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# On the Macroeconomic Consequences of Over-Optimism[†]

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*Analyzing International Monetary Fund (IMF) data, we find that overly optimistic growth expectations for a country induce economic contractions a few years later. To isolate the causal effect, we take an instrumental variable approach—exploiting randomness in the country allocation of IMF mission chiefs. We first document that IMF mission chiefs differ in their individual degrees of forecast optimism, yielding quasi-experimental variation in the degree of forecast optimism at the country level. The mechanism appears to run through excessive accumulation of debt (public and private). Our findings illustrate the potency of unjustified optimism and underline the importance of basing economic forecasts upon realistic medium-term prospects.* (*JEL* C53, E23, E27, E32, F33, H63)

Recent years have brought a revival of an old idea by Pigou (1927), that macroeconomic fluctuations can arise due to difficulties that agents encounter in forecasting the future. According to this view, over-optimism about the future may favor a short-term boom, but at the cost of a later reckoning period (often taking the form of a recession).[1] Alternatively, it is possible that optimism, even if unfounded, leads to a boom, as it may induce a switch to a higher equilibrium output path in environments with strong complementarities (Cooper and John 1988).

We examine these ideas by analyzing the impact of forecast errors on future macroeconomic outcomes. We base our analysis on forecasts made by the IMF. These data are available for all IMF member countries, yielding broad coverage. In addition, many member countries, investors, and private agents use IMF forecasts as an input to their financial decisions, which implies that they can affect macroeconomic outcomes. We, however, believe that our results are not specific to IMF forecasts but may apply to any forecast with sufficient prominence to affect aggregate decisions.

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[1] Beaudry and Portier (2004) and Blanchard, L'Huillier, and Lorenzoni (2013) provide a formalization of this idea, showing how noisy news about the future can give rise to business-cycle-type fluctuations with overoptimistic news giving rise to a delayed recession.

Our main finding is that past growth over-optimism reduces growth going forward. The mechanism seems to run through higher debt accumulation, both public and private. This leads to a build-up of fragilities in the system, setting the stage for an overaccumulation of debt if the elevated expectations fail to materialize. Recent history offers several prominent cases that fit this description:

- On January 19, 2017, *The Economist* wrote:

> 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. (https://www.economist.com/middle-east-and-africa/2017/01/21/mozambiques-default).

- When talking about Portugal's 2010 crash (which was fueled by large capital inflows), Reis (2013) notes how many believed back in 2008 that "the large amounts of borrowing from abroad could be the result of borrowing against future growth on account of economic convergence to the European core."
- In Argentina, investor excitement about President Macri's reforms enabled the country to delay fiscal adjustment—instead satisfying their financing needs by issuing $56 billion (about 9 percent of 2017 GDP) in external debt between January 2016 and June 2018, including a 100-year bond. Ensuing capital inflows led to exchange rate overvaluation, worsening Argentina's current account deficit (Congressional Research Service 2020, IMF 2018). The resulting situation of high external debt combined with a twin deficit left the country vulnerable when global financial conditions tightened—ultimately resulting in Argentina receiving the largest loan in IMF history in July of 2018.

By focusing on the effects of past growth forecast errors on future growth outcomes, our analysis is subject to an endogeneity problem (as our dependent and independent variables are mechanically related). We address this through an instrumental variable (IV) approach. First, we document the existence of IMF mission chief (MC) fixed effects: IMF forecasts for a country tend to become more optimistic if that country is assigned an MC who, on average, produced more optimistic forecasts when assigned to other countries. A subsequent IV regression suggests there is a causal link from growth over-optimism to future growth slumps. Results are robust to the inclusion of an additional instrument (exploiting the differential impact of oil price forecast errors between energy exporters and importers).

To the best of our knowledge, our empirical analysis on the medium-term macroeconomic consequences of forecast errors is a new contribution, as is the construction and utilization of our instrument.[2] Related papers include Bachmann and

[2] After completing a first draft of our paper, we became aware that our instrument is related to one used in the crime literature aiming to analyze the effects of detention—exploiting randomness in the allocation of judges with

Elstner (2015), who analyze welfare losses stemming from misallocation due to over-optimism/-pessimism at the firm level; Mian, Sufi, and Verner (2017), whose findings are suggestive of a role for flawed expectations in generating business cycles; Arezki, Ramey, and Sheng (2017), who study the effects of news shocks in open economies using information from oil discoveries; Tanaka et al. (2018), who study the impact of overly optimistic growth expectations on firm productivity growth; and Jochem and Peters (2017), who show that more optimistic firms are more highly leveraged. It also relates to contributions by Oh and Waldman (1990) and Rodríguez Mora and Schulstad (2007). The latter two papers are part of a literature with a shorter-run focus that analyzes the impact of expectational shocks (as measured by data revisions). The main takeaway from this literature is that "false announcements" regarding the US economy (where, e.g., date-$t$ GDP is announced to equal 100 at date $t$, but this number is being revised down to 99 at $t+1$) have a positive effect on subsequent US economic activity. Blanchard, Lorenzoni, and L'Huillier (2017) and Enders, Kleemann, and Müller (forthcoming) report similar findings using different methods on a more recent vintage of US data, while Di Bella and Grigoli (2018) confirm this result for a panel of countries.[3] Our paper is also related to Baqir, Ramcharan, and Sahay (2005), who analyze whether the level of ambition embodied in IMF programs affects growth. For a large sample of IMF program cases, they compare monetary and fiscal targets set in IMF programs with outcomes and document that more ambitious fiscal targets improve growth while more ambitious monetary targets are typically growth reducing.

Combining results of this literature with our contribution suggests a narrative where an overestimation of future growth could provide a short-run boost to the economy but is likely to lead to subsequent economic difficulties. The mechanism seems to involve forecasts affecting government policies and private sector decisions (with our results suggesting that over-optimism brings decisions that make the economy more recession prone). In this sense, we find that adopting a sufficiently long horizon when analyzing the effects of optimism is key for understanding its overall impact.

## I. IMF WEO Forecasts

Our empirical analysis is based upon forecasts made by the IMF's *World Economic Outlook* (WEO) publication (IMF 1990–2018). These forecasts are published twice a year (in April and October) and combine judgments by IMF staff with information from country authorities and economic and statistical models as well as other forecasters (IEO 2014).

The IMF WEO publication yields one of the few comprehensive forecasting databases, covering all 189 IMF member states. WEO forecasts furthermore play a central role in policy circles, with 88 percent of country authorities (strongly) agreeing

different propensities to punish, following Kling (2006).

[3] Cotton, Nakamura, and Steinsson (2018) question the usefulness of data revisions for such purposes. In light of their findings, the alternative approach we develop might have independent value. Benhabib and Spiegel (2019) offer another route, exploiting differences in sentiment driven by election outcomes.

with the statement that they "consider the WEO's projections to be the benchmark for assessing economic prospects" (IEO 2006). Genberg and Martinez (2014b), moreover, report that 64 percent of country authorities (strongly) agreed with the statement that they "use WEO forecasts to check the accuracy of [their] own forecasts," while 75 percent (strongly) agreed that "WEO forecasts are valuable inputs to the economic policy process in [their] country." As, for example, documented in the annex to Genberg, Martinez, and Salemi (2014), WEO forecasts are also taken into account in projections made by private sector forecasters.

