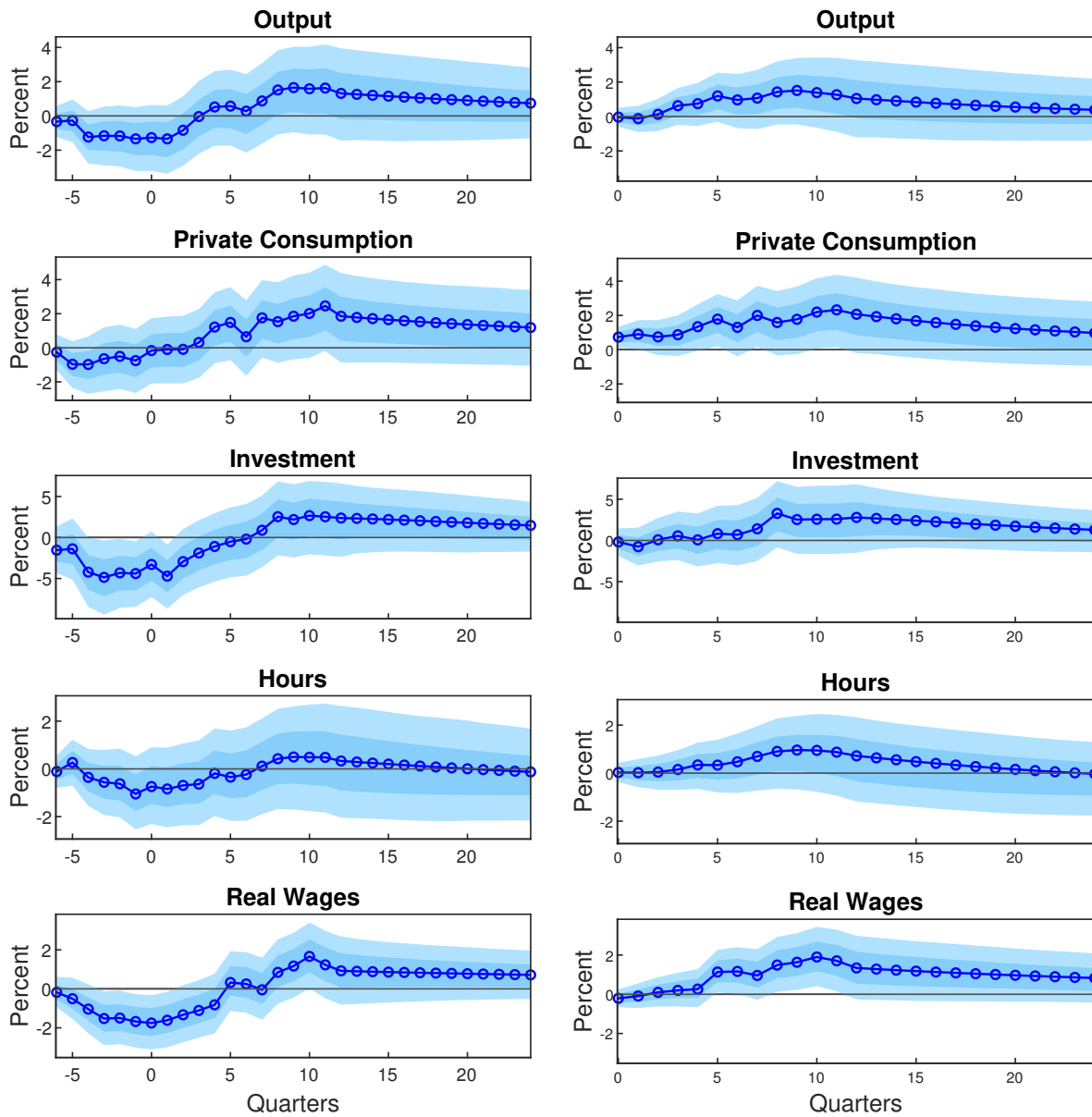
The development of private consumption is similar. We find negative anticipation effects. Consumption rises above its trend a bit earlier than output and remains above trend over the whole forecast horizon. We observe the largest negative anticipation effects for investment, which decreases by about five percent for several quarters. This result is remarkably similar to that obtained by Mertens and Ravn (2012) for the US. The strong response of investment is in line with the notion that capital is mobile and taxes have an impact on the rate of return to capital.

In addition to a direct effect, taxes may have an impact on the rate of return by affecting labor supply. In fact, we observe negative anticipation effects for hours worked, again similar to Mertens and Ravn (2012). On the other hand, there is no significant positive impact on hours worked post-implementation. This result can be explained with a Marshallian labor supply elasticity close to zero, which is in line with micro evidence (see e.g. Keane, 2011). While a permanent income tax reduc-

**Figure 4: The impact of a 1 percent tax cut**

**(a) Anticipated tax cut**

**(b) Tax cut without anticipation effects**



*Notes:* The figures shows the responses of macroeconomic variables to an exogenous tax cut corresponding to one percent of GDP. The panels on the left-hand side depict effects stemming from an anticipated tax cut announced six quarters before implementation. Anticipated tax changes are those exogenous tax changes for which the time between announcement and implementation exceeds 90 days. The panels on the right-hand side show effects for all exogenous tax shocks based on a specification which does not account for anticipation effects. Lines with circles indicate point estimates. Shaded areas denote 68 and 95 percent bootstrapped confidence intervals based on a nonparametric bootstrap with 10,000 replications.

