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

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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. An anticipated cut in taxes has a positive effect on output with a peak multiplier of 1.7, not observed until nine quarters after implementation. This positive effect is accompanied by significant negative anticipation effects on output, consumption, investment, hours worked and wages. Our results suggest that policymakers should take anticipation effects into account when implementing fiscal policy measures.

INTRODUCTION

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; Leeper *et al*. 2013), 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, for example, 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 (Feldstein 2002; Hall 2011; Correia *et al*. 2013). These policy measures rely on forward-looking agents and impact the prices that matter for intertemporal decisions. Therefore fiscal policy can, in principle, use anticipation effects to its advantage. On the other hand, if anticipation effects are not taken into account by policymakers, negative anticipation effects of fiscal policies originally intended to be expansionary might even prolong an economic downturn (House and Shapiro 2006; Mertens and Ravn 2012).

Despite the theoretical relevance of fiscal foresight, there is limited empirical evidence on the macroeconomic anticipation effects of tax policy changes. In this paper, we allow for anticipation effects and estimate the dynamic macroeconomic effects of tax shocks in Germany. We use a sample covering the years 1970 to 2017 and take into account the dates of announcements and realizations of changes to tax laws in a VAR model as in Mertens and Ravn (2012). Fiscal policy is generally endogenous to the business cycle. The reason is that policy measures are often implemented with the aim of stabilizing the economy. In the spirit of Romer and Romer (2010), we employ a narrative approach to

identify exogenous tax shocks. This approach uses explicit information on the motivation of tax law changes and thus assumes that exogenous tax shocks are directly observable, see the first subsection of Section II. We use a historical account of legislated tax changes that contains draft, announcement and implementation dates as well as the motivation and magnitude of all relevant tax changes in Germany in the sample period. It was constructed by Hayo and Uhl (2014) and extended by Christofzik and Elstner (2021) for the period from 2010 to 2017, and allows us to construct quarterly time series.

In the empirical fiscal policy literature, so far, the focus has been mainly on implications of anticipated 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 behaviour (D'Acunto *et al*. 2017, 2018; Crossley *et al*. 2014) find that the announcement of consumption tax increases leads to sizeable increases in goods purchases.

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 USA after 2001. Mertens and Ravn (2012) use the Romer and Romer (2010) dataset of narratively identified tax changes in the USA and construct a vector autoregression (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, demonstrating 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 setup that allows for anticipation effects. In 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 be partially offset by anticipation of tax shifts in the opposite direction.

The main contribution of this paper is to demonstrate the relevance of fiscal foresight for the case of Germany, verifying that the main results obtained by Mertens and Ravn (2012) for the USA also hold for this country. To our knowledge, this is the first paper to do so. Thereby we add to the small literature that provides direct evidence on the macroeconomic anticipation effects of tax changes. Germany is an interesting case as it is the largest European economy, it is part of a currency area—and thus does not have its own monetary policy—and it has close trade ties with many other countries, which can create spillovers. Compared to the USA, the strong manufacturing sector might cause different transmission effects. Additionally, we show that implementation lags of tax changes differ between the two countries.

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.

Taking into account anticipation effects can affect the estimate of the tax multiplier. Consequently, we also contribute to the literature on the magnitude of the tax multiplier in Germany.[1] There is no consensus on its size in the literature. 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 USA. Other studies apply to Germany a structural VAR (SVAR) approach in the spirit of Blanchard and Perotti (2002), and generally find rather small tax multipliers, in some cases even unexpected signs (Perotti 2005; Tenhofen *et al.* 2010; Hollmayr and Kuckuck 2018). Gechert *et al.* (2017) show that estimates between these two approaches are not necessarily comparable, while Gechert *et al.* (2020) provide additional evidence for the impact of social security shocks.

We find that a cut in taxes has a positive effect on output. For an anticipated tax shock, we estimate a peak tax multiplier of 1.7 nine quarters after implementation. However, this positive effect is accompanied by significant negative anticipation effects on output, consumption, investment, hours worked and wages. A substantial positive impact is not observed until several quarters after implementation. In line with Mertens and Ravn (2012), the largest negative anticipation effects are on investment.

The paper is structured as follows. Section I describes the dataset and our measure for legislated tax changes in Germany. We categorize these changes as unanticipated and anticipated based on their timing. Then we present the empirical specification. Section II reports our main results. We provide a number of robustness checks in Section III. Section IV concludes.

I. Empirical Strategy

Measuring tax policy changes

To study the effects of tax reforms on output prior to their implementation, we first need to identify discretionary tax changes that are unrelated to other factors influencing output in the short term. This serves to disentangle the effects of tax changes from underlying factors. Second, 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).

Romer and Romer (2009) identify the motivation for all major postwar tax law changes in the USA. Using these data, Romer and Romer (2010) estimate the impact of exogenous tax changes on real output in the USA, and find that tax increases 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 dataset of deficit-driven fiscal policy changes and estimate their impact on output.

We use information from the legislative process to identify exogenous tax changes and important dates in this process. We rely on the historical account of legislated tax changes by Uhl (2013) and Hayo and Uhl (2014), which was extended by Christofzik and Elstner (2021) for the period from 2010 to 2017. The series of tax changes is based on official government documents, in particular annual budget reports of the German Federal Ministry of Finance and draft bills.[2] The dataset includes estimations by the government of the impact of these tax changes on tax revenues. These estimations may vary between the draft, announcement and implementation dates. In our baseline specification, we use the estimations at implementation date.

Based on the underlying motive of the tax legislation, the tax changes are classified as either endogenous or exogenous.[3] In our analysis, we use solely tax changes identified as exogenous. These are either tax changes that are implemented in order to consolidate the budget, or measures that aim to increase 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 dataset contains 1353 exogenous tax measures.