This paper uses the October vintage of each WEO round (available annually since 1990), calculating $k$-year-ahead forecast errors by taking the October WEO vintage of year $T$ and comparing its prediction for real GDP growth in year $T+k$ with the subsequent year $T+k$ realization.[4] Over our sample period (1990–2016), WEO forecasts for real GDP growth have shown a tendency toward over-optimism: on average, the prediction for next year's growth rate has been 0.58 percentage points higher than the subsequent realization. Such an upward bias is also documented in IEO (2014), while Loungani (2001) shows that it is present in private sector forecasts as well.

The next section of this paper examines the effects of such forecast errors; Appendix A details the origin of the variables featuring in our analyses.

## II. Forecast Errors and Growth Outcomes

This section contains our main empirical analysis: investigating whether elevated growth expectations affect future growth outcomes.[5]

The most important input to our regressions is the IMF's WEO-implied forecast error for real GDP growth. We construct this forecast error variable as follows. Since many macro-relevant economic decisions (such as those related to investment and debt accumulation) are taken with a multiyear horizon in mind, we look beyond one-year-ahead forecasts. In particular, for each year $T$, we start by averaging the forecasted annual real GDP growth rate over the years $T+1$, $T+2$, and $T+3$ (thereby simultaneously incorporating the one-, two-, and three-year forecast horizons). Next, we calculate the average realized growth rate over those years. The forecast error then follows by subtracting the latter from the former. Hence, for any arbitrary horizon $h > 0$, we can define

(1) $$F_{h,t} \equiv \frac{1}{h}\sum_{j=1}^{h} g^f_{t+j|t} - \frac{1}{h}\sum_{j=1}^{h} g_{t+j},$$

where $g^f_{t+j|t}$ is the year $t$ forecast for annual real GDP growth in year $t+j$ while $g_{t+j}$ is the realized annual growth rate for year $t+j$.

[4] Taken to be the value in the WEO database vintage three years later. Using more recent vintages makes numbers incomparable over time due to definitional changes, statistical revisions, and so forth.

[5] Results are robust to analyzing the effect on the incidence of episodes of negative real GDP per capita growth (henceforth referred to as "recessions"); see Appendix B.

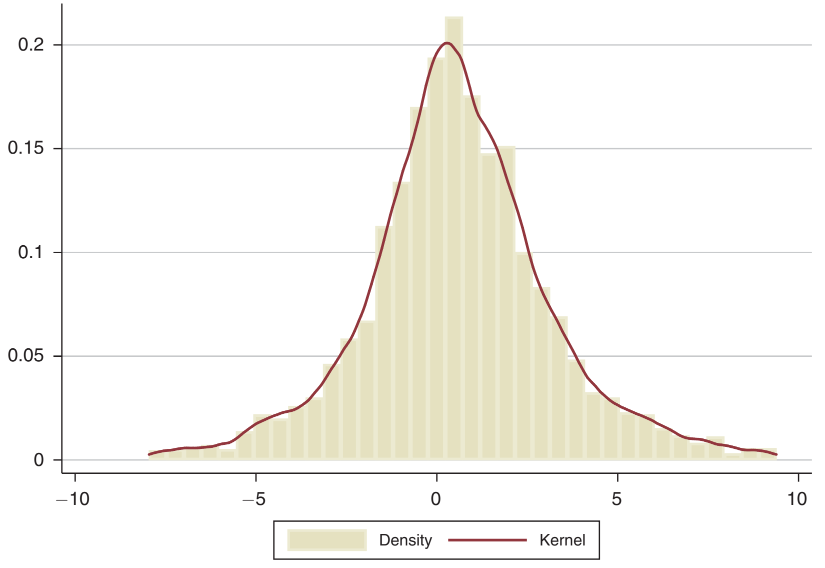


Figure 1. Kernel Density Plot for Forecast Error Variable $F_3$

We focus on the case where $h = 3$, which strikes a balance between taking a medium-run perspective (reflecting that many macroeconomic decisions are based upon expectations for multiyear horizons) and not losing too many observations at the beginning of our sample (lacking pre-1990 forecasts, we can only start calculating forecast errors from the year $1990 + h$ onward). Our findings are, however, robust to using other horizons (noting that we cannot go higher than $h = 5$ since the IMF WEO does not contain forecasts beyond the five-year horizon). Figure 1 depicts the distribution of our main forecast error variable $F_{3,t}$, confirming the aforementioned bias toward growth overestimation.[6]

We begin by asking what impact a forecast error stemming from year $T$ (for years $T+1, \ldots, T+h$) has on real growth in year $T+h$. Here, part of the forecast error is determined contemporaneously with the actual growth outcome.[7] This mechanical link between the dependent and independent variables of our regression gives rise to a simultaneity problem. To enable causal inference regarding the effect of over-optimism on later growth outcomes, we adopt an IV procedure. Hereto, we start by building an instrument that exploits the possibility that certain IMF MCs consistently display too much optimism or caution in their forecasts.[8]

Our sample covers all 189 IMF member states. Since IMF forecasts carry less weight in advanced countries for which other forecasts are readily available

[6] We drop observations that are more than 2 standard deviations (8.7 percentage points) from the mean since an investigation of large forecast errors shows that they often have reasons beyond economics (wars, pandemics, natural disasters, etc.). This shrinks our sample by about 3 percent.

[7] For example: when $h = 3$, the analysis looks at the impact of 1997 forecast errors (for the years 1998, 1999, and 2000) on growth in the year 2000. In this case, both the forecast error variable (featuring on the right-hand side of the regression) and the dependent variable (on the left-hand side) use growth outcomes from the year 2000.

[8] In the online Appendix, we outline the type of setup in which our IV strategy is able to identify whether over-optimism causes eventual downturns in economic activity.

(Genberg and Martinez 2014b), we report results for both our global sample and a restricted sample (of emerging and developing economies, where the channel is expected to be most relevant).

### A. *Results When Exploiting MC Fixed Effects*

Endogeneity is a challenge in regressions where the dependent variable is the rate of economic growth and the explanatory variable is the associated forecast error. To address this concern, we exploit the institutional setup of the IMF: IMF country teams are led by an MC. Together with other team members, so-called "desk economists," the MC determines the growth forecast. Given their leadership role, the MC's impact can be expected to be large—through both direct channels (instructing desk economists to change the growth forecast) and indirect ones (e.g., the MC setting a bullish/bearish tone about the country's prospects, to which other team members pander in an explicit or implicit attempt to please their boss). Forecasting philosophies differ across MCs in terms of preferred methods (ranging from econometrics to the use of judgment; see IEO 2014) as well as underlying principles: some individuals tend to have a more pessimistic outlook, while others will have a brighter view of the world. Some might see strategic reasons to be rosy in their forecasts, for instance, because they are afraid to "talk the economy down"; others might see this as less of a risk,[9] preferring to "round their forecast down" instead, e.g., in an attempt to prevent overborrowing.