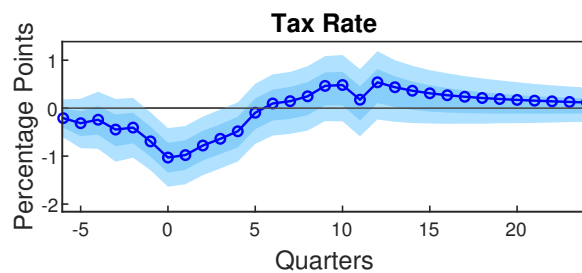
tion has little impact on hours worked in the long run, an anticipated tax reduction provides an incentive to shift hours of work intertemporally, away from the current periods with higher taxes than in the future. Finally, we find that a tax cut gives rise to a decline in real wages prior to the implementation, in line with reduced investment. After implementation, wages increase significantly.

### 3.2 Results for the Effective Tax Rate

Based on the results for the macroeconomic variables, we study how the average tax rate is affected by tax cuts. To this end, we re-estimate Equation (3) with the same parameters, including as variables the logarithms of real per capita GDP, real per capita investment, real per capita private consumption, and, in addition, the effective tax rate, defined as total tax revenue in relation to nominal GDP. The response of the effective tax rate is plotted in Figure 5.

Prior to the tax cut corresponding to one percent of nominal GDP, tax revenues relative to GDP decrease. This might reflect that activities that are directly affected by the tax cut are postponed until implementation. For instance, households delay consumption of certain goods and increase savings when expecting a consumption tax cut. In such case, the tax base decreases stronger than output. This would be in line with the prompt increase after implementation. Moreover, as the income tax is progressive, a decrease in real wages leads to a more than proportional decrease in tax revenues. The tax rate remains below its trend for only one year.

**Figure 5:** The impact of a 1 percent tax cut on tax revenue



*Notes:* The figure shows the responses of the effective tax rate (tax revenue in relation to nominal GDP) in percentage points to an exogenous anticipated tax cut announced six quarters before implementation corresponding to one percent of GDP. Anticipated tax changes are those exogenous tax changes for which the time between draft and implementation exceeds 90 days. Shaded areas denote 68 and 95 percent bootstrapped confidence intervals based on a nonparametric bootstrap with 10,000 replications.

## 4 Robustness

In this section, we show that our results are qualitatively robust to whether the anticipation horizon is defined as the lag between the date of the draft and the date of implementation or as the lag between the date of the official announcement and the date of implementation. Additionally, we show the robustness to changes in lag length and anticipation horizons. In additional exercises, we use alternative regression specifications and different variables to check whether the use of Equation (3) or the set of included variables drive our results and to facilitate comparisons with other studies. Finally, we show that the tax reforms that we have classified as exogenous cannot be predicted by past values of the variables included in the VAR.

### 4.1 Anticipation Based on the Draft Date

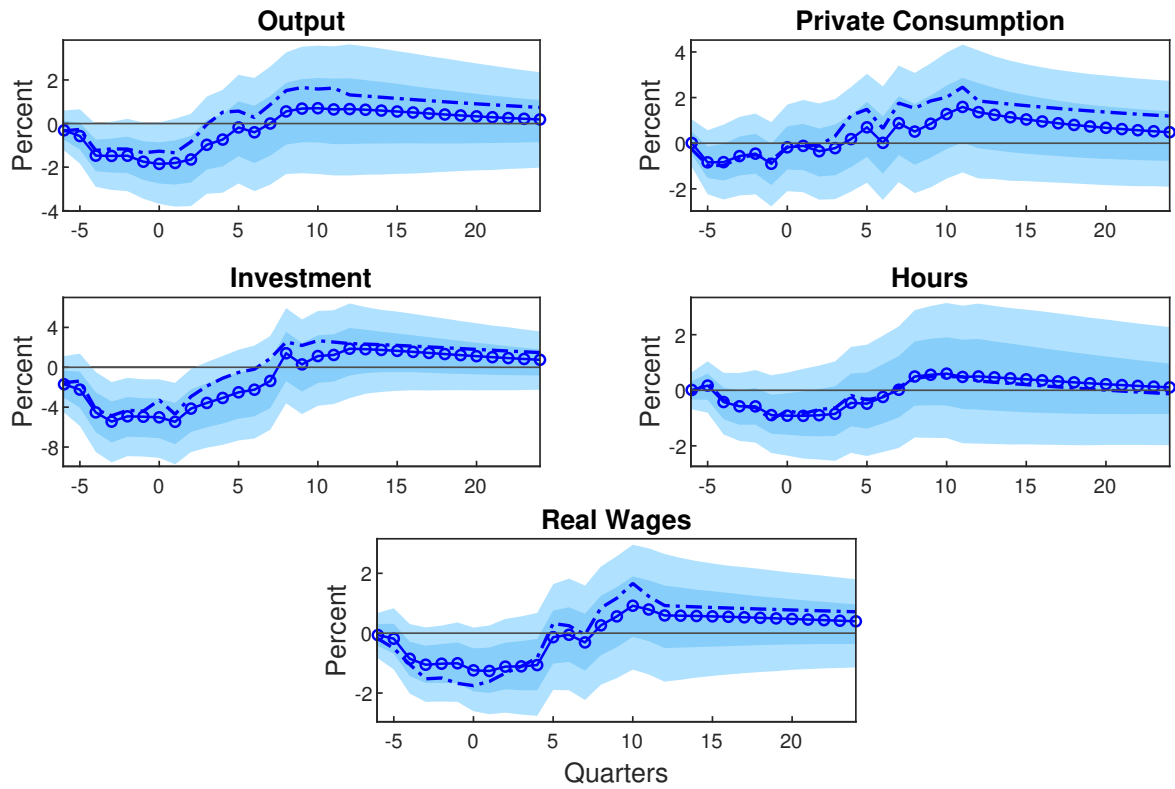
While our main results rest on anticipation horizons based on the official announcement date of tax changes, in Figure 6 the anticipation horizon is defined as the time between the date of the draft and the date of implementation. Compared to the baseline specification presented in Figure 4a, the pre- and post-implementation responses are generally similar. The observed expansion in output, consumption, and investment is less pronounced. However, similar to Figure 4a, the run-up to tax cuts is characterized by a slight downturn, which seems to be driven largely by a reduction in investment. Moreover, hours worked decline significantly before implementation and return to their previous level afterwards. Wages decrease significantly pre-implementation and increase beyond their previous level afterwards.

