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Risto Herrala and Fabrice Orlandi

## Win-Win? Assessing the global impact of the Chinese economy



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# Win-Win? Assessing the global impact of the Chinese economy

## Abstract

We study the global impact of the Chinese economy based on a novel indirect approach where the spillover effect is quantified from a forecast error model under relatively favorable identifying conditions. Findings from the real-time World Economic Outlook data over the period 2004–2015 indicate that an increase in economic growth in China had a negative impact on most other economies one to two years ahead. The estimations furthermore uncover evidence at the global level that spillover propagated by influencing prices, including global commodity prices, which tend to increase in reaction to accelerating economic growth in China.

JEL classifications: C2, F15, F440

Keywords: Chinese economy; global spillover; real time data

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# I Introduction

The global financial crisis brought into focus the importance of international spillover in macroeconomic policy, including monetary policy. Recently, the impact of the ongoing economic slowdown in China, the world's largest goods exporter, has emerged as a significant concern.<sup>1</sup> This paper presents estimates of the short-to-mid-term effect of real GDP growth in China on other countries.

The sharply divided discussion over spillover from the large Asian economy runs broadly along the lines of findings from the main alternative empirical approaches. The view that economic growth in China tends to benefit other economies is consistent with much of the previous econometric evidence developed from aggregated country-level time series (Dizioli et al., 2016; Feldkircher and Korhonen, 2014; Arora and Vamvakidis, 2011). The concern that Chinese economic growth comes at the expense of jobs elsewhere, especially in the developed world, finds support from studies using disaggregated data (Autor et al., 2016; Bloom et al., 2016; Pierce and Schott, 2016).

Some of this divergence in findings is symptomatic of the difficult identification and aggregation issues that challenge empirical work on spillover at the global level. Micro-data hold the promise of strong identification with quasi- experimental designs that utilize cross-sectional variation across agents. Due to the scarcity of suitable data, however, aggregation of the results to global, or even national, levels poses a non-trivial challenge. Aggregation is less of an issue in work that utilizes widely available country-level time series, but significant concerns have been voiced about the strength of identification in the context of the global economy, where the number of causal relationships is large, and, realistically, partly unknown to the econometrician (Bai et al., 2016). Standard regression techniques, including the Global Vector Autoregressive model (GVAR, Pesaran et al., 2004), are susceptible to degree-of-freedom problems and omitted variable bias. Moreover, the factor-augmented VAR (FAVAR, Bernanke et al., 2005) only resolves these issues under the strong assumption that the complex global web of interactions can be captured by a small number of factors.

To achieve strong aggregation and identification, we employ an original empirical strategy where spillover is quantified indirectly with a forecast error model. Data revisions of real GDP growth in China, the spillover source country, cause errors in forecasts of real GDP growth rates in spillover target countries. Formal analysis indicates that this channel can be exploited to quantify spillover. Estimation is performed by a parsimonious linear regression model of the forecast errors

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<sup>1</sup> Wall Street Journal (Jan. 31, 2019), "China's Slowdown Hits Growth Around the Globe." Financial Times (Jan. 14, 2019), "The Impact of China's Economic Slowdown is Spreading." BBC News (Jan. 4, 2019) "China's Economic Slowdown: How Worried Should We Be?" Scott, R., Mokhiber, Z. (2018), "The China Toll Deepens," Economic Policy Institute.

of real GDP growth in spillover target countries on the data revisions of real GDP growth in China. The more random the data revisions and better informed the forecasts, the lower the risk that spillover estimates are affected by omitted variable bias.

Our approach has less stringent identifying conditions than those required under similar leading alternative frameworks. In particular, the studies of Suarez Serranto and Wingender (SS&W, 2016) and Chodorow-Reich et al. (C-R et al., 2019) also use data revisions of the causing variable as “shocks” to identify causal effects. Unlike our approach, they use the caused variable rather than its forecast error as the endogenous variable of the regression model. We report spillover estimates based on these alternative approaches as well.

The present study also relates to the vast literature using forecast error models to uncover causal influence. The pioneers, Fama et al. (1969), used forecast errors in identification schemes in empirical finance. Subsequent researchers considered other fields (Roth, 2019; Kothari and Warner, 2006). This paper complements this work by broadening the domain in which forecast errors can be used to identify causal effects.

Additionally, Blanchard and Leigh (2013) show that a forecast error model can be analyzed using real-time data to reveal the presence of forecast bias, but do not attempt to quantify the causal effect. Our empirical specification, therefore, can be viewed as an extension of their framework.

This paper also relates to Denton and Kuiper (1965). Their result, almost trivial on its face, suggest that the data revision of an exogenous variable in the context of a linear forecast error model shifts the forecast by an amount equal to the loading of that variable in the model. While they do not discuss the issue further, their finding implies that regression analysis of forecast errors of a caused variable on data revisions of the causing variable(s) may reveal the causal relationship(s).

Intuition may, however, be easily misled in this case by the natural presupposition that the econometrician is interested in revealing the parameter used by the forecaster. This is not the case under our approach. The benefit of using forecast errors of the caused variable as the dependent variable instead of the caused variable directly (as in SS&W, 2016 and C-R et al., 2019) is not the forecasters’ presumed knowledge of the causal parameter, but rather their control of other causal factors besides the causal factor of interest. Even if the forecasters’ control of the other factors is imperfect, it may greatly mitigate nuisance influence of other causing factors, thereby shielding against the risk of omitted variable bias.

A novel empirical approach may therefore be warranted when the omitted variable bias is a significant concern under traditional approaches. Forecasts of the caused variable by well-informed professionals and the real-time data of the causing variable are also needed. Such is the case when studying GDP spillovers.

The World Economic Outlook database records the GDP growth forecasts of the IMF along with the real time data for over 170 countries. We use this data to estimate spillover of real GDP growth in China to each of these countries over the period 2004–2015.

Estimations with our novel approach yield the robust finding that GDP spillover from China was negative in most countries over the sample period. We further find a broad range of spillover strengths across countries, and that spillover from China is significant for about half of the 177 countries studied. The finding of significant across-country variation of spillover is consistent with such earlier empirical works as Furceri et al. (2016) and Dedola et al. (2017).

The paper also sheds new light on the spillover channels. We find that economic growth in China tends to be followed by a jump in GDP price inflation in other countries. Adding to previous evidence on the issue (Zhuo, 2018; Dieppe et al, 2018), the study furthermore yields evidence of a positive reaction of commodity prices to Chinese economic growth.

From a policy perspective, the results indicate that economic growth is not always a positive sum game, and that it can have negative externalities globally even if one abstracts from the issue of pollution. It also underscores the importance of international spillovers for monetary policy. Below we formalize the empirical approach, and then present the estimation results. We conclude with a discussion of future research directions.

## II The empirical approach

### A The estimable equation

Consider the model

$$Y_{t+1} = \alpha_1 + \alpha_2 \textit{China}_t + \alpha_3 \textit{Other}_t + \epsilon_{t+1}, \quad (1)$$

where  $Y$  indicates the real GDP growth rate of a country of interest;  $\textit{China}$  indicates the real GDP growth rate of China;  $\textit{Other}$  capture(s) all other systematic factors that cause  $Y$ ;  $\epsilon$  is white noise; and  $\alpha$  are unknown parameters. The focus of interest is the parameter  $\alpha_2$ , which indicates the causal influence of  $\textit{China}$  on  $Y$ , the (real GDP growth) spillover from China.

In principle, estimation of  $\alpha_2$  is a straightforward linear regression. However, construction of  $\textit{Other}$  is a difficult challenge as it encompasses a potentially large number of controls, some of which may be unknown. For the purpose of estimating  $\alpha_2$  for over 170 countries, the construction task is impracticable.

