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Data and code files to replicate the results reported in the paper are available here: https://github.com/heimbergecon/pubdebt-growth.

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Abstract

While the effect of higher public debt levels on economic growth has received much attention, the literature partly points to contradictory results. This paper applies meta-regression methods to 826 estimates from 48 primary studies. The unweighted mean of the reported results suggests: a 10 percentage points increase in public-debt-to-GDP is associated with a decline in annual growth rates by 0.14 percentage points, with a 95% confidence interval from 0.10 to 0.18 percentage points. However, we cannot reject a zero effect after correcting for publication bias. Furthermore, the meta-regression analysis shows that tackling endogeneity between public debt and growth makes estimates lean less towards the negative side. In testing for non-linear effects, our results do not point to a universal public-debt-to-GDP threshold beyond which growth slows: threshold estimates are sensitive to data and econometric choices. These findings imply a lack of evidence of a consistently negative growth effect of higher public-debt-to-GDP.

Keywords: Public debt; economic growth; meta-analysis

JEL classification: E62; F34; O11; O47

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1. Introduction

Public debt levels rose during the financial crisis of 2007/2008 and its aftermath; economic activity slumped, reflected in lower economic growth rates (see Figure 1). Against this background, researchers and policy-makers wanted to learn more about the potential impact of higher public-debt-to-GDP ratios on growth (e.g. Reinhart et al. 2012; Panizza and Presbitero 2013). In a major contribution, Reinhart and Rogoff (2010) provided long historical data series for the analysis of public-debt-to-GDP ratios and economic growth. Their finding that public-debt-to-GDP ratios above 90% are associated with markedly lower economic growth rates sparked a major debate. While several leading policymakers in the US and Europe directly referred to Reinhart and Rogoff (2010) in calling for immediate fiscal consolidation measures to reign in public debt (e.g. Konzelmann 2014), several groups of researchers used the data provided by Reinhart and Rogoff (2010) as well as newly constructed datasets to conduct extensive econometric tests on the impact of public debt levels on economic growth (e.g. Kumar and Woo 2010; Herndon et al. 2014; Pescatori et al. 2014; Eberhardt and Presbitero 2015; Amann and Middleditch 2020). However, the literature – including the most-cited papers – has partly reported contradictory results. Several papers argue that there is evidence for a negative causal effect of higher public-debt-to-GDP ratios on economic growth (e.g. Afonso and Jalles 2013; Woo and Kumar 2015; Chudik et al. 2017), and for a (close to) 90% threshold in the public-debt-to-GDP-ratio beyond which growth falls significantly (e.g. Caner et al. 2010; Checherita-Westphal and Rother 2012; Baum et al. 2013). While other studies acknowledge the stylised fact of a negative association between initial public debt levels and subsequent growth, they argue that the evidence for a causal effect running from higher public-debt-to-GDP to economic growth is weak at best (e.g. Panizza and Presbitero 2014; Ash et al. 2020). Furthermore, several authors point to systematic differences in the (non-linear) impact of public debt on growth across countries, implying a lack of evidence for universal thresholds in the public-debt-to-GDP ratio beyond which growth falters (e.g. Pescatori et al. 2014; Eberhardt and Presbitero 2015; Egert 2015a; Yang and Su 2018; Eberhardt 2019; Ash et al. 2020; Bentour 2021).

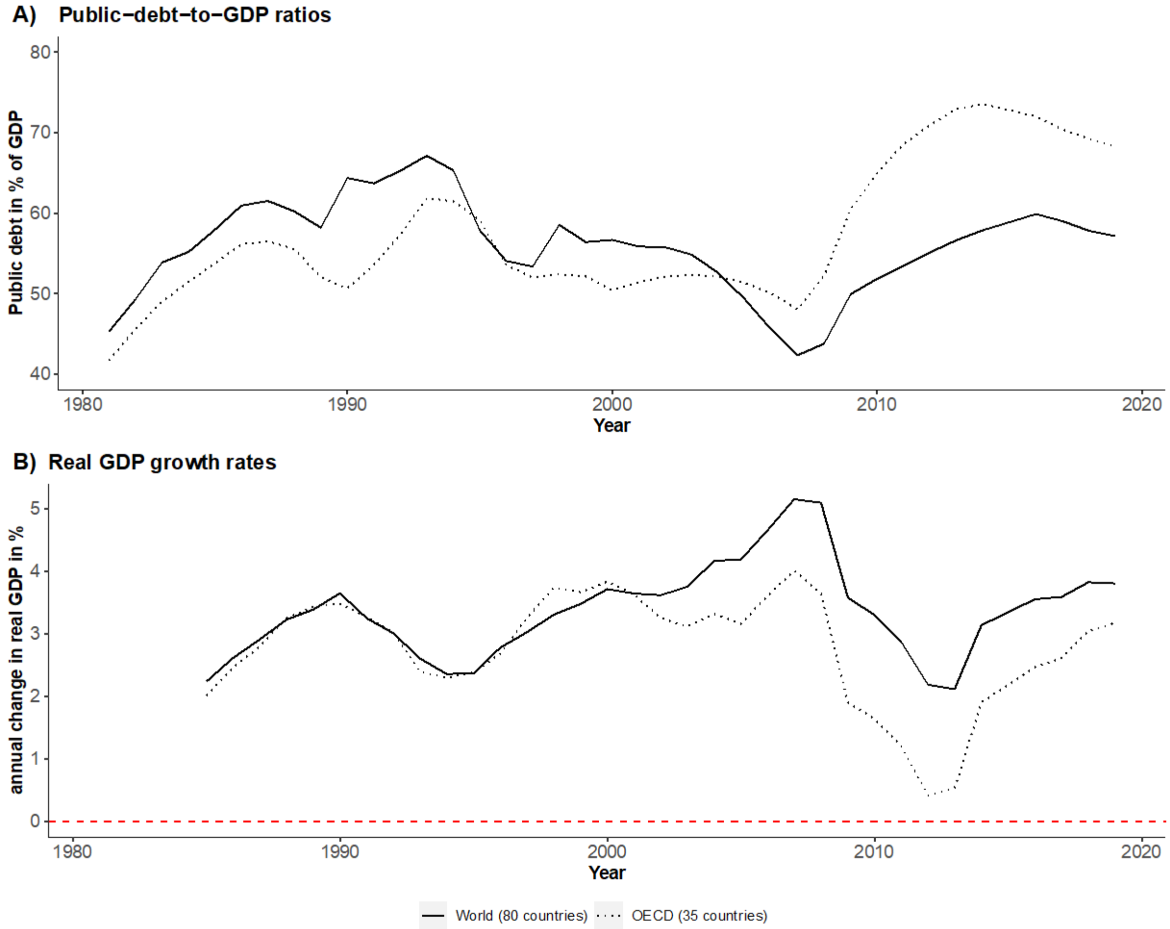
This paper contributes to the literature by developing the first comprehensive quantitative synthesis of the literature on the effect of public debt levels on economic growth by using the toolbox of meta-regression analysis (e.g. Stanley and Doucouliagos 2012).[1] In doing so, we construct and analyse a novel dataset with 826 estimates from 48 primary studies on the relationship between public debt and economic growth.[2] Since it is well-known that different data and econometric specification choices can have a sizeable impact, our analysis sets out to investigate the sources of variation in existing estimates. We tackle two main research questions. First, what can researchers and policymakers learn about the average effect size if each reported estimate is considered to be one piece of information that fits into a larger statistical picture by taking potential publication selection bias into account? Second, what factors contribute systematically to explaining heterogeneity in reported results on the relationship between public debt levels and growth? By using meta-regression methods, we are able to come up with model-

1 See Card et al. (2010), Havranek et al. (2015), Mengel (2018), Bajzik et al. (2020), Heimberger (2021a), Neisser (2021) and Imai et al. (2021) for a couple of selected and recently published meta-analyses on different topics.

2 566 estimates from 33 primary studies were included in the dataset on the linear impact of public debt on growth, and 260 estimates from 22 primary studies were considered concerning threshold effects of the public-debt-to-GDP ratio on growth. There is an overlap of 7 primary studies that were included in both datasets.

based predictions about the linear effect of higher public-debt-to-GDP ratios on growth while accounting for different data and specification choices, and we can also investigate the factors that affect threshold estimates in the public-debt-to-GDP ratio beyond which growth is reduced. In the context of answering our research questions, we pay attention to endogeneity and reverse causality issues between public debt and economic growth, to variations in the underlying data samples, the time horizon of the estimates (long-run vs. short-run), differences in model specification as well as publication characteristics such as citations and journal impact factors.

Figure 1 / Public-debt-to-GDP ratios (panel A) and real GDP growth rates (panel B) over the last decades (1982-2019): 5-year moving-average with GDP weights



Note that the data in this figure show a 5-year moving average for the respective country group. We also used GDP weights, i.e. countries with a higher GDP level receive more weight; vice versa, countries with a lower GDP levels receive less weight.
Source: IMF, World Bank; own calculations.

