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Government Debt and Inflation A Cross-Country Investigation of the Fiscal Theory of the Price Level

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Government Debt and Inflation

A Cross-Country Investigation of the Fiscal Theory of the Price Level

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Abstract

In the early 2020s sharp surge of inflation, unprecedentedly high levels of government debt and deficits fueled attention for the Fiscal Theory of the Price Level (FTPL). This theoretical framework for fiscally induced inflation is well-known and controversially discussed. However, empirical tests are scarce. This paper aims at testing the FTPL empirically by applying a tractable functional form of the intertemporal budget function to a wide range of crises in OECD countries between the years 1980 and 2023. The results imply that between 35 and 40 percent of excess government deficits and spending in times of crises are not financed by orthodox fiscal policy alternatives but instead result in higher prices. This provides empirical evidence for non-Ricardian fiscal policies in times of crises and strengthens the arguments of the FTPL.

Keywords: Fiscal Theory of the Price Level; inflation; fiscal policy; government debt

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1. Introduction

Monetary and fiscal policy are, at least in the public debate, but also in many academic contributions, well-separated policy agendas. When inflation spirals out of control, all eyes turn to central bankers, whereas the looming risk of a new sovereign debt crisis amplifies the calls for more prudent fiscal policy. The latter has received much attention by researchers in the last two decades (e.g., Heinemann et al., 2018; Yared, 2019), while inflation was little focused. For industrialized countries, constantly rising government debt seemed to be more worrying, illustrated, e.g., by the European debt crisis in the 2010s, while inflation remained relatively low during the great moderation. In the early 2020s, both, new record-high government debt levels and sharp, unexpected inflation surges hit many industrialized economies simultaneously. This provoked a new string of theoretical and empirical contributions on the consequences of unsustainably high government debt on inflation, cumulating in John Cochrane's seminal work on the *Fiscal Theory of the Price Level* (FTPL) (Cochrane, 2023). The central message of the FTPL, gaining much attention in describing the latest surge of inflation, is that rising price levels can be solely the result of fiscal policy and the outcome of eroded confidence that a government will be able to pay back its debt at least sometime in the distant future.

In this paper, we aim at testing the FTPL following the approach by Barro and Bianchi (2023) by applying a tractable functional form of the intertemporal budget constraint. However, our analysis is not restricted to the COVID19-crisis but considers a wide range of crises in OECD countries between 1980 and 2023. We are thus able to test the FTPL also for periods of lower inflation and contribute to a generalization of the empirical analysis.

The distinct effects of monetary and fiscal policy on inflation and the interaction between them are in fact well-researched mechanisms. In a stylized, Ricardian economy the two policies set their targets subsequently with a first mover advantage. The policy that selects its ideal policy instrument second must accept and consider the premises already defined. This makes the second policy "passive" and the first policy "active", disciplining the second one. Active monetary and passive fiscal policy can ensure price stability and at the same time enforce fiscal discipline. Fiscal policy can only increase its deficit as long as it is not threatening the inflation target of monetary policy. If passive fiscal policy exceeds its boundaries, default is the logical consequence in this Ricardian world. In an active fiscal and passive monetary policy situation, however, fiscal policy can choose any deficit level it desires, and monetary policy must adapt using an appropriate policy instrument to finance it, even if this causes higher inflation. If a fiscal shock is accompanied by tight money supply, monetary policy can delay but not avoid inflation.

Visualized in a real-world example, a deviation between the neutral and real interest rate (with output at its potential and inflation at target) indicates an increasingly active fiscal policy. Ideally, the neutral interest rate would be the result of an active monetary policy (defined first), and (passive) fiscal policy then adapts its debt (Sargent and Wallace, 1981; Leeper, 1991; Sims, 1994; Bolhuis et al., 2024).

The dynamics of active monetary and passive fiscal policy translate into central bank strategies to communicate effectively that they will never give in to excessive fiscal policy. Regarding certain shocks, however, inflating away fiscal expansion might be unavoidable or even desirable (Phelps, 1973; Calvo, 1978; Lucas and Stokey, 1983 based on Ramsey, 1927; Woodford, 2001). Bassetto (2006) narrows this conclusion to households' willingness to hold government debt and thus limiting the possibility of fiscal dominance in times of severe fiscal constraints.¹

The FTPL expands the scope of the monetary-fiscal-policy mix by dropping the premise of a Ricardian economy. So far, reactive (passive) monetary policy causes inflation mechanically. In a non-Ricardian economy, however, inflation may be caused solely by fiscal policy. Interpreting the intertemporal budget constraint as an intertemporal budget equilibrium function allows the price level to adjust to changes in discounted expected future surpluses. Now the intertemporal budget function is basically an asset valuation function. A stock price (real value of government debt) is determined by discounted expected future dividends (primary surpluses). Changes in primary surplus do not necessarily translate into changes in the price level instantly and one for one. The government's debt structure (nominal amount and yield to maturity) determines the necessary changes in the price level to bring the intertemporal budget function to an equilibrium. This determination of the price level does not rely directly on any monetary policy decision, and it thereby recognizes the importance of financial innovations with no backing by central bank money. Monetary policy remains, however, crucial since interest rate targeting defines the long-term trajectory of (expected) inflation. New information on expected future surpluses determines unexpected changes in the price level (Woodford, 1995; Cochrane, 2001, 2005, 2022a). While this view provides a plausible narrative for low inflation in the 2010s in the U.S., the European case is different, given multiple worrisome news on fiscal incidents in EMU member states (Hirsch et al., 2024).

¹ Focusing on monetary- or fiscal-led equilibria illustrates extreme cases for both alternatives. Cochrane (2022b) or Smets and Wouters (2024) introduced models with partial fiscal backing. Here, fiscal policy uses orthodox means to redeem most but not all of its debt. Smets and Wouters (2024) find that, since the 1960s, government debt was on average 80 percent backed by conventional fiscal policy.

Fiscal policy can only determine the price level if it is committed to its agenda, meaning that, under a non-Ricardian assumption, fiscal policy still has to move first (be active) and be committed to its selected policy. After the price level has subsequently been set, the intertemporal budget function is observationally equivalent in a Ricardian and a non-Ricardian world. This means for an empirical study that time-series analyses of fiscal parameters and the price level cannot falsify the FTPL. Instead, we need to introduce auxiliary assumptions that allow for a testable reaction function of the price level on exogenous fiscal shocks. Such a tractable functional form of the intertemporal budget equilibrium function in combination with plausible exogenous fiscal shocks allows to draw conclusions about the presence of non-Ricardian states and, hence, a price level reaction to fiscal shocks (Bohn, 1998; Canzoneri et al., 2001; Loyo, 1999; Woodford, 1999; Cochrane, 1999; Barro and Bianchi, 2023).² While using exogenous shocks of unexpected fiscal expansion is useful for our empirical design, the FTPL is not limited to explaining isolated inflation spikes. Incorporating the FTPL in a general equilibrium, new-Keynesian framework allows to model fiscal drivers of persistent inflation (Davig and Leeper, 2006; Bianchi and Ilut, 2017; Leeper et al., 2017; Cochrane, 2022b; Bianchi et al., 2023; Smets and Wouters, 2024).

While the FTPL offers a new and coherent view on the determination of the price level, considering modern fiscal and monetary policy aspects, it is not uncontested. Many criticisms of the FTPL circle around the question whether non-Ricardian regimes are possible and whether it is plausible to view the intertemporal budget constraint as an equilibrium function. The possibility to bring the intertemporal budget function into equilibrium by changes in the price level overdetermines the price level determination function (Buiter, 2002, 2023; Niepelt, 2004). Furthermore, the FTPL requires, according to its critics, unrealistic modelling of observations and expectations basically until the end of time using untestable assumptions. In contrast to mainstream models, the FTPL collapses if elements of surprise (change in expected future surpluses) are taken out of the model.

Even when taking the non-Ricardian case into account, HANK models suggest a much more moderate effect of deficits on inflation (while not rejecting the key message of the FTPL) (Angeletos et al., 2024a, b). HANK models highlight the self-financing capability of government deficits via the tax base channel (a consequence of the demand stimulating effect of extended government spending). Another concern with the FTPL is that the theory might be fragile to, e.g., low levels of debt or in a low-interest rate environment. The non-Ricardian case is

² Lawrence and Fitzgerald (2000) list most of the older literature in their summary article.

potentially not robust in determining the price level if certain variables take rather extreme values. Finally, empirical tests of the FTPL have not been fully supportive. In direct comparison, the Ricardian alternative seemed more realistic than the non-Ricardian alternative and cross-country studies beyond the U.S. case have not supported the FTPL empirically. This skeptical string of literature is not denying the interaction between fiscal policy and the price level per se. It also acknowledges that this correlation can be measured empirically. It assigns these observations, however, to Sargent and Wallace's (1981) unpleasant monetarist arithmetic in a Ricardian world (Lawrence and Fitzgerald, 2000; Canzoneri et al., 2001; Creel and Le Bihan, 2006; Bassetto and Cui, 2018).

With general agreement on the interaction between fiscal and monetary policy and contested positions regarding the FTPL's explanatory potential, recent literature brought a wide range of (empirical) evaluations on the most recent surge of inflation into light. One string of literature finds with different identification techniques that 30 to 80 percent of post-COVID inflation can be (causally) attributed to respective fiscal shocks. It remains an open question at this point though, whether this was a short-lived inflation spike or the starting point of fiscal inflation creeping into trend inflation. This literature emphasizes the importance of fiscal (and microeconomic) reforms to control inflation, especially on the last mile (Bianchi and Melosi, 2022; Anderson and Leeper, 2023; Blake and Zarazaga, 2024; Cochrane, 2024; Hazell and Hobler, 2024).

Moreover, the literature acknowledges that, in addition to fiscally induced inflation, other factors had an effect on recent price level increases. Supply constraints in the aftermath of the pandemic paired with a shift in consumer preferences from services to goods, energy shortages due to Russia's invasion into Ukraine and higher expected negative unemployment gaps can explain fractions of the post-COVID19 inflation surge. Increased public deficits affect inflation in this literature only in so far as they increase aggregated demand in an already strained aggregate supply situation (Ferrante et al., 2023; Bernanke and Blanchard, 2023; Gagliardone and Gertler, 2023; Bergholt et al., 2024; Crump et al., 2024; Dao et al., 2024; Giannone and Primiceri, 2024; Smets and Wouters, 2024).

The remainder of this paper is structured as follows: Section 2 introduces the empirical identification strategy proposed by Barro and Bianchi (2023) and presents the key assumptions that come with this approach. In Section 3, we describe our data and show how we aim to expand the Barro-Bianchi-approach to a multi-crises dataset. Defining transparently plausible criteria for crises (fiscal shocks), selection is crucial. In Section 4 we present our results and test the validity of this identification strategy beyond the in many regards unique COVID19 crisis. In Section 5 we apply our results to a battery of robustness checks, Section 6 concludes.

2. The Barro-Bianchi-Approach

Barro and Bianchi (2023) derive their tractable functional form of the intertemporal budget equilibrium function from fundamental FTPL functions, mostly referring to Cochrane (2001). They emphasize that their approach relies on a frictionless version of the FTPL with no rigidities and invariant GDP growth and real interest rates.³ Furthermore, this empirical identification strategy abstains from integration into a full model. It is the very basic idea expounded above to rearrange the key functions of the FTPL in such fashion to make them (a) empirically testable and (b) the results interpretable regarding plausibility of the non-Ricardian assumption. In the following, we will not repeat the entire derivation of Barro and Bianchi's approach in detail. Instead, we highlight the most important assumptions and restrictions.

The intertemporal budget function is the starting point:

(1)
$$\frac{B_t}{P_t} = \sum_{i=0}^{\infty} \frac{S_{t+i}}{(1+r)^i}$$

with B_t as nominal debt in period t and S_t the primary surplus, r the natural interest rate and P_t the price level. According to eq. (1), the real value of government debt, denominated by today's price level equals all future surpluses (the summation function goes to infinity) discounted at today's present value. The primary surplus S is defined as the government's total revenue (mostly taxes) minus total government expenditure, corrected for interest expenditures. We deviate slightly from Barro and Bianchi (2023) who consider only primary expenditure instead of primary surplus on the right-hand side of the equation. They argue that the fiscal reaction to the COVID19-crisis was overwhelmingly characterized by excess government spending with negligible changes in revenue. However, to bring this identification strategy closer to theory (e.g., Cochrane, 2005) and to be able to apply it more broadly to a wide range of crises, we stick with pure model theory and rely on primary surplus. We neglect revenue from seignorage due to data availability but could optionally integrate it as another source of revenue. It is assumed that the path for future taxes and government expenditure is fully known, such that we can use the true values of primary surpluses and neglect expectations.

³ Thus, it is a very basic approach to the FTPL as proposed in part I of Cochrane (2023).

Assume that an unexpected crisis occurs and lasts for M periods. The government either reacts to this crisis by some alteration of its spending behavior (e.g., expanding public spending) and/or is affected by a substantial revenue decline while expenditures remain constant. The changes can be formalized as:

(2)
$$\frac{\Delta B_t}{P_t} = \sum_{i=0}^M \frac{\Delta S_{t+i}}{(1+r)^i}$$

 ΔS is the deviation of the primary surplus from its non-crisis path. This change in surplus has consequences for the nominal value of government debt. These consequences are described by the deviation of nominal debt from its non-crisis path, ΔB . The fiscal consequences of the crisis are by assumption unknown before the crisis occurs in time *t* but are fully known in point *t* until the end of the crisis in *M*. After the crisis, fiscal policy is assumed to return to its pre-crisis path.

For the derivation proposed by Barro and Bianchi (2023) the following assumptions are additionally needed:

- We analyze fiscal reactions to a crisis in the absence of formal default, the no-Ponzi condition holds.
- Monetary policy actions do not impact the time path of real variables.
- The path of inflation is not affected by changes in real variables.
- The outbreak of a crisis provokes no changes in monetary policy.
- The real GDP growth rate g equals the real interest rate r.
- The real interest rate *r* equals the expected inflation π^* , therefore it is: $g = r = \pi^*$, all three variables are constant and do not change during the shock.⁴
- The government arranges its total nominal payments on debt as a constant share of GDP over time. The payment contains principal and coupon alike.