The dataset based on the narrative approach also provides detailed information about the timing of the legislative process. Three key dates in the legislative process can be discerned: the date at which the draft bill is introduced to be debated in the federal parliament, the announcement date, and the implementation date of the law.

The different dates are each associated with an expected revenue impact of the reform estimated by the government. This allows us to construct quarterly time series of tax changes at each of the three stages; see also Christofzik and Elstner (2021). If a tax change is temporary, then the measure is offset in the dataset by constructing an equal size change with the opposite sign at the expiration date. 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 a tax change at time t is defined as

$$\tau_t = \sum_{i=1}^{N_t} \tau_{i,t}, \tag{1}$$

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.[4] By aggregating the 1353 exogenous tax measures of our dataset, we end up with 84 out of 192 quarters (44%) 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 USA, 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.[5] 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 behaviour 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 the anticipation horizon between announcement and implementation, 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 1295 are anticipated. Note that the impact of policy changes in all cases refers to the estimated impact at implementation. For instance, the difference between the mean impact of unanticipated tax changes based on the draft date (−0.009) and unanticipated tax changes based on the announcement date (−0.002) is entirely due to the different samples. In some cases, a tax change is announced later than the implementation date. We categorize these retroactive measures as unanticipated.[6]

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

TABLE 1
DESCRIPTIVE STATISTICS OF EXOGENOUS TAX CHANGES

	All	Categorization based on draft date		Categorization based on announcement date	
		Unanticipated	Anticipated	Unanticipated	Anticipated
Tax policy measures					
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
Non-zero quarters					
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.

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 for this 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 in non-zero quarters (0.21) 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 anticipation horizons 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 defined 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 (1295 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 date, and two quarters based on the announcement date. Thus the anticipation horizon is considerably smaller than in the USA—Mertens and Ravn (2012) report a median anticipation 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, that is, 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 we limit the maximum anticipation horizon in the estimation equation (3) below.

TABLE 2
ANTICIPATION HORIZONS OF ANTICIPATED EXOGENOUS TAX CHANGES IN GERMANY IN QUARTERS

	Draft	Announcement
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 that we classify as anticipated—that is, 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.

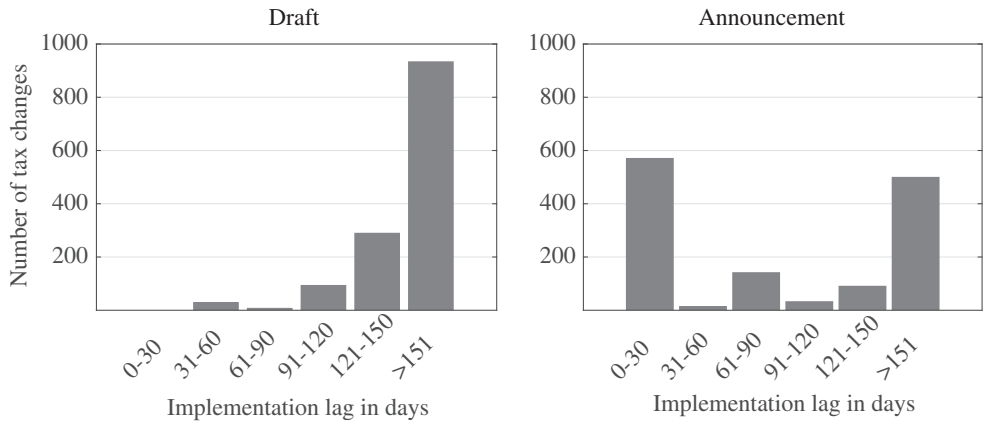

FIGURE 1. Distribution of implementation lags.*Notes*: The figure shows the distribution of implementation lags for all exogenous tax changes in Germany, expressed in days. In the left-hand panel, the implementation lag is the time between draft and implementation. In the right-hand panel, the implementation lag is the time between announcement and implementation. Own calculations.

Figure 1 illustrates the distribution of the implementation lags of the identified exogenous tax changes in days. The left-hand panel shows the distribution of implementation lags measured by the lag between draft date and implementation date. The majority of tax changes take more than five months to come into effect. In the right-hand 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 exogenous tax changes are implemented within 30 days after the announcement date. In the USA, the largest share of tax changes is associated with an implementation lag exceeding 151 days.

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

$$\tau^{a}_{t,i} = \sum_{j=0}^{M-i} s^{a,i+j}_{t-j}, \tag{2}$$

where $s^{a,i+j}_{t-j}$ denotes anticipated tax measures s announced at date $t-j$ with an anticipation horizon of $i+j$, and M is the largest implementation lag in the data.[7] Thus $\tau^{a}_{t,i}$ denotes the total tax liability change expected at date t to occur in i quarters. Figure 2 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 first quarter of 2001, in which numerous measures originating from the *Tax Reduction 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 Consolidation Programme*, driven in particular by the levy of a solidarity surcharge on personal income and