### 4.2 Robustness to Length of Anticipation Horizon

Figure 7 shows the responses to tax cuts allowing for different anticipation horizons. As expected, allowing for longer anticipation horizons leads to earlier anticipation effects. Moreover, as it is the case for the US (Mertens and Ravn, 2012), the negative anticipation effect on output is more pronounced when using a horizon of eight or even ten quarters. In practice, few tax law changes have such a long implementation lag. While the assumed anticipation horizon has an impact on the timing and magnitude of anticipation effects, the post-implementation effects are almost unchanged relative to the baseline specification and do not differ statistically significantly.

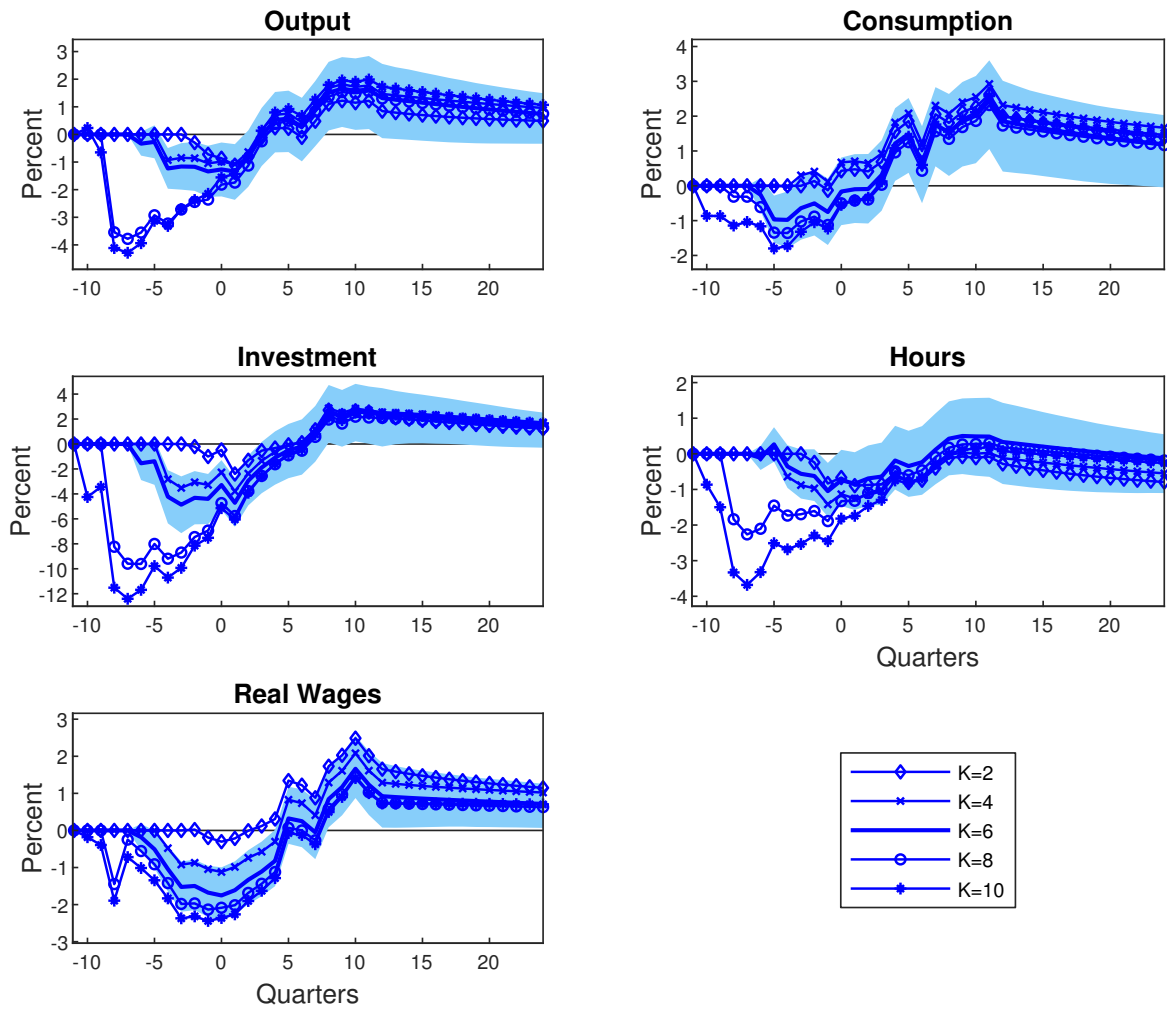


**Figure 6:** The impact of an anticipated 1 percent tax cut – Anticipation based on the draft date



*Notes:* The figure shows the responses of macroeconomic variables to an exogenous anticipated tax cut