MCs are periodically rotated to different country assignments. While this reallocation process has exogenous, random elements, it is not completely arbitrary (e.g., high-profile countries tend to be covered by more experienced MCs). To serve as a valid instrument in our paper, it is crucial that the MC allocation process does not systematically assign more optimistic MCs to countries that are more likely to be embarking on a boom-bust episode. We are comfortable with this assumption for several reasons. First, many other factors (orthogonal to optimism) seem to dominate the MC allocation process: relationships with IMF management, expertise, language skills, managerial strength, diplomatic skills, availability, and so forth. Second, it is not clear whether IMF decision makers have detailed knowledge about MC-specific degrees of optimism (for new MCs, such information would be particularly difficult to obtain). Third, even if they had such knowledge, it is not clear that they would want to assign "optimists" to countries that are on the verge of a boom-bust cycle. In fact, if a country is viewed to be on the cusp of such an episode, an organization like the IMF would have no special reason to assign it to an optimistic MC (as that would be seen as adding fuel to the fire). Consequently, we make the following assumption.

ASSUMPTION 1. *More optimistic MCs are not systematically allocated to countries embarking on a boom-bust cycle.*

[9] The example of former IMF chief economist Michael Mussa comes to mind, who once quipped that "we've been forecasting a slowdown in the US economy for a while now. And we will continue to forecast a slowdown until it happens."

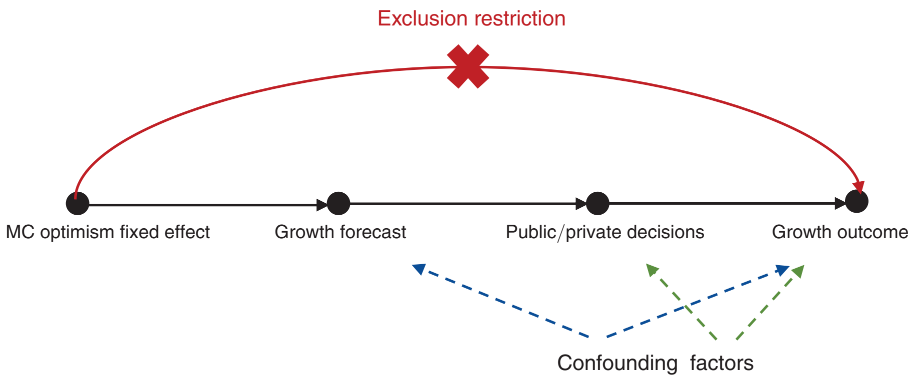


Figure 2. Causal Diagram

In addition, we require that the MC's degree of over-optimism does not have a direct effect on economic outcomes. This exclusion restriction also seems plausible, as it is hard to imagine that the mood of an MC (optimistic or pessimistic) has a direct macroeconomic effect (it seems safe to rule out that, e.g., an MC's body language in media interviews has a direct effect on growth). This is represented by the crossed-out arch-shaped red line in the causal diagram (Figure 2). It is furthermore hard to imagine that forecasts more generally have a *direct* causal effect on growth outcomes (holding economic decisions constant); a possible impact could run through public/private decisions, which are, in turn, shaped by the overall sentiment. As a result, Figure 2 does not contain a direct path from forecasts to growth outcomes. Instead, any effect works through ensuing actions (which are informed by the growth forecast), more on which in Section III. Recall how the presence of the aforementioned mechanical link between forecast errors and growth outcomes (illustrated by the blue dashed lines) creates the need for our IV strategy, relying on the pseudorandom variation provided by the MC fixed effects; there may be additional confounders that affect both public/private decisions and growth outcomes (captured by the green dashed lines, more on which in footnote 19 below).

To implement our approach, we use MC reallocations to estimate MC-specific degrees of growth forecast optimism. Hereto, we have constructed a new, comprehensive dataset on country assignments for IMF MCs since 1990. We managed to create our dataset by text mining IMF staff reports (mainly Article IV documents), which typically indicate the name of the MC on the first page of the introduction (exploiting the fact that the MC is always introduced by a standard sentence like "The mission was led by…").

Hereby, we have been able to compile a dataset that identifies the IMF MC for about 80 percent of the country-year pairs since 1990. Our database contains 705 unique MCs who have led IMF teams for 2.7 different countries on average; 475 MCs have led teams for multiple countries (which is necessary in order to identify our MC fixed effects). On average, the latter have led IMF teams for 3.5 different countries.

We estimate MC fixed effects using the approach of Bertrand and Schoar (2003), who identify manager fixed effects in a corporate context and start by regressing the one-year-ahead forecast error (as the one-year-ahead forecast typically receives the strongest input from an MC) on several observables:

$$F_{1,it}^{(k)} = \alpha_t + \delta_i + \lambda W_{it} + \mu^{(k)} + \epsilon_{it}. \tag{2}$$

Here, $\alpha_t$ are the year fixed effects, $\delta_i$ are country fixed effects, $W_{it}$ is a vector of time-varying country-level covariates, and $\epsilon_{it}$ is the error term. The main object of interest in (2) is $\mu^{(k)}$, MC $k$'s fixed effect (where overly optimistic MCs will have $\mu > 0$).

We subsequently use the MC fixed effects $\mu$ as an instrument for forecast errors in a regression where the left-hand-side variable $y_{it}$ is country $i$'s rate of real GDP growth in year $t$. Using $X_{it}$ to denote a vector of time-varying country-level covariates, we thus estimate first-stage regression (3) and second-stage regression (4):[10]

$$F_{3,it-3}^{(k)} = a_t + d_i + b\mu^{(k)} + cX_{it} + u_{it}, \tag{3}$$

$$y_{it} = \alpha_t + \delta_i + \beta\hat{F}_{3,it-3}^{(k)} + \zeta X_{it} + \varepsilon_{it}. \tag{4}$$

To further mitigate any mechanical relation that could generate problems, we follow a "jackknife" approach (Angrist, Imbens, and Krueger 1999) by inferring MC fixed effects from the MC's forecasts for countries *other than the country to which the fixed effect will be applied in regression* (2). So if we consider an MC who has headed IMF teams for Albania, Rwanda, and Thailand, the fixed effect that is used as an instrument in regression (3) for Rwandan forecast errors is only based upon the MC's forecasts for Albania and Thailand. In this way, no information from a country itself is applied back to that very same country—minimizing endogeneity of the instrument with respect to country outcomes.

When estimating MC fixed effects through equation (2), we include two covariates in the vector $W$: (i) a dummy variable equaling 1 if country $i$ was under an IMF program in year $t$ (as it has been argued that IMF forecasts are particularly optimistic in IMF programs; see GAO 2003) and (ii) purchasing-power-parity-adjusted real GDP per capita (as the level of development might play a role; see Genberg and Martinez 2014a). Table 1 shows results for the regression used to estimate MC fixed effects.

The average estimated MC fixed effect amounts to 0.34, while its standard deviation equals 2.6 (Figure 3 shows $\mu$'s distribution[11]).