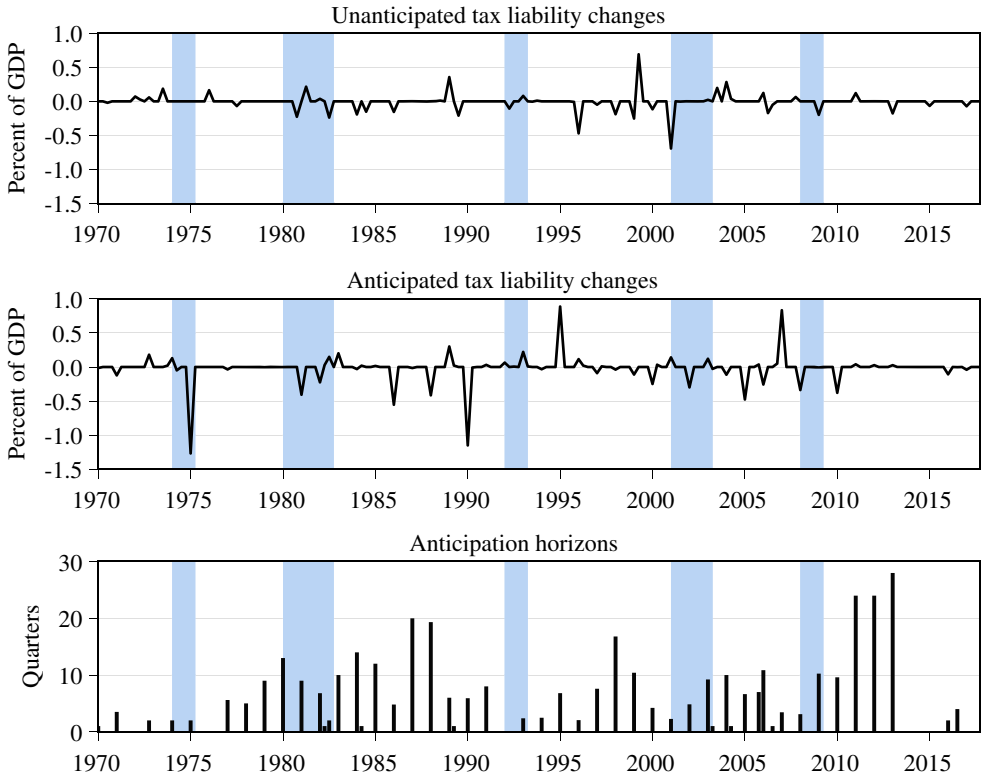

FIGURE 2. Unanticipated and anticipated tax changes based on the announcement date.*Notes*: The first two panels show unanticipated and anticipated legislative tax changes in Germany between 1970 and 2017. Anticipated tax changes are those exogenous tax changes for which the time between announcement 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.

corporate income tax in 1995, and from the *Budget Accompanying Act 2006*, which comprised an increase of the standard value-added tax rate by three percentage points.

Empirical specification

We incorporate the information on the timing of tax reforms to study anticipation effects empirically, following the approach taken by Mertens and Ravn (2012).

We base our analysis on the regression equation

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

where X_t is a vector that contains the logarithms of real per capita GDP, real per capita investment, real per capita private consumption, hours worked per capita, and real wages per employee.[8] 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.[9] 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 1. v_t contains this dummy in addition to a constant.[10]

Unanticipated tax changes are denoted by τ_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 in the previous subsection. For illustrative purposes, we report and describe the narrative data for the period Q1 2006 to Q1 2007 in Online Appendix C.1. Allowing for differential coefficients for the lags of anticipated and unanticipated tax changes in the VAR model makes our results directly comparable to those of Mertens and Ravn (2012).

For our main specification, we set the order of the lag polynomials $D(L)$ and $F(L)$ to 12, and the number of lags of the endogenous variables, $C(L)$, to 1. As shown in Online Appendix C.8, Schwarz's Bayesian information criterion suggests that one lag is the most parsimonious model. We choose a maximum anticipation horizon of K=6. In Section III, we show that the results are robust to varying these parameters.[11]

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 setup. 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 policymakers can use only the former. It should also be relevant for individuals when anticipating tax changes.

II. Empirical Results

Baseline results

Figure 3 reports our benchmark results. It shows the responses to a 1% decrease in tax revenue relative to GDP, based on the model specified in equation (3). The left-hand