The rest of the paper is structured as follows. Section 2 presents a literature survey on the theory and empirics of the impact of public debt and growth. Section 3 discusses the construction of the dataset. Section 4 develops the analysis on publication bias and presents the meta-regression analysis concerning the linear effect of public debt levels on growth. Section 5 presents the meta-regressions concerning threshold effects in the public-debt-to-GDP ratio beyond which growth slows down. Section 6 summarises and concludes.

2. Theory and literature survey

The literature on the nexus between public debt and economic growth has grown substantially in the aftermath of the financial crisis of 2007/2008 (e.g. Reinhart et al. 2012; Panizza and Presbitero 2013; Amann and Middleditch 2020). Conducting a comprehensive literature survey would be beyond the scope of this paper. The discussion in this section will focus on introducing those parts of the theoretical literature that have broadly guided the econometric testing, and on presenting and contextualising the most important elements of the relevant empirical literature.

2.1. THEORETICAL LITERATURE

The “conventional view” of government debt emphasises the positive aggregate demand effects of public debt in the short-run and crowding-out effects dampening economic activity in the long-run (Elmendorf and Mankiw 1999). In this view, an increase in the budget deficit raises disposable household income, especially when there is slack in the economy. The corresponding increase in income and wealth boosts the aggregate demand for goods and services. While the “conventional view” deems the economy to be Keynesian in the short-run, the outlook is still “classical” in the long-run: public savings fall due to the increase in the fiscal deficit, but private savings do not rise enough to compensate for the fall in public savings; therefore, national savings decline; total investment is reduced, resulting in a smaller capital stock and reduced output growth.

A negative long-run effect of public debt on growth can be due to crowding-out mechanisms: if increased fiscal deficits lead to higher interest rates, this may crowd-out investment in the private sector. Furthermore, net exports may fall due to an appreciation of the exchange rate. If more government debt is associated with higher inflation, this may also act as a drag on growth (e.g. Ash et al. 2020). Cochrane (2011) argues that the negative effect of a higher public debt level on growth can be quite large if higher debt boosts uncertainty and expectations of higher inflation and financial repression. In a more “unconventional view” of government debt, however, an initial increase in the fiscal deficit that initially leads to a higher public debt level may – especially when the output gap is large – not only temporarily boost aggregate demand, but increase the long-run growth rate through hysteresis (e.g. Delong and Summers 2012; Fazzari et al. 2020). Some endogenous growth models also yield results that are consistent with a positive effect of public debt on growth in the transition stage to the new steady-state, depending on what is actually being financed with the debt (Aizenman et al. 2007).

An idea that has strongly guided testing in the econometric literature is that there can be thresholds in the public-debt-to-GDP ratio beyond which growth is substantially reduced. However, finding fully specified theoretical models that yield predictions for such thresholds is not easy at all. In theory, non-linearities could arise because of public debt overhangs (e.g. Krugman 1988). Ghosh et al. (2013) provide a formal model where non-linearities arise as a tipping point is reached beyond which public debt turns unsustainable; however, their theoretical argument is not embedded in a growth framework. Checherita-Westphal et al. (2014) build a theoretical model in which public debt can only be issued to conduct spending on public investment; the public-to-private-capital ratio determines the optimal level of public-debt-to-GDP. In their model, the optimal level of debt-to-GDP that maximises GDP growth depends on the output elasticity of the capital stock. In building on Checherita-Westphal et al. (2014), Greiner (2013) argues that this theoretical result is driven by the assumption that the fiscal deficit is equivalent to public investment. Relaxing this assumption – i.e. allowing governments to take on debt for other things than public investment – yields a monotonic negative relationship between public-debt-to-GDP and the steady-state growth rate. Teles and Mussolini (2014) present a model with overlapping generations and endogenous growth, in which higher levels of public-debt-to-GDP extract some of the younger population's savings (which is needed to pay interest), thereby producing a crowing-out effect that reduces the impact of productive government spending on economic growth. Proaño et al. (2014) build a dynamic growth model in which public debt levels have a non-linear impact on economic activity through amplifications from the financial sector, with higher public-debt-to-GDP impairing growth only in times of financial stress. Alesina et al. (1992) argue that higher levels of public indebtedness can be linked to higher perceived default risk by investors, giving rise to non-linearities.

Another possible channel through which higher public debt levels may eventually have a negative growth effect is their impact on countercyclical fiscal policy: if higher debt levels constrain a government's ability to use expansionary fiscal policy in downturns, this may increase output volatility and thereby reduce growth (e.g. Ramey and Ramey 1995). In a world of multiple equilibria, a fully solvent government with a high level of public debt may decide to implement restrictive fiscal policies to reduce the probability that a sudden change in investor sentiments would push the country into the bad equilibrium. However, a government's ability and willingness to use expansionary fiscal policy in downturns arguably depends more on the monetary arrangements (in particular: on coordination with the central bank) and on public debt structures than on the actual level of the public-debt-to-GDP ratio (e.g. de Grauwe 2012).

2.2. EMPIRICAL LITERATURE

There are two major empirical arguments. First, the linear association between public-debt-levels and economic growth is negative, and this link can be interpreted as causal running from higher public debt levels to lower economic growth. Second, there is a threshold in the public-debt-to-GDP ratio beyond which countries experience significantly lower economic growth. The literature survey in this section, however, will show that a careful reading of the empirical literature on the nexus between public debt levels and economic growth suggests that the relationship between public debt and growth is less clear-cut and more nuanced than these major arguments suggest (especially when it comes to causal interpretations of the relationship between public debt and growth, differences in the underlying relationship across countries, and uncertainty around potential threshold effects). Against this background, conducting a quantitative synthesis of the literature by using meta-regression tools arguably provides added value.

2.2.1. Is there a negative (causal) effect of higher public debt levels on growth?

In an influential paper, Kumar and Woo (2010) start from the stylised fact that there is a negative correlation between initial government debt and subsequent real GDP per capita growth – indicating that an increase in the lagged public-debt-to-GDP ratio by 10 percentage points is, on average, associated with a decline in GDP growth rates by 0.25 percentage points (Kumar and Woo 2010, p. 8-9). However, the authors rightly note that this relationship ignores potential endogeneity issues between public debt and growth: the public-debt-to-GDP ratio and outcomes of economic growth may be jointly determined by third factors. Furthermore, interpretations of causality are anything but straightforward, since slumps in economic activity may largely be responsible for increases in public-debt-to-GDP – the so-called reverse-causality problem. Kumar and Woo (2010), however, argue that they address reverse causality and endogeneity issues by using lagged levels of public debt and a GMM estimation approach with suitable instrumental variables. In doing so, they report results suggesting that increases in public-debt-to-GDP are indeed linked with a slowdown in annual real GDP growth. Other empirical papers support this finding of a negative linear effect of the public-debt-to-GDP ratio on real GDP growth (Cecchetti et al. 2011; Afonso and Jalles 2013; Afonso and Alves 2015; Woo and Kumar 2015; Chudik et al. 2017).

However, Panizza and Presbitero (2014) call interpretations of a negative causal effect of the public-debt-to-GDP ratio on growth into doubt. They argue that the literature that has tried to address endogeneity by using lagged values of public-debt-to-GDP (Cecchetti et al. 2011), internal instruments via GMM estimation (Kumar and Woo 2010), or by instrumenting public debt with average debt-to-GDP ratios in other countries (Checherita-Westphal and Rother 2012) has failed to come up with results that convincingly address endogeneity. Therefore, Panizza and Presbitero (2014) propose a new strategy to address the endogeneity issue: using an external instrument for the public-debt-to-GDP ratio based on the fact that when there is public debt denominated in foreign currency, changes in the exchange rate of a country directly affect the public-debt-to-GDP ratio. They show that when they use their new instrumental variable, the association between debt and growth disappears so that there is no evidence of a causal negative effect of public debt on economic growth.

Ash et al. (2020) address endogeneity and reverse causality issues by using leads and lags of GDP growth in relation to public debt. They provide a comprehensive assessment of the impact of public debt on growth by using various datasets from influential papers in the literature, including Reinhart and Rogoff (2010), Cecchetti et al. (2011), Checherita-Westphal and Rother (2012) and Woo and Kumar (2015). They report findings according to which the relationship between the public-debt-to-GDP ratio and growth is close to zero since the 1970s, and there is no evidence of a causal effect of public debt on growth. Ash et al. (2020) argue that earlier results in the literature indicating a negative effect of public debt on growth are sensitive to small samples, outliers and peculiar econometric choices.

