IWH
Halle Institute for Economic Research
Member of the Leibniz Association

Discussion Papers

No. 25
December 2020

On the International Dissemination of Technology News Shocks

João C. Claudio, Gregor von Schweinitz

Authors

João C. Claudio
Halle Institute for Economic Research (IWH) – Member of the Leibniz Association, Department of Macroeconomics, and Leipzig University
E-mail: JoaoCarlos.Claudio@iwh-halle.de
Tel +49 345 7753 880

Gregor von Schweinitz
Halle Institute for Economic Research (IWH) – Member of the Leibniz Association, Department of Macroeconomics, and Leipzig University, Institute for Theoretical Economics
E-mail: gregorvon.schweinitz@iwh-halle.de
Tel +49 345 7753 744

The responsibility for discussion papers lies solely with the individual authors. The views expressed herein do not necessarily represent those of IWH. The papers represent preliminary work and are circulated to encourage discussion with the authors. Citation of the discussion papers should account for their provisional character; a revised version may be available directly from the authors.

Comments and suggestions on the methods and results presented are welcome.

IWH Discussion Papers are indexed in RePEc-EconPapers and in ECONIS.

Editor

Halle Institute for Economic Research (IWH) – Member of the Leibniz Association

Address: Kleine Maerkerstrasse 8
D-06108 Halle (Saale), Germany
Postal Address: P.O. Box 11 03 61
D-06017 Halle (Saale), Germany

Tel +49 345 7753 60
Fax +49 345 7753 820

www.iwh-halle.de

ISSN 2194-2188

On the International Dissemination of Technology News Shocks

Abstract

This paper investigates the propagation of technology news shocks within and across industrialised economies. We construct quarterly utilisation-adjusted total factor productivity (TFP) for thirteen OECD countries. Based on country-specific structural vector autoregressions (VARs), we document that (i) the identified technology news shocks induce a quite homogeneous response pattern of key macroeconomic variables *in each country;* and (ii) the identified technology news shock processes display a significant degree of correlation *across several countries*. Contrary to conventional wisdom, we find that the US are only one of many different sources of technological innovations diffusing across advanced economies. Technology news propagate through the endogenous reaction of monetary policy and via trade-related variables. That is, our results imply that financial markets and trade are key channels for the dissemination of technology.

Keywords: technology news shocks, technology spillover, structural VAR, international trade

JEL classification: E24, E32, F41

1. Introduction

The seminal contribution by Beaudry and Portier (2006) presents striking empirical evidence that changes in expectations about future production possibilities (i.e., through innovations in technology) can impact current US macroeconomic outcomes. Since then, several papers have further emphasized the cross-border effects of the so-called technology news shocks in the sense that technology might not only disseminate within a country, but also spillover to a neighboring country or region. However, the majority of these papers have focused on the spillover of US shocks to other countries.[1]

In this paper, we provide an international perspective on the propagation of technology news shocks within and across industrialized economies, with three contributions. *First*, we go beyond the usual analysis of technology news shocks stemming from one single country and the specific focus on spillovers in country-pair relationships. We identify technology news shocks in thirteen OECD countries. In order to do that, we construct a quarterly total factor productivity (TFP) measure adjusted for factor utilization for each country. *Second*, we demonstrate that technology news shocks generate a quite homogeneous response pattern of key macroeconomic variables *in each country*. We find that this pattern is in line with the literature that emphasizes permanent effects of technology news shocks rather than news-driven business cycles. *Third*, we find a significant degree of correlation between the identified technology news shocks *across several countries*. Most surprisingly, we find that the US are not the major source of technological innovations disseminating to other advanced economies. For instance, Germany also seems to play an important role in the diffusion process of technology.

Beyond these key contributions, we examine the underlying transmission channels through which anticipated technology shocks operate.[2] We find these shocks to induce disinflationary pressures that are immediately counteracted by an endogenous monetary policy response. This in turn leads, in the majority of the countries, to an immediate reaction on financial markets (Kurmann and Otrok, 2013; Kamber et al., 2017; Görtz et al., 2020). A second transmission channel can be found in the reaction of trade-related variables like trade openness and (total) trade in goods and services. Technology news shocks do not only lead to a higher level of (total) trade, but they also increase the degree of openness of the economies considered in our analysis. They explain about 50% of the forecast error variance of these two variables at longer horizons. These findings further strengthen the view that financial markets and trade are key channels through which technological innovations disseminate (Eaton and Kortum, 1996).

As far as the notion of news-driven business cycles is concerned, Beaudry and Portier (2006) show that technology news shocks induce a positive impact reaction and co-movement of stock prices, consumption, investment and hours worked. Even more interestingly, the authors find such shocks to be the major drivers of post WWII U.S. business cycles and therefore attribute a central role to supply-side disturbances as key determinants of short-run US macroeconomic fluctuations. Nevertheless, those findings have not remained unquestioned. A series of empirical contributions over the last years have shown that (i) while stock prices and consumption respond positively to an anticipated technology shock, investment and hours worked actually drop due to possibly more pronounced wealth effects and (ii) the role played by these shocks is actually emphasized only at medium-term frequencies, and not at business cycle frequencies (Barsky and Sims, 2011; Barsky et al., 2014; Forni et al., 2014; Kurmann and Sims, 2020; Miyamoto and

[1] See for example (Beaudry et al., 2011; Kosaka, 2013; Nam and Wang, 2014, 2015, 2018; Levchenko and Pandalai-Nayar, 2020).