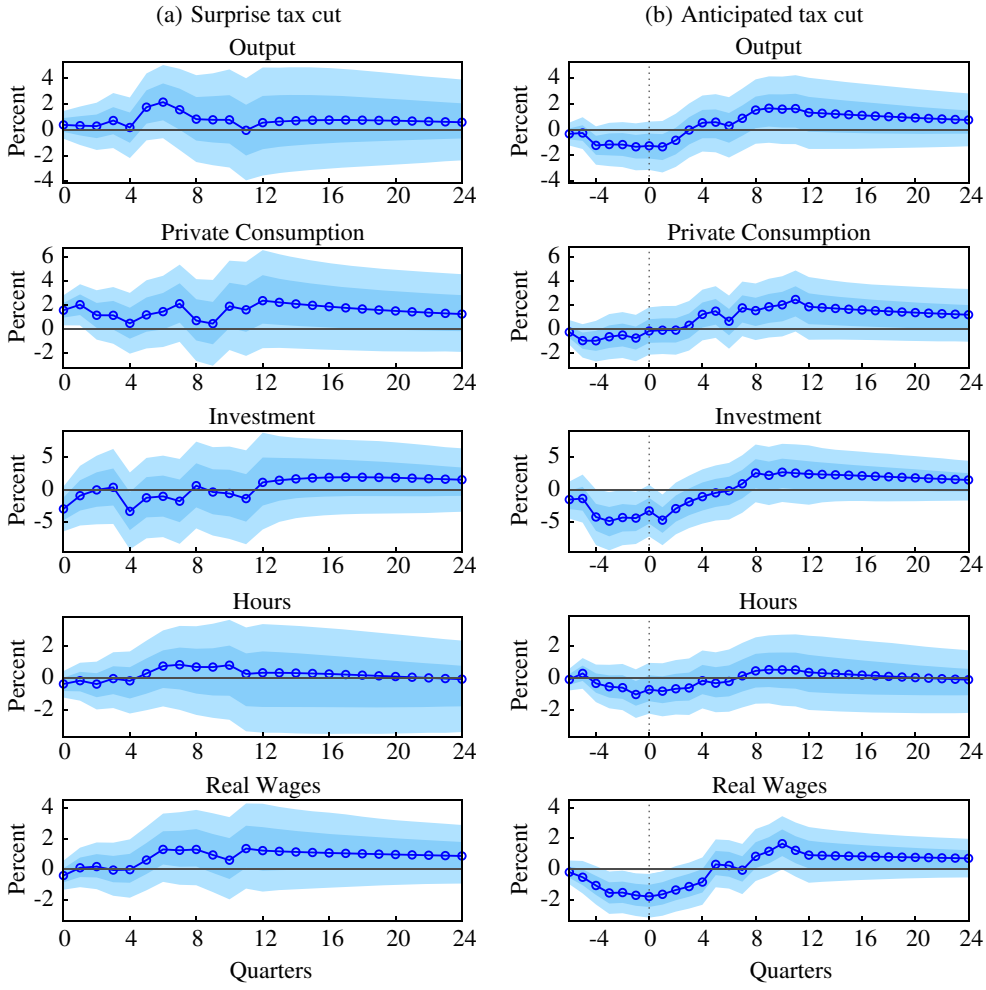

FIGURE 3. The impact of a 1% tax cut. *Notes*: The figure shows the responses of macroeconomic variables to an exogenous tax cut corresponding to 1% of GDP. The left-hand panels show effects stemming from a non-anticipated tax cut. Non-anticipated tax changes are those exogenous tax changes for which the time between announcement and implementation is less than 90 days. The right-hand panels 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. Lines with circles indicate point estimates. Shaded areas denote 68% and 95% bootstrapped confidence intervals based on a non-parametric bootstrap with 10,000 replications.

panels show responses for surprise tax cuts, and the right-hand panels show responses for anticipated tax cuts. 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, for example, by Mertens and Ravn (2012) with 10,000 replications. The left-hand panel in Figure 3 reports that surprise tax cuts induce a gradual increase in output. Peak effects occur six quarters after the tax cut and imply a peak multiplier of 2.2.[12] Consumption increases immediately at the implementation, while there are no clear reactions of investments and hours. Wages increase slightly starting five quarters after the implementation.

The right-hand panel of Figure 3 shows that anticipated tax cuts are associated with a significant decline in output before the 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. Mertens and Ravn (2011) use a dynamic stochastic general equilibrium model to study the channels through which anticipated tax cuts affect output. The sluggish adjustments can be explained with adjustment costs and habit formation. Our results show that output does not rise significantly above its trend until two years after the implementation. This also resembles findings by Mertens and Ravn (2012) for the USA. The implied peak tax multiplier of an anticipated tax cut for Germany based on the point estimate is 1.7, observed nine quarters after implementation

The other endogenous variables display the same pattern as output; they decrease prior to the implementation of the tax cut, and increase afterwards. However, hours worked and real wages do not rise significantly above their trend after the implementation. We observe the largest negative anticipation effects for investment, which decreases by about 5% for several quarters. This result is remarkably similar to that obtained by Mertens and Ravn (2012) for the USA. 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.

To gain more intuition about the results, consider an anticipated cut in labour income and corporate income tax rates. In our sample of 1353 exogenous tax changes, 1195 relate to corporate or personal income taxes. An expected increase in after-tax wages in the future leads to a drop in labour supply pre-implementation due to both a wealth effect and an intertemporal substitution effect. This drop in hours reduces returns on capital, which is one reason for the drop in investments. The expectation of lower taxes in the future also motivates firms to delay the purchase of capital goods. The theoretically expected pre-implementation response of real wages is ambiguous. On its own, the shift of the labour supply curve to the left exerts upward pressure on wages. On the other hand, decreased investments pre-implementation lower the marginal product of labour, leading to a shift of the labour demand curve to the left, which exerts downward pressure on wages. Our estimated negative anticipation effect on wages suggests that the latter channel dominates in Germany. Output decreases in line with the drops in wages, investments and hours prior to the implementation. In contrast to our finding for Germany, for the USA, Mertens and Ravn (2012) observe, if anything, an *increase* in wages pre-implementation.

We find significant negative anticipation effects on private consumption. The theoretical expectation of the pre-implementation response of consumption to income tax cuts is ambiguous as well. On the one hand, the expected increase in permanent income should lead to an increase in consumption before implementation. On the other hand, liquidity-constrained consumers reduce consumption if incomes drop due to lower returns to capital and lower wages. Moreover, if in-period utility is non-separable in consumption and leisure, and the two are substitutes, then a temporary drop in hours of work is accompanied by a drop in consumption. This assumption matches average lifecycle paths of consumption and leisure in microdata (Low 2005).

We observe negative anticipation effects for hours worked, again similar to Mertens and Ravn (2012). In contrast to that paper, we find no significant positive impact on hours worked post-implementation. This result can be explained with a Marshallian

labour supply elasticity close to zero, which is in line with micro evidence (see, for example, Keane 2011). While a permanent income tax reduction 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.

If instead of income taxes, consumption taxes (118 out of 1353 exogenous tax changes in our sample) are cut, then the theoretical expectation is that substitution effects lead to a decrease in private consumption prior to the implementation. The direct effect on labour supply, both pre- and post-implementation, is ambiguous. Due to the expected decrease in net consumer prices, one hour of work can buy more goods. This leads to a positive intratemporal substitution effect and a negative wealth effect on labour supply. Additionally, the decrease in pre-implementation demand for consumer goods is expected to lead to fewer hours worked pre-implementation.