A joint $F$-test that all MC fixed effects are equal to 0 is rejected at the 1 percent significance level (and inclusion of the MC fixed effects increases the adjusted $R^2$ from 0.09 to 0.13). Results furthermore suggest that once country and time fixed effects are accounted for, there is no strong evidence for excessive optimism during

[10] Following the related literature on judge fixed effects (recall footnote 2), we do not adjust our standard errors for the fact that our instrument is estimated (also see Dobbie and Song 2015, 1286, on this).

[11] In line with footnote 6, we drop estimates more than 2 standard deviations from the mean from our empirical analysis.

Table 1—OLS Regression (2), Used to Estimate MC Fixed Effects

| | Dependent variable: One-year-ahead forecast error $F_{1,it}$ | |
|---|---|---|
| | (1: without MC fixed effects) | (2: with MC fixed effects) |
| IMF program dummy | 0.361196 | 0.3606151 |
| | (1.56) | (1.26) |
| Real GDP per capita | −0.0000155 | −0.0000414 |
| | (−0.34) | (−1.17) |
| Mission chief fixed effects | No | Yes |
| Time and country fixed effects | Yes | Yes |
| Adjusted $R^2$ | 0.09 | 0.13 |
| Countries | 170 | 170 |
| Observations | 3,423 | 3,423 |

*Notes:* Estimates of OLS regression (2). The dependent variable is the one-year-ahead forecast error on annual real GDP growth. The forecast error is calculated as "forecast minus realization." This forecast error is regressed upon several controls and, in column 2, on IMF MC dummies. Numbers in parentheses represent $t$-statistics, calculated using robust standard errors.

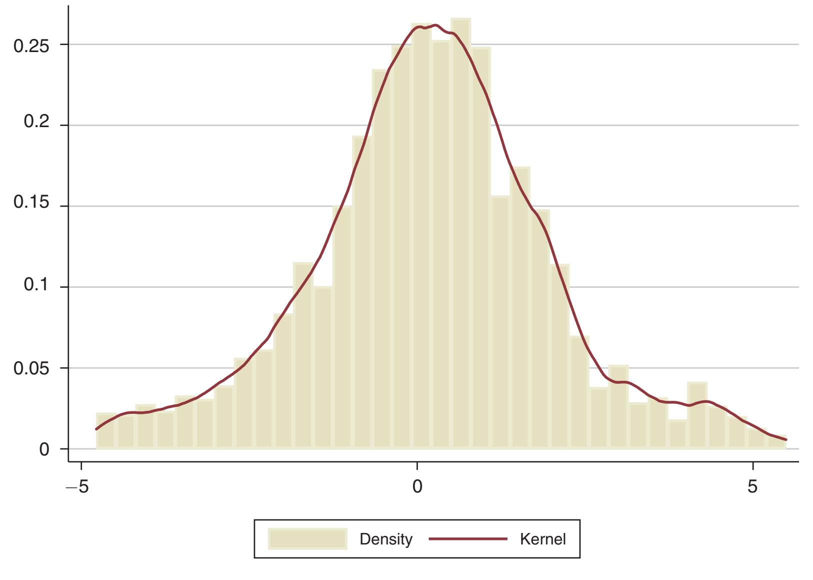


Figure 3. Kernel Density Plot for MC Fixed Effects $\mu$

IMF programs—a result that is in line with the recent studies of Luna (2014) and IMF (2019). The level of real GDP per capita does not seem to play a major role either.[12]

Table 2 contains results when the MC fixed effects $\mu$ from (1) are used to instrument forecast errors. The underlying first-stage regressions (see Table 2A) suggest

[12] When country fixed effects are dropped, the coefficient on the IMF program dummy turns significantly positive (0.43, $p$-value of 0.03), while that for real GDP per capita takes on a significantly negative value (−0.000015, $p$-value of 0.003)—pointing to larger forecast errors in less developed countries.

Table 2A—First-Stage Regressions

| | First stage—Dependent variable: Forecast error on growth "$F_{3,t-3}$" | | | | | |
|---|---|---|---|---|---|---|
| | (1: full) | (2: full) | (3: eme/dev) | (4: eme/dev) | (5: adv) | (6: adv) |
| Mission chief fixed effect "$\mu$" | 0.0982341 (3.52) | 0.0952006 (3.25) | 0.0783422 (2.51) | 0.0803141 (2.44) | 0.0849059 (1.48) | 0.0766778 (1.37) |
| Average growth in trading partners | | −0.2621982 (−3.61) | | −0.2261901 (−3.14) | | −0.5584623 (−3.69) |
| Percent change in terms of trade | | −0.0112075 (−4.99) | | −0.0110948 (−5.39) | | −0.0266762 (−1.28) |
| Time and country fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Cragg-Donald statistic | 13.23 | 11.62 | 6.40 | 6.16 | 2.58 | 2.19 |
| Countries | 183 | 167 | 148 | 133 | 35 | 34 |
| Observations | 2,704 | 2,465 | 2,105 | 1,893 | 599 | 572 |

*Notes:* Estimates of first-stage regression (3), employing the MC fixed effects as estimated via (2). The dependent variable is the three-year average forecast error on annual real GDP growth, calculated through equation (1). The forecast error is calculated as "forecast minus realization." Numbers in parentheses represent $t$-statistics, calculated using robust standard errors.

Table 2B—Instrumental Variable Regressions

| | Dependent variable: Real GDP growth rate | | | | | |
|---|---|---|---|---|---|---|
| | (1: full) | (2: full) | (3: eme/dev) | (4: eme/dev) | (5: adv) | (6: adv) |
| Forecast error on growth "$F_{3,t-3}$" | −1.388208 (−3.18) | −1.285326 (−2.72) | −1.402359 (−2.30) | −1.19759 (−1.97) | −1.992058 (−1.72) | −2.386024 (−1.73) |
| Average growth in trading partners | | −0.0339862 (0.22) | | 0.0367947 (0.23) | | −1.164853 (−1.32) |
| Percent change in terms of trade | | −0.0139767 (−2.44) | | −0.0136905 (−1.92) | | 0.0065408 (0.12) |
| Time and country fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Countries | 183 | 167 | 148 | 133 | 35 | 34 |
| Observations | 2,704 | 2,465 | 2,105 | 1,893 | 599 | 572 |

*Notes:* Estimates of IV regression (4), with forecast errors instrumented by MC fixed effects (Table 2A). The dependent variable is real GDP growth. Numbers in parentheses represent $z$-statistics, calculated using robust standard errors.

that our instrument is relevant.[13] In particular, the significantly positive estimates for the coefficients on $\mu$ are a strong finding since these fixed effects were obtained

[13] The Stock-Yogo 20 percent (15 percent) maximal bias critical value for the Cragg-Donald statistic equals 6.66 (8.96); cf. Stock and Yogo (2005). These hurdles are consistently passed in our full sample (as is the rule of thumb of ten), even when using heteroskedasticity-robust Kleibergen-Paap statistics (which are slightly lower than Cragg-Donald statistics). The critical values are not always passed in the smaller sample of emerging and developing countries, but findings are similar for both samples. In addition, reduced-form regressions (regressing $y$ directly on $\mu$, which is a recommended check; cf. Angrist and Krueger 2001 and Chernozhukov and Hansen 2008) confirm the sign and significance of our IV-based findings.

through a jackknife procedure, solely relying upon forecasts for "other countries" (recall the discussion under equation (4)). So in a sense, this is an out-of-sample forecasting exercise—showing that MCs who have been overly optimistic when assigned to countries other than A also tend to produce overly optimistic forecasts when assigned to country A. This is evidence in favor of the existence of "systematically optimistic/cautious" MCs.