Eberhardt and Presbitero (2015) estimate empirical specifications that allow for heterogeneity in the long-run relationship between public debt and economic growth across countries. They provide evidence for systematic differences in the impact of public debt on growth across countries. Therefore, the same policy response may not be appropriate in all countries. Other studies that allow the effect of public debt on growth to vary in different countries also stress this point (Bell et al. 2015; Sosvilla-Rivero and Gomez-Puig 2019; Bentour 2021).

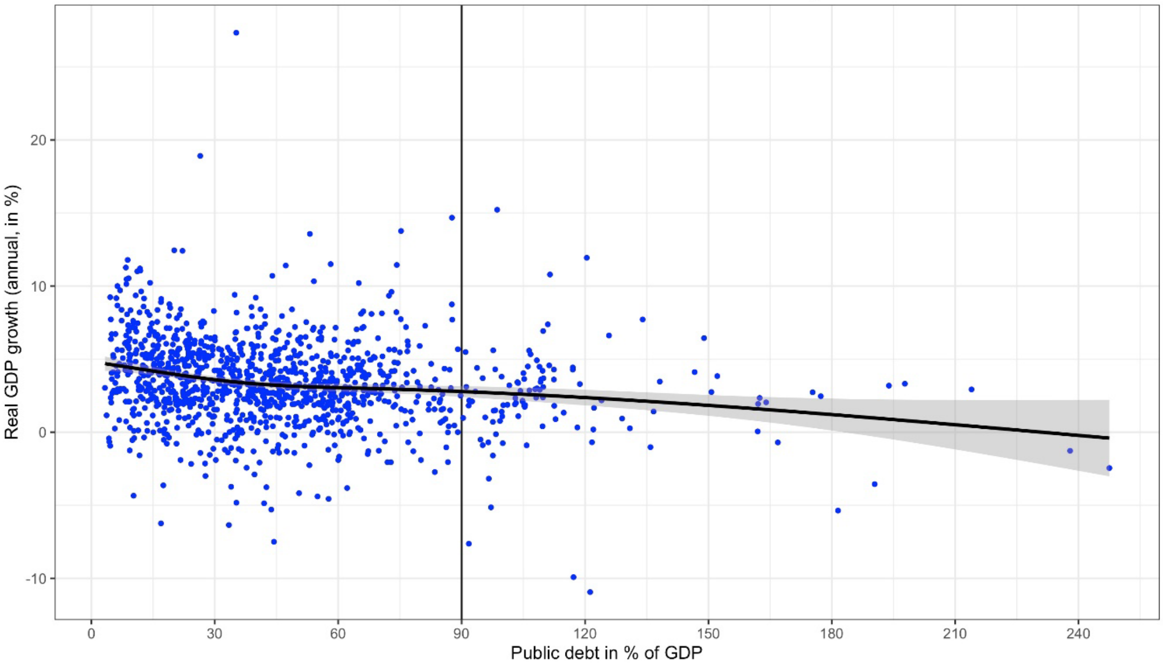
In sum, the literature has so far at least partly reported contradictory results, which also holds for some of the most cited and influential studies. It is clear that endogeneity and reverse causality issues are important in looking at the relationship between public debt and economic growth (e.g. Woo and Kumar 2015; Ash et al. 2020), and we will incorporate this point in our meta-regression analysis. Furthermore, parts of the empirical literature distinguish between short-run and long-run growth effects of public debt (e.g. Eberhardt and Presbitero 2015; Chudik et al. 2017) – a distinction that also plays a role in the theoretical literature (see section 2.1), so that accounting for the time horizon will also be relevant for the meta-regressions. Finally, the underlying country sample could play a role, as the effect of public debt on growth may vary in advanced countries compared to developing countries (e.g. Schclarek 2004). Our meta-regressions will therefore also consider whether different data choices systematically contribute to explaining heterogeneity. Beyond the issue of the linear impact of public debt on GDP growth, there is a growing literature on non-linearities in the relationship between public debt and growth, to which we turn next.

2.2.2. Are there threshold effects of the public-debt-to-GDP ratio on growth?

The study by Reinhart and Rogoff (2010) has proven influential – not only by attracting attention among other researchers but also because leading policy-makers in the US and Europe provided direct references to their main findings to justify fiscal consolidation measures (e.g. Blyth 2013; Konzelmann 2014). In making a significant contribution concerning the construction of long data series for the analysis of government debt and economic growth, Reinhart and Rogoff (2010) report descriptive statistics showing “that across both advanced countries and emerging markets, high [government] debt/GDP levels (90 percent and above) are associated with notably lower growth outcomes” (Reinhart and Rogoff 2010, p. 577), and they invite causal interpretations running from public debt to growth: “The nonlinear effect of debt on growth is reminiscent of ‘debt intolerance’ […] and presumably is related to a nonlinear response of market interest rates as countries reach debt tolerance limits.” (Reinhart and Rogoff 2010, p. 574).

Herndon et al. (2014) argue that the analysis by Reinhart and Rogoff (2010) is characterised by selective exclusion of data, coding errors, and unconventional decisions in weighting the summary statistics. After providing their corrections, Herndon et al. (2014) refute the argument that public-debt-to-GDP ratios above 90% are consistently related to lower GDP growth. Figure 2 shows the Reinhart-Rogoff data for 20 advanced countries over 1946-2009: when we use a smooth locally fitted regression line, we do not find evidence for non-linearity in economic growth rates close to a 90% public-debt-to-GDP ratio. There are fewer observations as public-debt-to-GDP increases, but the range of GDP growth rates remains wide even at higher public-debt-to-GDP ratios. Nevertheless, Figure 2 shows a downward-sloping regression line, indicating that higher public-debt-to-GDP ratios are, on average, indeed associated with lower GDP growth. This link, however, is difficult to interpret as a causal effect of public debt on growth. In particular, Irons and Bivens (2010) conduct Granger causality tests on data similar to those used in Reinhart and Rogoff (2010) and show that there is no evidence that public debt “Granger-causes” economic growth, but there is evidence for reverse causality (low growth leading to high debt).

Figure 2 / Public-debt-to-GDP and real GDP growth rates for 20 advanced countries over 1946-2009



Note: The regression line is based on a generalised additive model with local smoothing, and the smoothing parameter was selected by using the default method of cross-validation (Wood 2017). The country sample includes country-year observations for 20 advanced countries over 1946-2009: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, UK, USA.
Source: Reinhart-Rogoff (2010) data as used in Herndon et al. (2014).

Egert (2015a) puts the Reinhart-Rogoff data to the econometric test by using nonlinear threshold models: "90% is not a magic number. The threshold can be lower and the nonlinearity can change across different samples and specifications" (Egert 2015a, p. 10). Lee et al. (2017) also use the Reinhart-Rogoff dataset. By employing a median regression approach, they report that the debt threshold is not to be found at 90%, but at a much lower 30%. Several other papers – while making different choices in terms of data coverage and threshold estimation techniques – argue that there is no evidence for a universally applicable threshold effect of public debt on economic growth (e.g. Pescatori et al. 2014; Proaño et al. 2014; Eberhardt and Presbitero 2015; Egert 2015b; Chudik et al. 2017; Yang and Su 2018; Eberhardt 2019; Ash et al. 2020; Bentour 2021).

Notably, however, several other empirical papers lend support to the initial Reinhart and Rogoff (2010) finding of a (close to) 90% threshold in the public-debt-to-GDP ratio beyond which growth falls significantly. Cecchetti et al. (2011) consider a sample of 18 advanced countries from 1980 to 2010 to estimate a model with a quadratic public-debt-to-GDP term by using OLS, and they report a threshold value of around 85%. Checherita-Westphal and Rother (2012) cover 12 euro area countries over the period 1970-2008. They also estimate models with quadratic public debt terms for both annual data and multi-year data averages, and their results imply that the public-debt-to-GDP ratio has a negative impact on long-term growth at about 90% to 100% of GDP. They argue that tackling endogeneity by instrumenting each country's public-debt-to-GDP through either its time lags or through the average of the public debt levels of other countries in the sample indicates the robustness of their baseline findings.

Baum et al. (2013) estimate panel threshold models for 12 euro area countries over 1990 to 2010. They report that while the short-run impact of public debt on growth is positive, public-debt-to-GDP ratios above 95% have a negative impact on growth. Caner et al. (2010) use a larger sample of 101 advanced and developing countries over 1980 to 2008; they point to a public-debt-to-GDP threshold at 77% beyond which growth is significantly lower.

This literature survey on threshold effects points to substantial variation in reported results. Against this background, this paper contributes to the literature by providing meta-regression insights into how data and specification choices can help explain heterogeneity. It is important, however, to note that the inexistence of a universally applicable non-linearity in the relationship between public debt and growth could still be compatible with a significantly negative linear impact of higher public-debt on growth. Before section 5 investigates threshold effects in a meta-regression framework, sections 3 and 4 first turn to providing a quantitative synthesis of the literature on the linear impact of the public-debt-to-GDP ratio on economic growth.