announced six quarters before implementation corresponding to one percent of GDP. Anticipated tax changes are those exogenous tax changes for which the time between draft and implementation exceeds 90 days. Shaded areas denote 68 and 95 percent bootstrapped confidence intervals based on a nonparametric bootstrap with 10,000 replications. Dashed lines are the point estimates of the baseline results of the left-hand side in Figure 4a.

**Figure 7:** The impact of a 1 percent tax cut – Alternative anticipation horizons

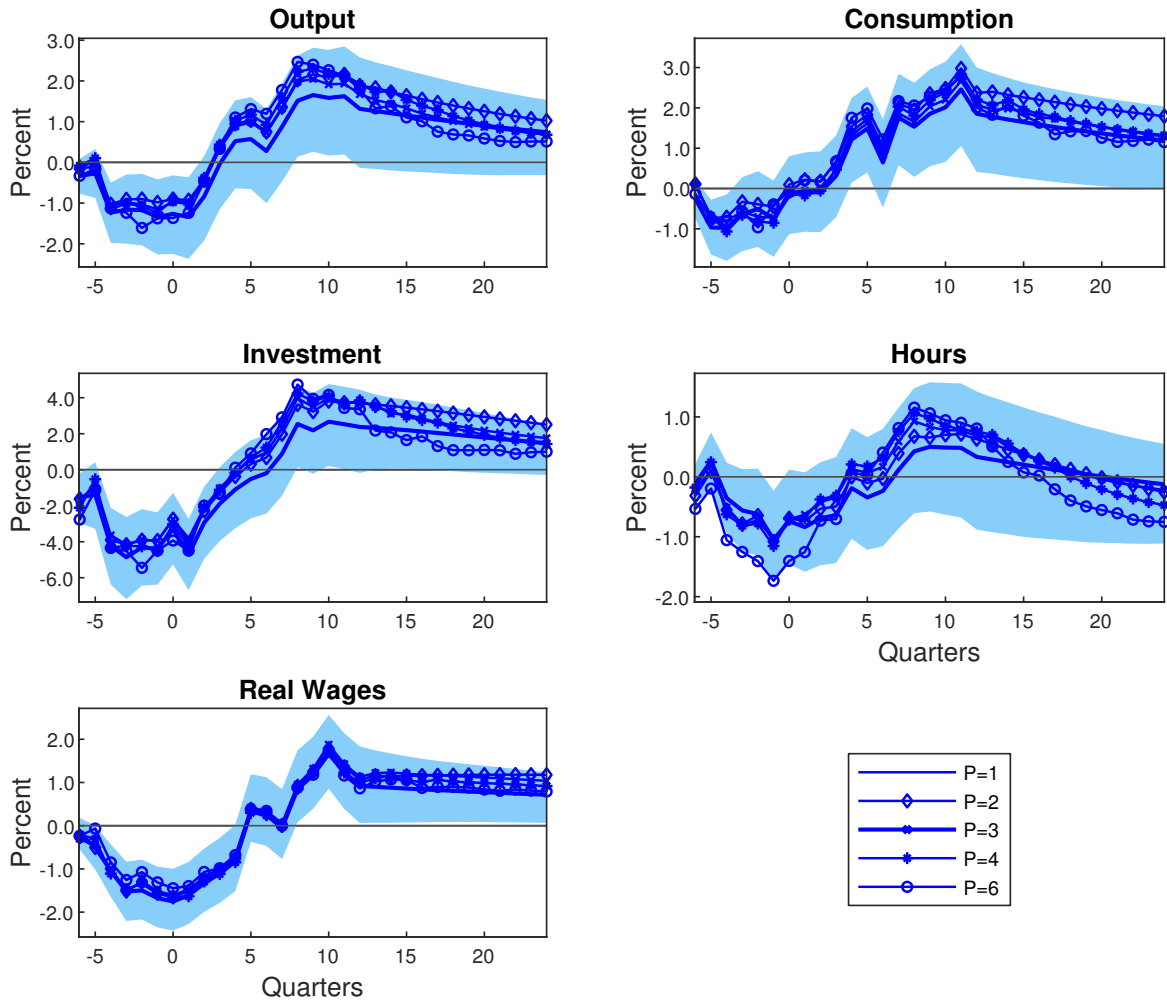


*Notes:* The figure shows the responses of macroeconomic variables to an exogenous anticipated tax cut corresponding to one percent of GDP with different anticipation horizons. Anticipated tax changes are those exogenous tax changes for which the time between announcement and implementation exceeds 90 days. Shaded areas denote 68 percent bootstrapped confidence intervals of the baseline specification ( $K = 6$ ).