It is well known that regression analysis using (1) could lead to poor results as the unmodeled correlation between *China* and *Other* may bias the estimate  $\widehat{\alpha}_2$ . The omitted variable bias is (Clarke, 2005):

$$\widehat{\alpha}_2 - \alpha_2 = \alpha_3 * \text{corr}(China_t, Other_t) \sqrt{\frac{\text{var}(Other)}{\text{var}(China)}} \quad (2)$$

where *corr* indicates correlation and *var* variance. Since *China* and *Other* are contemporaneous, and thereby affected by common economic events, they are likely to be correlated. Since *Other* is not observed, the size or direction of the omitted variable bias is unknown.

While lacking the knowledge to credibly estimate  $\alpha_2$  via (1), we note that the International Monetary Fund (IMF) seems much better positioned informationally. The IMF actively monitors the countries of interest and has access to a wide range of public and private information about them. Furthermore, since its country analysis has evolved under independent auditing and public scrutiny for decades, substantial institutional knowledge is likely embedded in its processes. The IMF publishes regularly GDP forecasts of its member countries. Might it therefore be possible to somehow extract the information needed to estimate  $\alpha_2$  from the IMF's GDP forecasts?

We investigate this question under the assumption of *linearity* that, like the spillover process (1), the IMF forecast process can also be approximated by a linear function:

$$F_t^{IMF} Y_{t+1} = \alpha_1^{IMF} + \alpha_2^{IMF} China_{t|t} + \alpha_3^{IMF} Other_{t|t}, \quad (3)$$

where  $F_t^{IMF}$  indicates a forecast based on information possessed by the IMF at  $t$ ;  $China_{t|t}$  and  $Other_{t|t}$  are real-time data of *China* and *Other* available to the IMF at  $t$ ; and  $\alpha^{IMF}$  are the (unknown) parameters embedded in the IMF forecast process which may or may not correspond with the correct parameters. We omit independent random noise from the right-hand side of (3) for simplicity. Please note one subtle point: since (3) is an approximation of the forecasting process of the IMF involving both judgement and models (rather than a statement about a specific econometric model), it may not be transparent even within the IMF.

By subtracting (3) from (1) and rearranging, we get the error of IMF forecasts:

$$\begin{aligned} (Y_{t+1} - F_t^{IMF} Y_{t+1} &= \alpha_1 - \alpha_1^{IMF} \\ &+ \alpha_2 (China_t - China_{t|t}) \\ &+ (\alpha_2 - \alpha_2^{IMF}) China_{t|t} \\ &+ \alpha_3 \left( Other_t - \frac{\alpha_3^{IMF}}{\alpha_3} Other_{t|t} \right) + \epsilon_{t+1}. \end{aligned} \quad (4)$$



The forecast error equation (4) shows that, indeed, access to IMF forecasts and real-time data affords the possibility to approach the estimation of  $\alpha_2$  from an alternative point of view. Rather than studying the influence of the Chinese economy on other countries directly via (1), one can approach the problem indirectly by studying IMF forecast errors. Part of that error is caused by data revisions regarding *China*. The spillover parameter  $\alpha_2$  can alternatively be interpreted as the strength of this channel.

However, Eq. (4) still includes the unobserved variable(s), namely ‘IMF modelling error’ regarding other factors except spillover ( $Other_t - \frac{\alpha_3^{IMF}}{\alpha_3} Other_{t|t}$ ). Does this mean that the indirect forecast error approach (4) is equally affected by missing variable bias as the traditional approach (1)?

If the unobserved IMF modelling error is omitted in empirical estimations, the estimate  $\widehat{\alpha}_2$  may, indeed, be biased. Based on Clarke (2005), the bias is:

$$\widehat{\alpha}_2 - \alpha_2 = \alpha_3 * \sqrt{\frac{\text{var}\left(Other_t - \frac{\alpha_3^{IMF}}{\alpha_3} Other_{t|t}\right)}{\text{var}(China_t - China_{t|t})}} * \left\{ \frac{\text{corr}\left(China_t - China_{t|t}, Other_t - \frac{\alpha_3^{IMF}}{\alpha_3} Other_{t|t}\right)}{1 - \text{corr}(China_{t|t}, China_t - China_{t|t})^2} - \frac{\text{corr}(China_{t|t}, China_t - China_{t|t}) * \text{corr}\left(China_{t|t}, Other_t - \frac{\alpha_3^{IMF}}{\alpha_3} Other_{t|t}\right)}{1 - \text{corr}(China_{t|t}, China_t - China_{t|t})^2} \right\} \quad (5)$$

From (5), we note that the omitted variable bias is negligible if data revisions and IMF modelling error are uncorrelated with each other and either is uncorrelated with the real-time variable. Under this *uncorrelated* assumption, the last two terms on the right of (5) (in curly brackets) vanish and take the omitted variable bias with them. Otherwise, the spillover estimate is expected to be biased.

For now, we keep an open mind that the *linear* and *uncorrelated* assumptions hold as a reasonable approximation. Accordingly, we use for the estimations the following regression model obtained from (4) by moving the unobserved term(s) in the residual:

$$Y_{t+1} - F_t^{IMF} Y_{t+1} = \beta_1 + \alpha_2 (China_t - China_{t|t}) + \beta_3 China_{t|t} + \varepsilon_{t+1}, \quad (6)$$

where  $\beta$  are parameters and  $\varepsilon_{t+1} = \alpha_3 \left( Other_t - \frac{\alpha_3^{IMF}}{\alpha_3} Other_{t|t} \right) + \varepsilon_{t+1}$ . Under the *linear* and *un-correlated* assumptions, regression analysis of (6) is expected to yield an unbiased estimate of  $\alpha_2$ .

As an aside, we note that the two assumptions do not guarantee that the real-time variable and the unobserved IMF modelling error are orthogonal. The estimate  $\widehat{\beta}_3$  may therefore be subject to omitted variable bias ( $\widehat{\beta}_3 \rightarrow \alpha_3 - \alpha_3^{IMF}$ ). The conditions under which  $\beta_3$  is unbiased are discussed by Blanchard and Leigh (2013).

## B Finite sample properties

It is therefore clear that the spillover parameter can be estimated from a forecast error model under suitable conditions. How do these conditions compare with competing approaches, in particular the “incumbent approach” of SS&W (2016) and C-R et al. (2019) in finite samples?<sup>2</sup>

We recall from previous discussion that both approaches are expected to yield unbiased estimates if data revisions are white noise. Furthermore, if data revisions are not white noise but IMF modelling error is white noise, then only the novel approach should yield unbiased estimates.

Previous empirical work, however, provides some evidence that data revisions are not always white noise (Aruoba, 2008), and that IMF forecasts can show some biases (IEO, 2014). To compare the omitted variable bias of  $\widehat{\alpha}_2$  under the two approaches when neither data revisions nor modelling error are white noise, we reformulate (6):

$$Y_{t+1} = \beta_1 + \alpha_2(China_t - China_{t|t}) + \beta_3 China_{t|t} + F_t^{IMF} Y_{t+1} + \varepsilon_{t+1} . \quad (7)$$

The restatement (7) of model (6) is similar in form to the incumbent approach, except that they omit  $F_t^{IMF} Y_{t+1}$  and  $\beta_2 China_{t|t}$  from the right-hand side. We abstract for now from the omission of the latter term which, as it turns out, only has a minor influence on the spillover estimate at the global level. Under this restriction, our comparison of the incumbent and novel approach boils down to the impact on  $\widehat{\alpha}_2$  of omission of  $F_t^{IMF} Y_{t+1}$  from the right-hand side of (7).