3. Constructing the dataset

This section explains how we identified the relevant econometric literature on the impact of public debt on economic growth and how we calculated comparable effect sizes from the reported information. Section 3.1 discusses our dataset in the context of the literature search and data collection process, and section 3.2 presents the meta-regression variables.

3.1. DATA COLLECTION

The search strategies for primary studies, the construction of the dataset, and the reporting of the final results are fully in line with established guidelines for conducting meta-analyses (Havranek et al. 2020). Appendix A presents the details concerning the criteria used in searching for papers and for including or excluding primary studies in the dataset. The central condition for including a study is that it reports results based on an econometric model where economic growth is the dependent variable and public debt-to-GDP is an explanatory variable. This means that, to be included, a study had to present findings from some variant of the following generic econometric model (we ignore subscripts for brevity):

$$g = \alpha_0 + \alpha_1 D + \alpha_x Z_x + \varepsilon \qquad (1)$$

where the dependent variable g is a measure of economic growth, D measures public debt-to-GDP, Z is a vector of other explanatory variables, and ε is the error term. Appendix F discusses further information on the inclusion of estimates on threshold effects in the public-debt-to-GDP ratio on economic growth.

We included 566 estimates from 33 primary studies in the dataset on linear impacts of public debt on growth (see Appendix B for a full list), and 262 estimates from 23 primary studies on threshold effects, which gives us a total of 826 estimates from 48 unique primary studies.[3] In what follows, this section focuses on the data concerning the linear effect of public debt on growth; details on the consideration of threshold effects will be presented in section 5.

Primary studies partly use different scales of the growth and public debt variables, so that it is not possible to directly compare all reported coefficients. We deal with this by transforming estimates where necessary such that the coefficient of interest α_1 – the linear (marginal) effect of the public-debt-to-GDP ratio on GDP growth – is standardised to reflect that a one percentage point increase in the public-debt-to-GDP ratio is associated with an x percentage point change in the rate of economic growth. Details of the standardisation process are available in Appendix A. In what follows, we denote the standardised coefficient of α_1 as SC.

[3] 7 primary studies were included both in the dataset on the linear impacts of public debt on growth as well as in the dataset on threshold effects. Figure A1 in appendix A presents a PRISMA flow chart on the various stages of the search process (Moher et al. 2009).

As a robustness check, we use the partial correlation coefficient (PCC) as an alternative standardised measure of the impact of public debt on growth. We can directly calculate the partial correlations by using the regression output reported in primary studies in terms of t-statistics and degrees of freedom.[4] The partial correlation coefficient is a unitless measure, which is bounded in the range from -1 to 1 (Stanley and Doucouliagos 2012, p. 25). A major drawback of using the partial correlation is that it lacks an economically intuitive and meaningful interpretation regarding its effect size. Therefore, we use the standardised coefficient estimates as described above – for more detail, see Appendix A – as our preferred estimates, because they provide direct information about the economic relevance of the coefficient of interest.

3.2. VARIABLES IN THE META-REGRESSION DATASET

Many of the variables we coded for the meta-regression analysis are categorical dummy variables, whose mean can be interpreted as the share of observations when a certain characteristic is present.

As we explained above, our main variable of interest in looking at the linear impact of public debt on growth is the standardised coefficient (SC). To test for potential publication selectivity, we collect the standard errors of this coefficient. As an alternative standardised effect size, we consider the partial correlation coefficient (PCC) together with its standard error. Appendix C presents a summary of the standardised coefficient, the partial correlation and all meta-regression variables, including their sample mean and standard deviation.

Country composition: The reported impacts of the public-debt-to-GDP ratio on economic growth could be influenced by the underlying data. If the level of development played a role, the relationship between public debt and growth would be influenced by the country sample. We thus control for whether an estimate uses a data sample of advanced countries, developing countries or a mix of countries. To distinguish between these three groups, we make use of the IMF's country classification (IMF 2021).

Time horizon of the estimate: We check if the study clearly states whether the reported estimate implies a long-run or short-run effect of public debt on growth. In doing so, we code three exclusive but mutually exhaustive dummy variables: LongRunExplicit refers to reported long-run effects of public debt on growth (e.g. via long-run coefficients in Error Correction models or Pooled Mean Group models, or by using multi-year averages of the data to filter out short-run fluctuations); ShortRunExplicit is about short-run effects (e.g. via short-run coefficients in dynamic models); and HorizonOther covers estimates that are unspecific concerning the time horizon.

Data and estimation details: We consider whether a study used cross-sectional data instead of panel data. Furthermore, we check whether the dependent variable (GDP growth) is used in per capita terms, since measurement of the dependent variable could make a difference (e.g. Baum et al. 2013; Woo and Kumar 2015). We also check whether accounting for potential reverse causality issues between public debt and growth has an impact on reported results: the LaggedPublicDebt dummy variable is set to one when a primary study includes a lag of the explanatory public debt variable. Notably, while using a lagged public debt variable may mitigate the endogeneity problem, it does not provide a comprehensive

4 The partial correlation is given by: $PCC = \frac{t}{\sqrt{t^2+df}}$ where *t* is the t-statistic of the coefficient of interest; and *df* denote the degrees of freedom of the regression model (Stanley and Doucouliagos 2012, p. 25).

solution, because addressing endogeneity requires a more direct instrumental variables approach (e.g. Panizza and Presbitero 2013). Therefore, we code the variable TacklingEndogeneity that is set to one if the econometric approach directly addresses potential endogeneity issues by instrumenting the public-debt-to-GDP ratio (e.g. via IV two-stage least squares or GMM estimation).

Publication characteristics: We consider several dimensions of the publication process: the mean year of the data in the underlying sample, which allows us to check whether the time dimension of the data matters; the journal impact factor of the journal in which the paper was published, which allows us to test whether estimates published in higher-impact journals are different;[5] and whether the authors of the primary studies consider estimates in their study to be preferable vis-à-vis other estimates.

[5] For studies that have not been published in a peer-reviewed journal, we code a value of 0.01.

4. Meta-regression analysis on the linear impact of public debt on growth

Is the literature on the impact of higher public debt levels on growth characterised by publication selection bias? What factors contribute to explaining the heterogeneity in reported results concerning the impact of public debt on economic growth? This section provides answers to these questions.

4.1. DISTRIBUTION OF ESTIMATES AND THEIR STANDARD ERRORS

We first look at the distribution of the standardised coefficients and the corresponding precision of the estimates. This allows us to obtain descriptive insights into the patterns of the data before we move to the results from the multivariate meta-regression models. Figure 3 reports information concerning the distribution of results based on 566 standardised coefficients of the linear impact of public debt on economic growth (on the horizontal axis) and the precision of these estimates (on the vertical axis), where precision is measured as the inverse of the standard errors of the standardised coefficients. [6] The unweighted mean (-0.014) and median (-0.010) point to a negative impact of the public-debt-to-GDP ratio on economic growth. The unweighted point estimate implies that, on average, a 10-percentage point increase in public debt to GDP is associated with a decline in economic growth rates by 0.14 percentage points per year – where the 95% confidence interval ranges from 0.10 percentage points to 0.18 percentage points. Taken at face value, this finding supports the stylized fact of a negative association between the (initial) public-debt-to-GDP ratio and economic growth (e.g. Kumar and Woo 2010; Cecchetti et al. 2011; Panizza and Presbitero 2014). Given that public-debt-to-GDP ratios in the G7 countries increased by 18.0 percentage points during the COVID-19 crisis year 2020 alone (IMF 2021), this would point to a potential forward-looking penalty for annual growth rates relative to maintaining public-debt-to-GDP levels at pre-COVID-19 levels.