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 and 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 4.

Figure 4(a) shows a decrease in tax revenues in percent of GDP after the implementation of a surprise tax cut. After seven quarters, the tax rate returns to its trend. Figure 4(b) shows that prior to an anticipated tax cut, the tax rate decreases. 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 a case, the tax base decreases more strongly 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

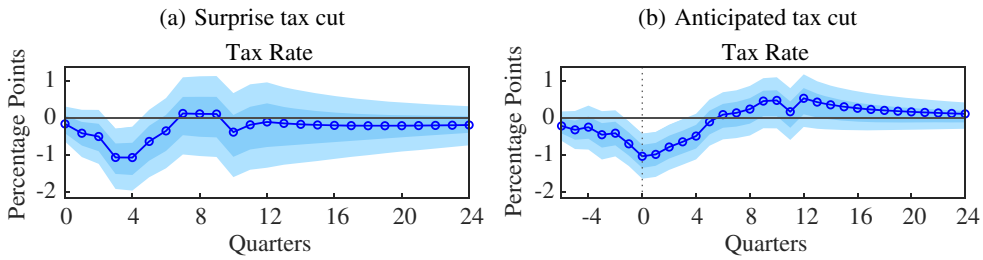

FIGURE 4. The impact of a 1% tax cut on tax revenue.*Notes*: The figure shows the responses of the effective tax rate (tax revenue in relation to nominal GDP) in percentage points to an exogenous tax cut corresponding to 1% of GDP. The left-hand panel shows the effect stemming from a non-anticipated tax cut. Non-anticipated tax changes are those exogenous tax changes for which the time between announcement and implementation is less than 90 days. The right-hand panel depicts the effect stemming from an anticipated tax cut announced six quarters before implementation. Anticipated tax changes are those exogenous tax changes for which the time between draft and implementation exceeds 90 days. Lines with circles indicate point estimates. Shaded areas denote 68% and 95% bootstrapped confidence intervals based on a non-parametric bootstrap with 10,000 replications.

leads to a more than proportional decrease in tax revenues. The tax rate remains below its trend for only one year.

III. Robustness

Robustness to the inclusion of fiscal policy variables

Figure 5 reports the responses based on a VAR that includes real per capita government spending and tax revenues as additional endogenous variables. For comparison, the dashed lines show the responses based on the baseline set of variables. For output, private consumption and investment, the post-implementation responses are slightly stronger but within the confidence bands of the baseline results. The peak multiplier of surprise tax changes is 2.4, and that of anticipated tax changes is 1.8.[13]

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 the implementation. Compared to the baseline specification presented in Figure 3(b), the pre- and post-implementation responses to an anticipated tax cut are generally similar. The observed expansion in output, consumption and investment is less pronounced. However, similar to Figure 3(b), 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.

Further robustness tests

Impulse response functions based on additional alternative specifications and sample restrictions are reported in the Online Appendix. In Online Appendix C.2 we do not allow for differential contemporaneous effects of anticipated and unanticipated tax changes, while still accounting for tax leads of the anticipated tax changes. The responses differ only marginally from those based on the main specification. In Online Appendix C.3, 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.[14] The impulse response functions lie within the confidence intervals of the baseline estimation. Demirel (2021), Fotiou *et al*. (2020) and Sims and Wolff (2018) find that the effects of tax changes depend on the state of the economy. Therefore, in Online Appendix C.4, we restrict the sample to the period 1970–2011, when the zero lower bound was not binding. Results are virtually unchanged. Moreover, the result of negative anticipation effects is robust to controlling for periods with high inflation rates (Online Appendix C.5). Online Appendix C.6 reports the responses based on different anticipation horizons from $K = 2$ to $K = 10$. As in Mertens and Ravn (2012), longer anticipation horizons lead to more pronounced negative anticipation effects. The impact of the anticipation horizon on estimated post-implementation effects is relatively small. Online Appendix C.7 shows responses based on different lag lengths of the endogenous variables. The responses to anticipated tax cuts all