Based on Clarke (2005), the omitted variable bias under the novel approach is expected to be smaller in absolute terms than in the incumbent approach if:

<sup>2</sup> See Eq. (6) of SS&W (2016) and C-R et al. (2019).

$$\sqrt{\frac{\text{var}\left(\alpha_3\left(\text{Other}_t - \frac{\alpha_3^{IMF}}{\alpha_3}\text{Other}_{t|t}\right)\right)}{\text{var}\left(\alpha_3\left(\text{Other}_t - \frac{\alpha_3^{IMF}}{\alpha_3}\text{Other}_{t|t}\right) + F_t^{IMF}Y_{t+1}\right)}} \cdot \text{abs}\left\{\frac{\text{corr}\left(\text{China}_t - \text{China}_{t|t}, \alpha_3\left(\text{Other}_t - \frac{\alpha_3^{IMF}}{\alpha_3}\text{Other}_{t|t}\right)\right)}{\text{corr}\left(\text{China}_t - \text{China}_{t|t}, \alpha_3\left(\text{Other}_t - \frac{\alpha_3^{IMF}}{\alpha_3}\text{Other}_{t|t}\right) + F_t^{IMF}Y_{t+1}\right)}\right\} < 1, \quad (8)$$

where *abs* gives the absolute value. For further reference, we refer to (8) as the condition that the forecaster is *well informed*.

Based on (8), being well informed is a combination of two (multiplicative) criteria. The

first variance criteria on the left  $\left(\frac{\text{var}\left(\alpha_3\left(\text{Other}_t - \frac{\alpha_3^{IMF}}{\alpha_3}\text{Other}_{t|t}\right)\right)}{\text{var}\left(\alpha_3\left(\text{Other}_t - \frac{\alpha_3^{IMF}}{\alpha_3}\text{Other}_{t|t}\right) + F_t^{IMF}Y_{t+1}\right)}\right)$  indicates that the smaller the

variance of IMF modelling error is relative to the variance of the sum of modelling error and forecasts, the better informed the forecaster and the more favorable the situation is for the novel approach. We note that the variance criteria are favorable to the novel approach except where the IMF modelling error and real time forecasts are strongly negatively covariant. Negative co-variation could arise if the IMF tended to systematically over/under estimate contributing factors that influence GDP growth positively/negatively ( $\text{Other}_{t|t} \neq \text{Other}_t$ ), or to over/underestimate the influence of positive/negative factors ( $\alpha_3^{IMF} \neq \alpha_3$ ).

The other criteria for being well informed relate to correlation of modelling error with data

revisions  $\left(\frac{\text{corr}\left(\text{China}_t - \text{China}_{t|t}, \alpha_3\left(\text{Other}_t - \frac{\alpha_3^{IMF}}{\alpha_3}\text{Other}_{t|t}\right)\right)}{\text{corr}\left(\text{China}_t - \text{China}_{t|t}, \alpha_3\left(\text{Other}_t - \frac{\alpha_3^{IMF}}{\alpha_3}\text{Other}_{t|t}\right) + F_t^{IMF}Y_{t+1}\right)}\right)$ . Based on these criteria, the smaller

the correlation between data revisions and IMF modelling error relative to the correlation between data revisions and the sum of modelling error and forecasts, the better informed the forecaster and the more favorable the situation for the novel approach. According to our interpretation, the correlation criteria indicate about how successfully the IMF manages to take into account the factors that drive data revisions in China in its forecasts of the spillover target countries. If data revisions and IMF modelling error are driven by different/similar factors, then this plays in favor of the novel/incumbent approach.

To summarize, if data revisions in China are not fully independent of other causal factors of GDP growth in spillover target countries, then only the novel approach can still produce unbiased estimates of spillover. The estimate is unbiased if GDP data revisions in China and IMF modeling

error regarding GDP growth in spillover target countries do not correlate. Otherwise, both the novel approach and the incumbent approach are expected to yield biased estimates of the spillover parameter. The bias under the novel approach is smaller than under the incumbent approach if the IMF is *well informed* in the sense that its modelling error is small or only weakly correlated with its own forecasts of spillover target countries, or data revisions in China. These qualities seem appropriate for an institution like the IMF with a long history and prominent role as global forecaster.

### III Empirical analysis

#### A The data

The estimation data is from the IMF World Economic Outlook (WEO) database, which provides access to forecasts and real-time data on annual real GDP growth of 170+ countries. The real-time data and forecasts are published twice a year around April and October. The estimation sample starts at 2004, which is the first year for which the necessary data are available for a large number of countries. The sample ends in 2015, allowing at minimum three years for the statistical authorities to provide a reasonable “final” estimate of real GDP growth by 2018, which we use as the final data year. The final data year is selected based on the dynamics of squared data revisions in the WEO. For the relevant estimation years, the median squared data revision is no longer increasing after three years from the first real-time data release.

When studying the data, one quickly confronts the fact that the data are never absolutely final. Data revisions can and do take place over many years, even decades, after the first real-time estimates appear. This issue may not significantly influence our findings, however, since independent random noise in the final data does not bias the spillover estimates. We nevertheless investigate the robustness of the estimation results to alternative definitions of “final.”

For the benchmark model, the final data and the real time data are taken from the April vintages. We use the term “observation year” to indicate the year when the WEO was published. Since the April WEO is prepared at the start of the year, we use as the dependent variable ( $Y_{t+1} - F_t^{IMF} Y_{t+1}$ ), the IMF forecast error of real GDP growth during the observation year. For example, the forecast error for the year 2014 is computed by diluting from the real GDP growth rates given in the April 2018 WEO regarding year 2014 the forecast of real GDP growth for the year 2014 observed in the April 2014 WEO. The data revision variable ( $China_t - China_{t|t}$ ) indicates the year that precedes the observation year. For example, the data revision for year 2013 is computed by diluting from the real GDP growth rate of year 2013 given in the April 2018 WEO the real-time estimate of real GDP growth in 2013 observed in the April 2014 WEO. Real-time GDP growth

( $China_{t|t}$ ) is measured correspondingly. For example, the real-time GDP growth rate of year 2013 is the real GDP growth rate of year 2013 observed in the April 2014 WEO.

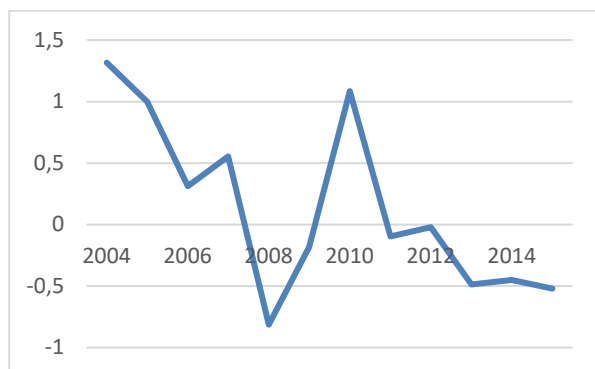
The IMF forecast errors and data revisions, on average, are positive at 0.17 pp and 0.5 pp, respectively (Table 1). The overall biases are not large in relative terms. The median forecast error is less than 5 percent of the median forecast, and Chinese GDP data revision is about 5 percent of the average Chinese (real-time) GDP growth rate. Identification of the spillover parameter is not sensitive to a positive or negative overall bias in forecasts or data revisions, which are captured by the constant term of the econometric model.