### 4.3 Robustness to Alternative Lag Lengths

Figure 8 shows the same impulse responses as Figure 4a but with varying lag lengths of the endogenous variables. In our baseline specification, we set the lag length to 1. The responses are strikingly similar when extending the lag length and lie within the 68-percent confidence bands of the baseline specification.

**Figure 8:** The impact of a 1 percent tax cut – Alternative lags of endogenous variables



*Notes:* The figure shows the responses of macroeconomic variables to an exogenous anticipated tax cut announced 6 quarters before implementation corresponding to one percent of GDP. Anticipated tax changes are those exogenous tax changes for which the time between announcement and implementation exceeds 90 days. Lines represent the point estimates based on different choices for the lags of endogenous variables  $P$ . Shaded areas denote 68 percent bootstrapped confidence intervals of the baseline specification ( $P = 1$ ).

## 4.4 Alternative Regression Specifications

In two additional tests, we use alternative regression specifications and different set of variables.

(i) In our main specifications, we adopt the methodology of Mertens and Ravn (2012). We further set up an alternative regression specification in which we summarize unanticipated and anticipated tax changes under  $\tau_t$ , i.e., we do not allow for differential contemporaneous effects of the two types of tax changes, while still accounting for tax leads of the anticipated tax changes  $\tau_t^a$ :

$$X_t = Av_t + Bt + C(L)X_{t-1} + D(L)\tau_t + \sum_{i=1}^K E_i\tau_{t,i}^a + e_t. \quad (5)$$

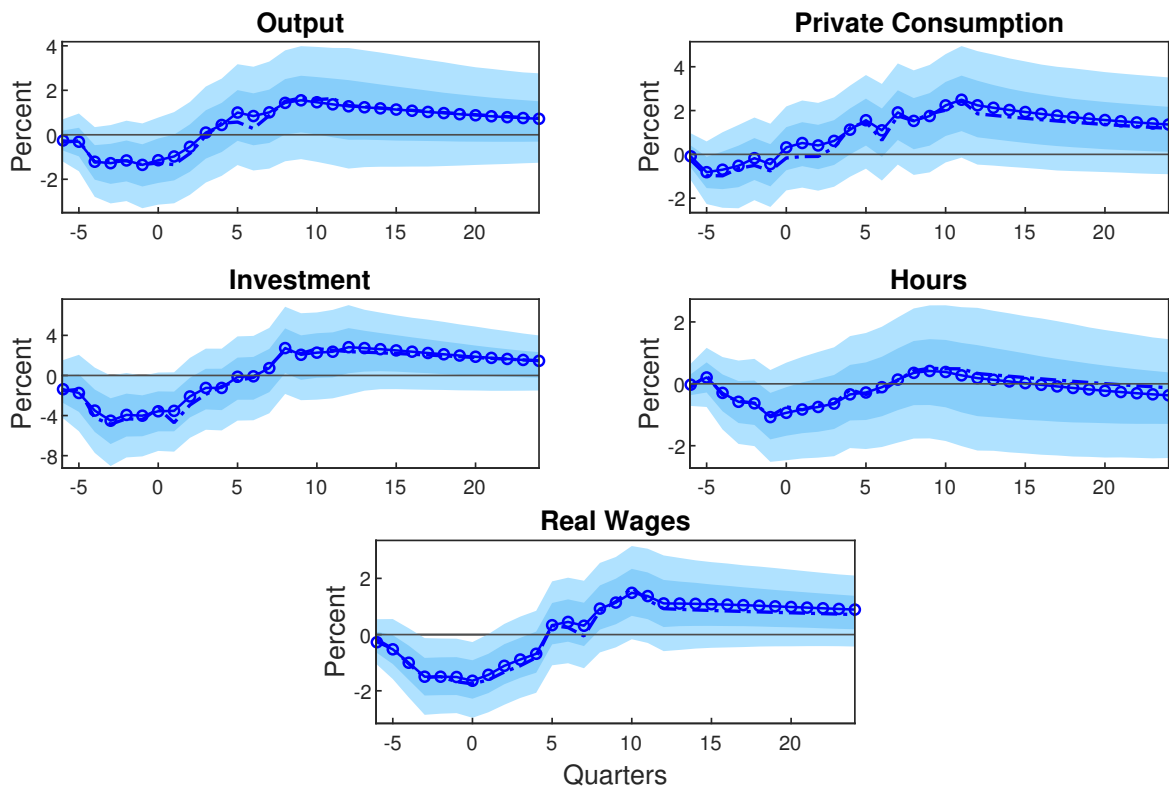
Figure 9 shows the responses of the macroeconomic variables based on this specification. It leads to effects that differ only marginally from those depicted in Figure 4a (dashed line). There is no statistically significant difference between the results.

