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Investor Sentiment, Sovereign Debt Mispricing, and Economic Outcomes

by Ramzy Al-Amine and Tim Willems

I N T E R N A T I O N A L   M O N E T A R Y   F U N D

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August 2020

Abstract

We find that countries which are able to borrow at spreads that seem low given fundamentals (for example because investors take a bullish view on a country's future), are more likely to develop economic difficulties later on. We obtain this result through a two-stage procedure, where a first regression links sovereign spreads to fundamentals, after which residuals from this regression are deployed in a second stage to assess their impact on future outcomes (real GDP growth and the occurrence of fiscal crises). We confirm the relevance of past sovereign debt mispricing in several out-of-sample exercises, where they reduce the RMSE of real GDP growth forecasts by as much as 15 percent. This provides strong support for theories of sentiment affecting the business cycle. Our findings also suggest that countries shouldn't solely rely on spread levels when determining their fiscal strategy; underlying fundamentals should inform policy as well, since historical relationships between spreads and fundamentals often continue to apply in the medium-to-long run.

JEL-Classification Numbers: E32, E37, E62, F34, H63.

Keywords: sentiment, sovereign debt, sovereign spreads, debt crises, over-optimism, growth forecasting.

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We find that countries which are able to borrow at spreads that seem low given fundamentals (for example because investors take a bullish view on a country's future), are more likely to develop economic difficulties later on. We obtain this result through a two-stage procedure, where a first regression links sovereign spreads to fundamentals, after which residuals from this regression are deployed in a second stage to assess their impact on future outcomes (real GDP growth and the occurrence of fiscal crises). We confirm the relevance of past sovereign debt mispricing in several out-of-sample exercises, where they reduce the RMSE of real GDP growth forecasts by as much as 15 percent. This provides strong support for theories of sentiment affecting the business cycle. Our findings also suggest that countries shouldn't solely rely on spread levels when determining their fiscal strategy; underlying fundamentals should inform policy as well, since historical relationships between spreads and fundamentals often continue to apply in the medium-to-long run.

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1 Introduction

Occasionally, countries consider themselves lucky when they can borrow at rates which are considered low given their fundamentals. This may for example happen when investors are optimistic about the country's future – a sentiment which then often spills over to the country's government and its citizens. In sharp contrast to these expectations, however, we find that episodes of sovereign spreads dropping to inexplicably low levels, are often harbingers of future economic difficulties. We arrive at this conclusion via a two-stage approach. First, we regress spreads on fundamentals to uncover an average relationship according to which sovereign debt is priced. We then go on to calculate deviations from this average (the "mispricing" component) and find, via a second-stage regression, that past negative residuals (spreads being *lower* than predicted by fundamentals) signal future difficulties (growth slowdowns and fiscal crises) after a delay of several years. To the extent that inexplicably low spreads are driven by an unduly positive sentiment surrounding a country, this suggests that a wave of over-optimism often brings subsequent damages.

The cost at which sovereigns can borrow in international capital markets varies greatly across time and space. Much of that heterogeneity is driven by variation in fundamentals, with countries being able to borrow more cheaply when characterized by stronger policies and buffers; the external environment matters as well. However, a substantial part of spread-heterogeneity is difficult to explain by fundamentals and seems driven by the sentiment surrounding a country instead (often relating to expectations about the country's future). Take Argentina, for example. Late 2015, the Argentinian population elected Mauricio Macri as their new President. This generated excitement among international investors, who widely saw Macri as a market-friendly reformer (CRS, 2018; Sturzenegger, 2019). On the back of such high hopes, investors were eager to lend to Argentina – enabling the country to avoid fiscal adjustment, instead meeting their financing needs by issuing $56 billion (about 9 percent of 2017 GDP) in external debt between January 2016 and June 2018 – partly even by placing an unprecedented 100-year bond. It however became increasingly difficult to rationalize these benign borrowing conditions based on existing fundamentals (as opposed to expectations about future ones) and soon the sentiment soured. Macri's reforms weren't bringing the immediate improvements investors (and Argentinians) had hoped for, which ultimately made Argentina lose access to international capital markets – leading to the largest loan in IMF history in July of 2018, followed by a restructuring process involving $65 billion in external debt.

Argentina is by no means unique on this. In January 2017, *The Economist* noted: "In 2014 Mozambique seemed a good place to host the IMF's 'Africa Rising' conference. The economy was buoyant, having grown by 7% a year for a decade. Offshore gas promised riches. Investors were optimistic, so much so that, in 2013, they snapped up $850m of bonds [...] But Mozambique's rise has halted. Those [bonds] were the first misstep in a widening scandal that led the government to say on January 16th that it would default." Similar examples abound on how, during much of the 2000s, countries in Southern Europe were able to borrow at rates similar to Germany's in expectation of future economic convergence (Bakker and Klingen, 2012; De Grauwe and Ji, 2012; Reis, 2013), making Frankel (2015) remark that convergence of Eurozone spreads (despite varying fundamentals) "was viewed as a good thing rather than a bad thing". Related narratives can be found on the adverse consequences following from mispricing of private sector assets (like subprime mortgages).

When a window of cheap borrowing opens, countries might feel tempted (or be advised) to exploit the opportunity and spend heavily – often on a combination of government investment and government consumption.[1] But when those bonds subsequently reach maturity years later, reality may have caught up, the yield discount might have disappeared, and refinancing the bond proves too expensive – creating economic difficulties. Once one recognizes the role that spreads play in determining subsequent policies, it may not come as a surprise that sovereign spreads typically do not perform well as crisis predictors (Berg, Borensztein, and Pattillo, 2004). More generally, our findings suggest that countries shouldn't overly rely on spread levels when determining their fiscal strategy; underlying fundamentals should inform policy as well, since often "this time turns out to be no different" and historical relationships between spreads and fundamentals continue to apply in the medium-to-long run.[2]

This paper builds upon two established literatures. First, it relates to papers analyzing sovereign spreads – both its determinants (cf. the large literature following Edwards (1984)) as well as its consequences for output fluctuations and the occurrence of fiscal crises (like Uribe and Yue (2006), Born et al. (2020), and Ciarlone and Trebeschi (2005)). Our paper suggests that it is important to distinguish between that part of the spread

[1] Collier (2017) has observed a bias towards government consumption, highlighting the case of Ghana where "between the discovery [of oil] in 2007 and the onset of extraction in 2011, the government responded to the pressure of exaggerated expectations by borrowing commercially on the international bond market, using the proceeds predominantly for consumption (...) [By] the end of 2014 an emergency IMF program was necessary" (p.224). Buffie and Krause (1989) give a similar description for Mexico following their 1977 discovery of oil; Harberger (1985) does so in relation to Venezuela.