Table 1 Data description

	median	N
IMF forecast error of real GDP growth at t	0.16	2119
Data revision of Chinese real GDP growth at t-1	0.51	12
Real-time Chinese real GDP growth at t-1	9.2	12
Real GDP growth at t	4.0	2119

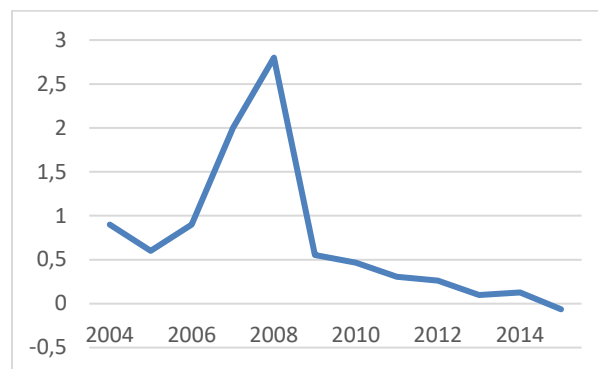
Notes: Data sample is 177 countries over years 2004–2015 unless data is missing.  
Data Source: IMF World Economic Outlook database, April vintages.

Figure 1 Average real GDP growth forecast error by the IMF



Notes: Average error in real GDP growth forecasts of the observation year across 177 countries given in the April WEO of that year. Real GDP growth given in the April 2018 WEO is used as the correct data. Units: pp. Data source: IMF World Economic Outlook database.

Figure 2 Revisions of Chinese real GDP growth rates by observation year



Notes: The observation for year t indicates real GDP growth data revision of year t-1. Real GDP growth given in the April 2018 WEO is used as the correct data. The real-time data are from the April vintage of each observation year. Units: pp. Data source: IMF World Economic Outlook database.

Average forecast errors (Figure 1) show positive autocorrelation. This feature may be benign from the point of view of the identifying assumptions, reflecting IMF modeling error of target countries that is not strongly correlated with data revisions in China. To investigate this, we suitably manipulate the latter variable.

Data revisions show a downwards trend, especially during the final part of the sample (Figure 2). We explore the implications of this issue by, among other things, de-trending techniques. We furthermore explore the sensitivity of the results to the two large positive observations by sub-sample estimations.

We note that the official compilation methods of GDP underwent substantial changes during the observation period (Holz, 2014). This plays in favor of the novel approach by introducing idiosyncratic variation in the exogenous variable. Note, for example the large positive data revisions during the observation years 2007 and 2008. Since observation years lead the real-time years by one year, the two large data revisions refer to real GDP growth in China in 2006 and 2007. At the time, real economic activity grew fast even by Chinese standards. The bulk of the data revisions regarding these years were executed in 2009–2011 based, in part, on the 2008 census data and a revision of that data in 2010, as well as the revision in 2009 of the Statistics Law (Holz, 2014).

## B Estimation results

Estimations with the novel approach yield the main finding (Table 2, Model 1) that the spillover parameter was negative for most countries over the period 2004–2015. In the benchmark model, the median spillover parameter estimate is at -0.11 over the 177 country-level models. The reduced model (Table 2, Model 2), where the real-time variable has been dropped, yields similar estimates for the median country.

Based on the  $R^2$  statistics, the benchmark models typically explain 14 percent and the reduced models about five percent of the variation in GDP growth rates within countries. The fourth column of Table 2 gives the sum of p-values of the spillover parameter across the country models as an indication of how many countries did not experience significant spillover from China. The number of countries in the benchmark model is 92, or just over one-half. The finding that spillover from China is significant for almost half of the countries is perhaps not surprising in light of China's share of one-fifth of global GDP and status as the largest goods exporter.

Table 2 Main estimation results

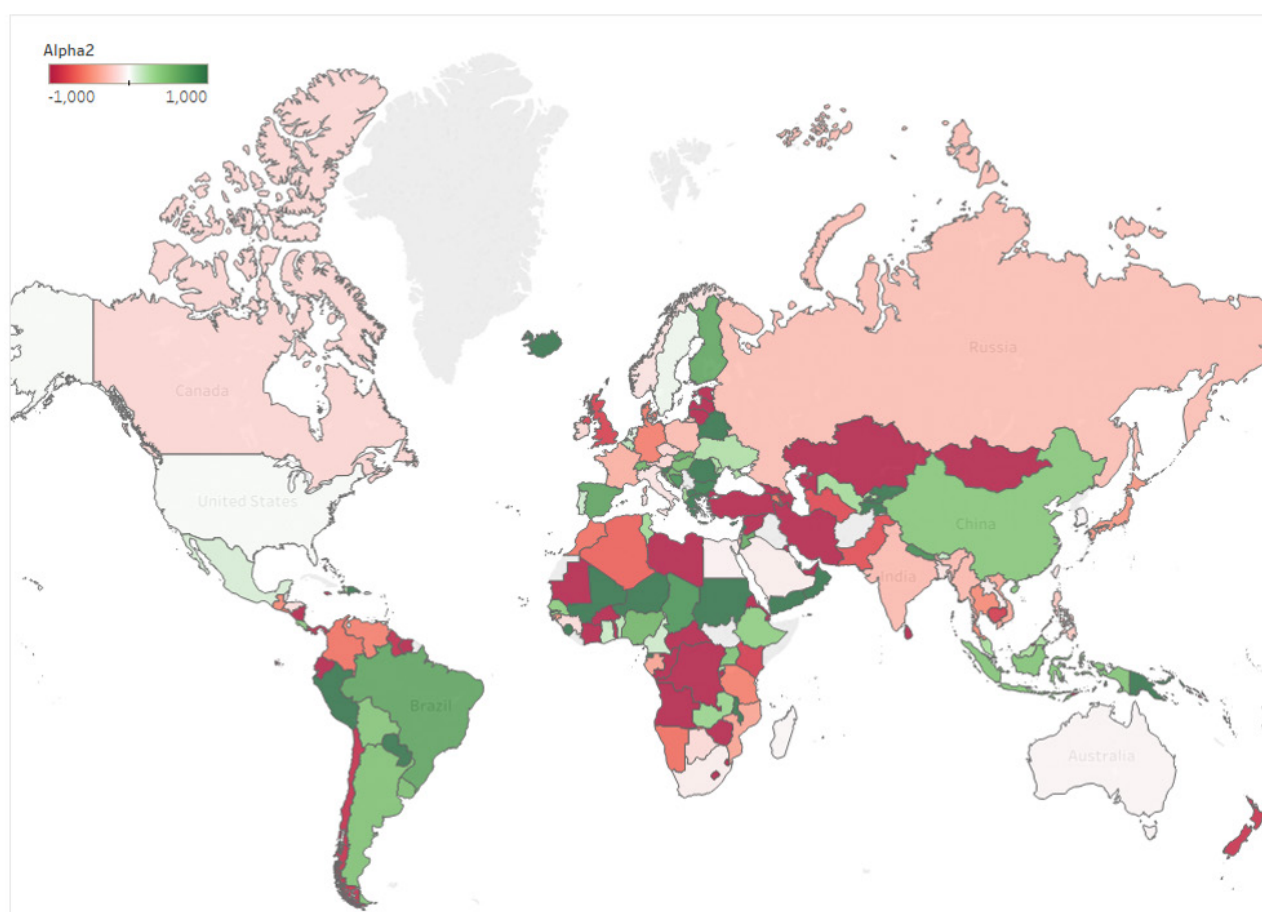
	$\widehat{\alpha}_2$	$R^2$	sum(p)
Model 1: benchmark	-0.11	0.14	92
Model 2: reduced, omits real-time variable	-0.08	0.05	85

Notes: The table is based on 177 country-level models of Eq. (6) estimated by OLS. The second column gives the median spillover estimate, while the third column shows the median  $R^2$ . The fourth column is the sum of p-values of the spillover parameter across the models. Estimations by OLS over observation years 2004–2015 unless data are missing. Data Source: WEO, April vintages.