However, Figure 3 reveals considerable dispersion in the results: the minimum standardised coefficient is -0.195 and the maximum is 0.100; the standard deviation is 0.046. The most precise estimates, which can be seen at the top of the plot, are close to the vertical zero effect line. The precision-weighted mean of the sample is -0.003, which is close to zero as the 95% confidence interval barely rules out a zero effect.[7] Figure 3 also shows that the reported standardised coefficients at the bottom of the plot are distributed asymmetrically – with a stronger mass of imprecise estimates located on the left side (representing negative growth impacts of public debt). These imprecisely estimated negative coefficients imply that the unweighted mean of the sample is more strongly negative than the precision-weighted mean.[8] The asymmetry of the distribution of reported estimates in Figure 3 could be driven by

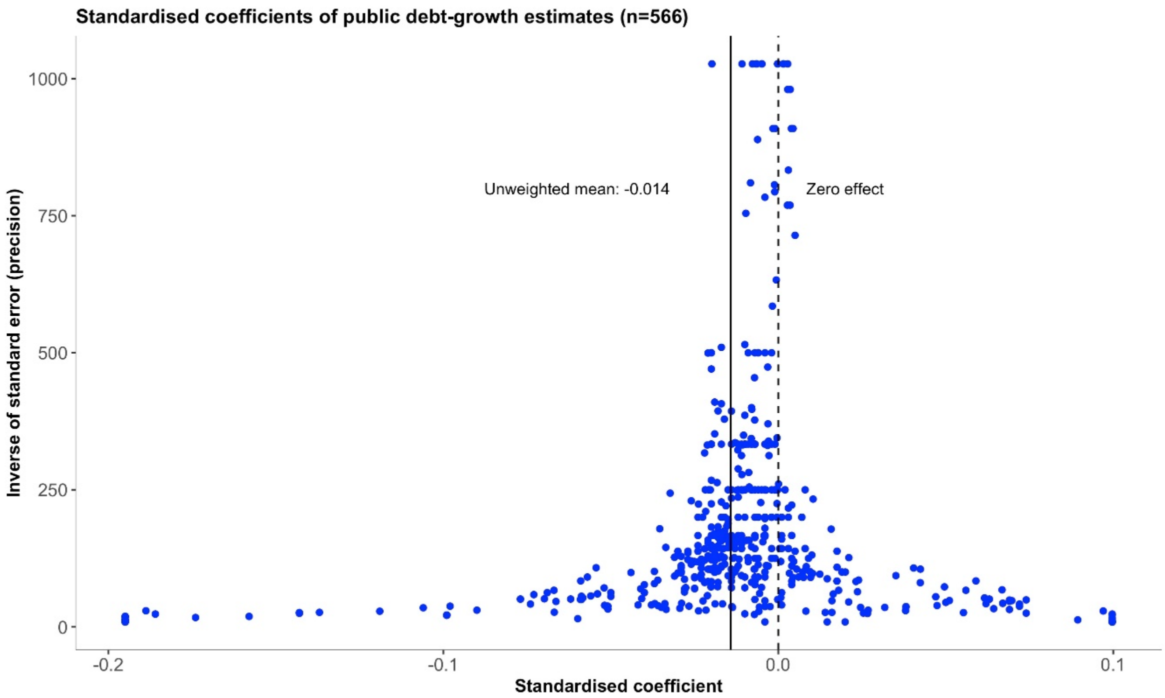
6 It must be noted that we winsorised the collected standardised coefficients and their standard errors at the 2nd and 98th percentile, respectively. We did so to limit extreme values in the data and to reduce the impact of potentially spurious outliers (e.g. Zigraiova et al. 2021). However, we conducted robustness checks: our findings are robust to different choices in winsorising the estimates.

7 The 95% confidence interval of the weighted mean ranges from -0.0034 to -0.0023.

8 Appendix D reports the distribution of reported results for our alternative standardised effect size, the partial correlation coefficient, and the corresponding precision of the estimates. The patterns of the data are consistent: when we use the

publication selection bias, i.e. a tendency to over-report results showing a negative impact of public debt levels on growth (e.g. Andrews and Kasy 2019) – an issue we investigate in the next section.

Figure 3 / Standardised coefficients of public debt-economic growth estimates



Note: The figure plots estimates (winsorised at the 2nd and 98th percentiles) of the standardised coefficients against the inverse of the corresponding standard errors (n=566). The solid vertical line shows the unweighted mean of the standardised coefficient; the dotted vertical line is the zero effect line.
Source: Own calculations.

4.2. PUBLICATION BIAS

This section presents the econometric analysis of whether there is evidence for publication bias. In general, publication bias is defined as a process where results are chosen for their statistical significance (e.g. Brodeur et al. 2016). Factors that could distort reported results include journal editors with a tendency for publishing those results that are statistically significant, and researchers deciding not to report statistically insignificant findings that would contradict accepted theory, so that statistically significant results are overall treated more favourably than insignificant results (e.g. Andrews and Kasy 2019).

To set the stage for the tests on publication selectivity, column (1) entails a t-test of the unweighted mean of the coefficient against zero. This results in a statistically significant negative relationship between public debt levels and economic growth, implying that a one percentage point increase in public-debt-to-GDP is associated with a decline in annual economic growth rates by 0.014 percentage points, with a 95% confidence interval from 0.010 to 0.018.

partial correlation coefficient, we also find more imprecise estimates on the left side of the plot, indicating a negative association of public debt with growth. Furthermore, the weighted mean of the partial correlation coefficient is significantly smaller than the unweighted mean as the most precise estimates are closer to zero.

Column (2) of Table 1 performs the Funnel-Asymmetry Precision-Effect test (FAT-PET), which allows us to formally assess the presence of publication selection bias (e.g. Stanley 2005). We run the following model:

$$SC_{ij} = \beta_0 + \beta_1 SE_{ij} + \varepsilon_{ij} \tag{2}$$

where SC_{ij} is the estimated standardised coefficient i from study j, SE_{ij} is its standard error, and ε_{ij} is a random sampling error. In this equation, the term $\beta_1 SE_{ij}$ controls for publication bias. The hypothesis test of β_1(H0: $\beta_1 = 0$) can be called the Funnel Asymmetry Test (short: FAT). If β_1 equals zero, we can conclude that there is no evidence for publication bias (Egger et al. 1997). Furthermore, testing the hypothesis that β_0 is zero (the Precision-Effect Test, in short: PET) allows us to check whether there remains an empirical effect of public debt on growth after correcting for publication bias. We need to take into account that the econometric estimates reported in the literature are based on different data and specification choices yielding various sources of heteroscedasticity. Therefore, these estimates must be expected to have different variances. We address this issue by estimating equation (2) via Weighted Least Squares (WLS) with the inverse of the variances as weights. WLS estimates assign more weight to those estimates that are more precise, because the information provided by more precise estimates is more valuable. Furthermore, we need to consider that most of the studies in our meta-study database report several estimates. We address potential dependence of the estimates within studies by clustering the standard errors at the study-level. Furthermore, we report results when only looking at a small sample of preferred estimates per study. This allows us to check for whether potential within-study dependence of the estimates is driving the results even after clustering the standard errors at the study-level.

Column (2) of Table 1 provides first evidence for the presence of publication selectivity: the association between the standardised coefficients and their standard errors is negative and statistically significant at the 1% level. In other words: there is a bias in favour of reporting negative associations of public-debt-to-GDP-ratios with economic growth. This means that researchers report positive and/or insignificant estimates less than they should according to econometric theory. The PET results in column (2) suggest that the average impact of higher public debt levels on economic growth cannot be statistically distinguished from zero once we correct for publication selectivity.

Columns (3) to (6) of Table 1 then report results from various robustness checks. Column (3) considers a much smaller sample, zooming in on the median estimates from the 33 primary studies. Column (4) also uses a reduced sample by looking at estimates that are preferred by the authors of the respective studies. Column (5) addresses the potential endogeneity problem that the standard error could be correlated with the error term via the choice of estimation techniques in the primary studies, leading to a biased estimate of β_1. We tackle this by an IV estimation: studies based on larger datasets tend to be more precise than those based on smaller samples, while the number of observations should be uncorrelated with methodological choices. We calculate the inverse of the square root of the number of degrees of freedom, an estimate that is directly proportional to the standard error (e.g. Gechert and Heimberger 2021) and use this as an instrument for the standard error. Column (6) reports FAT-PET results when we use our alternative standardised effect size, namely the partial correlation coefficient. Here, the quantities of the coefficients are not directly comparable to columns (1)-(5), but the signs and notions of statistical significance have a similar interpretation. It can be seen that the results from all these tests are consistent with the baseline FAT-PET in column (2), suggesting that there is publication bias in favour of reporting a negative association between public debt and growth, and the negative association disappears once we correct for this over-reporting, i.e. there is no evidence for a negative effect of public debt on growth.

Table 1 / Linear funnel asymmetry and precision effect tests

	(1) Unw. avg	(2) Base WLS	(3) Median	(4) Preferred	(5) IV	(6) PCC
β_1	---	-1.360***	-1.903***	-2.305***	-1.361**	-1.912***
[publication bias]	---	(0.359)	(0.607)	(0.374)	(0.640)	(0.518)
β_0	-0.014**	-0.001	0.002**	0.002***	0.006	0.015
[mean beyond bias]	(0.005)	(0.002)	(0.001)	(0.0004)	(0.008)	(0.026)
n	566	566	33	78	566	566

Note: This table reports several test results for publication selection bias and underlying effects beyond such a bias. The FAT (β_1) tests for the presence of publication selection bias. The PET (β_0) estimates the average effect of corporate taxes on economic growth after correcting for publication selection bias. The dependent variable is the standardised coefficient of the impact of public debt on economic growth rates – with the exception of column (6), which uses the partial correlation coefficient (PCC). Numbers in brackets are standard errors, which were clustered at the study level. All results except for column (1) were obtained by using Weighted Least Squares (weights based on the inverse of the variances). Column (1) serves as a comparison and shows the unweighted average (Unw. Avg) of the standardised coefficient, tested against zero. Column (2) refers to the baseline WLS results according to equation (2). In column (3), we used the median estimates of the 33 underlying primary studies. Column (4) zooms in on the preferred estimates of the respective primary studies. Column (5) refers to instrumental variable estimation (using the inverse of the square root of the degrees of freedom as an instrument for the standard error). Column (6) uses the partial correlation coefficient (PCC) instead of the standardised coefficient as the dependent variable. **, *** denote statistical significance at the 5% and 1% level, respectively.
Source: Own calculations.