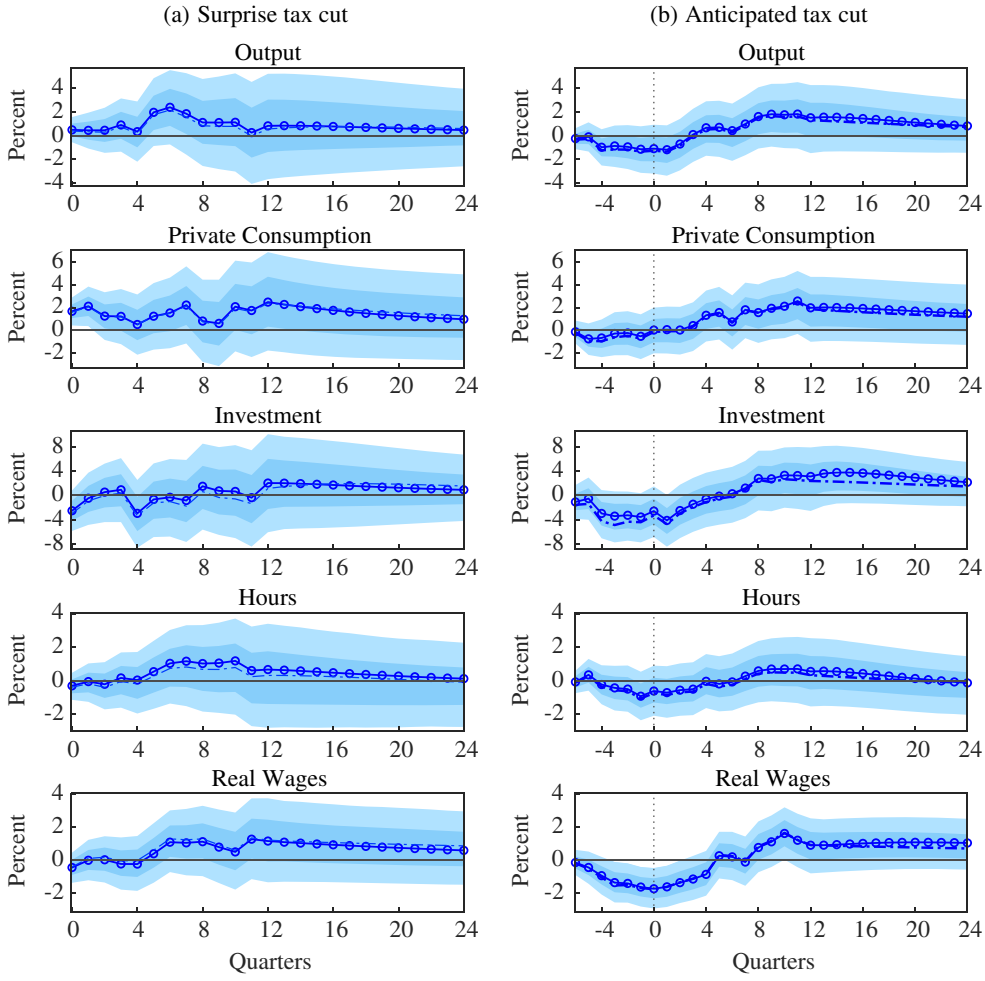

FIGURE 5. The impact of a 1% tax cut—specification with fiscal policy variables. *Notes*: The figure shows the responses of macroeconomic variables to an exogenous tax cut corresponding to 1% of GDP, based on the baseline specification where the vector of endogenous variables additionally contains the log real per capita values of government spending and tax revenues. The left-hand panels show effects stemming from a non-anticipated tax cut. Non-anticipated tax changes are those exogenous tax changes for which the time between announcement and implementation is less than 90 days. The right-hand panels 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. Lines with circles indicate point estimates. Shaded areas denote 68% and 95% bootstrapped confidence intervals based on a non-parametric bootstrap with 10,000 replications. Dashed lines are the point estimates of the baseline results in Figure 3.

lie within the 68% confidence interval of the baseline specification with one lag. We also show statistics for different lag selection criteria (Online Appendix C.8).

Online Appendix C.9 reports responses based on a specification that allows for different effects of tax cuts and tax increases. Anticipated tax cuts seem to drive our main results. For tax increases, we find a small but insignificant pre-implementation expansion in output, and no statistically significant post-implementation contraction. In Online Appendix C.10, we assess the relevance of the length of the implementation lag by

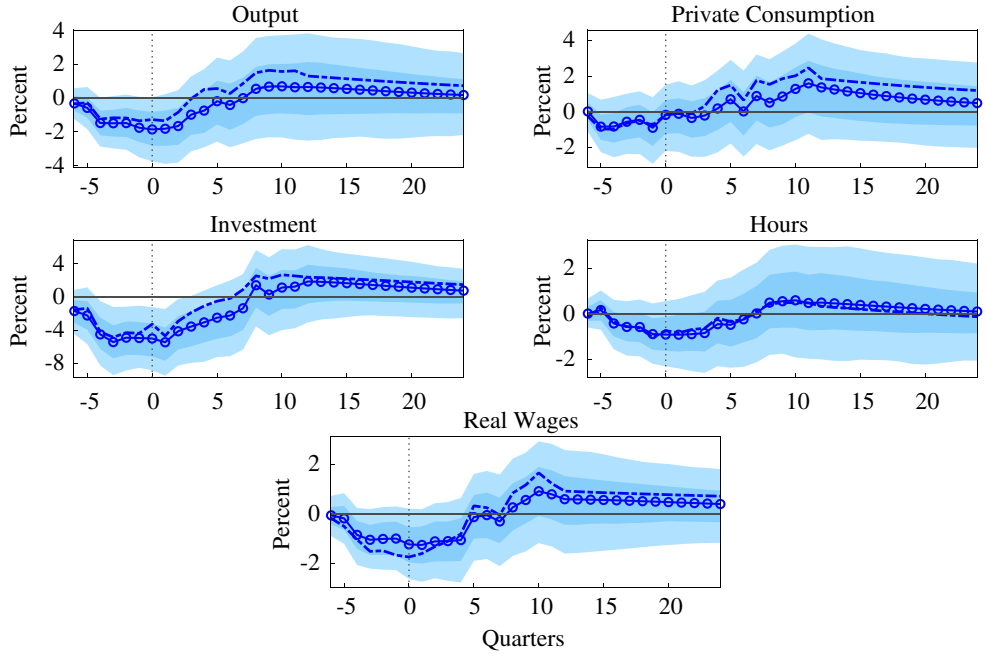

FIGURE 6. The impact of an anticipated 1% tax cut—anticipation based on the draft date.*Notes*: The figure shows the responses of macroeconomic variables to an anticipated exogenous tax cut announced six quarters before implementation corresponding to 1% of GDP. Anticipated tax changes are those exogenous tax changes for which the time between draft and implementation exceeds 90 days. Shaded areas denote 68% and 95% bootstrapped confidence intervals based on a non-parametric bootstrap with 10,000 replications. Dashed lines are the point estimates of the baseline results in Figure 3(b).

estimating the baseline specification with modified anticipated tax change series. Here we include only anticipated tax measures that have an implementation lag between 90 and 1838 days (95% quantile) or between 90 and 1403 days (90% quantile). The motivation for this robustness test is that tax changes announced far in the past might have only a small impact on agents' decision-making. The impulse responses to an anticipated tax cut using these series are similar to our baseline result. Finally, in Online Appendix C.11, following 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 the endogenous variables. We cannot reject the null hypothesis that the tax changes cannot be predicted, which lends additional credibility to the claim of exogeneity.