A detailed study of the country-level findings (Figure 3; Table A1 in Annex) is beyond the scope of the present paper. We note, however, that the overall cross-country pattern is not easily compatible with the view that spillover mainly reflects the bilateral trade relationships between the spillover source and target country. Indeed, the cross-country correlation between  $\widehat{\alpha}_2$  and bilateral goods trade with China, measured as share of exports to or imports from China to GDP of the spillover target country, tends to be quite weak (these calculations are not shown here in detail). We therefore suspect that spillover from China, at least at the horizons studied here, may mainly operate via other channels than through the direct trade channel.

What could this main spillover channel be? The visual provides the further clue that the bulk of large hydrocarbon producers, including Saudi Arabia, the US, Russia, Iran, Norway, the UAE, Canada, Australia, Kazakhstan, Venezuela, and Mongolia, show negative or negligible spillover. This result may seem counter-intuitive. Surely commodity producers benefit from increased production and the corresponding increased demand for natural resources from China?

Figure 3 Estimates of  $\alpha_2$  (indicating real GDP spillover from China)



Notes: Estimation based on Eq. (6) by OLS. Negative spillover in red and positive spillover in green. Spillover strength is indicated by shade. All spillover estimates in excess of 1 and below -1 are shown in the darkest shades. Data source: Own calculations. We thank Jonna Elonen-Kulmala and Tia Kurtti for the chart.

However, the increase in demand for commodities associated with a positive GDP shock in China presumably takes place prior to or simultaneously with the production process in China, where raw materials and energy serve as inputs. This dynamic is not captured in the present analysis, which focuses on what happens in the spillover target countries afterwards. The negative spillover parameter observed in commodity-producing countries may therefore reflect the second-round effect following a positive demand shock from China in these countries.

## C Robustness tests

Estimations with de-trended and demeaned data revisions (Table 3, Model 3) reinforce the finding, that the spillover parameter is mostly negative. We note that this model shows an even stronger negative spillover parameter for the median country than the benchmark model.

We also exploit the variation in real-time data to construct an alternative shock variable. The approach builds on a decomposition of the overall data revision used in the benchmark model ( $China_t - China_{t|t}$ ) into two components, i.e. the “late revision” that occurred after the October vintage of the observation year ( $China_t - China_{t|t\ Oct}$ ), and the “early revision” that occurred between the April and October vintages of the observation year ( $China_{t|t\ Oct} - China_{t|t\ Apr}$ ):

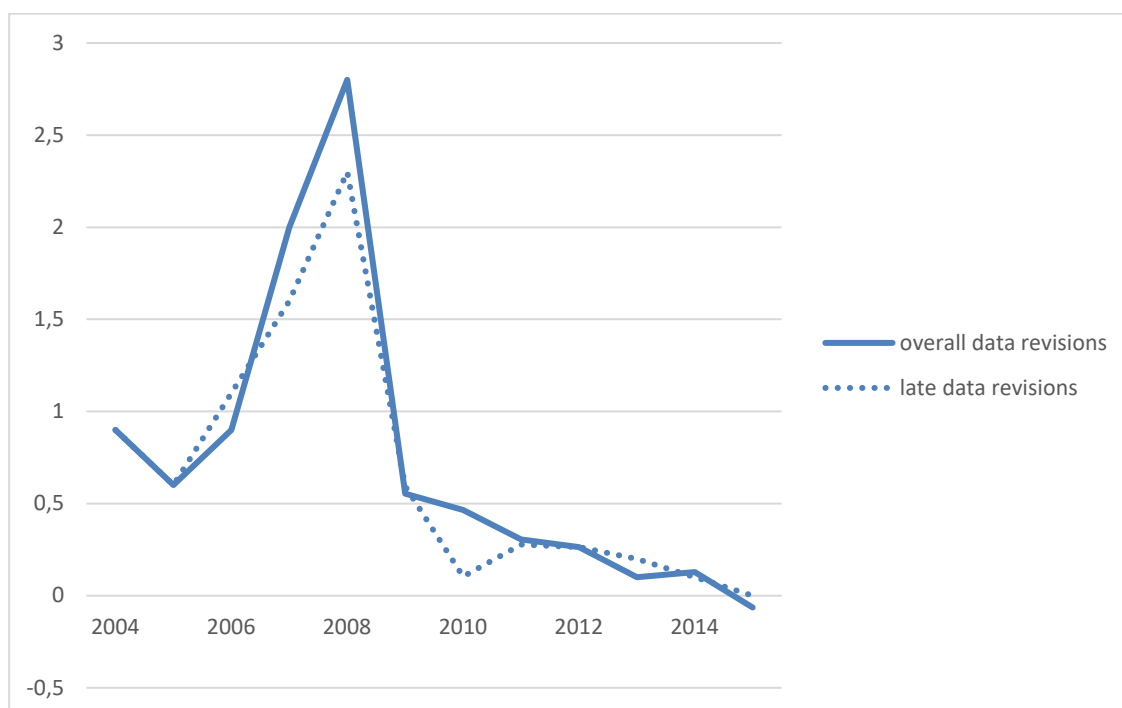
$$China_t - China_{t|t} \equiv China_t - China_{t|t\ Oct} + China_{t|t\ Oct} - China_{t|t\ Apr} \quad (9)$$

The two components of the data revision may have different statistical properties, reflecting the process by which GDP data is compiled in China (Holz, 2014). The early estimate of real GDP growth included in the April WEO is based on incomplete data. By the October WEO, the underlying data may be more complete. The Chinese statistical authorities have also had more time to use the various means at their disposal to estimate the systematic components from the still missing data. Confidence that the late revision is cleaned from the influence of systematic components is stronger than for the early release.

We therefore use the *late data revision* as an alternative shock variable (Table 3, Model 4; Figure 4). The finding that the spillover parameter is mostly negative is robust to the change in the shock variable. We, again, note somewhat larger (in absolute terms) negative spillover parameter estimates for the median country than in the benchmark model.



Figure 4 Overall and late data revisions of Chinese real GDP growth rates by observation year



Notes: The observation for year  $t$  indicates real GDP growth data revision of year  $t-1$ . Real GDP growth given in the April 2018 WEO is used as the correct data. The real time data for overall data revisions is from the April vintage and the real time data for the late data revision if from the October vintage of the observation year. Units: pp.

Data source IMF World Economic Outlook database.

We estimate the model using the subsample 2009–15, which excludes the two large positive data revisions (Table 3, Model 5). We omit the real time variable due to the small number of observations per country, and use the late revisions shock which exhibits less trending than the overall data revision in this subsample. The main finding that the spillover parameter is in most cases negative is robust to these changes.