To check the robust of the linear tests for publication selection bias, we apply non-linear methods. First, we use the approach suggested by Ioannidis et al. (2017), who focus only on the top 10% of estimates with the smallest standard error and report the weighted average from this subsample. This approach is robust to asymmetric tails of the funnel plot, which mostly show estimates with low power. Second, we produce non-parametric results that are robust to various assumptions regarding the functional form of publication selection bias and the underlying distribution of the true effect of public debt on growth. They are based on the method proposed in Furukawa (2019), using only a sub-sample of the most precise estimates – the so-called stem of the funnel plot. This sub-sample is determined by minimising the trade-off between variance per observation and publication bias. Using both the Ioannidis et al. (2017) and the Furukawa (2019) method supports our finding that there is publication selectivity, and the mean beyond bias cannot be distinguished from zero; detailed results are available in Appendix E. However, the estimates of the standardised coefficients and their standard errors could be jointly influenced by data characteristics and specification choices yielding an asymmetric distribution of reported findings. These sections, therefore, present meta-regression results investigating the sources of heterogeneity, in which we simultaneously control for publication selection bias and other confounding variables.

4.3. MULTIVARIATE META-REGRESSION ANALYSIS

We start from a standard assumption in the meta-regression literature: the linear effect of the public-debt-to-GDP ratio on growth, measured as the standardised coefficient i from study j, denoted SC_{ij}, is not only influenced by sampling error (ε_{ij}) but by a vector of 'moderator' variables (X_{ij}) that includes characteristics capturing differences in the underlying impact of the public debt level on economic growth (such as choices with regard to data, model specification and estimation approach). The meta-regression model can be written as follows:

$$SC_{ij} = \beta_0 + \beta_1 SE_{ij} + \beta_x X_{ij} + \varepsilon_{ij} \tag{3}$$

In this equation, the term $\beta_1 SE_{ij}$ again allows for publication selection bias. Note, however, that we also control for other moderator variables included in the vector Z.

Before presenting the meta-regression results, a couple of comments are in order so that the findings can be properly interpreted. Each of our models omits one category (as the reference category) from each of the two groups of mutually exclusive and jointly exhaustive dummy variables (see different country compositions of the underlying data and the time horizon of the estimates in Table A2 in appendix C). We have to omit these reference categories, because we would otherwise not be able to estimate our models due to perfect multicollinearity (e.g. Heimberger 2021b). A consequence of omitting these reference categories, however, is that the intercept β_0 cannot be interpreted as the 'true' impact of the public-debt-to-GDP ratio on economic growth, because it depends on the choice of reference groups. Other reference specifications would yield different estimates of the intercept β_0. Our reference specification is an estimate of the impact of the public-debt-to-GDP ratio on economic growth using a data sample consisting of advanced countries and being unspecific concerning the time horizon (long-run or short-run). Notably, however, the choice of these two omitted reference categories in no way influences any of the other estimated coefficients; it only shifts the reference value of the intercept β_0. Therefore, the coefficients of the moderator variables that will be estimated for the two groups of mutually exclusive and jointly exhaustive dummy variables allow us to make predictions regarding the impact of public debt on growth in a given setting of reference groups in comparison to an alternative setting. For example, the estimated average standardised coefficient for advanced countries compared with developing countries can be predicted by adding up the value of the intercept β_0 and of the developing countries coefficient; the estimate for long-run impacts of public debt on growth can be inferred by adding up the value of the long-run coefficient.

Table 2 shows the meta-regression results based on equation (3). Our preferred specification is column (1), which zooms in on the main factors discussed in the related literature in terms of having an impact on the relationship between public debt levels and growth. These include the state of economic development of the countries under investigation (advanced, developing or mixed country groups), the time horizon of the considered growth impacts of public debt (long-run, short-run or unspecific), whether estimates account for endogeneity between public debt and growth, and the journal impact factor as a proxy for "quality". Note that we use WLS with precision-weights, as Stanley and Doucouliagos (2017) show that the WLS estimator is to be preferred in comparison with other standard estimators in the meta-regression literature. Furthermore, we cluster the standard errors at the study-level to address potential dependence of the estimates within studies.

The reference specification represented by the constant – based on looking at only advanced countries and an unspecific time horizon of the estimate – points to a negative impact of public debt on growth close to zero. Notably, the standard error variable continues to be signed negatively, and it remains significant even after including all the other control variables. This suggests that there is indeed robust evidence for publication selection bias even after controlling for other data and specification choices that explain additional heterogeneity. For developing countries, reported results on the impact of public debt levels on growth seem to be similar as in advanced countries, as we can't find a sizeable and statistically significant coefficient. Similarly, we do not find evidence that the time horizon of the estimates has an impact on the results reported in the literature. In other words: long-run effects of public debt on growth are, on average, reported not to be significantly different from the effects in the short-run or in unspecific time horizons.

A major finding is that addressing the endogeneity between public debt and growth matters. Estimates that tackle endogeneity by using an instrumental variable approach report estimates that lean less towards a negative impact of public debt levels on growth. This implies that we can no longer rule out a zero effect when accounting for endogeneity. Furthermore, we find a significant positive coefficient of the journal impact factor, suggesting that estimates published in higher-impact journals tend to lean less towards reporting negative growth impacts of public debt. Finally, we find that studies that have been received more citations tend to report more negative effect of public debt levels on growth.

Column (2) of table 2 introduces additional moderator variables capturing details of estimation and data choices – including: the measurement of the GDP variable; whether a lagged public debt variable was used to address potential reverse causality; whether the sample is based on a cross-section instead of panel data; and the mean year of the underlying data sample. However, none of these variables exhibits a statistically significant coefficient, although they make a small contribution to explaining variation in the standardised coefficients. In column (3), we introduce two moderator variables capturing whether investment and inflation were controlled for in the underlying regression model, respectively. We find that holding investment or inflation constant does not have a systematic impact on the reported results. Therefore, there is no evidence that the growth impact of higher public debt levels runs through investment or inflation. In the extended specifications (2) and (3) of Table 2, we confirm the main previous findings from model (1). In particular, there is evidence for publication selection bias in favour of negative effects of public debt on growth; and tackling endogeneity between public debt and growth contributes to estimates that lean less towards the negative side.

Column (4) of table 2 provides a robustness check by only looking at a small sample of estimates that are preferred by the authors of primary studies compared to other reported estimates. By restricting the sample to preferred estimates only, we check whether potential dependence of estimates within studies – beyond what we can tackle by clustering the standard errors at the study level – has an impact on our meta-regression results. However, the results are strikingly similar to the baseline results in column (1), although the coefficient of the variable capturing endogeneity is even more strongly positive – which supports our finding that there is little to no evidence for negative growth effects of public debt on growth after correcting for publication bias, and that tackling endogeneity leads to less negative effect estimates. Finally, column (5) conducts another robustness check by using an alternative standardised effect size, namely the partial correlation coefficient (to allow comparisons with the standardised coefficient in our preferred specification). In using the partial correlation, the quantities of the coefficients are not directly comparable to columns (1)-(4), but the signs and notions of statistical significance allow for a similar interpretation. While using the partial correlation coefficient does not lend itself to an interpretation of the economic relevance of the coefficients, the results in column (5) confirm our main findings, although the coefficient of the journal impact factor loses significance, and there is some evidence that studies using more recent data tend to report estimates that lean more to the positive side.