Finally, we need one additional condition to derive a tractable form of the intertemporal budget function that allows us to test the presence of the non-Ricardian condition and, hence, the FTPL. In non-crisis times actual inflation (π) equals expected inflation, it is $g = r = \pi^* = \pi$. In a non-Ricardian regime, we should be able to observe actual inflation to be higher than the inflation target when fiscal policy deviates from its expected, non-crisis path, leading to:

 $g=r=\pi^*<\pi.$

 $^{^{4}\}pi^{*}$ can for example be an inflation expectation formed by observations in the past (as proposed by Barro and Bianchi, 2023). Alternatively, we deviate from the expectation term and propose the central bank's inflation target as another suitable π^{*} .

Given these assumptions the change in the nominal value of debt due to a fiscal shock can be formulated as:

(3)

$$\Delta B = B_t^0 \left[\left(\frac{1 + \pi^*}{1 + \pi_{t+1}} - 1 \right) + \left(\frac{(1 + \pi^*)^2}{(1 + \pi_{t+1})(1 + \pi_{t+2})} - 1 \right) + \dots + \left(\frac{(1 + \pi^*)^T}{(1 + \pi_{t+1}) \cdots (1 + \pi_{t+T})} - 1 \right) \right]$$

and be rearranged to:

(4)
$$\Delta \mathbf{B} = -\mathbf{B}_{\mathrm{t}}^{0} \cdot \frac{1}{2} T \cdot (\pi - \pi^{*})$$

with *T* describing the longest debt maturity and $T/_2$ approximating the average maturity. Note that B represents the total payments due for principal and coupon at a certain moment in time. The changes in the primary surplus ΔS on the right-hand side of eq. (2) are described by:

(5)
$$\Delta S = Y_t \cdot \sum_{i=0}^{M} \Delta \left(\frac{S_{t+i}}{Y_{t+i}} \right)$$

The deviation of the primary surplus in times of crises (the crisis lasts from t to M) from the times of no crisis can be described relative to GDP which grows constantly given the assumptions made above.

Barro and Bianchi (2023) next insert eq. (4) as ΔB and eq. (5) as ΔS in eq. (2) and rearrange it to describe the relationship between observable inflation's deviation from inflation expectation to fiscal policy, leading to:

(6)
$$\pi - \pi^* = \frac{\sum_{i=0}^{M} \Delta\left(\frac{S_{t+i}}{Y_{t+i}}\right)}{\frac{B_t^*}{P_t Y_t} \cdot \frac{T}{2}}$$

Eq. (6) states that the deviation of observable inflation from expected inflation can be explained by the aggregated deviation (over M periods) of primary surplus from its non-crisis path relative to the real value of debt (as share of GDP) before the crisis occurred times the average maturity of government debt. This equation implies: (a) that a higher deviation of primary surplus in times of crisis leads to higher deviations between observable and expected inflation, (b) that a higher pre-crisis level of debt leads to lower inflation deviation (less inflation is needed to bring debt back to its pre-crisis trajectory), and (c) that higher average maturity facilitates lower inflation as well (multiplicity of future inflation rates).⁵

⁵ For the theoretical justification, see Cochrane (2001). Liemen and Posch (2022) analyze the mechanisms of average maturity in a New Keynesian model with a fiscal block, supporting the dynamics outlined here. Mankart et al. (2022) analyze the effect of the maturity structure in a more mainstream approach, showing that short-term

To transform eq. (6) in an empirically applicable form it is furthermore worth emphasizing that the right-hand side – we call it in the remainder of the paper the *Composite Surplus Variable*, CSV – is an aggregation over the entire duration of the crisis. This, first, underlines the assumption of perfect foresight in the moment the crisis occurs and, second, implies that we are going to work with an unbalanced panel with only one observation per crisis. All other crisis years are incorporated in the composite fiscal variable but not estimated individually.

According to eq. (6) every deviation of primary surplus (relative to government debt and average maturity of debt) translates into higher inflation. This seems a bit extreme since there is also the possibility of orthodox fiscal policy (increased revenue, lower expenditures) after M. Evidence for the FTPL could, however, be provided if a fraction of excess government deficit is inflated away. This begs the question as to how large this fraction might be and whether it is statistically significantly different from zero (and one).

Putting the right-hand side of eq. (6) in a GLS regression function leads to:

(7)
$$\pi - \pi^* = \alpha + \beta \cdot CSV + \gamma \cdot X + \epsilon.$$

With the coefficient β we aim at estimating the fraction of CSV that translates into higher inflation and is therefore not subject to future adaptations in fiscal policy. The additional γX represents a vector of additional explanatory variables that may affect inflationary reactions during a fiscal shock, e.g., regional proximity to the shock (like the war in Ukraine), different government tiers responsible for monetary and fiscal policy (strict pegs), or the presence of fiscal rules. In difference to other contributions (e.g. Davig and Leeper, 2006; Bianchi and Ilut, 2017; Leeper et al., 2017; Bianchi and Melosi, 2022 or Blake and Zarazaga, 2024), Barro and Bianchi emphasize that they do not aim to incorporate the derived equilibrium function into a full model. We follow their approach in this regard as well.

<u>3. Descriptive Statistics and Identification Strategy Alterations</u></u>

Descriptive Statistics

In order to generalize the Barro-Bianchi-approach to a multi-crises setting, we need to expand our dataset to a longer time span and thus focus on an observation period from 1980 to 2023. Most fiscal variables are provided by the latest version of the IMF's *Public Finance in Modern History* dataset (Mauro et al., 2015), inflation observations are used from the OECD.STAT

debt has a higher effect on the fiscal multiplier than an increase in long-term debt since the former has a more direct effect on private consumption.

database and for the average maturity of government debt we use primarily the OECD's *Average Term to Maturity and Duration* dataset. Since the latter only provides observations until the year 2010, we expand this variable with manually collected observations from the IMF's annual *Fiscal Monitor* publications (2011 to 2023). While considering amounts and timing structure in the payment due (Macaulay duration of bonds) would theoretically be most accurate in the applied empirical framework, Barro and Bianchi (2023) show that the difference between Macaulay duration and average maturity is negligible. The correlation coefficient between both alternatives is 0.95 in their sample.

We furthermore collect manually the central banks' interest rate targets as an alternative π^* . In order so consider fiscal rules as one extension in Section 4, we use the IMF's *Fiscal Rules Dataset, 1985-2021* (Davoodi et al., 2022). Another extension is to control for the presence of strict monetary pegs; the respective dummy variable is provided by the 6th version of the *Macrohistory Database* (Jordà et al., 2017).

Following the data gathering process in Barro and Bianchi (2023) our dataset contains observations from 37 out of 38 OECD countries. We exclude Turkey due to bad data quality. The up to 17 countries that are OECD and EMU countries are summarized as one observation. The number of countries which constitute the EMU unit varies during our observation period and so does the composition of EMU observations. All variables except two are constructed as GDP weighted average over all EMU countries. The average maturity variable is instead debt weighted, and crisis indicators are considered as binary variables.

The most crucial step in the data selection process, however, is to define a variable that indicates plausibly when a crisis occurred. While Lucas and Stokey (1983) model the dominant role of fiscal policy in the presence of shocks, they also highlight that the fiscal reaction to the shock is state-contingent, depending on the shock's probability, predictability, cyclicality and other characteristics. Hence, defining a basket of shocks that are supposed to be analyzed empirically is not only suffering potential arbitrariness, but its selection is furthermore contested by theoretical appropriateness.

We propose two alternatives. First we state that every recession is defined as a crisis that probably comes with some sort of fiscal expansion. A recession is defined by at least two subsequent quarters with negative GDP growth, the data is provided by the OECD's *World Economic Outlook.* All variables are based on annual data and an observation is considered as crisis-observation when the dummy variable indicates a recession at some point during the respective year. Since codifying every recession as a crisis may suffer from potential endogeneity problems, we add the banking crisis variable from Jordà et al. (2017) as an alternative. They define a systematic banking crisis (henceforth referred to as *JST crisis*) as: "… major bank failures, banking panics, substantial losses in the banking sector, significant recapitalization, *and/or significant government intervention*. Importantly, this definition excludes the failures or losses of individual/small banks without systemic implications…" (Jordà et al., 2021).

all observations	Obs.	Mean	Std.Dev.	Min.	Max.
annual inflation growth rate (in %)	1,255	4.1%	5.6%	-4.5%	84.0%
gov. primary balance (in % of GDP)	1,236	0.6%	4.0%	-28.2%	25.7%
gov. primary expenditure (in % of GDP)	1,241	40.1%	8.8%	8.6%	65.1%
gov. debt (in % of GDP)	1,241	62.6%	38.0%	6.2%	261.3%
average maturity of gov. debt	1,006	6.1	2.4	0.4	20.0
observations considered in the $estimation$ with recession = 1					
annual inflation growth rate (in %)	361	5.4%	7.1%	-4.5%	84.0%
gov. primary balance (in % of GDP)	356	-0.9%	4.1%	-12.1%	25.7%
gov. primary expenditure (in % of GDP)	359	41.7%	9.0%	8.6%	65.1%
gov. debt (in % of GDP)	359	70.4%	45.2%	7.4%	261.3%
average maturity of gov. debt	297	6.7	2.6	0.4	20.0
observations considered in the $estimation$ with JST crisis = 1					
annual inflation growth rate (in %)	169	4.8%	4.0%	-1.2%	19.7%
gov. primary balance (in % of GDP)	167	-1.6%	4.1%	-11.4%	25.7%
gov. primary expenditure (in % of GDP)	170	42.0%	9.4%	13.7%	60.1%
gov. debt (in % of GDP)	170	72.8%	46.0%	17.0%	261.3%
average maturity of gov. debt	158	7.5	2.5	0.4	14.8

i.

Table 1: Descriptive statistics

<u>Notes:</u> Descriptive statistics for all observations and only observations for two distinct types of crises. All observations are gathered from 37 out of 38 OECD countries (all countries except Turkey due to data quality) between 1980 and 2023.

The JST crisis is basically a sub-sample of the recession sample. In only eight out of 170 cases the JST crisis indicates a crisis with no recession at the same time. On the other hand, in 200 cases the dataset contains a recession with no JST crisis or no JST crisis observation (total number of recession observations: 362). In 162 cases, both indicators mark a crisis, the correlation coefficient is 0.66. In both cases, the total number of observations is substantially reduced since every crisis that lasts for more than one year is only considered as one observation because we estimate the effect of the *aggregated* change in government surplus on *average* inflation. Therefore, the number of total (crisis) observations is reduced for recessions from 362

observations down to 156 (the average crisis lasts 2.14 years) and for JST crises from 170 observations down to 59 (the average crisis lasts 2.31 years). Table 1 summarizes the descriptive statistics for the most important variables considered in the baseline estimation and Table A-1 in the Appendix displays all variables considered. Figure 1 illustrates the differences between recession observations and the quasi-sub-sample for systematic banking crises.





<u>Notes:</u> Average observations for primary surplus (upper graph) in percent of GDP and inflation (lower graph) for the observations with no crisis (green bars), observations in a recession (orange bars, N=200) and observations in a recession with systematic banking crisis (grey bars, N=162). The number of observations with only systematic banking crises but no recession is too small for visualization (N=8). Black vertical lines represent in all three cases the 95% confidence interval.

The descriptive statistics point into the theoretically expected direction. In times of crisis primary surplus turns on average in a primary deficit (Figure 1, upper graph) and inflation is on average higher than in non-crisis times (Figure 1, lower graph). While the change in fiscal policy is evident, it is hard to tell graphically how substantial the change in inflation really is. These descriptions must be considered carefully though, because of endogeneity concerns. Fiscal variables contain the decline in GDP (the numerator of these variables) that defines a crisis and high inflation could (at least partially) represent a government induced demand effect.

Due to these mechanical relations and given that we consider non-normal times, we cannot rule out that tough crises lead to extreme reactions in some of our variables, thereby increasing the risk for statistical outliers. To ensure a coherent pool of observations during times of crises we winsorize our dataset at the 5th and 95th quantile. In contrast to common winsorizing techniques we cannot justify why observations below the 5th or above the 95th quantile should be standard-ized on the respective threshold. We therefore discard these observations altogether.

Identification Strategy Alterations

Expanding the identification strategy outlined above to a multi-crises setting that covers more than 40 years comes with five caveats. First, π^* in the dependent variable $\pi - \pi^*$ is codified as expected inflation formed by inflation observations from the past (between 2010 and 2019). This is (a) a rather rough approach to sketch expected inflation and (b) in a multi-crises dataset we need to be careful with this definition of expected inflation because time between two crises varies. Hence, we do not have a constant period with inflation observations prior to the crisis outbreak. We propose central banks' inflation target rate as an alternative π^* specification.

Second, regional proximity to Russia and Ukraine is less relevant for our own approach since these two countries have not been in a hot war with each other for most of our observation period. The same obviously holds true for controlling for COVID19 casualties. The last variation in data consideration is our focus on primary surplus instead of primary expenditure in the CSV's numerator. While Barro and Bianchi (2023) underpin their argument that the COVID19 fiscal shock was primarily expenditure driven with convincing descriptive statistics, we cannot systematically rule out the case of revenue shocks for the full observation period. We therefore return closer to theory and expand the analysis to primary surplus. We report, however, results for primary expenditure in the numerator of the variable of interest as well.⁶

⁶ In addition to the *Composite Surplus Variable* (CSV) aggerating *primary surplus*, we construct the *Composite Expenditure Variable* (CEV), incorporating *primary expenditure* observations.

Third, considering EMU as an individual unit is in this multi-crises approach problematic because the monetary union was established and expanded during our observation period. For all EMU member states, we have observations before they joined the union as well. We therefore estimate two alternatives. One, with the EMU as individual unit with varying composition over time, second, a set of estimations with all EMU member states considered individually.

Fourth, in a multi-crises dataset, we consider multiple observations for the same country over a relatively long period. This could mean that (a) our dataset (in an unbalanced panel) may contain *unobservable heterogeneity* across different countries, (b) that *endogeneity* might be an issue for our estimations or (c) bears the risk of *correlated error terms*.⁷ For the latter, we consider clustered standard errors on the country level. For the first issue, we estimate the models with Generalized Least Squares (GLS), thereby differentiating from Barro and Bianchi's OLS estimation. In absence of a suitable instrument (IV), the results for robust and bootstrapped Hausman tests are ambiguous. For the unbalanced panel random effects are practical and more efficient than fixed effects. This is particularly important for us as the sample is relatively small for some specifications (especially for estimating with systematic banking crises as crisis identifier). Barro and Bianchi (2023) use fixed effects for the balanced panel estimation. In our case, the robust and bootstrapped Hausman tests recommend fixed effects for balanced panel estimations as well. Therefore, we follow Barro and Bianchi (2023) for the balanced panel estimations.