Table 3 Selected robustness tests

	$\hat{\alpha}_2$	$R^2$	sum(p)
Model 3: de-trended	-0.47	0.17	80
Model 4: late revision shock	-0.21	0.15	89
Model 5: late revision shock, 2009–15	-0.17	0.19	70
Model 6: October vintages	-0.88	0.28	80
Model 7: global aggregates	-0.79	0.24	
Model 8: incumbent model, late revision shock	-0.49		

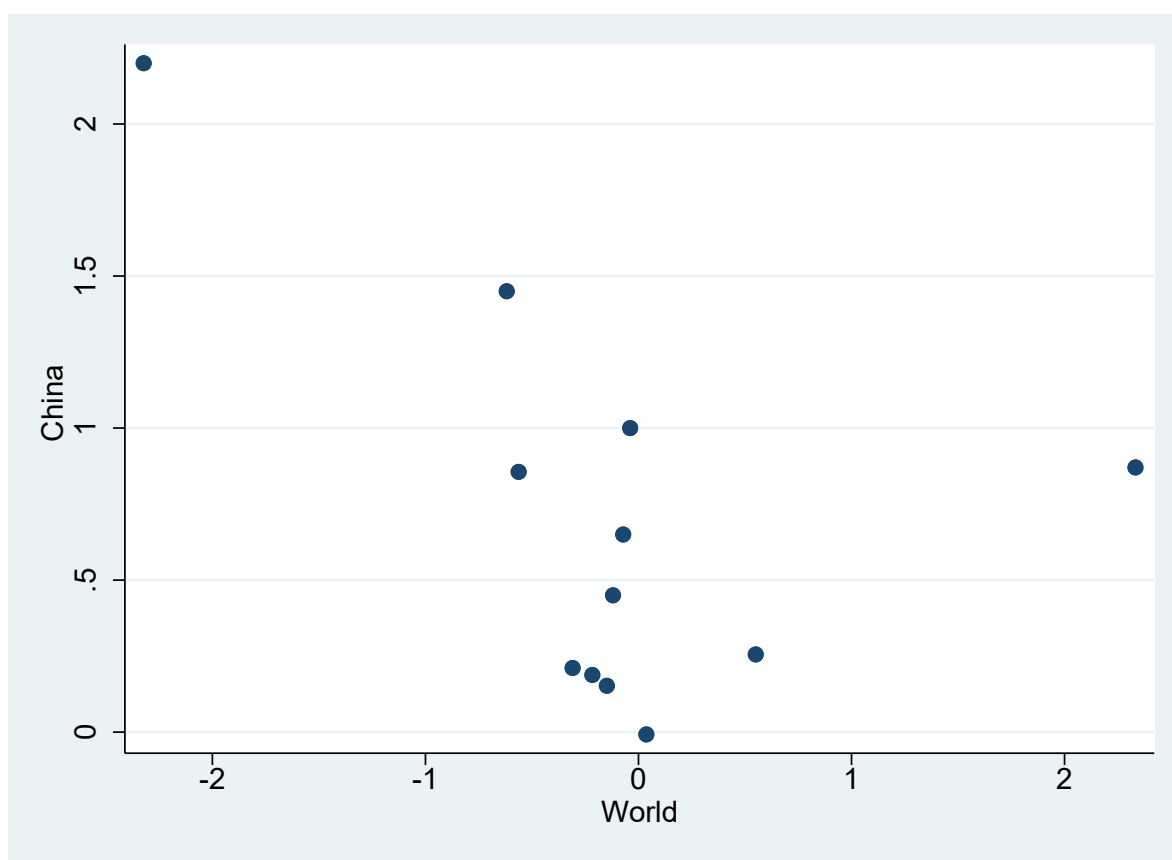
Notes: The second column gives the median spillover estimate and the third column the median  $R^2$  across countries. The fourth column is the sum of p values of the spillover parameter across countries. The global aggregates model 7 is from a regression of IMF forecast errors of world GDP on data revisions in China. The incumbent model 8 is like model 1 except that the endogenous variable is real GDP growth in spillover target countries and the exogenous variables are the demeaned and de-trended data revision, and the real time variable.

Data source: WEO. Estimations by OLS based on Eq. 7 at country level for 177 countries 2004–15 unless data are missing or otherwise stated.

We also estimate models based on the October vintages. The correct data regarding the years 2004–2015 are taken from the October 2018 data vintage. The dependent variable is the GDP forecast error during the year that follows the observation year. The explanatory variable is the data revision during the year that precedes the observation year. The estimations (Table 3, Model 6) reinforce the main finding of negative spillover. The model shows a considerably more negative spillover parameter for the median country than the benchmark model.

We estimate the model using the IMF forecast errors of real GDP growth of the world as the left-hand side variable (Table 3, Model 7). In this model, the forecast error is averaged over the two years, starting with the observation year. As the independent variable, we use the average data revision of Chinese real GDP growth during the two years preceding the observation year. The estimations confirm the robustness of the main finding of negative spillover to possible aggregation error and the lengthening of the forecast window. The negative correlation between IMF forecasts errors of global real GDP growth and data revisions in China is clearly visible in the data (Figure 5).

Figure 5 Data revision in China (vertical axis), and the IMF forecast error regarding world GDP growth during the following year



Notes: Data revisions are computed as average over two years before the observation year. Forecast errors are computed as average over the observation year and the following year. Unit: pp.  
Data source: IMF World Economic Outlook database, April vintages.

We also study the incumbent model, amended by the real time variable (Table 3, Model 8). Since this model is sensitive to correlation between data revisions and the other causal factors of GDP growth except spillover, we report results from the variant that uses as the shock variable the de-trended and de-measured data revision. The main estimation result with the amended incumbent model is in line with that of the novel approach.

## D The spillover channels

The WEO database provides possibilities to shed further light on the spillover process. To this end, we apply the decomposition of real GDP growth into nominal values and prices  $Y_{t+1} \equiv \frac{GDP_{t+1}}{GDP_t} * \frac{pGDP_t}{pGDP_{t+1}}$ , where  $GDP$  denotes the nominal GDP denominated in national currency, and  $pGDP$  its price index in spillover target countries. The spillover parameter can therefore be decomposed into a *value channel* and a *price channel*:

$$\begin{aligned} \alpha_2 &= \frac{\partial Y_{t+1}}{\partial China_t} = \frac{\partial \left( \frac{GDP_{t+1}}{GDP_t} * \frac{pGDP_t}{pGDP_{t+1}} \right)}{\partial China_t} & (10) \\ &= \frac{\partial \left( \frac{GDP_{t+1}}{GDP_t} \right)}{\partial China_t} \frac{pGDP_t}{pGDP_{t+1}} + \frac{\partial \left( \frac{pGDP_t}{pGDP_{t+1}} \right)}{\partial China_t} \frac{GDP_{t+1}}{GDP_t} \\ &= \text{value channel} + \text{price channel} \end{aligned}$$

The value channel indicates the contribution of changes in nominal GDP growth rates and the price channel the contribution of GDP price inflation to total spillover ( $\alpha_2$ ).

The WEO database includes real time data and forecasts of nominal GDP and GDP prices. The decomposition (10) can therefore be usefully studied based on the novel empirical approach. To arrive at an estimable equation, we first express the expected values of the two channels in terms of the expected values and covariances of their parts:

$$\begin{aligned} (a) \text{ value channel} &= E \left[ \frac{\partial \left( \frac{GDP_{t+1}}{GDP_t} \right)}{\partial China_t} \frac{pGDP_t}{pGDP_{t+1}} \right] = E \left[ \frac{\partial \left( \frac{GDP_{t+1}}{GDP_t} \right)}{\partial China_t} \right] E \left[ \frac{pGDP_t}{pGDP_{t+1}} \right] + \text{cov} \left[ \frac{\partial \left( \frac{GDP_{t+1}}{GDP_t} \right)}{\partial China_t}, \frac{pGDP_t}{pGDP_{t+1}} \right] & (11) \\ (b) \text{ price channel} &= E \left[ \frac{\partial \left( \frac{pGDP_t}{pGDP_{t+1}} \right)}{\partial China_t} \frac{GDP_{t+1}}{GDP_t} \right] = E \left[ \frac{\partial \left( \frac{pGDP_t}{pGDP_{t+1}} \right)}{\partial China_t} \right] E \left[ \frac{GDP_{t+1}}{GDP_t} \right] + \text{cov} \left[ \frac{\partial \left( \frac{pGDP_t}{pGDP_{t+1}} \right)}{\partial China_t}, \frac{GDP_{t+1}}{GDP_t} \right] \end{aligned}$$