[2] Also see Reinhart and Rogoff (2008) who note that "crisis-prone countries (...) tend to over-borrow in good times, leaving them vulnerable during the inevitable downturns. The pervasive view that 'this time is different' is precisely why it usually isn't different, and catastrophe eventually strikes again". Sturzenegger (2019) quotes Rüdiger Dornbusch: "Whenever I visit a country they always say 'You don't understand Professor Dornbusch, here it is different'... Well, it never is."

which is driven by fundamentals, and its more sentiment-driven "mispricing" component.

Second, we build upon papers analyzing the impact of sentiment on economic activity. So far, the associated theoretical literature has mostly focused on the behavior of the private sector – cf. the work on "Pigou cycles" (Beaudry and Portier, 2014), as well the ideas of Irving Fisher and Hyman Minsky (with Gennaioli, Shleifer, and Vishny (2012), Boz and Mendoza (2014), and Bhattacharya et al. (2015) providing recent formalizations). On the empirical front, López-Salido, Stein, and Zakrajšek (2017) show that past episodes of elevated sentiment in private credit markets lower future growth, with Krishnamurthy and Muir (2017) focusing on the link with financial crises. Different from these papers, we focus on sovereign borrowing behavior and find that an overly optimistic sentiment in the past regarding a country's outlook, often comes with negative repercussions in the future. Herewith, the current paper also relates to Beaudry and Willems (2018): they analyze the macroeconomic impact of IMF forecast errors (exploiting the pseudo-random allocation of IMF Mission Chiefs, who are shown to differ in their individual degrees of optimism) and report that economic crises are more likely to arise in countries where past growth expectations have been overly optimistic.

Adding to the existing literature, we go on to show that incorporation of a sentiment proxy has strong out-of-sample predictive power – lowering the root-mean-square error (RMSE) of quarterly growth forecasts by as much as 15 percent. This suggests that our sovereign debt mispricing measure has practical, out-of-sample relevance – not only enabling forecasters to increase accuracy, but also providing policy makers with an indicator upon which decisions to tighten/loosen macroprudential regulation can be based.

2 Data

At the core of this paper lies a dataset which includes sovereign spreads, national account series, and other variables. Different from most earlier analyses, our data are at the quarterly frequency. The spreads data cover a sample of advanced and emerging economies for the period 1995q1-2019q2 (albeit in an unbalanced way, leading to 5,871 country-quarter observations). Where available, we source spreads data from JPMorgan's Emerging Market Bond Index (EMBI, which measures spreads by comparing the yield on dollar-denominated government debt of a particular country, with the yield on US government bonds of similar maturity); we augment these observations with data for advanced economies where CDS spreads yield a measure of default risk (Born et al. (forthcoming; 2020) construct their dataset in a similar way). Appendix Table A1 lists all 89 countries included in the analysis.

In Section 5 we will furthermore analyze the occurrence of fiscal crises. Data on fiscal crises are only available at the annual frequency and are taken from IMF (2020).[3] They take the concept of "fiscal crisis" to include sovereign defaults, sovereign debt restructurings, reception of large official/bilateral financing packages (e.g. from the IMF), and inflationary episodes (as this is another way in which unsustainable debt can manifest itself; Sargent and Wallace (1981)).[4]

The nature and origin of other variables present in this paper is relatively standard and detailed in Appendix Table A2.

3 Analyzing the impact of sovereign debt mispricing on economic outcomes

Results are based upon a two-stage approach. The first stage estimates a regression which describes the "fundamental" determinants of sovereign spreads. This can be seen as an average relationship according to which sovereign debt has been priced over time and space. A second-stage regression subsequently analyzes the impact of first-stage residuals (the "mispricing" component, henceforth indicated by μ, which we think of as proxying sentiment towards a country) on future economic growth; Section 5 contains results when the second-stage dependent variable is a dummy capturing the onset of fiscal crises.

Our procedure – which can be seen as a "sovereign" equivalent to Gilchrist and Zakrajšek (2012, who analyzed the impact of corporate debt mispricing on economic activity) – can be summarized by the following set of regression equations:

$$\ln S_{it} = \alpha + \beta X_{it} + \mu_{it}, \tag{1}$$

$$y_{it} = \theta + \gamma W_{it} + \delta S_{it-1} + \kappa \mu_{it-\ell} + \varepsilon_{it}. \tag{2}$$

Here, S_{it} is country i's sovereign spread during period t, X_{it} a vector of variables ("fundamentals") that are believed to drive spread levels, W_{it} contains variables affecting the outcome variable y_{it} (which may also include lags of y and/or the variables in X),

[3]This dataset of fiscal crises builds on IMF staff reports, Gerling et al. (2017), and Beers and De Leon-Manlagnit (2019).

[4]IMF (2020) includes inflationary episodes if annual inflation doubled relative to the previous year and stands above 25 percent, or if inflation exceeds 100 percent. Episodes of large official/bilateral support are included if the support exceeds 5 percent of GDP, or if it takes the form of an IMF program with access greater than 100 percent of the country's IMF quota.

while $\mu_{it-\ell}$ is country i's ℓ-period lagged first-stage residual. A positive (negative) value of μ implies that the spread is higher (lower) than suggested by fundamentals. Regression (2) controls for lagged spread levels, as they are believed to have a direct, short-term impact on the dependent value of interest, for example through the working capital channel (Neumeyer and Perri, 2005).[5]

Following the literature (Edwards (1984); Eichengreen and Mody (2000); Dell'Ariccia, Schnabel, and Zettelmeyer (2006)), we employ pooled OLS – also since fixed effects are a "black box", picking up non-fundamental factors (which we want to capture in μ); reassuringly, however, our results are robust to adding fixed effects. We furthermore follow the literature by taking *logged* spreads to be the dependent variable in regression (1), as this leads to a better fit with the distribution of S_{it} being right-skewed (but results are robust to using the level and/or removing outliers). With respect to regression (2), which features a generated regressor (μ), Pagan (1984) has shown that standard errors are asymptotically valid under the null hypothesis that the associated coefficient κ is zero (which is the hypothesis we are interested in; also see Born et al. (2020)).[6] Following the recommendation in Bertrand, Duflo, and Mullainathan (2004) and Petersen (2009), we cluster standard errors at the country level. Inference is robust to two-way clustering (by country and by time).