IV. CONCLUSIONS

We have estimated the macroeconomic effects of tax changes in Germany. In our baseline estimation, the implied tax multiplier of an unanticipated tax shock—that is, the peak response of output to a tax cut corresponding to 1% of GDP relative to its trend—is 2.2. For an anticipated tax shock, the peak multiplier is 1.7. Table 3 compares our results for the peak tax multiplier with other studies for Germany. The maximum effect on GDP is in the same range as the estimates by Hayo and Uhl (2014) and larger than in the studies that follow the approach by Blanchard and Perotti (2002).

TABLE 3
OVERVIEW AND COMPARISON WITH OTHER TAX MULTIPLIER STUDIES FOR GERMANY

	Peak multiplier	Approach	Sample	Notes
Our results				
Baseline	2.2	Narrative	1970–2017	Unanticipated
	1.7	Narrative	1970–2017	Anticipated
	1.5	Narrative	1970–2017	All[a]
Fiscal policy variables[b]	2.4	Narrative	1970–2017	Unanticipated
	1.8	Narrative	1970–2017	Anticipated
	1.8	Narrative	1970–2017	All[a]
Other estimations for Germany				
Hollmayr and Kuckuck (2018)	0.6	Restrictions	1993–2017	
Gechert *et al.* (2017)	0.5	Narrative	1974–2013	
	≈ 0.6	Restrictions	1974–2013	
Hayo and Uhl (2014)	2.4	Narrative	1974–2010	VAR in growth rates
	1.6	Narrative	1974–2010	VAR in levels
Tenhofen *et al.* (2010)[c]	0.4	Restrictions	1974–2008	
Perotti (2005)[c]	≈ 0	Restrictions	1960–1989[d]	

Notes
[a]Specification that does not take into account anticipation effects (see Appendix B).
[b]See the first subsection of Section III.
[c]Estimates refer to the effect of changes in net revenues.
[d]Two subsamples: 1960–74 and 1975–89; multiplier is negative in some specifications.
The table summarizes peak tax multipliers for Germany from studies that use either a narrative identification approach (*Narrative*) or an approach in the spirit of Blanchard and Perotti (2002) (*Restrictions*). Note that estimates between these two approaches are not necessarily comparable; see Gechert *et al.* (2017).

However, the post-implementation effect does not tell the whole story. We estimate negative anticipation effects, observing significant contractions in output, private consumption, hours of work and real wages prior to implementation. The increase after implementation occurs gradually. For output, it takes two years to rise significantly above its trend after a tax cut is realized.

Our findings are broadly in line with those for the USA by Mertens and Ravn (2012), who find important negative anticipation effects of tax cuts on output, investment and hours of work. In contrast to that paper, we observe a pre-implementation decrease in real wages. Our results provide additional evidence for the importance of anticipation effects, and can be used for model evaluations. While anticipation effects are in line with forward-looking consumers, future research—building, for example, on Mertens and Ravn (2011)—should explicitly model expectation formation and the particular goods and labour markets implied by our estimated responses. The precise policy implications depend on the results of such modelling exercises. Nonetheless, a note of caution regarding fiscal policy with substantial implementation lags is warranted. When using conventional expansionary tax policy measures such as income tax cuts, policymakers need to take into account negative anticipation effects. In case of a long implementation lag, fiscal policies originally intended to be expansionary may actually make an economic slump worse. Analogously, announcing a tax cut during an economic upturn is not necessarily procyclical.

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 income and substitution effects, where the latter are determined by the change in the *marginal* tax rate. Future research could analyse differential effects of different kinds of taxes. It could also aim to characterize tax law changes more completely. Of course, the drawback is that long time series with detailed tax law changes needed for such an endeavour are not readily available.