Fifth, Barro and Bianchi (2023) allow crisis related, fiscally induced inflation to go beyond the crisis itself. Specifically, they estimate how excess government spending in 2020 and 2021 (relative to the pre-crisis debt level in 2019) affects average inflation between 2020 and 2023 (relative to average pre-crisis inflation between 2010 and 2019). So, the crisis lasts for two years (2020 and 2021) but the inflationary consequences are observable beyond the crisis itself (in 2022 and 2023 as well). This makes perfect sense from a theoretical point of view because the model suggests that inflation can fade out over time to maturity, and this is usually sometime in the future but does not end with the end of the crisis. Econometrically, however, we are constrained by the sequence of crises which probably occur in quicker succession than the average time to maturity lasts. Put differently, the next crisis occurs before debt from the last crisis reaches its average time to maturity. We therefore only focus on immediate inflationary reactions during times of crises. While this approach neglects fiscal effects on structural inflation (Cochrane, 2021, 2023, pp. 52) and rather focuses on unexpected changes, it is still feasible for

⁷ Appendix A-1 provides a more detailed elaboration on the potential issues and how we test for *endogeneity*, *multicollinearity*, *homoscedasticity* and *serial correlation* for our dataset.

the Barro-Bianchi-approach since their observation period can also not cover the entire time to maturity for COVID-related excess government debt.

4. Results

We begin with replicating the baseline results from Barro and Bianchi (2023), thereby restricting crisis observations to the years 2020 to 2022 and pre-crisis observations to the years 2010 to 2019. The results are presented in Table 2. They are very close to Barro and Bianchi's estimations; the significance levels and coefficients' signs are identical for all independent variables and coefficients only deviate in the second decimal place.⁸ All results are robust when we substitute CEV by CSV, only the coefficient signs turn. The coefficients' signs are negative for CSV, and this is what we would expect theoretically since higher expenditures lower the surplus or even turn it negative when a primary deficit occurs.

Next, we expand this baseline estimation to the full sample, covering crises between 1980 and 2023 (Table 3). The coefficients for estimating inflationary reactions to a fiscal shock in recessions are mostly a bit larger than estimation results for the COVID crisis exclusively.⁹ The Barro and Bianchi (2023) results suggest that around 40 percent of excess government expenditure linked to the COVID19 crisis is inflated away. When we expand the observation period to recessions between 1980 and 2023, the average share is similar for CEV (column (1) and (3)) and a bit lower for CSV. Only about 35 percent of excess primary surplus result in higher inflation. When we limit the sample to fiscal reactions during systematic banking crises the coefficients on fiscal expansion are larger, up to almost 80 percent (column (8)). This is somewhat surprising given that descriptive statistics in Section 3 rather pointed to higher inflation in the recession sample. For systematic banking crises the results differ substantially between CSV and CEV estimations. This strengthens our argument to primarily rely on the theoretically justified primary surplus alternative, since we cannot formulate a consistent expectation of the fiscal reaction across so many different crises. As explained in Section 3, we abstain from considering the Russian or Ukrainian border proximity due to minor relevance for the entire

⁸ Note that the paper by Barro and Bianchi (2023) was originally distributed in November 2023 but since then revised twice, in March 2024 and August 2024. When we cite this paper, we are usually referring to its latest version, revised in August 2024. However, Barro and Bianchi altered their estimation strategy slightly between March 2024 and August 2024. Due to practical reasons that we laid out in detail in Section 3, we stick with the estimation strategy from March 2024 (we only estimate inflationary reactions *during* a crisis, not beyond the crisis). For reasons of consistency, we therefore replicated the Barro and Bianchi results from March 2024 and when we compare our own results with theirs, we refer to the paper's version from March 2024.

⁹ In this first step, expected inflation π^* is defined in accordance with Barro and Bianchi (2023) by average inflation observations prior to the crisis.

observation period. R2 decreases notably from Table 2 to Table 3. This is logical since the variety of crises is larger in our estimations, making it more challenging to provide sufficient explanatory power by the empirical model.

Altering the dependent variable

The first point outlined in Section 3 for altering the empirical identification strategy are varying observation periods between two crises. The non-crisis periods that are used by Barro and Bianchi (2023) to form inflation expectations are not constant over different crises observations and this may distort the estimation of inflation expectation. To address this problem, we test central banks' inflation targets as an alternative π^* , making pre-crisis observations irrelevant. This also strengthens the theoretical argument that monetary policy is important for determining inflation expectations and debt maturity composition (Cochrane, 2022b).

Compared to the baseline results, the fraction of expansionary fiscal policy that is inflated away is mostly higher when measuring price level increases as the deviation between observable inflation and the central bank's inflation target (Table 4). This is plausible because during a recession inflation differs on average 2.1 percentage points from inflation expectations formed by pre-crisis observations and 3.2 percentage points from the inflation target. The only exception with higher coefficients in the baseline estimation are CSV coefficients for JST crises (column (6) and (7) in Table 3 and 4 respectively). The significant findings are less consistent though in this modification compared to the baseline findings. Statistically significant findings for CEV are only different from zero at a 10% or 5%-level and CSV findings are statistically significant at a 5% or 1%-level, yet not consistently across all specifications. Since this alteration is no substantial improvement compared to the baseline estimations, we return for the remainder of this paper to pre-crisis observations forming inflation expectations π^* .

Institutional design as additional independent variables

Inflationary reactions to a fiscal shock may be affected by the monetary policy framework. Depending upon whether monetary and fiscal policy are executed on the same governmental tier, it might be easier or more difficult for monetary policy to act credibly independently from fiscal policy. It may also have consequences as to whether monetary and fiscal policy goals are coherent or diverging.

Table 2: Replication of Table 5 in Barro and Bianchi (2023)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	СРІ	CPI	CPI	CPI	core CPI	core CPI	core CPI	core CPI
CEV	0.433** (0.165)		0.486*** (0.130)		0.382*** (0.128)		0.416*** (0.111)	
CSV		-0.478*** (0.122)		-0.499*** (0.0880)		-0.381*** (0.0996)		-0.394*** (0.0854)
Russian/ Ukrainian border			0.0279*** (0.00794)	0.0264*** (0.00627)			0.0181** (0.00679)	0.0167** (0.00608)
Constant	0.0210*** (0.00563)	0.0217*** (0.00426)	0.0142*** (0.00482)	0.0160*** (0.00335)	0.0148*** (0.00437)	0.0163*** (0.00347)	0.0104** (0.00412)	0.0127*** (0.00324)
s.e. of regression	0.0180	0.0156	0.0141	0.0112	0.0140	0.0127	0.0121	0.0109
log(likelihood)	53.01	55.92	58.47	63.08	58.09	60.02	61.58	63.70
R-squared	0.276	0.460	0.581	0.736	0.331	0.448	0.528	0.618
Observations	20	20	20	20	20	20	20	20

Robust standard errors in parentheses

*** *p*<0.01, ** *p*<0.05, * *p*<0.1

<u>Notes:</u> Columns (1), (3), (5), and (7) are replications with using the composite expenditure variable. Columns (2), (4), (6), and (8) are modifications with the composite surplus alternative. The years 2020 to 2022 are coded as crisis years, expected inflation π^* is calculated by average inflation observation between 2010 and 2019, OLS estimation. The respective results in Barro and Bianchi (2023) stem from the paper version revised in March 2024.

Table 3: Baseline estimation for the full observation period (1980 - 2023)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		recession			JST crisis			
	СРІ	CPI	core CPI	core CPI	СРІ	CPI	core CPI	core CPI
CEV	0.478*** (0.166)		0.413*** (0.142)		0.564* (0.309)		0.600*** (0.214)	
CSV		-0.371** (0.185)		-0.326*** (0.0967)		-0.709** (0.276)		-0.775*** (0.204)
Constant	-0.00331 (0.00343)	-0.000365 (0.00330)	-0.00125 (0.00223)	0.00181 (0.00225)	0.00401 (0.00695)	0.00152 (0.00611)	-0.000948 (0.00549)	-0.00394 (0.00471)
s.e. of regression	0.0232	0.0231	0.0173	0.0149	0.0141	0.0149	0.0102	0.0098
log(likelihood)	153.18	153.22	169.94	176.72	56.35	56.13	60.84	62.33
R-squared	0.1426	0.1178	0.1856	0.1842	0.2005	0.3112	0.2586	0.4546
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE
Observations	65	65	64	64	20	20	20	20
Number of countries	23	24	23	24	10	10	10	10

Robust standard errors in parentheses

***p < 0.01, **p < 0.05, *p < 0.1

<u>Notes:</u> Two crisis identifiers: recession and systematic banking crisis (JST crisis), EMU is considered as individual unit, constructed by weighted averages of its member states in the respective year. EMU member states are only considered individually as long as they are not members of the union. GLS estimation for unbalanced panel with random effects.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
	recession				JST crisis				
	CPI (target)	CPI (target)	core CPI (target)	core CPI (target)	CPI (target)	CPI (target)	core CPI (target)	core CPI (target)	
CEV	0.557** (0.273)		0.443* (0.244)		0.423 (0.310)		0.467 (0.403)		
CSV		-0.403 (0.304)		-0.621*** (0.197)		-0.731** (0.366)		-0.843** (0.375)	
Constant	0.0144** (0.00616)	0.0192** (0.00751)	0.0157** (0.00613)	0.00484 (0.00517)	0.0219*** (0.00566)	0.0176*** (0.00571)	0.0194*** (0.00674)	0.0154* (0.00889)	
s.e. of regression	0.0272	0.0265	0.0242	0.0205	0.0165	0.0133	0.0250	0.0126	
log(likelihood)	133.9680	134.8014	139.4338	137.7949	56.4146	59.1398	48.6692	56.5485	
R-squared	0.0920	0.0548	0.0713	0.2697	0.0788	0.2502	0.0674	0.2615	
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE	
Observations	62	62	62	57	21	21	21	20	
Number of countries	23	24	23	23	11	11	11	11	

Table 4: Extended baseline estimation for the full observation period (1980 - 2023)

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

Notes: Two crisis identifiers: recession and banking crisis (JST crisis). The two depended variables (CPI and core CPI) are the difference between observed inflation π , and the central bank's inflation target π^* . EMU is considered as individual unit, constructed by weighted averages of its member states in the respective year. EMU member states are only considered individually as long as they are not members of the union. GLS estimation for unbalanced panel with random effects.

We control for monetary policy characteristics by adding a dummy variable for the presence of a strict monetary peg. All countries are considered individually, and EMU participation is coded as a strict monetary peg not different from other strict pegs. This is different to the Barro-Bianchi-approach (and to our own estimations so far) who consider EMU as one unit constructed by the weighted average of its member states. However, this causes problems for interpreting our results since countries joined EMU at different moments in time and we have observations for all EMU countries before the union was formed. This means the fundamentals of EMU observations would vary across time and holding EMU membership constant over the entire observation period would also cause discretionary distortions. Furthermore, other countries with sovereign monetary policy, who pegged their currency to foreign monetary policy, have also limited the scope of national monetary policy.

The theoretical literature for this paper (Cochrane, 2005) and the assumptions we highlighted in Section 2 (monetary action has no impact on the time path of real variables; real interest rate *r* is constant and is not changed by the shock) emphasize the possibility that inflation is a purely fiscal phenomenon. This provokes the question as to how to justify the consideration of monetary policy institutions in the empirical research design. First, we consider central banks' decisions in the estimation directly when we substituted π^* with the central banks' inflation target. Second, while *r* is fixed before, after, and during a crisis it can vary between countries and take different values in the same country in two distinct crises (we construct and consider CSV (CEV) for every crisis individually). Third, advocates of the FTPL do not dismiss the important role of monetary policy but rather emphasize the importance of a well-balanced policy mix when it comes to tackling inflation. The long-term path to return to moderate inflation (tradeoff between long-term and short-term inflation) and sustainable public finances is substantially shaped by monetary policy (Cochrane, 2022b).

The second expansion regarding institutional characteristics as independent variables is the consideration of fiscal rules. The FTPL ultimately expresses doubts that a government will be able to pay back its debt at some point in the future. These doubts might be more persistent if no institutional framework guides public finances to sustainability in absence of inflation (although, to be fair, inflation is usually no aspect that is taken into consideration in fiscal rule designs). Kelemen and Teo (2014) for instance show theoretically that the disciplining effect of fiscal rules is caused by their ability to function as a focal point for the bond market, hence for the agents who form expectations about the government's ability to redeem its debt. Table 5 presents the results for both institutional expansions: controlling for monetary policy that is not nationally independent and the presence of fiscal rules.

The correlation between these two institutional variables is 0.54 and thus not too high. The observable correlation is probably driven by EMU member states since EMU participation is also coded as pegged monetary policy and one element in EMU foundation was the establishment of (supranational) fiscal rules. Graph A-1 in the Appendix visualizes the rule introduction and tightening over time for the G7 and Switzerland. We use the IMF's overall fiscal rule index (Davoodi et al., 2022), standardized on a scale between 0 and 1. Observations of fiscal rules vary considerably over time and between countries. The introduction of the Maastricht criteria in the year 1992 and the 2005 Stability and Growth Pact amendment are clearly visible in the graph for average constraints in EMU member states. Note that Davoodi et al. (2022) explicitly state that their index only addresses de jure but no de facto constraints. If a country is constraint

by multiple rules, all rules will be considered as weighted fraction in the index with the most constraining rule receiving the highest weighting.

Focusing first on the recession sample (Table 5, Part I), coefficient size for the excess government expenditure variable, proposed by Barro and Bianchi (2023), decreases substantially as compared to Table 3. Between 20 and 24 percent of excess government spending is not subject to orthodox financing. While most coefficients remain statistically significantly different from zero, the significance level is only between 5% and 10%. In comparison, the excess government surplus alternative, which represents a more general version of FTPL, provides much more consistent results. The effect size is, similar to Table 3, between 25 and 35 percent and all coefficients are highly significantly different from zero. These findings strengthen our decision to opt for a more general FTPL function form when considering many different crises.