In equation (2), economic considerations call for a meaningful lag ℓ: the idea after all is that *past* spread-mispricing (as captured by $\mu_{it-\ell}$) affects future outcomes by affecting the economy during an interim "build-up" period $(t-\ell, t)$, which only exists meaningfully for $\ell \gg 0$.[7] In line with López-Salido, Stein, and Zakrajšek (2017), our baseline will have ℓ equal to 2 years (8 quarters), but Section 5 documents robustness.

3.1 Sovereign spreads and their determinants

This section describes our first-stage regression, which is meant to link the observed spread level to economic fundamentals X_{it}. The fundamentals in this regression are fairly standard in the literature, and include both country-specific variables (debt-to-GDP ratio, terms of trade, inflation, and institutional quality[8]), as well as global ones (the VIX un-

[5] Uribe and Yue (2006) and Born et al. (2020) find empirically that higher spreads depress activity.

[6] Note how one can also use (1) to eliminate $\mu_{it-\ell}$ from (2) and directly estimate $y_{it} = \widetilde{\theta} + \gamma W_{it} + \delta S_{it-1} + \widetilde{\beta} X_{it-\ell} + \kappa \ln S_{it-\ell} + \varepsilon_{it}$ in one step (where $\widetilde{\theta} = \theta - \kappa\alpha$ and $\widetilde{\beta} = -\kappa\beta$). This produces the exact same results, also in terms of the out-of-sample forecasts in Section 4.

[7] Econometric considerations put the lower bound for this lag at 2 quarters: for $\ell = 1$, μ_{it-1} is already present in (2) as part of the actual spread S_{it-1} (recall equation (1)).

[8] As in IMF (2020), this is a simple average of the indicators "government effectiveness" and "regulatory quality" from the World Bank's Worldwide Governance Indicators database.

certainty index and S&P500 stock returns). While credit ratings or debt sustainability assessments by the IMF/World Bank might affect spreads as well, we do not include them as regressors, as they are not fundamentals themselves and may suffer from various biases: Ferri, Liu, and Stiglitz (1999) for example document a procyclical bias for credit rating agencies, arguing that they pay insufficient attention to fundamentals during economic booms, while Lang and Presbitero (2018) document biases in debt sustainability analyses by the IMF and World Bank. In particular, when sentiment is positive, credit rating agencies or international financial institutions might be reluctant to "rock the boat" and pop the bubble – pandering towards the optimistic consensus view instead (Genberg, Martinez, and Salemi, 2014); Bolton, Freixas, and Shapiro (2012) show that such incentives are particularly strong during boom periods. These possible biases notwithstanding, we will document in Section 5 that our results are robust to adding sovereign credit ratings to the list of covariates.

We also ran specifications featuring the Federal funds rate, but (in line with Longstaff et al. (2011)) we did not find it to add much once the VIX and US stock returns were accounted for (nor did second-stage results vary greatly with this).[9] The same goes for the current account deficit and FX reserve levels.[10] We also fail to find a major role for government deficits (in line with Edwards (1984)), potentially since its breakdown between government consumption and government investment has been shown to matter (Akitobi and Stratmann (2008); Peppel-Srebrny (2017)); data limitations however prevent us from examining this distinction in greater detail. Similarly, there is no consensus on the importance of economic growth in explaining spreads, with prominent studies like Edwards (1984) and Eichengreen and Mody (2000) finding no significant effect (which has led many others to abstract from including growth as a covariate in sovereign spread regressions). For this reason, our baseline specification does not feature real GDP growth, but second-stage results are robust to including this variable in the first stage (see Section 5 for results that are based on a less parsimonious first stage).

Table 1 summarizes our findings. All variables are significant and have the expected signs. While sovereign spreads are strongly related to fundamentals (the R^2 of our first-stage regression stands at 0.57), fundamentals do not seem to explain all variation. This

[9]Since one can debate the appropriateness of including the VIX as a first-stage control (it is arguably also capturing sentiment, albeit of a more global type rather than country-specific), we ran specifications without this variable as well. Our results continue to arise.

[10]While the conventional view is that FX reserve holdings lower sovereign spreads, Sturzenegger (2020) shows that the mode of reserve accumulation matters (unsterilized, or by issuing liabilities). Alfaro and Kanczuk (2009) discuss an additional channel: reserves are valuable as they enable consumption smoothing following default (which is likely to lead to a period of capital market exclusion), so a lower stock of reserves may assure lenders that default is not an option. Empirical evidence is mixed.

points to “sentiment” playing a significant role, a finding previously reported by e.g. Eichengreen and Mody (2000) and De Grauwe and Ji (2012).

The fact that a country is sometimes able to borrow at a spread which deviates from the value suggested by fundamentals, can for example be explained by geopolitics (Ambrocio and Hasan, 2019), by rating agencies or international organizations taking an unduly optimistic view,[11] or through investors/analysts buying into narratives about the future – leading to an “irrationally exuberant” outlook (like investors occasionally do with certain stocks). Linking the latter factor to the examples mentioned in the Introduction, the election of a new President improved sentiment on Argentina, the discovery of offshore gas did so for Mozambique, while hopes for Eurozone convergence gave investors a bullish outlook on many countries in Southern Europe during much of the 1990s and 2000s.

Table 1: OLS regression (1), used to capture the average relationship between spreads and fundamentals

Dependent variable: natural log of sovereign spread ($\ln S_{it}$)	
debt-to-GDP ratio	0.0047008* (1.87)
inflation	0.0061898** (2.43)
terms of trade	−0.0070032*** (−3.62)
governance index	−1.197624*** (−15.28)
VIX	0.0501941*** (10.36)
S&P 500 returns	2.596903*** (6.43)
constant	4.915916*** (16.44)
R^2	0.5694
countries	89
observations	4,651

Note: OLS estimates of regression (1) at the quarterly frequency. The dependent variable is the natural logarithm of the sovereign spread. Numbers in parentheses represent t-statistics, calculated using cluster-robust standard errors. * denotes significance at the 10% level, ** implies significance at the 5% level, *** indicates significance at the 1% level.