Turning back to our central question, second-stage results in Table 2B suggest that past over-optimism *reduces* future growth (also increasing recession odds; see Appendix B). This finding is robust to inclusion of various exogenous covariates that are believed to affect growth (see columns 2 and 4). It furthermore arises in our global sample (columns 1 and 2) as well as in the restricted sample featuring only emerging and developing economies (columns 3 and 4) where IMF forecasts tend to carry more weight. As columns 5 and 6 of Table 2A show, our instrument turns weak in the subsample of advanced countries (Cragg-Donald statistics fall below three).[14] In such situations, we know that the second-stage coefficient on the instrumented variable is biased upward (Bound, Jaeger, and Baker 1995), as a result of which the point estimates in columns 5 and 6 of Table 2B should be discarded. In any case, one would expect IMF forecasts to matter less in advanced economies for which alternative projections are readily available.

## B. *Incorporating an Oil-Price-Based Instrument*

Next to exploiting MC fixed effects, we can employ an additional instrument (which enables us to test overidentifying restrictions and obtain a better sense about the robustness of our findings): IMF forecast errors for energy prices. Energy price forecasts are made centrally (by the IMF's research department) and subsequently shared with country teams, who all use them as inputs to their country-specific macroeconomic frameworks. As a result, overestimation of future energy prices is likely to lead to overly optimistic growth projections in energy-exporting countries, and vice versa for energy-importing countries. Here, we build an instrument that exploits this insight.

We start by calculating the oil price forecast errors made by the IMF's research department over our sample period.[15] To ensure consistency with our growth forecast errors, we calculate them by averaging over a three-year horizon. Consequently, the forecast error made at time $t$ is calculated as (note the analogy with equation (1))

$$F_{3,t}^{oil} = \frac{1}{3}\sum_{j=1}^{3} p_{t+j|t}^{f} - \frac{1}{3}\sum_{j=1}^{3} p_{t+j}, \tag{5}$$

[14] This is to be expected, since many parties produce forecasts for this group of countries. In such an environment, IMF forecasts are reported to pander toward the consensus (IEO 2014, 15)—thus implying that the MC leaves a smaller mark on the IMF forecast.

[15] Ideally, we would like to base our analysis upon prices for all energy types, but historical forecast vintages are unfortunately not available at such a disaggregated level. We therefore proceed by using oil prices as a proxy, which seems reasonable given the relationship between oil prices and prices of other energy sources (Bencivenga, Sargenti, and D'Ecclesia 2010).

Table 3A—First-Stage and Instrumental Variable Regressions

| First stage—Dependent variable: Forecast error on growth "$F_{3,t-3}$" | | | | | | |
|---|---|---|---|---|---|---|
| | (1: full) | (2: full) | (3: eme/dev) | (4: eme/dev) | (5: adv) | (6: adv) |
| Mission chief fixed effect "$\mu$" | 0.186111 (5.43) | 0.1746104 (4.85) | 0.1756623 (4.28) | 0.171153 (3.95) | 0.0849358 (1.48) | 0.0763163 (1.36) |
| Oil-price-based instrument "$Z$" | 0.0034175 (1.92) | 0.0026224 (1.42) | 0.0025856 (1.21) | 0.0017439 (0.79) | 0.000549 (0.02) | −0.0004454 (−0.17) |
| Average growth in trading partners | | −0.2309111 (−3.03) | | −0.1712369 (−2.39) | | −0.557373 (−3.66) |
| Percent change in terms of trade | | −0.0162177 (−1.52) | | −0.0135512 (−1.19) | | −0.281953 (−1.17) |
| Time and country fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Cragg-Donald statistic | 19.30 | 15.38 | 11.07 | 9.39 | 1.29 | 1.10 |
| Countries | 127 | 119 | 92 | 85 | 35 | 34 |
| Observations | 1,947 | 1,810 | 1,348 | 1,238 | 599 | 572 |

*Notes:* Estimates of first-stage regression (3), employing the oil-price-based instrument described in Section IIB alongside the MC fixed effects as estimated via (2). The dependent variable is the three-year average forecast error on annual real GDP growth, calculated through equation (1). The forecast error is calculated as "forecast minus realization." Numbers in parentheses represent $t$-statistics, calculated using robust standard errors.

Table 3B—Instrumental Variable Regressions

| Second stage—Dependent variable: Real GDP growth | | | | | | |
|---|---|---|---|---|---|---|
| | (1: full) | (2: full) | (3: eme/dev) | (4: eme/dev) | (5: adv) | (6: adv) |
| Forecast error on growth "$F_{3,t-3}$" | −1.008264 (−4.23) | −0.9719984 (−3.62) | −0.934472 (−3.19) | −0.8618011 (−2.72) | −1.982551 (−1.71) | −2.525127 (−1.76) |
| Average growth in trading partners | | 0.0631699 (0.65) | | 0.1312953 (1.43) | | −1.243912 (−1.35) |
| Percent change in terms of trade | | −0.0047442 (−0.27) | | −0.0135111 (−0.70) | | 0.002631 (0.04) |
| Time and country fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Hansen $J$-statistic ($p$-value) | 0.043 (0.8362) | 0.195 (0.6588) | 0.266 (0.6061) | 0.810 (0.3681) | 1.219 (0.2696) | 1.230 (0.2674) |
| Countries | 127 | 119 | 92 | 85 | 35 | 34 |
| Observations | 1,947 | 1,810 | 1,348 | 1,238 | 599 | 572 |

*Notes:* Estimates of IV regression (4), with forecast errors instrumented by MC fixed effects and the oil-price-based instrument (Table 3A). The dependent variable is real GDP growth. Numbers in parentheses represent $z$-statistics, calculated using robust standard errors.

where $p$ $\left(p^f\right)$ is the (forecasted) oil price proxied by a simple average of Brent, WTI, and Dubai Fateh. We subsequently construct our instrument "$Z$" by interacting the oil price forecast error (which is obviously the same for all countries) with each country's energy balance in the year that the forecast was made (where the energy balance is defined as total energy exports minus total energy imports, relative to that country's total energy usage).[16] Consequently, a positive value for the instrument (driven by either overestimation of future oil prices in the case of an energy-exporting country or underestimation of future oil prices in the case of an energy-importing country) is likely to lead to overly optimistic growth projections at the country level.

Table 3 contains results when including this instrument next to the MC fixed effects. We continue to find that over-optimism reduces future growth (with a stronger first stage). Like in Section IIA, this result emerges in both the full and the emerging/developing sample and is robust to the inclusion of covariates. As before, our instrument turns weak for the advanced economy sample where IMF forecasts play a much smaller role. Since this estimation is overidentified, we can test whether our instruments are valid. As shown in Table 3B, Hansen's $J$-statistic suggests that our instruments are uncorrelated with the error term (where this test needs to assume that our MC fixed effects are exogenous).