The results are similar when core inflation (Table A-2 in the Appendix) substitutes CPI as dependent variable, but the coefficient size is usually a bit smaller. The results are consistent for the quasi-sub-sample with systematic banking crises (Table 5 and A-2, Part II) but the coefficient size is on average a bit higher.

The highly significant positive effect of fiscal rules is a bit puzzling. The effect is economically relevant, fiscal rules have a robust positive effect on the difference between expected and observed inflation between 1.1 and 3.5 percentage points. We would have expected that the existence of fiscal rules calms creditors' doubts that the government will pay back its debt at some point in the future and thereby mitigating the share of fiscal rules are a focal point for the bond market, it could, however, also be argued that fiscal rules make creditors more sensitive, leading to more nervous reactions in the price level when codified thresholds are exceeded.¹⁰ This, of course, is speculative and further research is needed to shed light on this positive and significant effect. The fiscal rule effect is robust when we consider the existence of a currency peg and fiscal rules in the same estimation.

Another caveat for interpreting the fiscal rule coefficients is the divergence between de jure and de facto constraints (see also the non-findings in Caselli and Reynaud, 2020). The sharp increase of the average EMU index value (Graph A-1 in the Appendix) in the year 2005 is primarily caused by Italy with an increase of more than 30 index points. However, this sharp increase in the index value did not substantially constrain Italy's fiscal policy in subsequent years.

¹⁰ This line of thought is supported by our arguments below for estimation results greater than one or coefficients not significantly different from one. See in this regard also Bassetto and Miller (2022), Jiang et al. (2024a, b).

Table 5: Extended baseline estimation for the full observation	period ((1980 - 2023)).
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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				reces	ssion			
	СРІ	СРІ	СРІ	СРІ	СРІ	СРІ	CPI	СРІ
CEV	0.293** (0.129)		0.236* (0.137)		0.272** (0.132)		0.201 (0.130)	
CSV		-0.371*** (0.104)		-0.316*** (0.109)		-0.354*** (0.106)		-0.266*** (0.101)
fiscal rule			0.0254*** (0.00639)	0.0240*** (0.00599)			0.0313*** (0.00783)	0.0294*** (0.00634)
currency peg					0.00638 (0.00513)	0.00267 (0.00411)	-0.0105* (0.00564)	-0.0112** (0.00489)
Constant	0.00248 (0.00237)	0.00236 (0.00183)	- 0.00753*** (0.00288)	-0.00739** (0.00338)	-0.000401 (0.00353)	0.000876 (0.00284)	-0.00546** (0.00262)	-0.00454 (0.00310)
s.e. of regression	0.0191	0.0193	0.0172	0.0173	0.0187	0.0195	0.0182	0.0184
log(likelihood)	249.36	249.34	245.92	248.98	249.98	247.40	242.23	244.18
R-squared	0.0859	0.1642	0.2089	0.2605	0.0887	0.1633	0.2279	0.2870
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE
Observations	99	99	94	95	98	98	93	94
Number of countries	34	34	33	33	33	33	32	32
		(10)	(11)	(12)	(12)	(14)	(15)	(10)
	(9)	(10)	(11)	(12) IST .	(13) antiata	(14)	(15)	(10)
	CDI	CDI	CDI	CDI	CDI	CDI	CDI	CDI
		CH		CFI		CH		CFI
CEV	0.345** (0.134)		0.316*** (0.0995)		0.338*** (0.120)		0.296*** (0.0933)	
CSV		-0.386*** (0.114)		-0.309*** (0.113)		-0.370*** (0.114)		-0.289*** (0.100)
fiscal rule			0.0244*** (0.00524)	0.0235*** (0.00551)			0.0364*** (0.00677)	0.0357*** (0.00757)
currency peg					0.00658* (0.00371)	0.00543 (0.00337)	-0.0124*** (0.00426)	-0.0126*** (0.00475)
Constant	0.0105*** (0.00340)	0.00965*** (0.00283)	-0.00223 (0.00395)	-0.00184 (0.00409)	0.00693 (0.00450)	0.00682* (0.00375)	-0.00103 (0.00410)	-0.000623 (0.00412)
s.e. of regression	0.0107	0.0114	0.0075	0.0077	0.0115	0.0117	0.0068	0.0071
log(likelihood)	105.08	104.13	116.20	115.57	104.37	103.87	117.99	117.08
R-squared	0.1761	0.2137	0.4919	0.4744	0.2371	0.2559	0.5717	0.5544
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE
Observations	34	34	34	34	34	34	34	34
Number of countries	18	18	18	18	18	18	18	18

Robust standard errors in parentheses

*** *p*<0.01, ** *p*<0.05, * *p*<0.1

<u>Notes:</u> Extensions by controlling for the presence of a currency peg and/ or fiscal rules. All countries are considered individually, no EMU unit. Presence of a monetary union is addressed by controlling for a strict monetary peg. GLS estimation for unbalanced panel with random effects.

The presence of a currency peg has, if any, a mild positive effect on the difference between observed and expected inflation.¹¹ The effect size is small and in most cases not significantly different from zero. Due to primarily non-significant findings, the interpretation of the estimated coefficients is difficult, but they could suggest that inflationary reactions to fiscal expansions in times of crises are higher for units with less national independence in their monetary policy. This would be in line with the findings of Barro and Bianchi (2023, pp. 32).

Isolate drivers of inflation

At this point we were able to show that Barro and Bianchi's (2023) tractable functional form reveals evidence for fiscally induced inflation in a broad set of crises. Strictly derived from theory, the empirical evidence is directly linked to the basic form of the FTPL. The composite fiscal variable, however, conceals direct fiscal policy consequences for inflation. We do not know yet whether excessive government spending, relatively low debt levels or short times to maturity cause the results presented so far.

To investigate more closely the individual contributions of distinct fiscal parameters to the overall composite effect, Barro and Bianchi (2023) suggest a more transparent approach by deriving a linear approximation of formula (6). We translate this approximation to our GLS model and estimate the individual effect of all three linearized components. As before, this approach is taken directly from Barro and Bianchi (2023, pp. 27) which is why we only cite their estimation function with our own notation here:

(8)
$$\pi - \pi^* \approx c + \left[\beta_S \cdot (S - \bar{S})\frac{\bar{\Omega}}{\bar{S}} + \beta_B \cdot (B - \bar{B})\frac{\bar{\Omega}}{\bar{B}} + \beta_{ATM} \cdot (ATM - \overline{ATM})\frac{\bar{\Omega}}{\overline{ATM}}\right]$$

Note that overlines indicate average values across the entire sample of the respective variable, omega-bar indicates the right-hand side of eq. (6) solely derived by sample average values. The variable ATM stands for average time to maturity and is equivalent to T/2 in eq. (6).

The results for eq. (8) as GLS with random effects are reported in Table 6. The estimation results for the recession sample are primarily driven by variation in the excess government surplus (expenditure) in times of crises (column (1) to (4)). Up to 75 percent of excess spending will not be repaid by low expenditures or higher revenue after the crisis. Instead, the additional spending translates into higher inflation than expected in absence of additional spending. The coefficients β_B and β_{ATM} are not statistically significantly different from zero, indicating that pre-crisis government debt and average time to maturity have no significant effect on inflation.

¹¹ The effect turns negative when we control for fiscal rules and currency pegs in the same estimation.

Table 6: Estimation function in linearized form

		rece	recession JST crisis			JST crisis		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	СРІ	core CPI	СРІ	core CPI	СРІ	core CPI	СРІ	core CPI
$(Exp-avg.Exp) \cdot (\Omega/avg.Exp)$	0.754*** (0.193)	0.504*** (0.139)			0.274 (0.274)	0.245 (0.221)		
$(B-avg.B) \cdot (\Omega/avg.B)$	-0.102 (0.232)	-0.209 (0.202)			0.0531 (0.571)	-0.0119 (0.479)		
$(ATM-avg.ATM) \cdot (\Omega/avg.ATM)$	0.555 (0.568)	0.200 (0.518)			0.319 (0.606)	0.160 (0.779)		
$(S-avg.S) \cdot (\Omega/avg.S)$			-0.725*** (0.219)	-0.455*** (0.142)			-0.474* (0.256)	-0.429** (0.209)
$(B-avg.B) \cdot (\Omega/avg.B)$			0.282 (0.203)	0.361* (0.210)			0.0319 (0.526)	0.0991 (0.453)
$(ATM-avg.ATM) \cdot (\Omega/avg.ATM)$			-0.868 (0.752)	-0.398 (0.692)			-0.301 (0.735)	-0.126 (0.955)
Constant	0.00489 (0.00309)	0.00613** (0.00280)	0.00298 (0.00247)	0.00522** (0.00242)	0.00726 (0.00532)	0.00378 (0.00513)	0.00657 (0.00497)	0.00300 (0.00481)
s.e. of regression	0.0204	0.0139	0.0213	0.0149	0.0154	0.0135	0.0152	0.0128
log(likelihood)	161.09	178.68	158.72	176.43	53.95	56.26	54.26	56.79
R-squared	0.2662	0.2186	0.2885	0.2160	0.0705	0.0410	0.1522	0.1142
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE
Observations	65	64	65	64	20	20	20	20
Number of countries	24	24	24	24	11	11	11	11
Test: chi-squared distribution	23.3092	25.6300	21.4550	24.8392	2.1822	2.6195	4.8505	5.7021
Test: p-values	0.0000	0.0000	0.0001	0.0000	0.5355	0.4541	0.1831	0.1270

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

<u>Notes:</u> Ω is the composite fiscal variable solely derived by sample average values. EMU is considered as individual unit, constructed by weighted averages of its member states in the respective year. EMU member states are only considered individually as long as they are not members of the union. GLS estimation for unbalanced panel with random effects.

The results are less clear for the JST quasi-subsample. The coefficients for excess government spending are not statistically significantly different from zero; β s is only different from zero at a 5% or 10%-level. The different results are confirmed by a Wald test with chi-squared distribution (last two lines in Table 6).¹² Based on the Wald test in columns (1) to (4), we reject the null hypothesis that β s, β B, and β ATM are the same in absolute values. At least one coefficient (the government spending coefficient) is thus different from the other two coefficients. We cannot reject this null hypothesis for systematic banking crises (Table 6, columns (5) to (8)).

Panel estimation

Lastly, we replicate and extend the balanced panel estimation approach by Barro and Bianchi (2023). Like in Table 2, the replication results in Table 7 are very close to the original and only vary slightly. Almost 50 percent of excess government expenditure is inflated away in real terms, proximity to the Russian or Ukrainian border has a highly significant positive effect and the effect size is between 8.5 percent in the Barro and Bianchi estimation and 7.5 percent in our own estimation. The estimation results for core inflation are equally similar.¹³

	(1)	(2)	(3)	(4)
	СРІ	CPI	core CPI	core CPI
CEV	0.485*** (0.0612)		0.406*** (0.0436)	
CSV		-0.657*** (0.0686)		-0.507*** (0.0493)
Russian/ Ukrainian border	0.0772*** (0.00923)	0.0735*** (0.00901)	0.0394*** (0.00657)	0.0379*** (0.00649)
Constant	0.0205*** (0.00114)	0.0218*** (0.00111)	0.0182*** (0.000811)	0.0192*** (0.000799)
s.e. of regression	0.0176	0.0170	0.0125	0.0123
log(likelihood)	720.11	680.77	811.97	763.71
R-squared	0.384	0.451	0.363	0.416
Observations	270	252	270	252

Table 7: Replication of balanced panel estimations in Barro and Bianchi (2023)

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

<u>Notes:</u> Columns (1) and (3) replications with using the composite expenditure variable. Differences in the results may be caused by the neglection on observations for Costa Rica due to data availability. Columns (2) and (4) are modifications with the composite surplus alternative. The years 2020 to 2022 are coded as crisis years, expected inflation π^* is calculated by average inflation between 2010 and 2019, OLS estimation with fixed effects. The respective results in Barro and Bianchi (2023) stem from the paper version revised in March 2024.

¹² In contrast to Barro and Bianchi (2023), the Wald test is based on a chi-squared distribution not an F-distribution since we apply this test to a GLS with random effects and not an OLS as Barro and Bianchi (2023) do.

¹³ All comparisons in results and methodology refer to Barro and Bianchi (2023), revised March 2024.

The results for CSV and CEV in Table 8 are only statistically different from zero when fiscal rule and peg controls are considered as well. Interpreting these findings is in any case difficult due to the caveats of observational equivalence, discussed in Section 1. When we use recessions as crisis indicator, the results do, however, support our findings from the baseline unbalanced panel estimation. CSV and CEV coefficients are significantly different from zero, but the coefficient sizes are much larger than the results in Table 5 and A-2 (Table 8, columns (1) to (8)).

The JST crisis quasi-subsample is less convincing, the coefficients for both fiscal variables of interest are either not significantly different from zero (when estimated without controls) or larger than one. The latter is implausible since the tractable functional form of the intertemporal budget function aims at estimating the fraction of excess government spending that is inflated away. Besides theoretical concerns, e.g., observational equivalence, the panel estimation could also suffer from too few observations of systematic banking crises. While the panel dataset contains 229 observations of recessions, its only 99 systematic banking crisis observations (on more than 1,000 total observations). Given these problems, we discard balanced panel estimations with systematic banking crisis as crisis identifier for the remainder of this paper.

While the coefficient signs are consistent with unbalanced panel estimation results for CSV and CEV, the coefficient signs turn from positive to negative (consistently and robustly) for both institutional variables, monetary peg and fiscal rules. Given the methodological concerns raised above, these results should not be overstated but a negative effect of fiscal rules on inflation is in a FTPL context more intuitive and in line with the fiscal rule literature than the positive effect from Table 5. If anything, this result strengthens the conclusion that the effect of fiscal rules on inflation in a non-Ricardian framework remains puzzling.