[11] See Eichengreen and Mody (2000) and Dell’Ariccia, Schnabel, and Zettelmeyer (2006) for evidence that the “non-fundamental” part of a sovereign credit rating exists and has an effect on spreads. Ratings can end up including a non-fundamental component because of strong personal convictions of individuals responsible for the rating, strategic motives (Ferri, Liu, and Stiglitz, 1999; Bolton, Freixas, and Shapiro, 2012), while studies have also documented the existence of a “home bias” for sovereign rating agencies (Fuchs and Gehring, 2017).

3.2 Effects on real GDP growth

Now that we have an empirical specification relating spreads to fundamentals, we can also calculate the "mispricing" component μ and analyze the growth impact of the latter via the second-stage regression. As earlier studies have found quarterly real GDP growth rates to be described well by an $AR(2)$ (Chauvet and Potter, 2013), we place that structure at the core of our regression. Since spreads themselves may have an independent and rapid effect on real GDP growth through the working capital channel (Neumeyer and Perri, 2005), we control for last period's spread level S_{it-1} in our regression (aiming to shut down the working capital channel, so that our estimate for κ is focused on capturing the pure effect of past sentiment[12]). Our second-stage regression (2) thus reads:

$$y_{it} = \theta + \gamma_1 y_{it-1} + \gamma_2 y_{it-2} + \delta S_{it-1} + \kappa \mu_{it-\ell} + \varepsilon_{it}.$$

Table 2 summarizes results. In our baseline, we lag the mispricing term by 8 quarters (2 years), but results are robust to different lags or to taking a moving average (see Section 5). Estimates suggest that the coefficient on lagged spread-mispricing is significantly positive: this implies that countries which have been surrounded by an optimistic sentiment ($\mu < 0$), get to experience lower rates of economic growth in the future (and vice versa). In terms of magnitude, the point estimate for κ (the coefficient on μ_{it-8}) suggests that being able to borrow at a "discount" (lower spread than suggested by fundamentals) of one standard deviation (0.873), reduces real GDP growth by an average 0.180 percentage points two years later ($0.873 \times 0.206 = 0.180$). For comparison, our estimates imply that a one standard deviation increase in the spread S_{it} (491.815 basis points[13]), is expected to reduce real GDP growth by 0.225 percentage points.

Note that the direction of our finding is a strong signal, for at least two reasons. First, having $\mu_{it-8} < 0$ means that, 8 quarters ago, country i was facing favorable borrowing conditions (given its fundamentals). One would think that this could boost future growth

[12] Herewith, we aim at a different question relative to Uribe and Yue (2006) and Born et al. (2020): they focus on analyzing the impact of borrowing costs (rather than that of pure sentiment) and thus do not aim to shut down the working capital channel. Our approach of controlling for S_{it-1} is unlikely to block this channel completely, but this turns our results into a conservative lower bound. After all, we find that if the non-fundamental part of the spread works to lower borrowing costs, this tends to *reduce* future growth. Since taking a cost perspective suggests that cheaper borrowing should *boost* future growth (as it leaves more room for investment), our results – described in detail below – constitute a strong finding.

[13] If we were to exclude outliers, a one standard deviation increase would be lower. For example, calculating the standard deviation after dropping the top 5 percentile of all spread observations, halves the standard deviation to 223.331 basis points. Consequently, the reported impact on real GDP growth can be seen as a conservative estimate regarding the importance of the mispricing component (the distribution of which is not skewed) relative to that of the spread level itself.

(as cheaper borrowing – whether or not because of strong fundamentals – leaves more fiscal room for productive investment), but our results suggest the exact opposite (recall footnote 12). Second, if one believes that spreads are co-determined by the market's expectation of a country's economic future, having $\mu < 0$ points to the market seeing reason to expect strong future growth in the associated country (e.g. in anticipation of the activation of a recently-discovered oil well). Again, our results point in the other direction – to growth slowing down in those very countries.[14] Section 5 shows that this is not a statistical artifact stemming from the country experiencing higher growth during the interim period ($\hat{\kappa}$ is positive for all lags around our baseline of $\ell = 8$ quarters).

Column (2) examines potential asymmetries (through interaction with a dummy which takes the value 1 in periods where $\mu < 0$) and finds that the effect is most severe when countries are able to borrow at a discount; episodes where countries are only able to borrow at a "penalty" ($\mu > 0$), seem less harmful. When $\mu < 0$, a one standard deviation spread discount is expected to reduce growth by 0.270 percentage points – an effect which is larger than that for the spread level itself (0.225). This suggests that a country is particularly at risk when investors take a bullish view with respect to that country's future development – essentially betting on an upcoming structural break, not backed by current fundamentals. The opening of such a borrowing window often induces countries to indebt themselves heavily (exploiting the window for which the favorable borrowing conditions last) and our results suggest that such borrowing sprees often do not end well. This result fits with available narrative evidence (which has mostly emphasized the potentially harmful effects of overly cheap borrowing; cf. Weder di Mauro and Zettelmeyer (2017: 13)) and can be seen as evidence that there are real economic forces at play, as opposed to mere statistical biases (since there is no reason why the latter would suggest an *asymmetric* effect).

These findings give rise to questions regarding the underlying driving forces. Since sovereign spreads affect a country's cost of external borrowing, it is natural to look for a link with that variable. Unfortunately, quarterly data on external debt flows (consistent across all countries in our sample) are difficult to obtain, but we can easily incorporate a major driver of external borrowing: current account deficits. When spreads are low relative to fundamentals, it becomes particularly tempting for a country to finance its current account deficit through external borrowing. Column (3) therefore interacts the mispricing term μ with the ratio of the current account deficit-to-GDP ("CAD/GDP").

[14] If one believes that $\mu_i < 0$ is a sign of the market having a positive growth outlook on country i, one can interpret this paper's results as pointing to the difficulties associated with forecasting medium-term growth. This relates to findings that forecasters typically underestimate the forces of mean reversion when it comes to predicting future growth rates, effectively over-estimating the probability with which structural breaks materialize (see Pritchett and Summers (2014) and Hellwig (2018)).