We then use the same econometric technique as before to estimate the partial derivatives

$\left( E \left[ \frac{\partial \left( \frac{GDP_{t+1}}{GDP_t} \right)}{\partial China_t} \right] \text{ and } E \left[ \frac{\partial \left( \frac{pGDP_t}{pGDP_{t+1}} \right)}{\partial China_t} \right] \right)$ . The estimated equations are:

$$(a) \frac{GDP_{t+1}}{GDP_t} - F_t^{IMF} \frac{GDP_{t+1}}{GDP_t} = \beta_{1,value} \quad (12)$$

$$+ \alpha_{2,value} (China_t - China_{t|t})$$

$$+ \beta_{3,value} China_{t|t} + \varepsilon_{t+1,value}$$

$$(b) \frac{pGDP_t}{pGDP_{t+1}} - F_t^{IMF} \frac{pGDP_t}{pGDP_{t+1}} = \beta_{1,price}$$

$$+ \alpha_{2,price} (China_t - China_{t|t})$$

$$+ \beta_{3,price} China_{t|t} + \varepsilon_{t+1,price}$$

where  $\alpha, \beta$  are estimated parameters and  $\varepsilon$  are residuals. Standard linear regression yields estimates

on how Chinese economic growth impacts changes in nominal GDP growth rates  $\left( E \left[ \frac{\partial \left( \frac{GDP_{t+1}}{GDP_t} \right)}{\partial China_t} \right] =$

$\hat{\alpha}_{2,value}$ ) and GDP price inflation  $\left( E \left[ \frac{\partial \left( \frac{pGDP_t}{pGDP_{t+1}} \right)}{\partial China_t} \right] = \hat{\alpha}_{2,price} \right)$  in spillover target countries.

In this manner, we obtain estimates of the ‘price effect’

$$\left[ \frac{\partial \left( \frac{pGDP_t}{pGDP_{t+1}} \right)}{\partial China_t} \right] E \left[ \frac{GDP_{t+1}}{GDP_t} \right] = \widehat{\alpha_{2,price}} E \left[ \frac{GDP_{t+1}}{GDP_t} \right] \quad \text{and} \quad \text{‘value effect’} \quad E \left[ \frac{\partial \left( \frac{GDP_{t+1}}{GDP_t} \right)}{\partial China_t} \right] E \left[ \frac{pGDP_t}{pGDP_{t+1}} \right] =$$

$\alpha_{2,value} E \left[ \frac{pGDP_t}{pGDP_{t+1}} \right]$ . The covariance terms in (11) remain unknown. Their combined effect is computed as residual from (10).

Implementing this empirical strategy yields the main result that, on the global scale, the price effect is negative accounting for a large proportion of total spillover (Table 4). In the benchmark model, the price effect is dominant in the sense that it is larger in absolute terms than the sum of the value effect and the combined effect. In the alternative model, where we use the late data revision as the shock variable, the price effect is larger in absolute terms than the value effect, but not as large as the combined effect.

Based on the two models, we conclude that an acceleration in real GDP growth in China spills over to other countries by increasing their GDP price inflation one year ahead. The increase in the price deflator exerts downward pressure on the real GDP growth rates in these countries. The estimations furthermore indicate a negative value effect, reflecting a change in economic activity in the spillover target countries in the aftermath of a GDP shock from China.

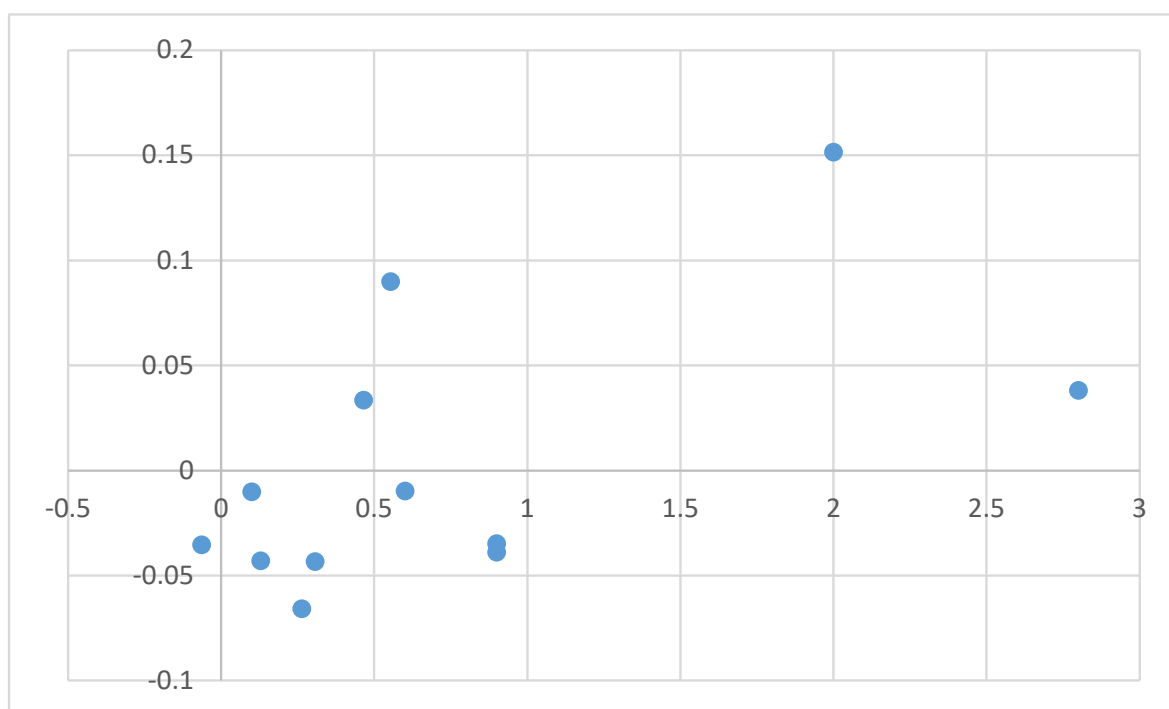
Table 4 Spillover channels

	$\hat{\alpha}_2$	Value effect	Price effect	Combined effect
Benchmark	-0.09	-0.018	-0.045	-0.041
Late revisions shock	-0.22	-0.03	-0.06	-0.11

Notes: Estimations by OLS based on Eq. 11 at country-level for 177 countries 2004–15.  
Data source: WEO.

To shed further light on the price channel, we study forecasts and real time data of commodity prices. The data exhibit a clear pattern (Figure 6) in which upward data revisions of Chinese real GDP growth are followed by a positive forecast error of global commodity prices. The correlation between the two series is close to 60 percent. Based on regression analysis with the novel approach (not shown), an increase in Chinese real GDP growth by one percent causes an increase in world commodity prices by over 4 percent one year ahead. The quantitative estimate is well in line with the recent findings by Dieppe et al (2018).

Figure 6 Data revision in China (horizontal axis) and the IMF forecast error regarding world commodity prices



Notes: Data in pp. Forecast errors and data revisions are computed as difference between 2018 and the observation year.  
Data source: WEO, April vintages.

## IV Discussion and concluding remarks

The robust result of negative spillover from China strengthens the previous findings about the negative effects of Chinese economic development on various industries and sectors in other countries. Based on our study, negative spillover is not limited to market segments such as labor markets, certain sectors, or even countries. Instead, negative spillover seems to be the norm, and not the exception, globally one to two years ahead.

So is negative spillover a China-specific phenomenon or is it applicable to other countries as well? We speculate that, at least at the time horizon used here, spillover from most countries tends to be negative. This view is based on the finding that spillover propagates largely via the price channel. Future studies will hopefully explore this issue further.