<u>Table 8:</u> Panel estimation with fixed effects for the full observation period (1980 - 2023)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				reces	ssion			
	СРІ	CPI	CPI	CPI	core CPI	core CPI	core CPI	core CPI
CEV	0.130 (0.287)		0.735*** (0.211)		-0.0721 (0.271)		0.516*** (0.186)	
CSV		-0.146 (0.292)		-0.916*** (0.213)		0.106 (0.279)		-0.640*** (0.192)
fiscal rule			-0.0359*** (0.00825)	-0.0362*** (0.00843)			-0.0400*** (0.00857)	-0.0402*** (0.00876)
currency peg			-0.0205** (0.00964)	-0.0206** (0.00956)			-0.0260** (0.0108)	-0.0260** (0.0108)
Constant	0.0408*** (0.000977)	0.0408*** (0.000854)	0.0526*** (0.00570)	0.0526*** (0.00557)	0.0367*** (0.000936)	0.0367*** (0.000827)	0.0548*** (0.00627)	0.0548*** (0.00618)
s.e. of regression	0.0512	0.0512	0.0323	0.0322	0.0380	0.0379	0.0277	0.0276
log(likelihood)	1937.38	1937.42	2153.51	2155.76	2288.76	2288.80	2279.44	2279.97
R-squared	0.0003	0.0002	0.0688	0.0697	0.0001	0.0003	0.0836	0.0834
individual-specific effects	FE							
Observations	1,255	1,255	1,094	1,094	1,241	1,241	1,085	1,085
Number of countries	37	37	35	35	37	37	35	35

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)		
		JST crisis								
	CPI	CPI	CPI	СРІ	core CPI	core CPI	core CPI	core CPI		
CEV	0.477 (0.391)		2.703*** (0.385)		-0.490 (0.446)		1.849*** (0.394)			
CSV		-0.571* (0.326)		-2.792*** (0.356)		0.377 (0.387)		-1.965*** (0.368)		
fiscal rule			-0.0346*** (0.00843)	-0.0355*** (0.00847)			-0.0390*** (0.00917)	-0.0398*** (0.00921)		
currency peg			-0.0209** (0.00944)	-0.0215** (0.00930)			-0.0262** (0.0107)	-0.0266** (0.0106)		
Constant	0.0410*** (0.000212)	0.0409*** (0.000197)	0.0533*** (0.00543)	0.0537*** (0.00532)	0.0367*** (0.000245)	0.0367*** (0.000236)	0.0553*** (0.00609)	0.0556*** (0.00601)		
s.e. of regression	0.0512	0.0512	0.0323	0.0322	0.0379	0.0379	0.0277	0.0276		
log(likelihood)	1937.27	1937.40	2147.75	2149.68	2289.11	2288.96	2274.93	2275.62		
R-squared	0.000	0.001	0.128	0.135	0.001	0.001	0.203	0.208		
individual-specific effects	FE									
Observations	1,255	1,255	1,094	1,094	1,241	1,241	1,085	1,085		
Number of countries	37	37	35	35	37	37	35	35		

Robust standard errors in parentheses

*** *p*<0.01, ** *p*<0.05, * *p*<0.1

<u>Notes:</u> Extensions by controlling for the presence of a currency peg and fiscal rules. All countries are considered individually, no EMU unit. Presence of a monetary union is addressed by controlling for a strict monetary peg.

Given the unsatisfying results for the JST crises sample, the notion persists that investigating the FTPL in a time series model is not convincing. The balanced panel estimation alternative briefly presented by Barro and Bianchi (2023) is at best partially applicable with a careful selection of a suitable crisis definition criterion, but the approach is not generally applicable.

Coefficients greater one

We close this section by discussing the consequences of coefficients of the fiscal composition variable that are not statistically significantly different from one. In the panel estimation (Table 8, columns (11), (12), (15), and (16)) we can even observe coefficients greater than one and in some other cases a Wald test fails to reject the hypothesis that the observed coefficient is different from one (e.g. Table 4, columns (1) and (7) and later in some robustness checks). This is worth mentioning because β in eq. (7) estimates the average fraction of excess government surplus (expenditure) that is subject to non-Ricardian fiscal policy. Fractions larger than one are implausible. One possible explanation could be some sort of "overshooting", meaning that once a certain threshold in fiscal policy is exceeded markets adjust their expectations about the government's ability to pay back its debt at some point in the future not only regarding the crisis-related fiscal expansion but beyond it. While the results presented here can only be suggestive for this line of thought, the respective literature points in a similar direction.

The debt level can be viewed as sustainable for a long time but suddenly turn into the bad outcome that is supposed to be avoided (default or sharp surges of inflation). The Ricardian literature aims at specifying fiscal limits. The fiscal limit's distribution is country-specifically determined and state contingent. It considers parameters like economic fundamentals, degree of countercyclical fiscal policy and policy behavior more generally. The fiscal limit distribution defines risk premia on government bonds and once they begin to rise they increase rapidly, increasing the default risk substantially (Bi, 2012; Bi and Leeper, 2013; Codogno and Corsetti, 2022). In the non-Ricardian, FTPL framework this sudden turn can be modeled as a bursting bubble, potentially triggered via an information channel that is neglected by investors most of the time. Even in a Ricardian setting, investors may fail to consider the intertemporal budget constraint properly and thereby overprice the value of government bonds (Brunnermeier et al., 2022; Bassetto and Miller, 2022; Jiang et al., 2024a, b).

5. Robustness Checks

For the robustness checks we continue with the same procedure: first we replicate the robustness checks proposed by Barro and Bianchi (2023) and apply them to our extended dataset (endogeneity test, restricted model alternatives, altering independent variables). Then we expand the battery of robustness checks with additional tests on outliers and reverse causality.

Endogeneity

The interaction between GDP growth and inflation could potentially affect our estimations in two different ways. First, variation in fiscal variables could be driven by variation in the respective denominator. Second, GDP growth and inflation on one side as well as GDP growth and fiscal expansion on the other side could correlate over a business cycle.

A first look at the correlation results eases these concerns. The average aggregated GDP growth over M periods long recessions (systematic banking crises) correlates with the CSV 0.18 (0.30) and the CEV -0.02 (-0.14), respectively. When we decompose CSV and CEV, the correlation coefficient for the average aggregated GDP growth rates over crises with the individual components never exceeds 0.4. To deepen the test of this potential distortion, we follow the Barro-Bianchi-approach and estimate the effect of GDP growth on our variables of interest, CSV and CEV. Table A-3 provides estimation results for the average effect of cumulated GDP growth during a crisis on cumulated surplus (expenditure) growth and on CSV (CEV) respectively. Barro and Bianchi (2023) want to know whether the economic decline from the years 2019 to 2020 resulted in a cumulative expenditure increase between 2020 and 2022. We cannot replicate this approach exactly in a multi-crises sample since we do not know when the most severe economic decline is observable and how long it takes the government to respond with expansionary fiscal policy. Instead, we lag the GDP growth rate by one period, indicating that fiscal policy reacts to economic development. Our results follow the findings from Barro and Bianchi (2023) for the COVID19 crisis.

The GDP growth rate (lagged) has no systematically significant effect on fiscal variables. The highly significant positive effect on primary surplus (Table A-3, column (3)) is in line with our theoretical concerns and emphasizes the need for this robustness check. The same holds for estimating CSV in the JST crises sample. While the significant effect of lagged GDP growth on the numerator of our composite fiscal variable is not too worrying, the significant effect of GDP growth on CSV is a proper, yet isolated, caveat for our empirical results. Barro and Bianchi (2023) rationalize the non-findings with the negative association of pre-crisis debt (as share of GDP) with GDP growth. Considering the pre-crisis debt-to-GDP-ratio in the denominator of

the composite fiscal variables has an inverse effect, mitigating the relationship between economic decline and fiscal expansion.

Additionally, we expand the baseline estimation from Table 3 by cumulated GDP growth rates. The results are reported in Table A-4 in the Appendix. While GDP growth has a statistically significant positive effect on the difference between observable and expected inflation, the changes in coefficients' sizes and significance levels for CSV and CEV are minor when recessions define crises (Table A-4, columns (1) to (4)). When systematic banking crises constitute the sample, the absolute values for CSV and CEV coefficients decrease substantially, but they are still within a reasonable margin. Only CEV has no longer a significant explanatory effect on changes in CPI during systematic banking crises. The cumulated GDP growth rate turns from a positive sign to a negative sign in the systematic banking crises and it is less consistently significantly different from zero.

The overall consistent coefficients for both composite fiscal variables are in line with the findings from Barro and Bianchi (2023) for the COVID19 crisis. Due to the overall consistent CSV and CEV coefficients we follow Barro and Bianchi's conclusion that estimation results of CSV and CEV do not proxy general economic conditions.

Restricted model alternatives

Composite surplus and composite expenditure variables are directly derived from the basic FTPL approach (part I in Cochrane, 2023). The results for isolated fiscal drivers on inflation in Section 4 clearly showed, that higher than expected inflation is predominantly caused by variation in excess government spending, while the level of debt and average time to maturity prior to the crisis are less informative. To reevaluate these findings and to assess the question whether variation in all three fiscal components is actually superior for evaluating fiscal roots of inflation, we compare unrestricted and restricted model alternatives.

We follow the two-step procedure proposed by Barro and Bianchi (2023). First, we re-estimate the baseline estimation by restricting one element at a time in the composite fiscal variable to its sample mean. We test whether the estimated coefficients are still significantly different from zero when the respective parameter cannot add any variation to CSV (CEV) observations. Secondly, we compare the new results with the respective baseline results from Table 3 and apply a Likelihood Ratio (LR) test to determine whether exclusion of the respective variation worsens the model fit substantially. If the LR is statistically significantly higher than the chi square distribution threshold for one degree of freedom (since we only restrict one fiscal parameter at

a time), we reject the hypothesis that keeping the respective fiscal parameter constant would not worsen our estimation. Table A-5 in the Appendix reports the results.

The results show some interesting variation compared to the findings from Barro and Bianchi (2023) for the isolated COVID19 crisis. When we restrict the pre-crisis debt level or average time to maturity to the respective sample mean, CSV (CEV) coefficients remain statistically significantly different from zero in all specifications. However, the coefficient is no longer significantly different from zero anymore when we put the numerator (cumulated excess government surplus (expenditure)) to its sample mean. This finding is robust, consistent across all specifications and confirms our findings for isolated fiscal drivers of inflation in Section 4.

Interestingly and in contrast to Barro and Bianchi (2023) the LR test is partially supporting the hypothesis that a restricted model is not detrimental to the unrestricted model. When crises are defined by recessions, constant ATM or constant pre-crisis debt levels do usually not worsen the estimation results compared to the unrestricted model (columns (2), (3), (5), (6), (14), (15), and (18) in Table A-5 in the Appendix). The results by Barro and Bianchi (2023) suggest that restricting any fiscal parameter to its sample mean worsens the estimation. In these cases, the log likelihoods are larger than the values in the unrestricted model (Table 3). The high p-values for the respective LR test suggests that keeping the respective fiscal parameter constant would not worsen the model fit. However, this is hard to tell with an LR test when the log likelihood is larger in the restricted than the unrestricted model.

The relative likelihood of the Akaike Information Criterion (AIC) addresses this issue, and its results shed some new light on the restricted model alternatives. Keeping the numerator constant is worsening the model's quality, the AIC relative likelihood is close to zero. A model with constant pre-crisis debt or ATM is, however, in some cases actually preferable to the unrestricted model. The high AIC relative likelihood values (greater than one) indicate that these models have a better fit than the unrestricted alternative. The results for JST crises are in this regard particularly puzzling. While unrestricted models perform worse when estimating CPI variation (columns (7) to (12)), the unrestricted models are favorable when estimating core inflation variation (columns (19) to (24)).

Overall, there is, however, no consistent evidence that some restricted models would be favorable. Besides well-founded doubts for pre-crisis government debt and average time to maturity, all three fiscal parameters contribute some explanatory value to the CSV (CEV), especially since we consider multiple alternatives as crises selection criteria and variables of interest. We find not enough evidence to alter our baseline model.

Alter independent variable

The substantial explanatory power of cumulated excess government surplus (expenditure) for isolated estimations and in restricted models provokes the question as to which degree the measured effect is rather an indication for Keynesian demand effects than evidence for FTPL validity.¹⁴ We address this issue, in accordance with Barro and Bianchi (2023), by substituting the right-hand term of eq. (6) with only the numerator of this term. The results are reported in Table A-6 in the Appendix.

In difference to the results from Barro and Bianchi (2023) for the COVID19 crisis the isolated excess government surplus (expenditure) remains highly significantly different from zero but, in accordance with Barro and Bianchi (2023), the economic effect is much smaller than our findings in Table 5. The coefficient size is on average 3.5 times smaller for excess government surplus (expenditure) compared to CSV (CEV) and at least 1.6 times smaller. The same holds for all estimation results for the JST sub-sample: the significance level remains consistent, but the economic effect is substantially smaller. These results connect the explanatory power of cumulated excess government surplus (expenditure) with FTPL expectations very well. While Keynesian demand effects potentially influence our results and contribute substantially to observable variation, they cannot explain the largest share of our results. Our results suggest stronger FTPL validity than higher inflation via expanded, government-induced demand.

Alter dependent variable

The chosen observation period (1980 to 2023) is favorable for our exercises for multiple reasons. We cover a period with unprecedentedly high structural government debt, besides (or leading to) a high concentration of fiscal rule introductions. This observation period includes multiple forms of strict currency pegs which impose various restrictions on central banks' room for maneuver. The most consequential form is the centralized monetary and de-centralized fiscal policy in EMU which is founded and established during our observation period. The downside of our observation period, however, is that inflation was only of minor relevance in many OECD countries during this time. This observation period neither includes uncontrolled sharp inflation spikes like they have been common in the first half of the 20th century, nor persistently high inflation rates like in the 1970s (Bordo and Levy, 2021; Cochrane, 2022a; Schmitt-Grohé and Uribe, 2022 and Sims, 2024). The period between 1980 and 2023 has instead been characterized by little variation in inflation, structurally declining interest rates and perhaps, as some

¹⁴ Briodeau and Checherita-Westphal (2024) investigate how high inflation rates affect fiscal policy. Below certain thresholds they identify decreasing (increasing) effects of inflation on government expenditure (revenue), but once these thresholds are exceeded, government expenditure increases beyond inflation rates and revenue declines.

even argue, secular stagnation (Summers, 2014, 2015; Rachel and Summers, 2019). In this setting, it is an open question by which degree (low) inflation is inherently dynamic with past inflation shaping today's price level. To test this effect on our baseline results we alter the estimation function by estimating only average observable inflation during the crisis (π) and consider observed inflation in non-crisis years (π^*) as additional independent variable. The results are reported in Table A-7 in the Appendix.