Table 2: OLS regression (2), analyzing impact of past spread-mispricing on real GDP growth

	Dependent variable: real GDP growth (y_{it})			
	(1: baseline)	(2: sign of μ)	(3: CAD/GDP)	(4: DEF/GDP)
y_{it-1}	0.8869012*** (17.14)	0.8848846*** (17.19)	0.8805322*** (17.19)	0.8959092*** (17.25)
y_{it-2}	−0.0585542 (−1.13)	−0.0597324 (−1.15)	−0.0546642 (−1.04)	−0.0647883 (−1.25)
S_{it-1}	−0.0004574 (−1.64)	−0.0004567 (−1.61)	−0.0004543 (−1.64)	−0.0004311* (−1.69)
μ_{it-8}	0.2063282*** (4.10)	0.0515458 (0.80)	0.180464*** (3.54)	0.1986631*** (3.96)
$(CAD/GDP)_{it-8}$			−0.0110134 (−1.60)	
$(DEF/GDP)_{it-8}$				−0.018155 (−1.49)
$\mu_{it-8} \times \mathbb{I}\{\mu_{it-8} < 0\}$		0.2581765*** (2.45)		
$\mu_{it-8} \times (CAD/GDP)_{it-8}$			0.0238574** (2.24)	
$\mu_{it-8} \times (DEF/GDP)_{it-8}$				0.0118626 (0.97)
constant	0.5764751*** (5.26)	0.6711882*** (5.41)	0.5875962*** (5.74)	0.5585442*** (5.50)
R^2	0.7481	0.7486	0.7499	0.7528
countries	79	79	79	79
observations	3,835	3,835	3,835	3,825

Notes: OLS estimates of regression (2) at the quarterly frequency. The dependent variable is real GDP growth. Numbers in parentheses represent t-statistics, calculated using cluster-robust standard errors. * denotes significance at the 10% level, ** implies significance at the 5% level, *** indicates significance at the 1% level.

Results strongly indicate that the detrimental effects of being able to borrow at a discount are largest in countries running current account deficits, suggesting that undue optimism is particularly harmful for countries in a position of external deficit; the (lagged) current account-to-GDP ratio itself does not have a significant impact on growth. At this stage, one may worry about a high degree of comovement between μ and CAD, but this correlation stands at only 0.0335 in our sample – rendering multicollinearity not much of a concern. It also suggests that situations where countries can borrow at a discount, are not strongly associated with current account deficits per se. However, when these phenomena do occur jointly, the future consequences are likely to be negative. Herewith, our results are also informative on the question as to when current account deficits should be of concern (Milesi-Ferretti and Razin, 1996; Obstfeld, 2012) – namely when external borrowing conditions seem inexplicably favorable given fundamentals. Such a situation may induce a country to finance the current account deficit through (cheap) external borrowing, but that sets the country up for adjustment challenges when fundamental forces kick in and the spread discount disappears.

Column (4) suggests that such an interaction does not exist for government deficits ("DEF"). Quarterly data on the latter are not widely available, and numbers tend to be incomparable as countries differ in accounting practices (how much they push off-budget), but we can proxy it via the change in the debt-to-GDP ratio (which is less vulnerable to accounting differences). When adding this variable and its interaction with μ to the baseline, neither is significant – indicating that the main channel runs through external borrowing (and that domestic government borrowing is of secondary importance in relation to μ). This is intuitive as μ mainly captures mispricing of *external* debt.

4 Out-of-sample performance

So far, we have established a statistically significant effect of past sovereign debt mispricing, on future growth. However, given the nature of the question central to this paper, statistical significance cannot substitute for out-of-sample predictive power (which can tell us something about the external validity of our results).

This section therefore describes several cross-validation exercises, which confirm the relevance of past sovereign debt mispricing. In all cases, we will evaluate model performance against an $AR(2)$ model – as this has been proven to be a rather hard-to-beat forecasting model for quarterly real GDP growth (Chauvet and Potter, 2013). For completeness, we also consider an $AR(2)$ augmented with lagged spread levels only (i.e.: $y_{it} = \theta + \gamma_1 y_{it-1} + \gamma_2 y_{it-2} + \delta S_{it-1} + \varepsilon_{it}$), which does not consider our mispricing variable μ_{it-8}. This exercise confirms the relevance of the latter. Across all models, we do not include fixed effects, so as to avoid complications stemming from the Nickell bias. While this likely means that our resulting model is not optimized, finding the optimal forecasting model is not our objective: we are merely interested in comparing three different models – and the absence of fixed effects across all competing specifications, does not favor one model over the others.

To compare the out-of-sample performance of the three competing models, we first implement a "leave-one-out" cross-validation procedure which follows an iterative process where the estimation sample consists of all countries in the dataset *except for* country J, while the testing sample is country J. The procedure thus starts by dropping country J, then estimates the first- and second-stage regression, after which the coefficients are used to predict next-quarter growth in the excluded country J (projecting on $y_{Jt-1}, y_{Jt-2}, S_{Jt-1}, \mu_{Jt-8}$).

Table 3 summarizes results (obtained after looping over all countries in the sample). Our model, which augments a standard $AR(2)$ with lagged spreads (S_{it-1}) and past mispricing (μ_{it-8}), reduces the RMSE by about 15 percent (from 2.03, to 1.72). In practice, beating an $AR(2)$ has proven difficult out-of-sample. Chauvet and Potter (2013) survey the literature on this result and consider a wide variety of (sophisticated) models themselves – finding that, when it comes to forecasting US real GDP growth, an $AR(2)$ is hard to beat with an RMSE of 2.15. Most alternative models they consider (DSGE, VAR, Markov switching models) yield a higher RMSE; for their sample, only an AR-dynamic factor model with Markov switching is able to lower the RMSE of forecasts, by 10 percent (to 1.92).[15] Our (much simpler) procedure improves upon the $AR(2)$ benchmark by 15 percent. As the bottom row of Table 3 shows, the improvement is largely driven by inclusion of past mispricing (as captured by μ_{it-8}); just adding lagged spreads to the baseline $AR(2)$ does not lower the RMSE.

Table 3: Leave-one-out out-of-sample RMSE of different models for one-quarter ahead growth forecasts

model	RMSE	ratio to $AR(2)$
$y_{it} = \theta + \gamma_1 y_{it-1} + \gamma_2 y_{it-2}$	2.03	1.00
$y_{it} = \theta + \gamma_1 y_{it-1} + \gamma_2 y_{it-2} + \delta S_{it-1} + \kappa \mu_{it-8}$	1.72	0.85
$y_{it} = \theta + \gamma_1 y_{it-1} + \gamma_2 y_{it-2} + \delta S_{it-1}$	2.03	1.01

Notes: RMSE of forecasts generated according to a leave-one-out procedure, whereby the model is estimated on a sample excluding country J, to predict one-quarter ahead growth for country J. Results loop over all countries in our sample. To ensure a fair comparison across rows, all RMSEs are calculated over the same observations.