(ii) Our analysis is based on the historical account of legislated tax changes by Hayo and Uhl (2014), which was extended by Christofzik and Elstner (2020) for the period 2010 to 2017. Hayo and Uhl (2014) study the macroeconomic effects of tax changes, using a different framework than ours. They specify a regression equation without considering anticipation effects. In this equation all exogenous tax shocks enter with the same coefficient, similar to Equation (4). To check the robustness of our results and to make our results more comparable, we run additional regressions.

First, we estimate Equation (4) with the same parameters as before, not distinguishing between anticipated and non-anticipated tax changes and using the same endogenous variables set as Hayo and Uhl (2014), i.e., log real per capita values of GDP, tax revenues and government spending, a short-term interest rate and the inflation rate.

Figure 10b shows the response of GDP following an exogenous tax cut corresponding to one percent of GDP, based on this specification. The peak output effect of 1.7 is very similar to the results by Hayo and Uhl (2014) who obtain a value of 1.6 when they estimate a VAR in levels.

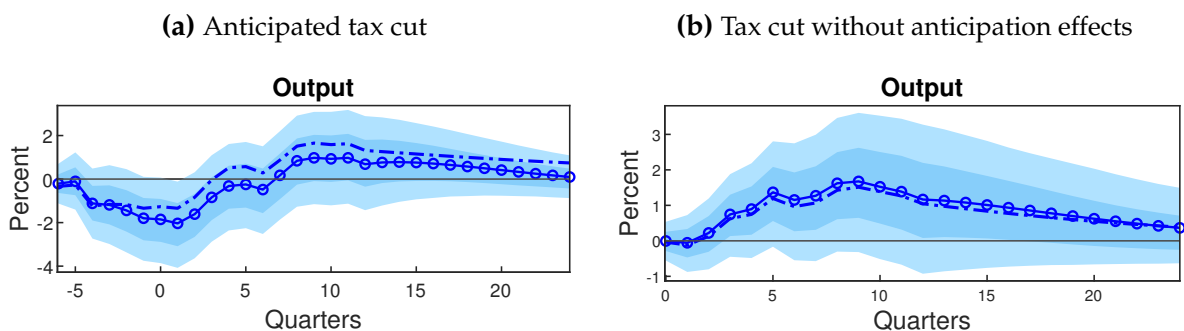
**Figure 9:** The impact of an anticipated 1 percent tax cut – Alternative specification



*Notes:* The figure shows the responses of macroeconomic variables to an exogenous anticipated tax cut announced 6 quarters before implementation corresponding to one percent of GDP. Anticipated tax changes are those exogenous tax changes for which the time between announcement and implementation exceeds 90 days. Shaded areas denote 68 and 95 percent bootstrapped confidence intervals based on a nonparametric bootstrap with 10,000 replications. Dashed lines are the point estimates of the baseline results in Figure 4a.

Second, we estimate our baseline Equation (3), using the set of endogenous variables of Hayo and Uhl (2014). Figure 10a shows the response of GDP following an anticipated tax cut announced six quarters before implementation. The result is qualitatively similar to our baseline results. Prior to the implementation, output decreases significantly and rises gradually afterwards.

**Figure 10:** The impact of a 1 percent tax cut on GDP - Different specifications and variables



*Notes:* The figure shows the responses of real GDP per capita to an exogenous tax cut corresponding to one percent of GDP. The right figure shows effects for all exogenous tax shocks without considering anticipation effects. The left figure shows effects stemming from an anticipated tax cut announced six quarters before implementation. Anticipated tax changes are those exogenous tax changes for which the time between announcement and implementation exceeds 90 days. Lines with circles indicate point estimates. Dashed lines are baseline point estimates of Figure 4a and 4b, respectively. Shaded areas denote 68 and 95 percent bootstrapped confidence intervals based on a nonparametric bootstrap with 10,000 replications.

## 4.5 Predictability of Exogenous Tax Shocks

Our identification strategy relies on the assumption that exogenous tax changes are uncorrelated with the contemporaneous error term in Equation (3). By nature, this assumption cannot be tested. Nonetheless, as in Mertens and Ravn (2012), Cloyne (2013), and Hayo and Uhl (2014), we test whether the exogenous tax shocks at announcement date can be predicted by past values of  $X_t$ . Failure to reject the null hypothesis that the tax changes cannot be predicted might lend additional credibility to the claim of exogeneity. For this purpose, we separately take into account all exogenous tax changes. Within these categories we distinguish between anticipated, unanticipated, and all tax changes.