Expected, previously observed inflation has in all specifications a highly significant positive effect on explaining observable inflation during the crises. Coefficient sizes are between 0.51 and 0.64 and, thus, substantially lower than the unit coefficients found by Barro and Bianchi (2023) for the effect of inflation between 2010 and 2019 on inflation during the COVID crisis. While trend inflation had a unit coefficient effect on inflation during the COVID crisis the trend effect is on average only half as large when we expand the observation period to 1980 to 2023. Like in Barro and Bianchi (2023) the coefficient size and significance level are very similar for estimating CPI and core inflation and the results are robust for recessions and JST crises alike. Substituting the dependent variable $\pi - \pi^*$ for only π and adding π^* as an additionally independent variable has almost no effect on coefficient sizes and significance levels for CSV and CEV. In this specification R2 increases strongly as compared to the baseline results in Table 3 and 5, indicating that considering trend inflation improves the model's explanatory power substantially. After these four robustness checks, replicated from Barro and Bianchi (2023), we conclude this chapter by adding three additional tests to the battery of robustness checks.

Excluding major crises

One main motivation for this paper was to investigate whether the findings from Barro and Bianchi (2023) are externally valid beyond the COVID19 case or whether inflation is only describable as an FTPL phenomenon in unique historical events like the pandemic. Due to the extension of the observation period our sample size for recessions more than tripled compared to the analysis in Barro and Bianchi (2023). However, since the pandemic came with a severe recession in all countries considered, our estimations contain the same observations as the one in Barro and Bianchi (2023)¹⁵ and it is an open question to what degree our results are driven by this unique event. More broadly, it is an open question as to how much global crises, like COVID19 or the Great Financial Crisis (GFC), affect our results. In Table A-8 in the Appendix we present the results after excluding one of the two large crises.

¹⁵ Roughly one third (half) of all recession (JST crisis) observations are COVID-observations.

Observations from the pandemic play a crucial role for our overall results. As column (1) to (4) in Table A-8 show, almost all fiscal coefficients are no longer statistically significantly different from zero and the very low R2 values indicate the disappearance of any explanatory power of the model. CSV has no longer significant explanatory power when crises are identified by recessions, excluding COVID19. Around 20 percent of CEV is inflated away due to higher core inflation rates but we cannot find equally significant effects for CPI. The results are equally weak for JST crises (column (9) to (12)) but the reactions in core inflation to fiscal expansions are strengthened.¹⁶ It seems as if without the COVID19 observations core inflation might be better suited to identify FTPL evidence than CPI.

CPI and core CPI differ by the exclusion of food and energy prices. When comparing differences between these two variables, the right-hand side of the estimation function does not change. It seems as if the variation in food and energy prices during COVID19 (and, probably more importantly, Russia's war against Ukraine) substantially contributed to variation in the π – π^* dependent variable for headline inflation. Eliminating these observations from the sample leads to non-findings in CPI estimations. Core inflation, however, is not affected by food and energy price variations in recent years, providing an overall less volatile measurement of the price level. Core inflation observations are so consistent that taking one crisis (e.g. the COVID19 pandemic) out of the estimation is not discarding our results.

It is furthermore explicitly the COVID19 crisis that contains these anomalies. When we exclude observations from the GFC (Table A-8, column (5) to (8) and (13) to (16)), the coefficients are significantly different from zero again, and the coefficient sizes are similar to the ones from the baseline estimation in Table 3.

We considered re-estimating the model by excluding both major crises at the same time. Especially the sample size for systematic banking crises becomes so small (between nine and six observations) that we discarded the idea that this exercise could be of any informative value.

Jackknife test

In addition to discretionarily excluding observations from specific crisis, we apply a Jackknife test to more generally control for outliers. Table A-9 in the Appendix provides results for the Jackknife test with respect to the baseline results from Table 5. The results are basically unchanged, indicating that outliers do not drive the overall results. This is not surprising since we already justified in Section 3 that we winsorize the sample at the 5th and 95th percentile to

¹⁶ We discuss our thoughts for explaining coefficients greater than one at the end of Section 4.

eliminate massive turmoil observations. During these times, we cannot rule out all different sorts of confounders which may affect our variables of interest.

Reverse Causality

Keynesian demand effects of expansionary fiscal policies can theoretically explain that higher public spending causes higher inflation. We considered this alternative above when we altered the variable of interest and found limited empirical evidence for this channel. Causality could, however, also go the other way. Higher inflation raises, among other things, prices for government consumed goods and costs in the welfare system rise in accordance with inflation. Since crises last for M periods and, in our identification strategy, changes in fiscal policy are only considered as cumulated values, we cannot rule out this kind of reverse causality.

The cumulated excess government surplus (expenditure) on the right-hand side as well as the average values that define π and π^* on the left-hand side of eq. (6) are, however, in the unbalanced panel inapplicable for intertemporal shifts which are usually applied to test for reverse causality. Cumulated or average values would either, for large M, not change the observable values (substantially), even with an intertemporal shift over multiple periods or, for low M, even a one-year shift could distort the average value substantially. Capturing intertemporal shifts properly is further complicated by the baseline unbalanced panel estimation. Therefore, we do not test reverse causality in the baseline framework but rely instead on the panel estimation alternative (with its well-known disadvantages) and put the results in perspective to these estimations. We re-estimate the panel estimation from Table 8 with lagged independent variables (prices in t cannot affect public costs in t-1).

The results are displayed in Table A-10 in the Appendix. The results in absence of control variables for fiscal rules and currency pegs (Table A-10, columns (1), (2), (5), and (6)) suggest that reserve causality might be an issue. However, the respective coefficients were also insignificantly different from zero in the panel baseline estimation in Table 8. So, we cannot draw any conclusion from estimations without considering fiscal rules and currency pegs.

The preservation of high significance levels and economically relevant effects in columns (3), (4), (7), and (8) suggest that reverse causality is *not* a major issue. Fiscal variation in t-1 cannot be affected by higher prices in t, thus these significant findings suggest that variation in fiscal parameters drive inflation and not the other way around. The effect size is smaller when using lagged fiscal variables, but still substantial. On average between 32 and 76 percent of lagged excess fiscal policy is inflated away. The effect size is between 52 and 92 percent for balanced panel estimations with fixed effects in Table 8. The consistently low R^2 values indicate that

estimating any interaction with lagged fiscal variables is not expedient. As discussed for panel estimations in Section 4, we forego to consider any further panel estimations for the JST subsample since the respective results were already poor in the regular panel estimation.

While these results for reverse causality are encouraging, the caveats of using balanced panel estimations for testing FTPL validity must be kept in mind. We have discussed these caveats in Section 1 and 4. Comparing the results for unbalanced panel estimations (Table 5) and panel estimations (Table 8), emphasize these problems. This is, after all, the reason why Barro and Bianchi (2023) proposed the theory driven empirical exercise that is in the center of this paper.

The procedure for deriving the composite fiscal variables makes common placebo tests impractical. CSV (and CEV) as derived in eq. 6 portray fiscal performance in times of crisis *relative* to non-crisis times. A classical placebo test would assign the treatment to a not-treated point in time in order to assess whether the estimated effect remains significant. If that is the case, something else but not the perceived treatment causes the (significant) findings. If we applied this logic to the case at hand, we would construct composite fiscal variables for non-crisis times and estimate the effect on inflation deviation. However, both the composite fiscal variable and the dependent variable $\pi - \pi^*$ would describe the change of the respective observation relative to the time before the non-crisis period began, so relative to observations in the last crisis. Such a variable is impossible to interpret. The issue is persistent for both, balanced and unbalanced panel datasets. The rolling regressions in Barro and Bianchi (2023, pp. 28) address the issue for a single crisis but it is not possible to extend their approach to a multi-crises dataset. In order to roll the regression forward from 2010 to 2022 they implicitly assume that no other crisis affected the sample during this time. While this might be plausible for a single crisis, it is impractical for our case with multiple crises hitting different countries at different points in time.

6. Conclusion

With this paper we contribute an empirical investigation to the fiscal effects on inflation in the light of the Fiscal Theory of the Price Level. The FTPL was first formulated in the 1990s but received more attention lately when record high government debt levels fell together with an unexpectedly sharp inflation surge in the early 2020s. Cochrane (2023) provides a thorough theoretical basis that allows for investigating fiscal effects on the price level in a modern and realistic institutional framework. Due to observational equivalence, it is not trivial to test the FTPL empirically, common time-series approaches usually deny any conclusions regarding the validity of the FTPL compared to mainstream theories.

Barro and Bianchi (2023) offer a new empirical test strategy. They derive a composite fiscal variable from the basic, frictionless FTPL with no rigidities and implement this variable in an OLS model to estimate the fraction of unexpected fiscal expansion that is not financed by orthodox fiscal means (higher revenue or lower expenditures in the future) but is instead inflated away. We take the Barro-Bianchi-approach and ask whether it is, with some minor alterations, applicable to a more general set of crises, i.e., recessions or systematic banking crises.

Our results confirm Barro and Bianchi (2023) but provide additional evidence for the FTPL. Between 35 and 40 percent of higher-than-expected government spending in a recession is inflated away on average. For systematic banking crises the fraction of monetary financing of fiscal expansion is even larger, up to almost 80 percent. The results are primarily driven by lower primary surplus (higher government spending), indicating that it is neither pre-crisis gov-ernment debt nor maturity structure of debt that causes the results. We also show that higher prices are not simply caused by higher (government induced) aggregate demand. The demand effect is much lower than our findings for unsustainable fiscal policy. Moreover, our results are more robust for estimating core inflation deviation, e.g., when we substitute expected inflation by central banks' inflation target or when we exclude COVID19 observations.

We expand Barro and Bianchi (2023) considering institutional settings like the presence of fiscal rules or limited national autonomy in monetary policy because of strict currency pegs or membership in the European Monetary Union. Institutional properties have no substantial effect on the composite fiscal variable. While constrained monetary policy has no robust and consistent effect on fiscally induced inflation, the presence of fiscal rules increases excess inflation. We argue that this finding could be evidence for the fiscal rules' function as focal point for creditors, but the finding requires a deeper empirical investigation by future research.

A wide battery of robustness checks largely confirms our results. We show that evidence for the FTPL is not ruling out mainstream government demand effects on inflation and reverse causality issues do not question our results. Endogeneity is not a major problem for our crisis selection procedure. Future research should test this empirical research strategy for alternative crises samples and investigate more thoroughly the theoretical and empirical reasons for fractions of fiscal expansion financed by inflation greater than one. The fact that our empirical findings are consistent and robust for an observation period with relatively calm inflation in our sample further strengthens the validity of the FTPL. Future research could, however, take prewar data into consideration since this period was regularly characterized by sharp, unexpected inflation shocks followed by a quick return to more moderate inflation rate.

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Appendix

Text A-1: Analytical description of data properties

The empirical identification strategy applied in this paper is new and we had to make some alterations to transform it from a cross-sectional identification strategy to our application with multiple crises per country over a more than forty years observation period. Furthermore, defining the considered crises is a critical aspect of our approach and beyond theoretical reason we also need to ensure that our sample at hand is applicable to the empirical approach suggested by Barro and Bianchi (2023). For these reasons, we analyse in this section the data properties more thoroughly by investigating *endogeneity*, *multicollinearity*, *heteroscedasticity* and *serial correlation* concerns. We apply various tests which are applicable to balanced and unbalanced panels and in absence of a proper instrumental variable (IV).

We use a *Wooldridge Test without IV* and *Mundlak's approach* to test whether our model specifications (models with random effects or fixed effects estimations) suffer from additional endogeneity which we are unable to address with the model alternatives at hand. The Wooldridge Test without IV is applied to fixed effects estimations. This test uses future covariates to estimate today's error term in order to assess whether future observations have an influence on today's estimation results:

(A.1)
$$\hat{u}_{i,t} = \beta X_{i,t} + \gamma X_{i,t+1} + \epsilon_{i,t}$$

If this was the case, the estimation would suffer from endogeneity biases (Wooldridge, 1995). Because of the dynamic estimation, we can only apply this test to the balanced panel of our dataset.

In Mundlak's approach we explicitly add the individual mean of potentially endogenous variables into an estimation with random effects in order to see whether such a (quasi fixed effect) variable still contributes significantly to explaining the variable of interest:

(A.2)
$$y_{it} = \beta X_{i,t} + \gamma \overline{X}_i + \epsilon_{i,t}$$

If this is not the case, the endogeneity concerns can be abandoned (Mundlak, 1978). Both tests show for various specifications no systematic signs for endogeneity problems. Neither CSV nor CEV estimations show any noticeable problems in balanced and unbalanced panel estimations for both, random and fixed effects specifications.¹⁷

¹⁷ All results reported and described here refer to (if not explicitly named differently) balanced and unbalanced panel datasets for the full observation period (1980 to 2023). The unbalanced panel dataset is composed with recessions as crisis identifier. All results will be made available upon request.

To assess the potential issue of perfect multicollinearity we first estimate our model in a pooled OLS regression and then derive the *Variance Inflation Factors* (VIF). Additionally, we perform a simple *correlation analysis*. We find no indications for (perfect) multicollinearity with VIF never higher then 1.9 and all relevant correlation coefficients are lower than 0.4.

Heteroscedasticity and serial correlation should in our case probably be an issue we need to account for. We use a modified *Wald Test* to identify heteroscedasticity in our data (Greene, 2000 pp. 153 and 598; Baum, 2001). Across all different model specifications, we reject the null hypothesis that the error term for every unit *i* equals the error term for the entire sample, thereby revealing the heteroscedastic properties of our data. Given the (unbalanced) panel structure it is very likely that heteroscedasticity comes with serial correlation. We test serial correlation with a *Breusch-Godfrey Test* and then, more explicitly, test additionally our random effects specification with a *Baltagi-Wu LBI Test* and the fixed effects specification with a *AR(1) Autocorrelation Test*.

For the Breusch-Godfrey Test we regress lagged residuals on residuals in the balanced panel (Godfrey, 1976; Breusch, 1978; Greene, 2000, pp. 541). We find that yesterday's residuals significantly explain today's residuals across all specifications. The locally best invariant (LBI) test from Baltagi and Wu (1999) for estimations with random effects and the AR(1) Autocorrelation Test for fixed effects confirm these results. All three tests find serial correlation properties in our data, resulting in clustered standard errors on country level in all our specifications.