Second, we cut the sample along the temporal dimension. We consider all observations prior to date T as part of the estimation sample. Resulting coefficients are subsequently used to predict next-quarter growth in the testing sample (which spans T-2019q2). We repeat this procedure for all T between 2015q1 and 2016q4 and average over the resulting RMSEs. Results are depicted in Table 4. Again we find that our augmented model

[15] Chauvet and Potter (2013) also report that Blue Chip forecasts (a composite from about 50 major banks, financial/industrial/consulting firms, and universities) come with a similar RMSE of 1.92.

($AR(2)$ with lagged spreads S_{it-1} and past mispricing μ_{it-8}) is able to reduce the RMSE by 15 percent, relative to the $AR(2)$ benchmark. Just augmenting the $AR(2)$ with lagged spreads reduces the RMSE of forecasts by only 5 percent, confirming that information on past mispricing is a valuable addition when predicting future outcomes.

Table 4: Time-based out-of-sample RMSE of different models for one-quarter ahead growth forecasts

model	RMSE	ratio to $AR(2)$
$y_{it} = \theta + \gamma_1 y_{it-1} + \gamma_2 y_{it-2}$	2.17	1.00
$y_{it} = \theta + \gamma_1 y_{it-1} + \gamma_2 y_{it-2} + \delta S_{it-1} + \kappa\mu_{it-8}$	1.86	0.85
$y_{it} = \theta + \gamma_1 y_{it-1} + \gamma_2 y_{it-2} + \delta S_{it-1}$	2.07	0.95

Notes: RMSE of forecasts generated according to a time-based procedure, whereby the model is estimated on a sample containing data up to time period T, to predict one-quarter ahead growth for periods $> T$. Results average over all T between 2015q1 and 2016q4. To ensure a fair comparison across rows, all RMSEs are calculated over the same observations.

5 Robustness and further results

We have found our results to be highly robust to different lag lengths ℓ and to deploying a less parsimonious specification in our first-stage regression. Results also continue to hold when considering a different dependent variable of interest (the eruption of fiscal crises).

In our baseline specification for real GDP growth, we considered a lag ℓ of 8 quarters (2 years) – motivated by prior evidence (obtained through very different methods) presented in López-Salido, Stein, and Zakrajšek (2017). Figure 1 however illustrates that our results are by no means unique to this particular lag choice. In fact, results suggest that the effect is maximized at an horizon of 6 quarters. When moving away from $\ell = 6$ in either direction, the estimate for κ is decreasing in both size and significance.[16] The dynamics of unfounded optimism seem to be such that it is able to peter out and slow growth over the course of about 4 to 14 quarters – a finding also consistent with Beaudry and Willems (2018) who document an impact at the 3-year (12-quarter) horizon.

Second, our baseline results come from a reasonably parsimonious first-stage specification. While it features many of the most commonly included covariates (and while it comes with a high R^2 of 0.57), one might worry that our findings are driven by an omitted variable bias. This seems unlikely though. To see why, take the example of μ_{it}

[16]Results are also robust to taking a moving average (MA) of past μ's. For example, when the MA-window is set to include $[\mu_{it-14}, \mu_{it-2}]$, $\hat{\kappa} = 0.1548611$ (p-value = 0.001).

being < 0 (so the country is borrowing at a spread lower than suggested by included fundamentals). If this is due to an omitted fundamental variable in our first stage (possibly a country-specific factor that could even be a fixed effect), it must be that there is some fundamental, absent from the first stage, on which country i looks strong (otherwise μ_{it} would not be < 0). One would then however expect that country i's *strength* along this omitted fundamental, leads to *higher* future growth. Our results however point in the exact opposite direction, so if there is an omitted variable bias, it is likely to work *against* our second-stage result. When we artificially create an omitted variable bias by dropping one of the fundamentals featuring in our first-stage regression, we indeed find that our estimate of κ moves towards zero, while increasing RSMEs in our out-of-sample forecasting exercises. This supports the hypothesis that the presence of an omitted variable bias in our first stage, works against our second-stage results.

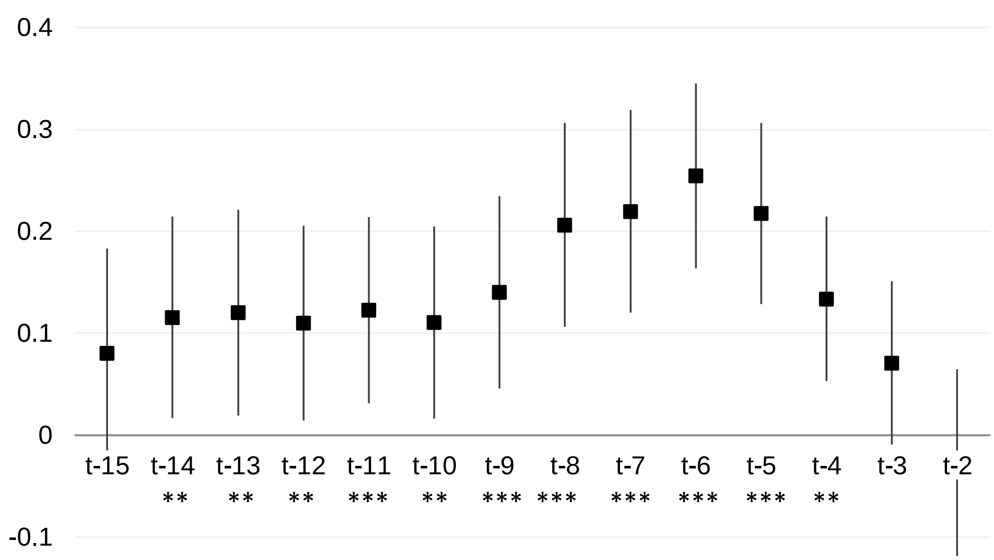

Figure 1: Estimates of κ in OLS regression (2) at the quarterly frequency when the dependent variable is real GDP growth, varying the lag ℓ in $\mu_{it-\ell}$. Squares indicate the point estimate; lines capture the 95% confidence interval, calculated using cluster-robust standard errors. * denotes significance at the 10% level, ** implies significance at the 5% level, *** indicates significance at the 1% level.