Our finding that the price channel is a significant globally, and perhaps even the main spillover channel, is unexpected as earlier discussion tended to emphasize trade and employment effects. This finding places the issue of Chinese spillover in the domain of central banking. The ongoing slowdown in China adds to the challenges of central banks (e.g. Bank of Japan, Eurosystem central banks) already struggling with low inflation. More generally, the findings emphasize the need to account for spillover in monetary policy analysis. All major central banks closely monitor international developments. The methodologies presented in this paper hopefully prove useful to quantify their impact on the domestic economy.

The novel empirical approach seems well suited to study spillover from other countries, as well as other types of spillovers and causal effects. To this end, it harnesses the long experience and special position of the IMF in the global economy. Our analysis motivates the release of real-time datasets by professional forecasters such as the IMF in order to promote understanding of the world economy.

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Table A1 Spillover estimates

Country	iso	$\alpha_2$ benchmark	Country	iso	$\alpha_2$ benchmark
Albania	ALB	0.29	Congo, Democratic Republic of	COD	-1.14
Algeria	DZA	-0.69	Congo, Republic of	COG	-1.21
Angola	AGO	-2.59	Costa Rica	CRI	0.42
Antigua and Barbuda	ATG	1.61	Côte d'Ivoire	CIV	-1.86
Argentina	ARG	0.48	Croatia	HRV	0.55
Armenia	ARM	-0.63	Cyprus	CYP	1.71
Australia	AUS	-0.03	Czech Republic	CZE	-0.16
Austria	AUT	-0.16	Denmark	DNK	-0.60
Azerbaijan	AZE	-2.94	Djibouti	DJI	-1.10
Bahamas, The	BHS	-3.30	Dominica	DMA	2.89
Bahrain	BHR	0.72	Dominican Republic	DOM	1.40
Bangladesh	BGD	-0.11	Ecuador	ECU	-1.06
Barbados	BRB	-1.18	Egypt	EGY	-0.05
Belarus	BLR	2.36	El Salvador	SLV	-0.61
Belgium	BEL	0.19	Equatorial Guinea	GNQ	4.43
Belize	BLZ	-0.03	Eritrea	ERI	-3.60
Benin	BEN	0.20	Estonia	EST	-5.22
Bhutan	BTN	0.15	Ethiopia	ETH	0.39
Bolivia	BOL	0.45	Fiji	FJI	-0.12
Bosnia and Herzegovina	BIH	0.83	Finland	FIN	0.67
Botswana	BWA	-0.15	France	FRA	-0.35
Brazil	BRA	0.68	Gabon	GAB	-0.40
Brunei Darussalam	BRN	-1.70	Gambia, The	GMB	2.24
Bulgaria	BGR	0.96	Georgia	GEO	-2.38
Burkina Faso	BFA	-1.43	Germany	DEU	-0.58
Burundi	BDI	-1.28	Ghana	GHA	0.16
Cambodia	KHM	-0.86	Greece	GRC	1.26
Cameroon	CMR	0.14	Grenada	GRD	0.16
Canada	CAN	-0.16	Guatemala	GTM	-0.52
Cape Verde	CPV	0.27	Guinea	GIN	-0.14
Central African Republic	CAF	-3.06	Guinea-Bissau	GNB	-0.46
Chad	TCD	0.79	Guyana	GUY	-0.95
Chile	CHL	-0.93	Haiti	HTI	0.26
China	CHN	0.44	Honduras	HND	-0.13
Colombia	COL	-0.62	Hong Kong SAR	HKG	-0.41
Comoros	COM	-1.40	Hungary	HUN	0.53
Iceland	ISL	2.44	Mozambique	MOZ	-0.40
India	IND	-0.30	Myanmar	MMR	-0.30
Indonesia	IDN	0.46	Namibia	NAM	-0.64
Iran, Islamic Republic of	IRN	-1.42	Nepal	NPL	0.84
Ireland	IRL	-0.16	Netherlands	NLD	0.07
Israel	ISR	-0.23	New Zealand	NZL	-0.91
Italy	ITA	-0.10	Nicaragua	NIC	-1.18
Jamaica	JAM	-1.01	Niger	NER	1.89

Country	iso	$\alpha_2$ benchmark	Country	iso	$\alpha_2$ benchmark
Japan	JPN	-0.46	Nigeria	NGA	0.57
Jordan	JOR	0.65	Norway	NOR	-0.11
Kazakhstan	KAZ	-1.21	Oman	OMN	1.01
Kenya	KEN	-0.86	Pakistan	PAK	-0.77
Kiribati	KIR	-1.13	Panama	PAN	-0.97
Korea	KOR	-0.02	Papua New Guinea	PNG	1.82
Kuwait	KWT	-3.48	Paraguay	PRY	1.70
Kyrgyz Republic	KGZ	2.10	Peru	PER	1.20
Lao People's Democratic Republic	LAO	-0.07	Philippines	PHL	-0.23
Latvia	LVA	-4.18	Poland	POL	-0.29
Lebanon	LBN	4.08	Portugal	PRT	0.14
Lesotho	LSO	-1.60	Qatar	QAT	3.14
Libya	LBY	-19.38	Romania	ROU	1.59
Lithuania	LTU	-1.27	Russia	RUS	-0.28
Luxembourg	LUX	0.24	Rwanda	RWA	0.26
Macedonia, Former Yugoslav Republic of	MKD	1.16	Samoa	WSM	-1.97
Madagascar	MDG	-0.03	São Tomé and Príncipe	STP	-0.78
Malawi	MWI	2.59	Saudi Arabia	SAU	-0.06
Malaysia	MYS	0.27	Senegal	SEN	0.41
Maldives	MDV	2.26	Seychelles	SYC	-3.93
Mali	MLI	1.52	Sierra Leone	SLE	2.50
Malta	MLT	1.93	Singapore	SGP	-0.93
Mauritania	MRT	-3.18	Slovak Republic	SVK	0.68
Mauritius	MUS	-0.09	Slovenia	SVN	1.35
Mexico	MEX	0.11	Solomon Islands	SLB	-1.39
Moldova	MDA	0.26	South Africa	ZAF	-0.06
Mongolia	MNG	-2.42	Spain	ESP	0.69
Morocco	MAR	-0.63	Sri Lanka	LKA	-1.57
St. Kitts and Nevis	KNA	2.06	Tunisia	TUN	0.30
St. Lucia	LCA	1.80	Turkey	TUR	-3.22
St. Vincent and the Grenadines	VCT	-1.64	Turkmenistan	TKM	-0.79
Sudan	SDN	2.78	Uganda	UGA	0.49
Suriname	SUR	-1.43	Ukraine	UKR	0.24
Swaziland	SWZ	-1.27	United Arab Emirates	ARE	-2.64
Sweden	SWE	0.04	United Kingdom	GBR	-0.83
Switzerland	CHE	0.57	United States	USA	0.01
Syrian Arab Republic	SYR	-1.07	Uruguay	URY	0.51
Taiwan Province of China	TWN	-0.09	Uzbekistan	UZB	0.28
Tajikistan	TJK	1.10	Vanuatu	VUT	1.76
Tanzania	TZA	-0.57	Venezuela	VEN	-0.56
Thailand	THA	-0.52	Vietnam	VNM	-0.38
Timor–Leste, Dem. Rep. of	TLS	-1.20	Yemen, Republic of	YEM	2.08
Togo	TGO	-1.64	Zambia	ZMB	0.34
Tonga	TON	1.75	Zimbabwe	ZWE	-7.22
Trinidad and Tobago	TTO	-0.20			

Notes: Estimations based on Model 1 (Table 2). Data source own calculations.

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