Table 2 / Meta-regression results, linear effect of public debt levels on growth

	(1) base	(2) data + est.	(3) add. control	(4) prefer.	(5) PCC
Constant	0.003	0.001	0.003	0.002	0.109
	(0.003)	(0.005)	(0.002)	(0.002)	(0.069)
StandardError	−0.859**	−0.937***	−0.915**	−1.345***	−1.497**
	(0.329)	(0.301)	(0.339)	(0.294)	(0.707)
DevelopingCountriesOnly	−0.002	−0.003	−0.003	0.003*	−0.170**
	(0.003)	(0.003)	(0.004)	(0.002)	(0.082)
MixofCountries	−0.004	−0.004	−0.004	−0.008**	−0.060
	(0.004)	(0.004)	(0.004)	(0.003)	(0.047)
LongRunExplicit	−0.002	−0.001	−0.002	−0.001	−0.018
	(0.003)	(0.003)	(0.002)	(0.002)	(0.032)
ShortRunExplicit	−0.002	−0.001	−0.002	−0.001	−0.017
	(0.003)	(0.003)	(0.002)	(0.002)	(0.021)
TacklingEndogeneity	0.006***	0.009***	0.008***	0.011***	0.065**
	(0.002)	(0.003)	(0.003)	(0.003)	(0.026)
NormalizedImpactFactor	0.009**	0.007**	0.009**	0.010**	0.099
	(0.004)	(0.003)	(0.004)	(0.004)	(0.084)
Citations	−0.002***	−0.002***	−0.002***	−0.002***	−0.029**
	(0.0004)	(0.001)	(0.0005)	(0.0004)	(0.012)
GrowthPerCapita		0.003			0.007
		(0.003)			(0.057)
LaggedPublicDebt		0.002			0.039
		(0.003)			(0.065)
CrossSection		−0.004			−0.157
		(0.004)			(0.118)
MeanYearData		0.0001			0.002***
		(0.0001)			(0.0004)
Investment			0.0002		
			(0.003)		
Inflation			0.004		
			(0.002)		
Observations	566	566	566	78	566
R^2	0.391	0.406	0.413	0.584	0.279
Adjusted R^2	0.382	0.393	0.402	0.536	0.264

Note: The dependent variable is the standardised coefficient, with the exception of column (5), which uses the partial correlation coefficient (PCC). Numbers in brackets are standard errors, which were clustered at the study level. All results were obtained by using Weighted Least Squares (weights based on the inverse of the variances). The first column shows our preferred baseline specification (base). The second column starts from the baseline specification and additionally controls for estimation and data details; column (3) controls for publication characteristics. The fourth column reports results from the baseline specification when we only look at preferred estimates from the 33 primary studies, and column (5) uses the partial correlation coefficient as the alternative effect size. *, **, *** denote statistical significance at the 10%, 5% and 1% level, respectively.
Source: Own calculations.

5. Threshold effects

We have shown so far that there is little to no evidence for a negative linear effect of public debt on growth once we correct for publication selection bias and tackle endogeneity; however, this could still be consistent with the existence of (universal) thresholds in the public-debt-to-GDP ratio beyond which growth falls significantly. Therefore, we now turn to providing a meta-regression analysis of the literature on such non-linearities (see section 2.2.2 for a qualitative survey). Comparing standardised estimates across threshold studies is quite simple: we no longer have to rely on the standardised coefficients (see section 3.1), because we can directly compare the public-debt-to-GDP ratios that are reported as different thresholds values in the underlying studies. Notably, we only include threshold estimates that were estimated endogenously, i.e. we exclude threshold values that were set exogenously by the researchers themselves instead of being determined by model estimation (e.g. Reinhart and Rogoff 2010; Kumar and Woo 2015; Bentour 2021).[9] However, since almost all primary studies providing estimates on relevant public-debt-to-GDP thresholds do not report standard errors or t-values, we follow Stanley and Doucouliagos (2012, p. 73) who stress that in such a case the square root of the number of observations can serve as a proxy for precision.[10] Furthermore, we use the same moderator variables as in the meta-regression analysis on the linear impact of public debt on growth above (see section 4.3). However, we do not distinguish the time horizon of the estimates, since it is not accounted for in the threshold literature. Instead, we consider an additional dimension when it comes to different econometric choices in the underlying studies by checking whether the threshold was estimated based on a model specification with a squared public-debt-to-GDP term (e.g. Cecchetti et al. 2011; Checherita-Westphal and Rother 2012). Other studies covered in our dataset use endogenous (panel) threshold models (e.g. Baum et al. 2013; Proaño et al. 2014; Egert 2015b). By including a dummy variable for threshold estimates from squared public debt terms, we are able to assess whether this econometric choice systematically affects the size of reported thresholds. A summary and descriptive statistics of all meta-regression variables are available in appendix G.

Figure 4 shows the distribution of the 260 threshold estimates that we collected from 22 primary studies (see appendix H for a list of all included studies), where the public-debt-to-GDP ratio beyond which growth falls is on the horizontal axis, and the vertical axis depicts the precision of these estimates proxied by the square root of the sample size in the underlying study. The unweighted mean is a threshold in the public-debt-to-GDP ratio of 59.8. However, the threshold estimates are widely scattered: the minimum is 8.4 and the maximum is 147.5; the median is 53.5. It can also be seen that the more precise estimates are spread all over the map, which is an indication that they are not distributed around a “true” effect so that data and specification choices could have a large impact.

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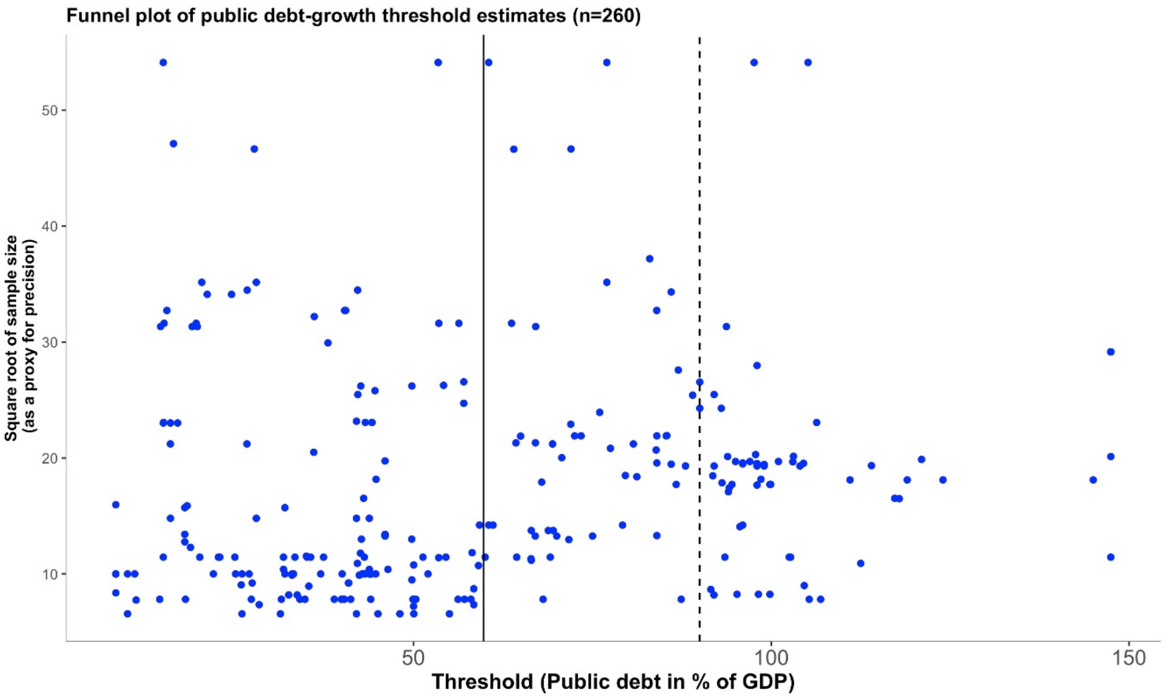
9 We also focus on single threshold estimates, i.e. we exclude estimates based on specifications with multiple thresholds (e.g. Egert 2015a).

10 Gechert (2015) has also relied on the same precision proxy in providing a meta-analysis on the size of fiscal multipliers.

Against this background, we turn to the meta-regression analysis. We again estimate our models based on equation (3), where the dependent variable is now the threshold-estimate of the public-debt-to-GDP ratio, and the standard error variable – which allows for potential publication selection bias (see section 4.1) – is the inverse of the square root of the sample size. We again use WLS with precision-weights, and the standard errors are clustered at the study-level.

Figure 4 / Estimates of thresholds in the public-debt-to-GDP ratio beyond which growth is reduced



Source: Own calculations.

Table 3 shows the meta-regression results for the threshold estimates. Our preferred specification is column (1), which focuses on the main moderator variables discussed in the literature. The reference specification represented by the constant looks at advanced countries only; here, our meta-regression model predicts an average public-debt-to-GDP threshold of 46.4 while holding other moderator variables constant. The proxy variable for the standard error is far from significant. This suggests that publication bias is not a significant issue in the literature estimating threshold effects, which contrasts with the previously analysed set of estimates on the linear effect of public debt levels on growth. However, we can also see that data and specification choices have a major impact on the reported threshold estimates. We find that threshold estimates for developing countries are 18.4 percentage points lower. Most importantly, our results show a large and significant coefficient of 40.9 for using a squared public debt term in estimating the threshold, which would give us a model-based prediction for advanced countries of a public-debt-to-GDP threshold at 87.3% beyond which growth is reduced. In other words, our results show that endogenous threshold estimates broadly consistent with the 90% non-linearity emphasised by Reinhart and Rogoff (2010) are due to a specific econometric choice. Ash et al. (2020) argue, however, that threshold estimates based on including squared debt terms may be unreliable, because they are highly sensitive to outliers. Another feature of estimating quadratic functions is that the slope around the peak is necessarily close to zero, which implies that changes in the public-debt-to-GDP ratio are unlikely to have a large impact on growth. Furthermore, Eberhardt (2019, p. 1564) makes the case that “if variables are nonstationary, then the popular implementations of nonlinearity in the debt-growth literature (squared debt terms or debt terms interacted with threshold dummies) are invalid, since these transformations of the variable are not defined within the (co-)integration framework.” Our meta-regression results call for caution in relying on squared debt threshold estimates, since the estimated threshold values are more much higher than estimates derived from (panel) threshold models, and outliers and/or estimation problems due to unit roots in the underlying data may be driving at least parts of these differences. We find none of the other moderator variables in column (1) of table 3 to be significant. The variable on whether endogeneity issues between public debt and growth are tackled by using instrumental variables is, however, almost significant at the 90% confidence interval (p-value: 0.101) – which provides some (weak) evidence that accounting for endogeneity is systematically related to somewhat smaller threshold estimates.