Resulting estimates of $\beta$ (which are close to 1) suggest that overestimating average annual growth over a 3-year horizon by 1 percentage point (with 40 percent of country-year pairs showing an overestimation larger than this) reduces real GDP growth 3 years later by about 1 percentage point on average. With unconditional real GDP growth rate in our sample standing at 3.7 percent, this reflects a sizable effect.

In Appendix C, we show that our findings are robust to moving away from IV-based methods—mitigating endogeneity by lagging the forecast error variable (as to break the mechanical link between the dependent and independent variables).

At the same time, our results imply that cautious forecasts can help to prevent recessions. Van der Ploeg (2010) provides a theoretical analysis, while Frankel (2011) highlights the example of Chile in the 2000s: he argues that Chile was able to moderate the global-financial-crisis-induced downturn because they had been basing their budget decisions upon a conservative estimate of the price of copper (an important Chilean export product). As a result, Chile entered the crisis with ample fiscal space (a government-debt-to-GDP ratio below 5 percent), which enabled their government to implement countercyclical fiscal policy when external conditions deteriorated. In line with this narrative, the econometric study by Romer and Romer (2018) finds that countries that enter financial crises with lower debt-to-GDP ratios experience a smaller subsequent decline in output—a result they also attribute to the availability (and subsequent deployment) of fiscal policy space.

## III. Inspecting the Mechanism

The previous sections provided evidence that overly optimistic growth expectations make countries more likely to experience future growth slowdowns. This begs the question of what drives this result. It is hard to imagine that excessive optimism *by itself* would set the stage for recession odds to increase; a channel that runs through the subsequent implementation of certain government policies or private

[16] Data come from Bogmans, Kiyasseh, and Matsumoto (2019) and are based upon IEA (2018).

sector decisions seems more plausible (recall Figure 2). In theory, one can think of at least two (non-mutually-exclusive) channels that could convert an overly optimistic sentiment into a later slump:

- Overly optimistic forecasts make agents *overestimate the present value of their wealth*, setting the stage for overborrowing (as the anticipated future income stream, against which borrowing is thought to occur, might not arrive; see, e.g., Mansoorian 1991 or Akıncı and Chahrour 2018). Borrowing plans for, especially, the public sector are often calibrated upon debt sustainability frameworks, most of which (including the IMF's) have debt-to-GDP ratios at their core (Debrun et al. 2020). This implies that an overestimation of future GDP easily translates into a future debt stock that is undesirably high relative to GDP (as the latter ends up disappointing in cases where expectations were too optimistic). In parallel to this effect on borrowers, a positive sentiment surrounding a country will also make lenders more willing to supply funds—enabling a borrowing spree. This narrative fits well with the examples of Argentina, Mozambique, and Portugal presented in the introduction.
- An overly optimistic sentiment might bring a false sense of security and an accompanying *underestimation and underpricing of risks*, leading to an unwarranted increase in leverage. Such a development can be driven by demand (from borrowers) and by supply (through lenders) as well as by a loosening of prudential standards (by regulators). In this category, the (now-falsified) belief that "US house prices never go down"[17] contributed to the development of the subprime mortgage industry, whose subsequent difficulties (when US house prices *did* fall) triggered the global financial crisis in 2007–2008. This narrative is often linked to the ideas of Fisher (1933), see e.g. Boz and Mendoza (2014), and Minsky (1992), see e.g. Bhattacharya et al. (2015).

Based upon the causal diagram in Figure 2, one may be inclined to think that the effect of forecast optimism on growth outcomes should disappear if the regression were to control for the mediating public and private sector decisions. Finding suitable proxies for the latter is, however, difficult, since available data are equilibrium objects (observable subject to noise) rather than direct policy choices; endogeneity of policies poses another challenge, creating a need for yet an additional instrument.[18]

[17] See Greg Ip, "The Worst Ideas of the Decade: Housing Prices Always Rise" (https://www.washingtonpost.com/wp-srv/special/opinions/outlook/worst-ideas/housing-bubble.html), for an account of this belief.

[18] To see this, suppose that outcomes $y$ are related to policies $p$ and other factors $x$ (e.g., trading partner growth or investment decisions) via $y = \beta p + x$. Policies, in turn, are affected by exogenous optimism (captured via $\mu$), political preferences $v$, and the aforementioned other factors $x$ affecting outcomes, so $p = \alpha_1 \mu + \alpha_2 v + \alpha_3 x$. Finally, policies are only observed with error $\varpi$, so $p^* = p + \varpi$. If one next adds observed policies $p^*$ as a control to our baseline regression, thus estimating $y = \vartheta_1 \mu + \vartheta_2 p^* + \varepsilon$, one can show that the estimate for $\vartheta_1$ has plim $\alpha_1(\Sigma_\varpi - \alpha_3 \Sigma_x) / (\alpha_2^2 \Sigma_v + \alpha_3^2 \Sigma_x + \Sigma_\varpi)$, where $\Sigma_q$ is the variance of variable $q$. Hence, when exogenous optimism affects policies ($\alpha_1 \neq 0$), controlling for policies will not manage to uncover that $\vartheta_1 = 0$ unless policies are exogenous to $x$ ($\alpha_3 = 0$) *and* policies can be measured without error ($\Sigma_\varpi = 0$). Since these conditions are unlikely to hold in reality, just controlling for policies is not enough; to move forward along these lines, one would need to find an extra instrument to resolve the additional endogeneity problem (represented by the green dashed lines in Figure 2).

Table 4. IV Probit Regressions

| | Dependent variable: Crisis dummy (1 = onset of crisis) | | | | | |
|---|---|---|---|---|---|---|
| | (1: fiscal crisis) | (2: fiscal crisis) | (3: bank/ curr. crisis) | (4: bank/ curr. crisis) | (5: BoP crisis) | (6: BoP crisis) |
| Forecast error on growth "$F_{3,t-3}$" | 0.3278953 (2.30) | 0.3506703 (2.15) | 0.3553154 (3.97) | 0.3650777 (3.91) | −0.0433515 (−0.14) | −0.181226 (−0.64) |
| Average growth in trading partners | 0.1441591 (3.52) | 0.1484423 (3.53) | 0.1540699 (5.39) | 0.1578306 (5.57) | 0.0072497 (0.08) | −0.028436 (−0.34) |
| Percent change in terms of trade | −0.0045759 (−0.40) | −0.0022374 (−0.17) | 0.0012289 (0.15) | 0.0010729 (0.13) | −0.0070022 (−0.57) | −0.0093372 (−0.87) |
| Recession dummy | | −0.3664191 (−0.89) | | −0.33316528 (−0.95) | | 0.6648157 (1.72) |
| Countries | 117 | 117 | 120 | 120 | 117 | 117 |
| Observations | 1,413 | 1,413 | 1,806 | 1,804 | 1,401 | 1,401 |

*Notes:* IV probit regressions analyzing the impact of past forecast errors on the incidence of future crises, with forecast errors instrumented by MC fixed effects and the oil-price-based instrument. The dependent variable is a crisis dummy, which takes the value 1 in year $T$ if that year brought the onset of a new crisis. Numbers in parentheses represent $z$-statistics, calculated using robust standard errors.