First, we estimate a linear regression of the tax changes on four lags of log per capita values of GDP, investment, and consumption, hours worked, and log real wages per employee as well as a linear trend. The left column of Table 3 shows the p-values of an F-test that the coefficients of the lags of  $X_t$  are zero. Second, we estimate an ordered probit, where the dependent variable is defined as follows:

$$y_t^{i,j} = \begin{cases} -1 & \text{if } \tau_t^{i,j} < 0 \\ 0 & \text{if } \tau_t^{i,j} = 0 \\ 1 & \text{if } \tau_t^{i,j} > 0, \end{cases} \quad (6)$$

**Table 3:** Predictability of exogenous tax changes

	Linear	Ordered Probit
<i>Exogenous tax changes</i>		
All tax changes	0.875	0.918
Unanticipated tax changes	0.938	0.499
Anticipated tax changes	0.669	0.903
<i>Structural tax changes</i>		
All tax changes	0.911	0.966
Unanticipated tax changes	0.984	0.893
Anticipated tax changes	0.776	0.775
<i>Consolidation tax changes</i>		
All tax changes	0.771	0.689
Unanticipated tax changes	0.382	0.080
Anticipated tax changes	0.914	0.951

*Notes:* This table reports the outcomes of tests of the predictability of the exogenous tax measures dated by the announcement date. The tests are specified with the null hypothesis that four lags of log GDP per capita, log investment per capita, log consumption per capita, log hours worked per capita and log real wages are jointly equal to zero. For the linear model, the values denote the p-values of F-tests of  $H_0$ ; for the ordered probit model, the values denote p-values of likelihood ratio tests of  $H_0$ .

where  $\tau_t^{i,j}$  denotes a tax change of type  $i$  (anticipated, unanticipated, all) with motivation  $j$  (exogenous, structural, consolidation). The second column of Table 3 shows the p-values of a likelihood ratio test that the coefficients of the lags of  $X_t$  equal zero. Neither test provides evidence that the tax changes can be predicted by past values of  $X_t$ , thus supporting our claim that the tax changes are exogenous.

## 5 Conclusions

In this paper, we estimate the macroeconomic effects of tax changes in Germany. In our baseline estimation, the implied tax multiplier of an anticipated tax shock, i.e., the peak output of a tax cut corresponding to one percent of GDP relative to its trend, is 1.7. However, the post-implementation effect does not tell the whole story. We estimate significantly negative anticipation effects. We observe contractions in output, consumption, investment, and hours worked. The increase after implementation occurs gradually. Therefore, for output it takes two years after a tax cut is realized to

rise significantly above its trend. These results are qualitatively in line with those obtained for the US by Mertens and Ravn (2012).

The policy implications are twofold: First, when using conventional expansionary tax policy measures such as income tax cuts, policy makers need to take into account negative anticipation effects. In case of a long implementation lag, seemingly expansionary tax policy measures may actually worsen an economic slump. This should be taken into account, e.g., when considering tax policy measures that are intended to cushion the economic consequences of the COVID-19 pandemic. Analogously, announcing a tax cut during an economic upturn is not necessarily procyclical. Second, policy makers can exploit such anticipation effects and use implementation lags to boost the economy. For instance, a pre-announced tax increase has an immediate expansionary effect on consumption, investment and output. Considering anticipation effects, expansionary fiscal policy can—at least in principal—be budget neutral, for instance through an immediate decrease in consumption tax rates followed by a pre-announced tax increase.

This paper presents stylized facts in the sense that tax policy measures are characterized through their effect on the average tax rate. In practice, the effect of tax law changes depends on an income effect and a substitution effect, where the latter is determined by the change in the *marginal* tax rate. Future research could analyze differential effects of different kinds of taxes. It could also aim at characterizing tax law changes more completely. Of course, the draw-back is that long time series with detailed tax law changes needed for such an endeavor are not readily available. A second line of future research could aim at modelling the particular goods and labor markets implied by the estimated responses.



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## A Data Definitions and Sources