Columns (2) and (3) of table 3 introduce the same additional moderator variables capturing details of estimation and data choices as in table 2.[11] In column (2), we find that measuring GDP growth in per capita terms has a sizeable impact on the estimated public-debt-to-GDP threshold, which is predicted to be about 20.6 percentage points lower. Furthermore, our results indicate that the coefficient of the mean year of the underlying data sample is positive and significant – suggesting that using more recent data yields somewhat higher threshold estimates. It must also be noted that the coefficient of the developing countries variable loses significance in column (2), suggesting that the evidence for lower thresholds in developing countries compared to advanced countries is not entirely robust to introducing additional moderator variables. In column (3), we find that neither controlling for investment nor considering inflation in the underlying threshold model systematically moderates the reported results. Finally, column (4) of table 3 restricts our sample to preferred estimates only, which broadly confirms the findings from our preferred specification in column (1). However, the coefficients of tackling endogeneity and the journal impact factor in column (4) are larger in absolute size and also statistically significant.[12]

In sum, our meta-regression results indicate that the level of public-debt-to-GDP thresholds estimated in the primary literature is not robust to (small) differences in data and econometric choices. This point has already been made by some of the existing studies (e.g. Egert 2015a, Bentour 2021), but our meta-regression analysis provides more comprehensive evidence. The (close to) 90% threshold emphasised by Reinhart and Rogoff (2010) and some successor studies (Cecchetti et al. 2011; Checherita-Westphal and Rother 2012) is certainly not a “magic number”, but driven by peculiar data and econometric choices that have been contested (e.g. Eberhardt 2019; Ash et al. 2020).

[11] In table 3, we do not include a dummy variable for whether cross-sectional data were used. The reason is simply that none of the 260 estimates in our dataset is based on using cross-sectional data.

[12] Notably, we are not able to use the partial correlation as an alternative standardised effect size in investigating the literature on threshold effects. The reason is that we do need to have information on t-values to calculate the partial correlation, because this information is simply lacking in most cases.

Table 3 / Meta-regression results on threshold effects of public debt levels on growth

	(1) base	(2) data + est	(3) add. control	(4) prefer.
Constant	46.421***	62.120***	47.879***	37.294*
	(11.611)	(10.170)	(11.835)	(20.008)
SEProxy	−85.225	−140.249*	−80.922	−46.816
	(92.180)	(79.231)	(88.779)	(216.722)
DevelopingCountriesOnly	−18.439***	−9.710	−16.966**	−21.581
	(5.550)	(7.173)	(6.161)	(24.614)
MixofCountries	6.265	12.394*	7.143	19.735
	(6.454)	(6.131)	(6.877)	(13.133)
SquaredTerm	40.912***	33.919***	40.398***	47.073***
	(4.314)	(3.770)	(4.382)	(10.921)
TacklingEndogeneity	−7.549	−6.752	−7.836*	−35.619**
	(4.387)	(4.057)	(4.451)	(15.362)
NormalizedImpactFactor	3.065	−11.496	2.667	22.187*
	(9.510)	(6.980)	(9.132)	(12.618)
Citations	1.452	2.094	1.077	0.778
	(2.005)	(1.365)	(2.286)	(3.545)
GrowthPerCapita		−20.592***		
		(5.770)		
LaggedPublicDebt		−4.983		
		(7.069)		
MeanYearData		0.198**		
		(0.070)		
Investment			3.243	
			(5.161)	
Inflation			−3.604	
			(5.626)	
Observations	260	260	260	33
R^2	0.360	0.401	0.362	0.433
Adjusted R^2	0.342	0.377	0.340	0.274

Note: The dependent variable is the standardised coefficient, with the exception of column (4), which uses the partial correlation coefficient (PCC). Numbers in brackets are standard errors, which were clustered at the study level. All results were obtained by using Weighted Least Squares (weights based on the inverse of the variances). The first column shows our preferred baseline specification (base). The second column starts from the baseline specification and additionally controls for estimation and data details; column (3) controls for publication characteristics. The fourth column reports results from the baseline specification when we only look at preferred estimates from the 22 primary studies. *, **, *** denote statistical significance at the 10%, 5% and 1% level, respectively.
Source: Own calculations.

6. Conclusions

This paper has analysed the impact of public debt on growth by providing the first comprehensive quantitative synthesis of the existing literature. We apply meta-regression methods to a novel data set consisting of a total of 828 estimates from 48 different primary studies. In doing so, we show that the unweighted mean of the results reported in the literature suggests that higher public-debt-to-GDP is associated with lower real GDP growth: a 10 percentage points increase in the public-debt-to-GDP ratio is related to a 0.14 percentage points decline in annual economic growth rates; the 95% confidence interval ranges from 0.10 percentage points to 0.18 percentage points. However, this relationship is unlikely to represent a causal effect of public debt on real GDP growth: the literature reports fewer zero and positive growth effects of public debt levels on growth than it should based on econometric theory, i.e. there is evidence for publication selectivity in favour of negative growth effects of public debt levels. Once we correct for this bias, we cannot reject a zero average effect. Furthermore, the meta-regression results show that tackling endogeneity between public debt and growth matters, as it leads to estimates that lean less towards the negative side.

Another main finding is that there is little to no evidence for a universal threshold in the public-debt-to-GDP-ratio beyond which growth falls. We show that threshold estimates prominently identified at 90% or above (e.g. Checherita-Westphal and Rother 2012; Baum et al. 2013; Cecchetti et al. 2011) are highly sensitive to peculiar data and specification choices. As has been argued by Eberhardt (2019) and Ash et al. (2020), thresholds based on estimation via squared public debt terms may be driven by problems with non-stationarity of the underlying data and/or by outliers (e.g. Eberhardt 2019; Ash et al. 2020). A cautious reading of the meta-analytical evidence suggests that the econometric literature has so far not provided robust evidence for universal threshold effects across countries when it comes to the impact of the public-debt-to-GDP ratio on growth. Existing threshold estimates of the public-debt-to-GDP ratio are widely scattered in a range from 8.4% to 147.5% of GDP, and they are strongly influenced by (seemingly minor) choices in terms of data and econometric approaches: while the econometric approaches used in the literature assume the existence of endogenous thresholds, there is a lack of evidence on actually existing uniform debt thresholds. Country-specific nonlinearities in the relationship between public debt and growth may still exist, but if so they are more complex than what has often been suggested by referring to the stylised facts presented in Reinhart and Rogoff (2010). In any case, the meta-regression evidence clearly shows that a 90% public-debt-to-GDP threshold is not a “magic” number, which confirms and echoes previous findings in this regard (e.g. Pescatori et al. 2014; Eberhardt and Presbitero 2015; Egert 2015a; Egert 2015b; Yang and Su 2018; Eberhardt 2019; Ash et al. 2020; Bentour 2021).

Our findings refer to the average effect of public debt on growth. Given the variance, there may be country cases with positive or negative growth effects of higher public debt levels. Similarly, our finding that there is a lack of robust evidence concerning a consistently negative effect of high public-debt-to-GDP ratios does not imply that countries are able to sustain any level of public debt. Governments may still be confronted with country-specific unsustainable debt levels, in particular if interest payments increase strongly (e.g. Eichengreen et al. 2019). However, the meta-regression evidence suggests that – given the further increase in public-debt-to-GDP ratios against the background of the Covid-19 crisis in most countries – there is no evidence for a general urgency to bring down public debt levels to avoid a drag on growth. A cautious reading of the existing evidence calls for caution when it comes to “one-size-fits-all” fiscal policy prescriptions in response to high public-debt-to-GDP ratios such as the simultaneous drive towards fiscal consolidation in Europe from 2010 onwards (e.g. Blyth 2013; Blanchard and Leigh 2013; Konzelmann 2014; Fatas and Summers 2018).

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