With no serious endogeneity or multicollinearity problems we can still perform a Hausman Test to determine whether random effects are in our case more efficient or whether we need to consider fixed effects. To address heteroscedasticity and serial correlation properly, we use a robust Hausman test (Hausman, 1978; Greene, 2000, pp. 576; Baum et al. 2003). This approach provides ambiguous results for the balanced panel but shows conclusively for the unbalanced panel that fixed effects are overidentifying restrictions and estimating our model with random effects is more efficient. Due to the lack of consistency of random effect in the balanced panel, we only apply random effects to the unbalanced panel but fixed effects for the balanced panel. The latter is consistent with Barro and Bianchi's (2023) approach for panel estimations.

Lastly, we are concerned that the datasets might be too small to meet the asymptotic assumptions of the Hausman Test. This is particularly worrying when the Hausman Test compares different model specifications for the unbalanced panel dataset. We address this last concern with a bootstrapping extension for the Hausman Test with 10,000 repetitions (Henderson et al., 2008; Amini et al., 2012; Bole and Rebec, 2013). The results from the robust Hausman Test

persist in this extension as well, confirming fixed effects for balanced and random effects for unbalanced panel estimations.



Graph A-1: Visualization of fiscal rule index (Davoodi et al., 2022)

<u>Notes:</u> Development of de jure fiscal constraints over time (1985 to 2021) for the G7 and Switzerland. Overall index in Davoodi et al. (2022), standardized on a scale between 0 and 1. EMU line depicts equally weighted average across all 17 EMU member states considered in this dataset.

all observations	Obs.	Mean	Std.Dev.	Min	Max
annual inflation growth rate (in %)	1,255	4.1%	5.6%	-4.5%	84.0%
annual core inflation growth rate (in %)	1,241	3.6%	4.1%	-4.3%	32.9%
central bank inflation target (in %)	1,256	2.1%	0.6%	0.0%	7.0%
annual real GDP growth rate (in %)	1,216	2.4%	2.9%	-10.9%	24.6%
gov. primary balance (in % of GDP)	1,236	0.6%	4.0%	-28.2%	25.7%
gov. revenue (in % of GDP)	1,256	40.6%	9.2%	12.2%	63.9%
gov. primary expenditure (in % of GDP)	1,241	40.1%	8.8%	8.6%	65.1%
gov. interest expenditure (in % of GDP)	1,236	3.2%	2.3%	-4.5%	12.7%
gov. debt (in % of GDP)	1,241	62.6%	38.0%	6.2%	261.3%
average maturity of gov. debt	1,006	6.1	2.4	0.4	20.0
Fiscal rule (index variable)	1,113	0.4	0.3	0.0	1.0
Strict monetary peg (dummy variable)	1,238	0.4	0.5	0.0	1.0
observations considered in the estimation with recession = 1					
annual inflation growth rate (in %)	361	5.4%	7.1%	-4.5%	84.0%
annual core inflation growth rate (in %)	356	4.4%	4.7%	-4.3%	32.9%
central bank inflation target (in %)	362	2.1%	0.6%	0.0%	6.5%
annual real GDP growth rate (in %)	348	0.4%	3.7%	-10.9%	16.3%
gov. primary balance (in % of GDP)	356	-0.9%	4.1%	-12.1%	25.7%
gov. revenue (in % of GDP)	362	40.7%	9.3%	13.0%	63.9%
gov. primary expenditure (in % of GDP)	359	41.7%	9.0%	8.6%	65.1%
gov. interest expenditure (in % of GDP)	356	2.8%	2.3%	-4.5%	12.7%
gov. debt (in % of GDP)	359	70.4%	45.2%	7.4%	261.3%
average maturity of gov. debt	297	6.7	2.6	0.4	20.0
Fiscal rule (index variable)	319	0.5	0.4	0.0	1.0
Strict monetary peg (dummy variable)	355	0.5	0.5	0.0	1.0
$observations\ considered\ in\ the$ $estimation\ with\ JST\ crisis\ =\ 1$					
annual inflation growth rate (in %)	169	4.8%	4.0%	-1.2%	19.7%
annual core inflation growth rate (in %)	169	3.8%	2.8%	-0.7%	13.8%
central bank inflation target (in %)	170	2.1%	0.5%	0.0%	3.0%
annual real GDP growth rate (in %)	158	1.6%	4.6%	-10.9%	16.3%
gov. primary balance (in % of GDP)	167	-1.6%	4.1%	-11.4%	25.7%
gov. revenue (in % of GDP)	170	40.3%	9.3%	15.3%	63.9%
gov. primary expenditure (in % of GDP)	170	42.0%	9.4%	13.7%	60.1%
gov. interest expenditure (in % of GDP)	167	1.8%	1.7%	-4.5%	10.1%
gov. debt (in % of GDP)	170	72.8%	46.0%	17.0%	261.3%
average maturity of gov. debt	158	7.5	2.5	0.4	14.8
Fiscal rule (index variable)	165	0.6	0.4	0.0	1.0
Strict monetary peg (dummy variable)	163	0.5	0.5	0.0	1.0

Table A-1: Descriptive statistics for all observation, recession and banking crisis observations

Notes: All observations gathered from 37 out of 38 OECD countries (all countries except Turkey due to data quality) between 1980 and 2023.

Table A-2: Extended baseline estimation for the full observation period (1980 – 2023)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				reces	ssion			
	core CPI	core CPI	core CPI	core CPI	core CPI	core CPI	core CPI	core CPI
CEV	0.202** (0.0905)		0.165* (0.0955)		0.184** (0.0907)		0.129 (0.0917)	
CSV		-0.281*** (0.0804)		-0.243*** (0.0909)		-0.269*** (0.0821)		-0.213** (0.0871)
fiscal rule			0.0153*** (0.00565)	0.0143*** (0.00458)			0.0208*** (0.00674)	0.0191*** (0.00548)
currency peg					0.00261 (0.00394)	-0.000615 (0.00309)	-0.00918** (0.00442)	-0.00960** (0.00393)
Constant	0.00295* (0.00178)	0.00244 (0.00160)	-0.00287 (0.00275)	-0.00325 (0.00254)	0.00163 (0.00287)	0.00233 (0.00230)	-0.00124 (0.00235)	-0.00115 (0.00244)
s.e. of regression	0.0142	0.0141	0.0129	0.0127	0.0142	0.0144	0.0141	0.0139
log(likelihood)	277.94	280.19	271.87	276.33	278.02	277.05	266.53	270.69
R-squared	0.0686	0.1773	0.1434	0.2304	0.0591	0.1742	0.1780	0.2741
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE
Observations	99	99	94	95	98	98	93	94
Number of countries	34	34	33	33	33	33	32	32

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
				JST	crisis			
	core CPI	core CPI	core CPI	core CPI	core CPI	core CPI	core CPI	core CPI
CEV	0.335*** (0.111)		0.322*** (0.108)		0.338*** (0.115)		0.296*** (0.104)	
CSV		-0.394*** (0.135)		-0.349** (0.153)		-0.399*** (0.131)		-0.327*** (0.120)
fiscal rule			0.0135*** (0.00493)	0.0117** (0.00534)			0.0325*** (0.00798)	0.0312*** (0.00894)
currency peg					-0.00142 (0.00472)	-0.00254 (0.00384)	-0.0189*** (0.00413)	-0.0189*** (0.00437)
Constant	0.00343 (0.00271)	0.00240 (0.00204)	-0.00363 (0.00384)	-0.00331 (0.00355)	0.00414 (0.00505)	0.00373 (0.00370)	-0.00232 (0.00424)	-0.00226 (0.00374)
s.e. of regression	0.0097	0.0104	0.0086	0.0089	0.0095	0.0102	0.0067	0.0072
log(likelihood)	108.84	107.42	112.95	112.43	109.61	108.20	119.86	118.76
R-squared	0.1791	0.2473	0.2924	0.3225	0.1829	0.2593	0.5360	0.5650
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE
Observations	34	34	34	34	34	34	34	34
Number of countries	18	18	18	18	18	18	18	18

Robust standard errors in parentheses

*** *p*<0.01, ** *p*<0.05, * *p*<0.1

<u>Notes:</u> Crisis identifier recession (upper table) and systematic banking crisis (lower table), core inflation. Extensions by controlling for the presence of a currency peg and/ or fiscal rules. All countries are considered individually, no EMU unit. Presence of a monetary union is addressed by controlling for a strict monetary peg. GLS estimation for unbalanced panel with random effects.

Table A-3: Robustness Check - Endogeneity test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		recess		JST crisis				
	primary expenditure	CEV	primary surplus	CSV	primary expenditure	CEV	primary surplus	CSV
cumulated GDP growth	-0.204 (0.263)	-0.0141 (0.101)	2.732*** (0.391)	0.111 (0.0828)	-0.680 (0.419)	-0.136 (0.0972)	-0.632 (0.763)	0.176** (0.0775)
Constant	0.0566*** (0.0102)	0.0154*** (0.00225)	-0.0446** (0.0225)	-0.0142*** (0.00320)	0.0518*** (0.0133)	0.0133*** (0.00298)	0.00518 (0.0405)	-0.0154*** (0.00321)
s.e. of regression	0.0628	0.0194	0.1035	0.0235	0.0713	0.0144	0.1220	0.0133
log(likelihood)	85.68	159.72	53.09	147.73	24.93	57.11	13.03	58.91
R-squared	0.0042	0.0003	0.1788	0.0123	0.0969	0.0953	0.0302	0.1717
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE
Observations	63	63	63	63	20	20	20	20
Number of countries	21	21	22	22	10	10	10	10

Robust standard errors in parentheses

*** *p*<0.01, ** *p*<0.05, * *p*<0.1

<u>Notes:</u> Regression results for the effect of cumulative GDP growth on independent variables from the baseline estimation. EMU is considered as individual unit, constructed by weighted averages of its member states in the respective year. EMU member states are only considered individually as long as they are not members of the union.

Table A-4: Robustness Check - Endogeneity test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		rece	ssion			JST	crisis	
	CPI	CPI	core CPI	core CPI	CPI	CPI	core CPI	core CPI
CEV	0.526*** (0.157)		0.488*** (0.139)		0.357 (0.250)		0.436*** (0.133)	
CSV		-0.417** (0.191)		-0.352*** (0.0933)		-0.501** (0.252)		-0.646*** (0.155)
cumulated GDP growth	0.175* (0.0919)	0.186** (0.0944)	0.217*** (0.0733)	0.170** (0.0667)	-0.417* (0.214)	-0.411** (0.204)	-0.322 (0.202)	-0.345** (0.173)
Constant	-0.00412 (0.00339)	-0.00106 (0.00348)	-0.00156 (0.00243)	0.00191 (0.00241)	0.0122* (0.00727)	0.00972 (0.00733)	0.00548 (0.00595)	0.00213 (0.00611)
s.e. of regression	0.0230	0.0221	0.0151	0.0141	0.0124	0.0119	0.0096	0.0075
log(likelihood)	149.88	150.52	172.68	172.50	57.61	59.01	61.12	64.20
R-squared	0.1848	0.1471	0.2610	0.2289	0.3842	0.4424	0.4312	0.5599
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE
Observations	63	63	62	62	20	20	20	20
Number of countries	21	22	21	22	10	10	10	10

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

<u>Notes</u>: Baseline estimations (Table 3) with additional control for cumulative GDP growth. EMU is considered as individual unit, constructed by weighted averages of its member states at the respective year. EMU member states are only considered individually as long as they are not members of the union. GLS estimation for unbalanced panel with random effects.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	
			rece	ssion			JST crisis						
	СРІ	СРІ	CPI	CPI	CPI	CPI	СРІ	CPI	CPI	CPI	CPI	CPI	
	numerator constant	debt constant	ATM constant	numerator constant	debt constant	ATM constant	numerator constant	debt constant	ATM constant	numerator constant	debt constant	ATM constant	
CEV	-0.0576 (0.0694)	0.603*** (0.186)	0.543*** (0.154)				-0.472 (0.353)	0.371** (0.181)	0.513** (0.236)				
CSV				0.0943 (0.106)	-0.573*** (0.190)	-0.669*** (0.225)				0.585 (0.438)	-0.513** (0.206)	-0.720*** (0.267)	
Constant	0.00578** (0.00251)	-0.00398 (0.00322)	-0.00464 (0.00380)	0.00644** (0.00291)	-0.000993 (0.00342)	-0.00464 (0.00421)	0.0203*** (0.00466)	0.00467 (0.00642)	0.00422 (0.00655)	0.0203*** (0.00466)	0.00314 (0.00601)	0.00138 (0.00592)	
s.e. of regression	0.0249	0.0227	0.0224	0.0252	0.0217	0.0219	0.0137	0.0133	0.0137	0.0137	0.0133	0.0146	
log(likelihood)	147.95	154.88	155.74	147.75	156.94	156.76	56.55	57.08	56.61	56.55	57.41	56.45	
p-value	0.0012	1.0000	1.0000	0.0009	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	
Relative likelihood (AIC)	0.0053	5.4240	12.8477	0.0042	41.3966	34.2622	1.2233	2.0877	1.3003	1.5210	3.5880	1.3753	
R-squared	0.0121	0.1833	0.2059	0.0168	0.1929	0.2044	0.0278	0.1564	0.1721	0.0278	0.2454	0.2954	
Observations	65	65	65	65	65	65	20	20	20	20	20	20	
Number of countries	23	23	23	24	24	24	10	10	10	10	10	10	

Table A-5, part I: Robustness Check - Restricted models, with CPI as dependent variable

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

Notes: EMU is considered as individual unit, constructed by weighted averages of its member states in the respective year. EMU member states are only considered individually as long as they are not members of the union. GLS estimation with random effects.