**Table A1:** Macroeconomic variables: description and sources

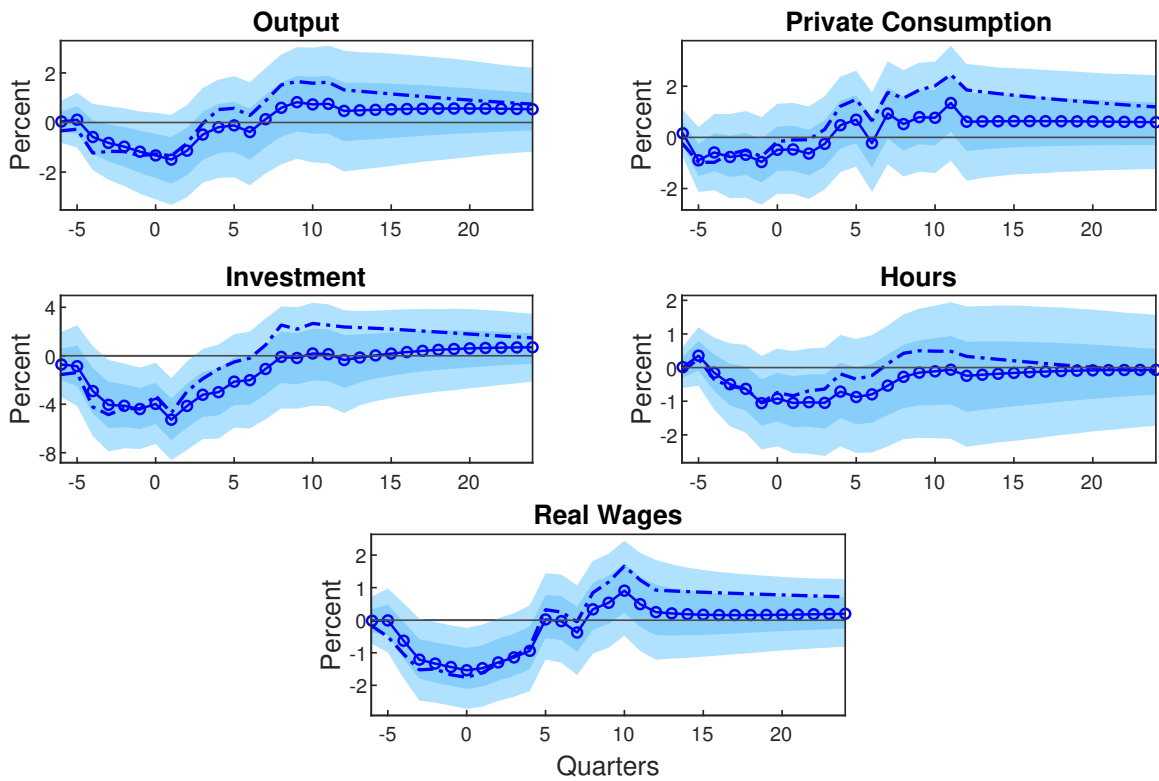
Variable	Description	Source
Gross domestic product per capita	Real gross domestic product (GDP) divided by total population; GDP data are chained volume (base year=2010); post-1991 data are extended backwards by using the growth rates of the pre-1991 data; quarterly; seasonally- and working day adjusted; period Q1 1970 to Q4 2017, Fachserie 18 Reihe 1.3 (Table 2.3.2) and Reihe S. 28 (Table 2.3.2)	DESTATIS (Federal Statistical Office)
Consumption per capita	Real private consumption divided by total population; consumption data are chained volume (base year=2010); post-1991 data are extended backwards by using the growth rates of the pre-1991 data; quarterly; seasonally- and working day adjusted; period Q1 1970 to Q4 2017, Fachserie 18 Reihe 1.3 (Table 2.3.2) and Reihe S. 28 (Table 2.3.2)	DESTATIS
Investment per capita	Gross fixed capital formation divided by total population; investment data are chained volume (base year=2010); post-1991 data are extended backwards by using the growth rates of the pre-1991 data; quarterly; seasonally- and working day adjusted; period Q1 1970 to Q4 2017, Fachserie 18 Reihe 1.3 (Table 2.3.2) and Reihe S. 28 (Table 2.3.2)	DESTATIS
Hours	Total hours worked divided by total population; post-1991 data are extended backwards by using the growth rates of the pre-1991 data; quarterly; seasonally- and working day adjusted; period Q1 1970 to Q4 1970, Fachserie 18 Reihe 1.3 (Table 2.1.8) and Reihe S. 28 (Table 2.1.7)	DESTATIS
Real wages	Total compensation divided by total employees; price adjusted by the implicit GDP deflator; post-1991 data are extended backwards by using the growth rates of the pre-1991 data; quarterly; seasonally- and working day adjusted; period Q1 1970 to Q4 1970, Fachserie 18 Reihe 1.3 (Tables 2.2.3 and 2.2.6) and Reihe S. 28 (Tables 2.2.3 and 2.2.6)	DESTATIS
Population	Population; thousand persons; quarterly; seasonally adjusted; post-1991 data (referring to reunited Germany) are extended backwards by using the growth rates of the pre-1991 data that refer to Western Germany only; Fachserie 18 Reihe 1.3 (Table 2.1.7) and Reihe S. 28 (Table 2.1.6)	DESTATIS

*Notes:* All series were downloaded from the cited sources in February 2019 at the most recent vintage available at that time.

## B Alternative Sizes of Tax Shocks

In our baseline estimations, we use the revenue estimates made at the implementation stage of the tax change. These estimations may differ from those conducted at the draft or announcement stage. Therefore, we check whether changing this assumption affects our results. In an alternative specification, we evaluate the unanticipated shocks with the revenue estimates conducted at the implementation date, but use estimates published at the announcement stage in case of anticipated tax changes. Figure A1 shows the impulse responses and compares them to our baseline results. The impulse response functions lie within the confidence intervals of the baseline estimation.

**Figure A1:** The impact of an anticipated 1 percent tax cut – Alternative tax change size



*Notes:* The figure shows the responses of macroeconomic variables to an exogenous anticipated tax cut announced 6 quarters before implementation corresponding to one percent of GDP. Anticipated tax changes are those exogenous tax changes for which the time between announcement and implementation exceeds 90 days. Shaded areas denote 68 and 95 percent bootstrapped confidence intervals based on a nonparametric bootstrap with 10,000 replications. Dashed lines are the point estimates of the baseline results in Figure 4a.