Faced with these difficulties, looking at outcomes is a practical way forward, teaching us more about the potential mechanisms at play. After all, if public overborrowing plays a significant role, waves of excessive optimism should increase the incidence of fiscal crises; likewise, in case the mechanism runs via private overborrowing (be it due to borrowers, lenders, and/or regulators), waves of undue optimism can be expected to be followed by more banking/currency crises (which often coincide; see Kaminsky and Reinhart 1999); in situations of external overborrowing (public or private), balance of payment (BoP) crises should become more likely to arise.[19] The estimates reported in Table 4 speak to this point by taking the dependent variable to be a dummy that takes the value 1 in years that bring the onset of the various aforementioned crises. To account for the binary nature of the outcome variable, we employ Newey's (1987) IV probit estimator.[20]

Estimation results in Table 4 suggest that past over-optimism makes the arrival of fiscal and banking/currency crises more likely (columns 1–4). There is no evidence for a significant effect on the arrival of BoP crises (columns 5–6). Note that specifications in even columns, which include a recession dummy as a covariate, suggest that results are robust to explicitly controlling for the state of the economy.

Columns 1 and 2 point to public sector overborrowing as a possible mechanism through which past over-optimism can have negative repercussions for future growth

[19] The banking/currency crisis dummies are taken from Laeven and Valencia (2013), while the fiscal crisis dummies (capturing sovereign defaults, episodes of public arrears accumulation, etc.) stem from Gerling et al. (2017). We proxy BoP crises by the presence of an IMF program (as the existence of a BoP need is a necessary condition to qualify for an IMF program by the Fund's Articles of Agreement). Data on IMF programs come from the IMF's Monitoring of Fund Arrangements (MONA) database.

[20] To avoid the incidental parameters problem, results in Table 4 do not include time and country fixed effects (although results are largely robust to their inclusion).

outcomes. It provides support for the aforementioned popular narrative whereby optimism about future growth induces governments to borrow against a prospective income stream—creating debt vulnerabilities and potentially endangering the country's future growth potential (as high public debt may hamper long-run growth; see Eberhardt and Presbitero 2015). This holds particularly true if the optimistic outlook subsequently does not materialize, in which case the sovereign is left with a larger debt-to-GDP ratio relative to the perfect foresight benchmark that it might have been aiming for (given the deterministic nature of most debt sustainability frameworks; see Debrun et al. 2020).

Columns 3 and 4 similarly suggest that excessive borrowing by the private sector plays a role. An excessive build-up of private credit is consistent with the firm-level evidence in Jochem and Peters (2017), who report that firms with a more optimistic outlook are more highly leveraged. Relatedly, Diamond, Hu, and Rajan (2020) show how optimism about the future may lead to a build-up of fragilities through riskier financing strategies in the corporate sector (also see Asriyan, Laeven, and Martín 2019 and Gorton and Ordoñez 2020, who construct credit models in which the quality of information is falling during booms—leading to worse lending decisions).

The fact that these results survive when adding a recession dummy as a covariate (columns 2 and 4) means that past over-optimism makes future fiscal and banking/currency crises more likely to arise *even when controlling for the state of the economy*. This suggests that a more optimistic sentiment might indeed lead to policies and decisions that induce public and private overborrowing—bringing greater fragilities to government finances and the banking system.

## IV. Conclusion

Using a broad sample of IMF member countries, this paper finds evidence for a causal link from overly optimistic growth forecasts to future growth slowdowns and recessions. We arrived at this conclusion by taking an IV approach, which exploited the observation that IMF MCs appear to differ in their degree of forecast optimism/caution. As MCs are periodically reassigned to different countries in a pseudorandom fashion, we obtain quasi-experimental evidence on the effect of varying the degree of forecast optimism/caution at the country level.

Our finding that over-optimism brings economic damage in later years is highly robust: to different estimation methods (IV, OLS, probit, logit) and different dependent variables (recession dummies, growth rates) as well as subsamples (with our results continuing to hold for the subsample containing only emerging and developing economies, where IMF forecasts carry the largest weight).[21]

We also find that fiscal problems and banking/currency crises are more likely to arise in countries where past growth forecasts have been overly optimistic—sug-

[21] Of note, we fail to detect a causal impact from forecast errors to future growth outcomes when the sample is restricted to advanced economies only. This comforts us, as it is hard to imagine that IMF forecasts matter enough for advanced economies to materially affect their course. Things are different for less developed countries, where the IMF is often a rare provider of external forecasts.

gesting that the underlying mechanism runs through higher debt accumulation by both public and private agents. Both seem to welcome positive news about the future by borrowing more (while creditors become more willing to lend). If the expected rise in income subsequently fails to materialize, the amount of debt accumulated turns out to be excessive (relative to income realizations), and negative dynamics set in.[22]

Our results illustrate the potency of (nonmaterializing) optimism shocks and underline the importance of basing policy upon realistic (or even cautious) medium-term macroeconomic forecasts. Specifically, our finding regarding the impact of over-optimism on future growth slowdowns provides support for existing models in the news/noise tradition. On the normative front, it suggests that basing policies upon cautious growth forecasts can contribute to safeguarding economic stability; in addition, macroprudential policy might need to tighten during waves of over-optimism (which most people associate with boom periods; see Schaal and Taschereau-Dumouchel 2020). This runs against the policy prescription following from standard models with endogenous borrowing constraints (where overborrowing takes place during recessions) but is optimal in an economy where shocks to growth are sufficiently persistent (Flemming, L'Huillier, and Piguillem 2019).

A separate contribution of this paper has been the construction of a new database on IMF MC assignments and illustrating how these data can be used to yield IVs that can answer macroeconomic questions. The current application has focused on identifying the causal impact of forecast errors, but a similar approach can be employed to answer different questions (e.g., related to the size of fiscal multipliers).

## Appendix A. Origin of Variables

Table A1—Origin of Variables

| Variable | Origin | Remarks |
|---|---|---|
| Real GDP growth | IMF WEO | |
| Forecast error on growth | IMF WEO | Constructed as described in Section II |
| Average growth in trading partners | IMF WEO | Partner-weighted real GDP growth, three-year average |
| Percent change in terms of trade | IMF WEO | Three-year average |
| Fiscal crises | Gerling et al. (2017) | |
| Banking/currency crises | Laeven and Valencia (2013) | |
| IMF programs | IMF MONA | |
| Energy balance | Bogmans et al. (2019) | |

[22] This was already hypothesized by Fisher (1933, 341), who noted that "overconfidence seldom does any great harm except when … it beguiles its victims into debt." In line with this narrative, Zhou Xiaochuan (governor of the People's Bank of China until March 2018) expressed his worries over excessive debt accumulation in China around the 2017 Communist Party Congress, adding that "if we are too optimistic when things go smoothly, tensions build up, which could lead to a sharp correction, what we call a 'Minsky Moment.' That's what we should particularly defend against."