Our baseline conclusions are also supported by Appendix Tables A3 and A4, which contain results for a richer (less parsimonious) first stage. Relative to our baseline (displayed in Table 1), the regression underlying Table A3 adds real GDP growth, the quarter-on-quarter change in the debt-to-GDP ratio, the ratio of the current account to GDP, the ratio of FX reserves to GDP, and the US Federal funds rate. It also includes S&P's

sovereign credit rating (converted to a numerical scale where 1 = default and 21 = AAA; using ratings by Fitch or Moody's produces nearly identical results). Table A4 shows that our main finding continues to arise.

Finally, our mispricing variable also seems to affect other dependent variables of interest. Figure 2 shows the estimate of κ (for different choices of the lag ℓ, now expressed in years; see footnote 17) when the dependent variable is a dummy variable that takes the value 1 in years of the eruption of fiscal crises.[17]

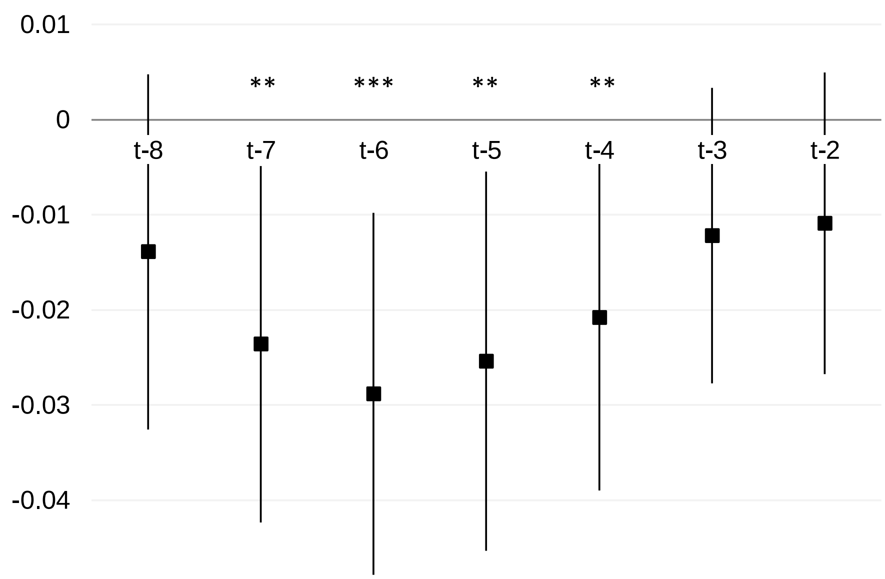

Figure 2: Estimates of κ in OLS regression (2) at the annual frequency when the dependent variable is a fiscal crisis dummy, varying the lag ℓ in $\mu_{it-\ell}$. Squares indicate the point estimate; lines capture the 95% confidence interval, calculated using cluster-robust standard errors. * denotes significance at the 10% level, ** implies significance at the 5% level, *** indicates significance at the 1% level.

Here, we again find a significant effect (with episodes of having $\mu_{t-\ell} < 0$ making the eruption of future fiscal crises more likely), but at longer lags (of 4-7 years).[18] This delay accords well with the average maturity of government debt[19] and indicates that the effect of borrowing at a discount is first to slow down growth (which is a rather gradual event),

[17]Unfortunately, this dummy series is only available at the annual frequency. Results are generated via a second-stage regression where, in terms of equations (1) and (2), $W = X$. The second-stage regression thus includes the same control variables as the first stage (since the determinants of spreads, which proxy default risk, are also the likely determinants of fiscal crises), augmented with last period's spread level S_{t-1} and the past mispricing variable $\mu_{t-\ell}$. Figure 2 varies the lag length ℓ in years.

[18]Results are for the linear probability model, but findings are robust to estimating via logit or probit.

[19]OECD data for selected countries suggests an average maturity of government debt of 5.6 years.

after which – in some cases – a more dramatic event follows in the form of a fiscal crisis (e.g. when the time comes to refinance the debt that was contracted while the country was unduly favored by markets). The data herewith suggest that the growth slowdown is not a side effect of the arrival of a fiscal crisis, but rather that the growth slowdown pre-dates (and may be the cause of) fiscal difficulties which sometimes arise later on – potentially via debt overhang. It furthermore shows that our findings are not specific to considering real GDP growth as the dependent variable.

6 Conclusion

This paper has shown that undue optimism surrounding a country (as proxied by countries being able to borrow at spreads lower than seem warranted by fundamentals) tends to be followed by adverse economic outcomes (lower economic growth and, with a further lag, increased incidence of fiscal crises). In various cross-validation exercises, we show that accounting for sovereign debt mispricing has strong out-of-sample predictive power – reducing the RMSE of real GDP growth forecasts by as much as 15 percent relative to an $AR(2)$ model (which has proven hard to beat in practice). This is strong evidence that sentiment affects the business cycle, with waves of undue optimism often bringing economic damages later on. At the same time, our results imply that "bond vigilantes" should not lose sight of fundamentals (both for their own sake, as well as that of borrowing countries), especially in the face of optimistic narratives regarding a country's future.

The effect appears to be asymmetric, stemming solely from the harmful effects of borrowing at spreads *lower* than warranted by fundamentals. This aligns well with anecdotal evidence, while also suggesting that there are real economic forces at play (as opposed to mere statistical biases, which would not bring an asymmetric effect). Our results thus suggest that episodes during which a country finds itself able to borrow at rates which are low given fundamentals, may actually turn out to be a gift much like the mythological Trojan horse. Governments are generally thought to try and influence rating agencies to obtain a better rating and thereby lower borrowing costs (De Moor et al., 2018). Our findings however point out that such effects may be counterproductive and backfire, as better ratings cannot substitute for solid fundamentals in the medium-to-long run (while they might induce harmful over-borrowing whilst the spread-discount lasts). Our results furthermore call for caution when anchoring fiscal rules in spread levels. Although spreads can in principle serve like a useful "summary statistic" capturing the various determinants of fiscal space (Hatchondo, Martinez, and Roch, 2017), the presence of sentiment-like factors may call for paying attention to fundamentals as well.