	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
			rece	ssion			JST crisis					
	core CPI numerator constant	core CPI debt constant	core CPI ATM constant	core CPI numerator constant	core CPI debt constant	core CPI ATM constant	core CPI numerator constant	core CPI debt constant	core CPI ATM constant	core CPI numerator constant	core CPI debt constant	core CPI ATM constant
CEV	0.0487 (0.0457)	0.439*** (0.160)	0.408*** (0.107)				-0.147 (0.428)	0.339** (0.136)	0.436** (0.187)			
CSV				-0.0604 (0.0697)	-0.390*** (0.122)	-0.433*** (0.125)				0.183 (0.531)	-0.491*** (0.159)	-0.626*** (0.214)
Constant	0.00403** (0.00191)	-0.000623 (0.00227)	-0.00152 (0.00238)	0.00543** (0.00260)	0.00236 (0.00238)	0.000150 (0.00262)	0.00982* (0.00563)	0.000918 (0.00549)	0.00100 (0.00580)	0.00982* (0.00563)	-0.000961 (0.00481)	-0.00174 (0.00507)
s.e. of regression	0.0190	0.0171	0.0171	0.0163	0.0152	0.0149	0.0137	0.0108	0.0116	0.0137	0.0102	0.0117
log(likelihood)	163.71	170.48	170.67	171.61	176.29	176.79	57.18	60.09	59.18	57.18	61.17	59.84
p-value	0.0004	1.0000	1.0000	0.0014	0.3538	1.0000	0.0069	0.2216	0.0688	0.0013	0.1273	0.0258
Relative likelihood (AIC)	0.0020	1.7137	2.0792	0.0060	0.6505	1.0721	0.0259	0.4738	0.1911	0.0058	0.3127	0.0833
R-squared	0.0149	0.1649	0.2029	0.0095	0.1750	0.1674	0.0006	0.1342	0.1361	0.0006	0.2483	0.2687
Observations	64	64	64	64	64	64	20	20	20	20	20	20
Number of countries	23	23	23	24	24	24	10	10	10	10	10	10

Table A-5, part II: Robustness Check – Restricted models, with core inflation as dependent variable

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

Notes: EMU is considered as individual unit, constructed by weighted averages of its member states in the respective year. EMU member states are only considered individually as long as they are not members of the union. GLS estimation with random effects.

<u>Table A-6</u>: Robustness Check – Variation in the model – Part I, excess government primary surplus (expenditure) as independent variable

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				rece	ssion			
	СРІ	CPI	CPI	CPI	core CPI	core CPI	core CPI	core CPI
excess prim. expenditure	0.152*** (0.0266)		0.125*** (0.0242)		0.0997*** (0.0198)		0.0787*** (0.0183)	
excess prim. surplus		-0.165*** (0.0332)		-0.142*** (0.0288)		-0.101*** (0.0250)		-0.0814*** (0.0221)
fiscal rule			0.0271*** (0.00708)	0.0240*** (0.00682)			0.0213*** (0.00526)	0.0195*** (0.00516)
currency peg			-0.00921** (0.00457)	-0.00739* (0.00414)			-0.00946** (0.00382)	-0.00824** (0.00357)
Constant	-0.00165 (0.00205)	-0.00138 (0.00175)	-0.00803*** (0.00222)	-0.00806*** (0.00233)	2.14e-05 (0.00185)	0.000464 (0.00174)	-0.00395* (0.00218)	-0.00386* (0.00233)
s.e. of regression	0.0194	0.0193	0.0187	0.0185	0.0148	0.0149	0.0146	0.0146
log(likelihood)	293.56	296.37	276.02	279.53	321.58	322.81	299.46	302.20
R-squared	0.2203	0.2194	0.3510	0.3418	0.1636	0.1454	0.2963	0.2657
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE
Observations	117	118	107	108	116	117	106	107
Number of countries	34	34	32	32	34	34	32	32

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
				JST	crisis			
	СРІ	CPI	СРІ	СРІ	core CPI	core CPI	core CPI	core CPI
excess prim. expenditure	0.0643** (0.0252)		0.0521** (0.0213)		0.0584*** (0.0226)		0.0446*** (0.0163)	
excess prim. surplus		-0.108*** (0.0327)		-0.0847*** (0.0284)		-0.0934*** (0.0271)		-0.0710*** (0.0189)
fiscal rule			0.0280*** (0.0100)	0.0269*** (0.00968)			0.0324*** (0.00679)	0.0322*** (0.00673)
currency peg			-0.000803 (0.00806)	-0.00121 (0.00783)			-0.0160*** (0.00522)	-0.0170*** (0.00530)
Constant	0.00816** (0.00382)	0.00641 (0.00398)	-0.00355 (0.00628)	-0.00444 (0.00668)	0.00204 (0.00397)	0.000858 (0.00408)	-0.00291 (0.00591)	-0.00331 (0.00616)
s.e. of regression	0.0157	0.0155	0.0104	0.0107	0.0137	0.0143	0.0112	0.0119
log(likelihood)	96.48	96.92	109.05	109.58	101.38	99.96	108.73	107.47
R-squared	0.0992	0.1693	0.3720	0.4049	0.0905	0.1515	0.3668	0.4029
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE
Observations	35	35	35	35	35	35	35	35
Number of countries	18	18	18	18	18	18	18	18

Robust standard errors in parentheses

*** *p*<0.01, ** *p*<0.05, * *p*<0.1

<u>Notes</u>: EMU is considered as individual unit, constructed by weighted averages of its member states in the respective year. All countries are considered individually, no EMU unit. GLS estimation for unbalanced panel with random effects.

<u>Table A-7:</u> Robustness Check – Variation in the model – Part II, average inflation in no crises periods as additional control variable and only average inflation in crisis periods as dependent variable

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		rece	ssion			JST	crisis	
	CPI (only crisis)	CPI (only crisis)	core CPI (only crisis)	core CPI (only crisis)	CPI (only crisis)	CPI (only crisis)	core CPI (only crisis)	core CPI (only crisis)
CEV	0.469*** (0.153)		0.382*** (0.125)		0.520*** (0.167)		0.577*** (0.220)	
CSV		-0.296* (0.151)		-0.228* (0.120)		-0.672*** (0.158)		-0.808*** (0.223)
inflation (no crisis period)	0.513*** (0.130)	0.516*** (0.148)	0.521*** (0.139)	0.552*** (0.151)	0.501*** (0.0695)	0.511*** (0.0454)	0.622*** (0.0979)	0.639*** (0.0696)
Constant	0.0117** (0.00505)	0.0158*** (0.00490)	0.0114** (0.00464)	0.0140*** (0.00508)	0.0178*** (0.00285)	0.0152*** (0.00192)	0.00817*** (0.00282)	0.00401 (0.00354)
s.e. of regression	0.0212	0.0218	0.0190	0.0200	0.0071	0.0062	0.0082	0.0055
log(likelihood)	162.16	160.15	169.52	166.07	71.36	74.59	68.54	73.53
R-squared	0.5054	0.4525	0.5656	0.5372	0.7287	0.8279	0.7398	0.8477
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE
Observations	66	66	66	66	21	21	21	21
Number of countries	23	24	23	24	11	11	11	11

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

<u>Notes</u>: EMU is considered as individual unit, constructed by weighted averages of its member states in the respective year. EMU member states are only considered individually as long as they are not members of the union. GLS estimation for unbalanced panel with random effects.

Table A-8: Robustness Check	- Exclude most influential cri	ises, full observation	period (1980 – 2023)
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	drop COVID crisis					drop Great Fi	nancial Crisis	5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				rece	ssion			
	СРІ	СРІ	core CPI	core CPI	СРІ	CPI	core CPI	core CPI
CEV	0.113 (0.0956)		0.202** (0.0863)		0.528*** (0.174)		0.458*** (0.151)	
CSV		-0.0111 (0.0982)		-0.130 (0.128)		-0.423** (0.212)		-0.382*** (0.111)
Constant	-0.00691** (0.00282)	-0.00534 (0.00367)	-0.00355 (0.00220)	-0.00188 (0.00278)	-0.00207 (0.00390)	0.000962 (0.00350)	0.000729 (0.00321)	0.00228 (0.00251)
s.e. of regression	0.0148	0.0156	0.0154	0.0130	0.0245	0.0249	0.0144	0.0161
log(likelihood)	128.76	126.24	127.56	132.97	124.77	125.71	147.44	145.85
R-squared	0.0071	0.0001	0.0519	0.0419	0.1536	0.1490	0.1852	0.2302
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE
Observations	47	47	46	46	54	55	53	54
Number of countries	19	20	19	20	23	24	23	24

		drop CO	VID crisis		drop Great Financial Crisis				
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	
				JST	crisis				
	СРІ	СРІ	core CPI	core CPI	СРІ	CPI	core CPI	core CPI	
CEV	0.635 (0.413)		1.074*** (0.306)		0.653* (0.325)		0.625** (0.263)		
CSV		-0.524 (0.309)		-0.887*** (0.239)		-0.866** (0.274)		-0.894*** (0.240)	
Constant	-0.00141 (0.00682)	-0.00164 (0.00700)	-0.00589 (0.00490)	-0.00630 (0.00522)	0.00109 (0.00780)	-0.00333 (0.00660)	-0.000990 (0.00672)	-0.00623 (0.00547)	
s.e. of regression	0.0188	0.0188	0.0150	0.0151	0.0182	0.0163	0.0167	0.0138	
log(likelihood)	29.20	29.19	31.67	31.63	42.50	44.28	43.89	46.92	
R-squared	0.1334	0.1316	0.4078	0.4037	0.2599	0.4077	0.2764	0.5050	
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE	
Observations	11	11	11	11	16	16	16	16	

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

<u>Notes</u>: EMU is considered as individual unit, constructed by weighted averages of its member states in the respective year. EMU member states are only considered individually as long as they are not members of the union. GLS estimation for unbalanced panel with random effects.

Table A-9: Robustness Check – Jackknife test for JST crises sub-sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
	recession									
	СРІ	CPI	СРІ	CPI	core CPI	core CPI	core CPI	core CPI		
CEV	0.293** (0.134)		0.201 (0.123)		0.202** (0.0973)		0.129 (0.0922)			
CSV		-0.371*** (0.128)		-0.266** (0.114)		-0.281*** (0.0798)		-0.213** (0.0854)		
fiscal rule			0.0313*** (0.00976)	0.0294*** (0.00980)			0.0208*** (0.00786)	0.0191*** (0.00672)		
currency peg			-0.0105 (0.00755)	-0.0112* (0.00598)			-0.00918 (0.00673)	-0.00960* (0.00544)		
Constant	0.00248 (0.00295)	0.00236 (0.00246)	-0.00546 (0.00388)	-0.00454 (0.00514)	0.00295 (0.00211)	0.00244 (0.00183)	-0.00124 (0.00335)	-0.00115 (0.00378)		
s.e. of regression	0.0191	0.0193	0.0182	0.0184	0.0142	0.0141	0.0141	0.0139		
log(likelihood)	249.36	249.34	242.23	244.18	277.94	280.19	266.53	270.69		
R-squared	0.0859	0.1642	0.2279	0.2870	0.0686	0.1773	0.1780	0.2741		
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE		
Observations	99	99	93	94	99	99	93	94		
Number of countries	34	34	32	32	34	34	32	32		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
	(1)	(2)	(3)	$(3) \qquad (4) \qquad (5) \qquad (0)$				(0)		
	СЫ	СРІ	СРІ	CDI CDI coro CDI			core CPI	core CPI		
CEV	0.401***	err	0.270***	CI I	0.545***	core er r	0.426***			
CEV	(0.146)		(0.370^{***})		0.545*** (0.109)		(0.436^{***})			
CSV		-0.575*** (0.141)		-0.391*** (0.119)		-0.667*** (0.0829)		-0.527*** (0.183)		
fiscal rule			0.0323*** (0.00797)	0.0297*** (0.00883)			0.0254* (0.0131)	0.0218 (0.0146)		
currency peg			-0.00993*** (0.00362)	-0.00902** (0.00402)			-0.0159*** (0.00397)	-0.0135** (0.00641)		
Constant	0.00795** (0.00375)	0.00672* (0.00333)	-0.00151 (0.00524)	-0.000965 (0.00549)	-0.000395 (0.00348)	-0.00231 (0.00288)	-0.00285 (0.00465)	-0.00368 (0.00460)		
s.e. of regression	0.0087	0.0092	0.0060	0.0067	0.0059	0.0055	0.0042	0.0045		
log(likelihood)	109.67	109.46	119.78	118.05	118.54	121.50	126.64	127.14		
R-squared	0.2882	0.3595	0.6065	0.5853	0.3525	0.5284	0.6139	0.6938		
individual-specific effects	RE	RE	RE	RE	RE	RE	RE	RE		

Robust standard errors in parentheses

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*** *p*<0.01, ** *p*<0.05, * *p*<0.1

Number of countries

<u>Notes</u>: EMU is considered as individual unit, constructed by weighted averages of its member states in the respective year. EMU member states are only considered individually as long as they are not members of the union. GLS estimation with Jackknife and random effects.

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<u>Table A-10:</u> Panel estimation with random effects and lagged independent variables (CSV and CEV) for the full observation period (1980 - 2023) with crisis identifier recession

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)			
	recession										
	СРІ	СРІ	CPI	СРІ	core CPI	core CPI	core CPI	core CPI			
lagged CEV	-0.0576 (0.251)		0.617*** (0.163)		-0.326 (0.238)		0.322** (0.146)				
lagged CSV		0.0632 (0.246)		-0.766*** (0.156)		0.379 (0.243)		-0.422*** (0.149)			
fiscal rule			-0.0399*** (0.00845)	-0.0402*** (0.00864)			-0.0437*** (0.00882)	-0.0441*** (0.00900)			
currency peg			-0.0197* (0.00969)	-0.0198** (0.00961)			-0.0249** (0.0108)	-0.0250** (0.0108)			
Constant	0.0409*** (0.000875)	0.0408*** (0.000748)	0.0527*** (0.00563)	0.0527*** (0.00551)	0.0369*** (0.000839)	0.0369*** (0.000749)	0.0550*** (0.00619)	0.0550*** (0.00613)			
s.e. of regression	0.0516	0.0516	0.0319	0.0319	0.0379	0.0379	0.0271	0.0270			
log(likelihood)	1,874.12	1,874.10	2,098.64	2,099.91	2,222.81	2,222.76	2,227.27	2,227.17			
R-squared	0.0001	0.0003	0.0744	0.0749	0.0036	0.0046	0.0956	0.0956			
individual-specific effects	FE										
Observations	1,219	1,219	1,061	1,061	1,205	1,205	1,052	1,052			
Number of countries	37	37	35	35	37	37	35	35			

Robust standard errors in parentheses

*** *p*<0.01, ** *p*<0.05, * *p*<0.1

<u>Notes</u>: Extensions by controlling for the presence of a currency peg and/ or fiscal rules. All countries are considered individually, no EMU unit. Presence of a monetary union is addressed by controlling for a strict monetary peg. JST crises observations are not reported due to bad fit of this sub-sample for panel estimation. GLS estimation with fixed effects.

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