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NOTES

1. Recent estimates of the tax multiplier for the USA include those by Demirel (2021) and Fotiou *et al*. (2020). Ramey (2019) provides an overview of the recent literature on the fiscal multiplier.
2. A similar dataset that includes Germany in the sample was constructed by Devries *et al*. (2011) and extended by Alesina *et al*. (2020). That dataset, however, presents data at an annual frequency and focuses on the implementation date only. We construct a quarterly time series based on the dataset by Hayo and Uhl (2014) and Christofzik and Elstner (2021), and consider the announcement of tax changes.
3. Explanations and motives for tax changes are drawn primarily from the budget report or government statements (Hayo and Uhl 2014).
4. 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 implementation date to the surprise tax changes and the revenue estimate at the announcement date to the anticipated tax changes; see Online Appendix C.3. This specification does not change our conclusions from the main section.
5. In a robustness check, we estimate the effects based on anticipation between draft and implementation; see the second subsection of Section III.
6. 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 dataset. 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.
7. 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).
8. Mertens and Ravn (2011) show that this equation can be derived as an approximation of the observables in a dynamic stochastic general equilibrium model that incorporates stochastic shocks to tax rates.
9. Using a quadratic trend changes the results only marginally.
10. We use seasonally adjusted data. Additionally including quarterly dummies has a negligible impact on the impulse response functions, which are available from the authors on request.
11. We estimate equation (3) by OLS and construct impulse response functions from these estimates. Error bands are constructed using a residual-based bootstrap procedure with 10,000 replications.
12. Table C4 in Online Appendix C.12 reports point estimates and standard errors for the multipliers in all quarters displayed in Figure 3. In that table, we also show the multipliers based on a VAR that ignores anticipation effects of tax changes and does not distinguish between anticipated and surprise tax changes as is done in most of the literature on fiscal multipliers. In Figure A1 in Appendix B below, we show responses to a tax cut based on that specification. It yields a peak multiplier of 1.5, which is close to the 1.6 peak multiplier estimated in the specification in levels in Hayo and Uhl (2014); see Table 3 in Section IV. Online Appendix C.13 reports the difference between post-reform responses to anticipated and surprise tax cuts based on our main specification.
13. Table C4 in Online Appendix C.12 additionally reports the point estimates and standard errors for the multipliers shown in Figure 5, as well as multipliers based on a specification including these variables, but not accounting for anticipation effects or distinguishing between anticipated and unanticipated tax shocks. The peak multiplier based on that specification is 1.8, which, again, is close to the 1.6 peak multiplier estimated in the specification in levels in Hayo and Uhl (2014).
14. In that specification, as in the baseline specification, we use only one set of estimates for the revenue impact of tax changes. Using the same dataset, future research could use the different estimated impacts at the draft, announcement and implementation stages. For instance, the difference between estimated impacts at announcement and implementation stages can be used to construct an additional set of surprise tax shocks.

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SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article:

C.1 Example for Narrative Data
C.2 Alternative Regression Specification
C.3 Alternative Sizes of Tax Shocks
C.4 Excluding Periods with Binding Zero Lower Bound
C.5 Controlling for High Inflation Periods
C.6 Robustness to the Length of the Anticipation Horizon
C.7 Robustness to Alternative Lag Lengths
C.8 VAR Lag Selection
C.9 Asymmetric Responses
C.10 No Anticipation to Tax Changes with Long Implementation Lag Lengths
C.11 Predictability of Exogenous Tax Shocks
C.12 Tax Multipliers
C.13 Difference Between Anticipated and Surprise Shocks

APPENDIX: DATA DEFINITIONS AND SOURCES

TABLE A1
MACROECONOMIC VARIABLES: DESCRIPTION AND SOURCES

Variable	Description
Gross domestic product per capita	Real gross domestic product (GDP) divided by total population. GPD 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).
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).
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).
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).
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).
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 West Germany only. Fachserie 18 Reihe 1.3 (Table 2.1.7) and Reihe S. 28 (Table 2.1.6).

Notes
All series were downloaded from Destatis (Federal Statistical Office) in February 2019 at the most recent vintage available at that time.

APPENDIX: NEGLECTING ANTICIPATION EFFECTS

Figure A1 shows impulse responses to a cut of all exogenous taxes based on a specification that does not distinguish between anticipated and unanticipated tax changes, and does not allow for anticipation effects prior to implementation, as is commonly done in the literature (see, for example, Cloyne 2013; Hayo and Uhl 2014).

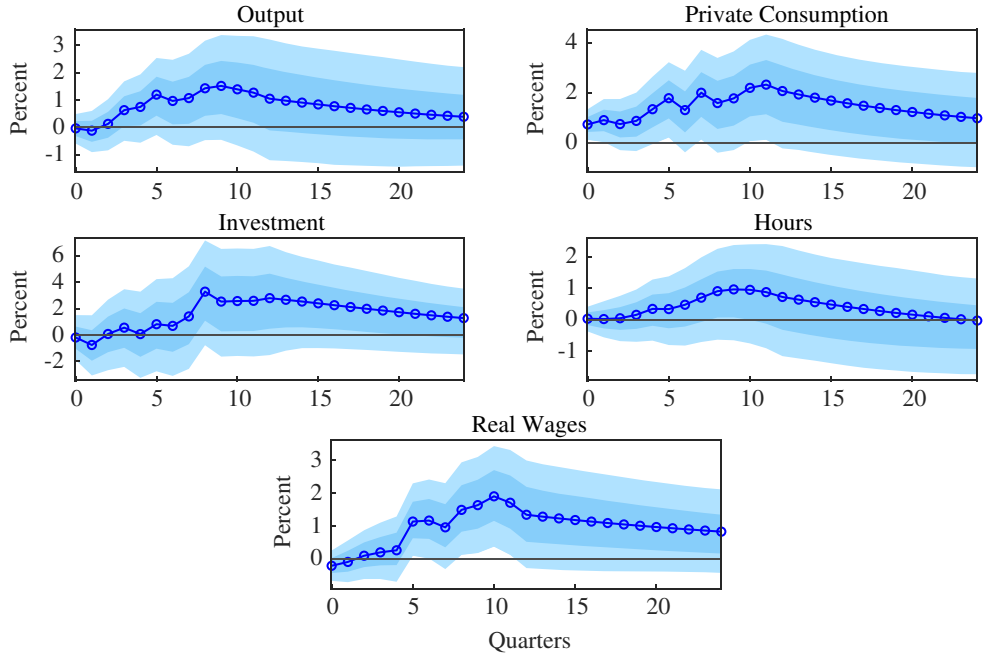

FIGURE A1. The impact of a 1% tax cut—without anticipation effects.*Notes*: The figure shows the responses of macroeconomic variables to an exogenous tax cut corresponding to 1% of GDP. The panels show effects for all exogenous tax shocks based on a specification that does not account for anticipation effects. Lines with circles indicate point estimates. Shaded areas denote 68% and 95% bootstrapped confidence intervals based on a non-parametric bootstrap with 10,000 replications.