The exact mechanism by which an episode of inexplicably cheap borrowing brings economic difficulties deserves further consideration in future studies. Evidence presented in this paper points to an unduly positive sentiment being especially harmful in countries running large current account deficits, which are often financed by large inflows of external borrowing (particularly if this option looks extraordinarily cheap). The responsible channel could be economic in nature, political, or a combination thereof (Weder di Mauro and Zettelmeyer, 2017: 13).

In terms of economic channels, it may be that a wave of over-optimism induces heavy borrowing leading to classic debt overhang (Myers, 1977; Krugman, 1988), or that it leads to "investment hangover" (Beaudry, Galizia, and Portier (2018); Rognlie, Shleifer, and Simsek (2018)). A surge in financial inflows could also bring real exchange rate over-valuation, eroding the country's level of competitiveness (as with Dutch disease) and potentially bring an inefficient allocation of resources (Reis, 2013; Benigno and Fornaro, 2014; Benigno, Converse, and Fornaro, 2015; Gopinath et al., 2017).

As to the more political channels, it may be that the contracted funds are not being put to good use – either because they fuel corruption ("institutional Dutch disease") or because they flow to finance government consumption rather than productive investment (recall footnote 1; Gelb (1988) also describes how oil windfalls frequently lead to grandiose, non-productive investments). The availability of financing could furthermore mask inefficiencies and buy countries time, enabling them to postpone needed reforms (Fernández-Villaverde, Garicano, and Santos (2013) highlight this channel for the Eurozone). Finally, a bullish sentiment can also provide a false sense of security – leading to riskier policies in other areas (e.g. through an unwarranted loosening of macroprudential regulation or via the implementation of more populist policies, which are easier to finance when sentiment is positive). When the country's fundamentals catch up and the spread discount disappears, the country is likely to struggle to adjust to its new reality.

On the normative (and practical) front, our results can be interpreted as calling for countercyclical macroprudential regulation (i.e. regulation which tightens during boom periods when foreign borrowing is cheap, $\mu < 0$), as such a policy of "leaning against sentiment" might bring stability gains (Flemming, L'Huillier, and Puguillem, 2019). Since our mispricing proxy μ can be calculated in real time, continuous tracking of this variable may be able to guide discussions as to whether a tightening of macroprudential measures is warranted. Incorporating information from this variable in forecasting models may furthermore prove to be an easy path towards producing more accurate forecasts.

7 Appendix: list of included countries, origin of variables, and robustness checks

Table A1: 89 countries included in the analysis

Algeria	Bulgaria	El Salvador	Italy	Netherlands	Romania	Thailand
Angola	Canada	Estonia	Jamaica	New Zealand	Russia	Trinidad&Tobago
Argentina	Chile	Finland	Japan	Nigeria	Saudi Arabia	Tunisia
Armenia	China	France	Jordan	Norway	Serbia	Turkey
Australia	Colombia	Germany	Kazakhstan	Oman	Slovak Rep.	UAE
Austria	Costa Rica	Greece	Kuwait	Pakistan	Slovenia	UK
Azerbaijan	Croatia	Guatemala	Latvia	Panama	South Africa	Ukraine
Bahrain	Cyprus	Hungary	Lebanon	Paraguay	South Korea	US
Belarus	Czech Rep.	Iceland	Lithuania	Peru	Spain	Uruguay
Belgium	Denmark	India	Malaysia	Philippines	Sri Lanka	Venezuela
Belize	Dominican Rep.	Indonesia	Mexico	Poland	Suriname	Vietnam
Bolivia	Ecuador	Ireland	Morocco	Portugal	Sweden	
Brazil	Egypt	Israel	Namibia	Qatar	Switzerland	

Table A2: origin of variables

Variable	Origin	Remarks
sovereign spread	Bloomberg	see Section 2
real GDP growth	Haver	
debt-to-GDP	IMF	
inflation	Haver	CPI-based
terms of trade$^{\diamond}$	Haver	
governance index$^{\diamond}$	World Bank	see footnote 8
current account-to-GDP	IMF	
VIX	St Louis Fed	
S&P 500 returns	Bloomberg	
sovereign credit rating	Bloomberg	
fiscal crisis dummy	IMF (2020)	see Section 2

Note: For variables indicated by $^{\diamond}$, quarterly values are not available and the annual value is used for all quarters of the associated year.

Table A3: OLS regression (1), used to capture the average relationship between spreads and fundamentals with additional covariates

	Dependent variable: natural log of sovereign spread ($\ln S_{it}$)
debt-to-GDP ratio	-0.0015111 (-1.33)
inflation	0.0076616^{**} (2.59)
terms of trade	-0.0051727^{***} (-2.81)
governance index	-0.0823701 (-0.74)
VIX	0.0375399^{***} (8.80)
S&P 500 returns	1.19756^{***} (3.39)
real GDP growth	-0.0215626^{***} (-3.05)
change in debt-to-GDP	0.0067865^{*} (1.92)
current account-to-GDP	-0.12601^{**} (-2.55)
FX reserves-to-GDP	0.0013224 (0.55)
S&P sovereign credit rating	-0.2279616^{***} (-12.53)
US Fed funds rate	-0.1877812^{***} (-5.70)
constant	8.134497^{***} (24.31)
R^2	0.7811
countries	78
observations	3,963

Note: OLS estimates of regression (1) at the quarterly frequency. The dependent variable is the natural logarithm of the sovereign spread. Sovereign credit rating is converted to a numerical scale, with higher numbers representing a better rating (default = 1 and AAA = 21). Numbers in parentheses represent t-statistics, calculated using cluster-robust standard errors. * denotes significance at the 10% level, ** implies significance at the 5% level, *** indicates significance at the 1% level.

Table A4: OLS regression (2), analyzing impact of past spread-mispricing on real GDP growth

Dependent variable: real GDP growth (y_{it})	
y_{it-1}	0.9172771*** (16.50)
y_{it-2}	−0.0763577 (−1.33)
S_{it-1}	−0.0003941 (−1.51)
μ_{it-8}	0.1504278** (2.49)
constant	0.5119853*** (5.06)
R^2	0.7568
countries	76
observations	3,577

Notes: OLS estimates of regression (2) at the quarterly frequency. The dependent variable is real GDP growth. Numbers in parentheses represent t-statistics, calculated using cluster-robust standard errors. * denotes significance at the 10% level, ** implies significance at the 5% level, *** indicates significance at the 1% level.

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