## Appendix B. Results When the Dependent Variable Is a Recession Dummy

Here, we display the equivalents of our key tables when our baseline dependent variable (real GDP growth, featuring in Tables 2 and 3) is replaced by a recession dummy, which takes the value 1 when real GDP per capita growth is negative. To account for the binary nature of the dependent variable, we employ Newey's (1987) IV probit estimator. As one can see, the negative impact of past over-optimism on future outcomes continues to arise.

Table B1— IV Probit Regressions

| Dependent variable: Recession dummy (1 $=$ presence of recession) | | | | |
|---|---|---|---|---|
| | (1: MC fixed effects as instrument) | | (2: both instruments) | |
| Forecast error on growth "$F_{3,t-3}$" | 0.4121804 | 0.4652131 | 0.2389639 | 0.4232543 |
| | (5.58) | (36.14) | (1.59) | (3.32) |
| Average growth in trading partners | | 0.2293333 | | 0.11434 |
| | | (5.85) | | (2.26) |
| Percent change in terms of trade | | 0.0040258 | | 0.008187 |
| | | (4.30) | | (1.37) |
| Countries | 182 | 166 | 127 | 120 |
| Observations | 2,695 | 2,459 | 1,944 | 1,809 |

*Notes:* IV probit regressions analyzing the impact of past forecast errors on the incidence of recessions, with forecast errors being instrumented with MC fixed effects (column 1) and MC fixed effects plus the oil-price-based instrument (column 2). The dependent variable is a recession dummy (as opposed to real GDP growth, which features in Tables 2B and 3B), where recessions are defined as years during which real GDP per capita growth was negative. Numbers in parentheses represent $z$-statistics, calculated using robust standard errors.

## Appendix C. OLS Estimation Results

Our results indicate that past overestimations of growth increase the odds of growth slowing down (and the country ending up in recession). These results were established by addressing the endogeneity problem through an IV approach. However, as already emphasized in the seminal IV contribution of Sargan (1958) and reiterated in a recent paper by Young (2017), IV estimates are inefficient if the endogeneity problem is not severe, and better information can sometimes be obtained by deploying OLS. This is potentially true in the present setup, where the endogeneity problem can be mitigated by lagging the forecast error variable instead of using an IV approach. While some lagging was already present in the IV regressions for economic reasons (many of the relevant decisions are based upon multiyear horizons), introducing further lags may allow us to proceed without relying on IV estimates.

Let's indicate these further lags by $k > 0$. In that case, we can analyze the impact of year $T$ forecast errors (for years $T+1, T+2, \ldots, T+h$) on growth outcomes in year $T+h+k$. We then thus ask whether a recession in, say, the year 2001 (for $k = 1$), 2002 ($k = 2$), or 2003 ($k = 3$) was more likely to arise if growth expectations in 1997 for the years 1998, 1999, and 2000 were overly optimistic. Having $k > 0$ cuts the mechanical relationship between the growth outcome (which now lies in the year 2001 or beyond) and previous forecast errors (constructed by

Table C1—Average Annual Growth Rate of Real Per Capita GDP in Years Prior to Recessions

| Average in nonrecession years | Average $(T-4), (T-3), (T-2)$ | Average $(T-3), (T-2), (T-1)$ |
|---|---|---|
| 3.8 percent | 3.6 percent | 3.9 percent |

*Notes:* In these calculations, year $T$ always denotes the first year of a recession (defined as years during which real GDP per capita growth was negative). All years $(T-4), \ldots, (T-1)$ are recession free by construction (to enable a clean comparison).

Table C2—OLS Regressions When Forecast Error Is Lagged $(k+3)$ Periods, Global Sample

| Dependent variable: Real GDP growth | | | |
|---|---|---|---|
| | (1: $k = 3$) | (2: $k = 2$) | (3: $k = 1$) |
| Forecast error on growth "$F_{3,t-3-k}$" | −0.129093<br>−1.62 | −0.185263<br>−2.34 | −0.2875159<br>−5.06 |
| Time and country fixed effects | Yes | Yes | Yes |
| Countries | 185 | 186 | 187 |
| Observations | 3,536 | 3,715 | 3,891 |

*Notes:* OLS regressions analyzing the impact of past forecast errors on future growth. The dependent variable is real GDP growth. Endogeneity is mitigated by lagging the forecast error variable. Numbers in parentheses represent $t$-statistics, calculated using robust standard errors.

only using data from *before* the year 2001). This opens the door to an analysis that does not rely on IV.

However, such a strategy can still be subject to endogeneity issues if economies systematically started losing momentum in years prior to a growth slump (as that would mechanically generate positive forecast errors in years before a downturn). To look at this possibility, we examined the growth behavior of all IMF member states over the period 1990–2016 prior to recessions. As can be seen in Table C1, the average per capita growth rate remains fairly stable in the run-up to a recession,[23] suggesting that recessions arrive rather suddenly (a feature also described in the literature on business cycle asymmetries; see, e.g., Acemoglu and Scott 1997). This contributes to the fact that recessions are typically hard to predict—even one year ahead. While this does not guarantee that there isn't some third factor—distinct from optimism—systematically affecting past forecast errors as well as current recessions, it does suggest that such a problem may be somewhat minor.

Proceeding with OLS gives rise to the following estimation results for the global sample (results are fully robust to using logit/probit on a recession dummy or to focusing on the restricted sample of emerging and developing countries only). As Table C2 illustrates, our main result survives when considering $k$ up to two lags. In that case, we are analyzing the impact of 1995 forecast errors for the years {1996, 1997, 1998} on real GDP growth for the year 2000—a temporal distance that seems long enough to mitigate endogeneity concerns. (Claiming the opposite would imply that data from 1996 to 1998 should already contain signals that a recession will

[23] In many emerging markets, growth frequently even accelerates just before a recession (underlining that these countries often get to experience *sudden* stops). This biases OLS results in a downward direction.

Table C3—OLS Regressions with Covariates When Forecast Error Is Lagged $(k+3)$ Periods, Global Sample

| Dependent variable: Real GDP growth | | | |
|---|---|---|---|
| | (1: $k = 3$) | (2: $k = 2$) | (3: $k = 1$) |
| Forecast error on growth "$F_{3,t-3-k}$" | $-0.0738884$ | $-0.1291015$ | $-0.2706438$ |
| | $(-1.38)$ | $(-2.50)$ | $(-4.73)$ |
| Average growth in trading partners | 0.283612 | 0.227599 | 0.1571967 |
| | (2.70) | (2.69) | (2.62) |
| Percent change in terms of trade | 0.0044323 | 0.0036816 | 0.0011393 |
| | (1.89) | (1.57) | (0.50) |
| Time and country fixed effects | Yes | Yes | Yes |
| Countries | 169 | 169 | 169 |
| Observations | 3,194 | 3,350 | 3,496 |

*Notes:* OLS regressions analyzing the impact of past forecast errors on future growth whilst controlling for covariates. The dependent variable is real GDP growth. Endogeneity is mitigated by lagging the forecast error variable. Numbers in parentheses represent $t$-statistics, calculated using robust standard errors.

arrive in the year 2000. Judging from the difficulties that forecasters experience in predicting growth outcomes at such horizons, this does not seem to be the case.)

Table C3 shows similar findings when including controls.

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