[2] The terms "technology news shocks" and "anticipated technology shocks" and "productivity news shocks" are used interchangeably. Such shocks encompass not only news about future technological improvements, but also reflect a slow dissemination process of specific innovations within technology and investment communities (Rotemberg, 2003; Mansfield, 1989).

Nguyen, 2020).[3]

We document that the above-mentioned empirical findings actually hold not only in the US, but in a much larger set of advanced economies. More specifically, our results suggest that technology news shocks play a crucial role in driving macroeconomic variables internationally at the medium-run, but, with very few exceptions, less influence is observed on business cycle fluctuations (Forni et al., 2014). In most countries, we find positive impact reactions in stock prices and consumption, while investment and hours worked respond indeed negatively on impact. These results are at odds with Kamber et al. (2017), who not only find technology news to induce a positive reaction and co-movement of key macroeconomic variables in Australia, Canada, New Zealand and UK, but also to be the major driver of business cycle fluctuations in these countries. In this respect, a key distinction has to be made in terms of the underlying identification strategies. While Kamber et al. (2017) impose sign-restrictions on their VAR model based on a theoretically motivated co-movement of macroeconomic aggregates in small open economies, our identification procedure builds on the forecast error variance maximization originally proposed for the identification of technology shocks (Uhlig, 2003, 2004; Francis et al., 2014) and further implemented in the technology news literature (Barsky and Sims, 2011; Kurmann and Otrok, 2013; Barsky et al., 2014; Nam and Wang, 2015; Kurmann and Sims, 2020).

Independent of the identification approach at hand, a crucial step to recover technology news shocks is the choice of a productivity measure. Labor productivity has long been employed in the literature on technology shocks, because it is easily computable and (annual) TFP measures adjusted for factor utilization first became available for the US through the work by Basu et al. (2006). The recent literature on technology (news) shocks in the US has employed the quarterly utilization-adjusted TFP constructed for the US business sector and regularly updated by Fernald (2014), because theoretical models show that capacity utilization reacts to technology news shocks on impact (Jaimovich and Rebelo, 2009). Therefore, TFP measures not adjusted for factor utilization tend to react contemporaneously to such shocks as well. Nevertheless, to our knowledge no comparable measures exist for the other OECD countries in our sample.[4] For this reason, we construct the Solow residual based a Cobb-Douglas production function. We use data on hours worked to control for labor effort. In addition, we control for the utilization of the capital stock following the approach suggested by Imbs (1999). By considering total hours worked and utilization of the capital stock in the economy, we are able to *partially* clean the initially computed Solow residual by removing possible labor and capital hording decisions of firms.[5] For comparability, we also compute labor productivity as output divided by the total number of hours worked in the economy, i.e., we consider both the extensive and intensive margins of labor (Ohanian and Raffo, 2012). The rationale for computing two productivity measures relates to two potential caveats intrinsic to each series. Utilization-adjusted TFP measures are, on the one hand, conceptually preferable to labor productivity, because the latter can also be affected by long-run trends like permanent changes in capital tax rates and population, which are orthogonal to technological progress (Uhlig, 2004; Francis and Ramey, 2005, 2009). In addition, labor productivity reflects not only changes in technology, but also changes in the input factors (Chang and Hong, 2006). On the other hand, the utilization-adjusted TFP measures computed by Fernald (2014) are vulnerable to measurement errors (and thus revisions), which can in principle

[3]Ben Zeev and Khan (2015) contribute to this debate by further demonstrating that investment-specific technology news shocks (i.e., anticipated changes in technology embodied in capital goods) rather than neutral technology news shocks are responsible for driving U.S. business cycles.

[4]The only exception is Kamber et al. (2017), who provide quarterly utilization-adjusted TFP measures for Australia, Canada, New Zealand and the UK from 1989 onwards. Both in terms of cross-sectional and time coverage, this sample is too small for our analysis. In a recent work, Levchenko and Pandalai-Nayar (2020) also compute a new quarterly utilization-adjusted TFP measure for Canada starting in the end of the 60's.

[5]As we do not control for imperfect competition and non-constant returns due to lack of existing data, our technology measures are not entirely purified in the sense of Basu et al. (2006).

also affect the identification of technology news shocks (Cascaldi-Garcia, 2017; Kurmann and Otrok, 2017; Kurmann and Sims, 2020). In terms of the main (technology) sources driving both measures, labor productivity captures both neutral and investment-specific technological innovations, while TFP encompasses by construction only neutral technology (Fisher, 2006; Ben Zeev and Khan, 2015). Despite these concerns, we show that the identified technology news shocks are not only nearly identical in our US model once the most recent vintage of utilization-adjusted TFP by Fernald (2014) is replaced either by our utilization-adjusted TFP measure or by labor productivity, but also in all the other countries considered. Thus, the relevancy of the above shortcomings seems to depend on the exact research question.

The remainder of the paper is organized as follows. Section 2 presents the data and methodology employed in our analysis. Section 3 provides a discussion of our results based on the baseline and alternative specifications. Section 4 concludes.

2. Data and methodology

2.1. Data

As our baseline, we estimate medium-sized country-specific VARs with nine variables for thirteen OECD countries. The variable selection is based on the current standard in the literature on technology news shocks (Barsky and Sims, 2011; Kurmann and Otrok, 2013; Kurmann and Sims, 2020). Our main variable of interest is either *utilization-adjusted TFP* or *labor productivity*, which we construct as described in subsection 2.2. Beyond this core variable, we consider log real consumption per capita, log real stock prices, inflation, a three-month interest rate, the slope of the term structure of interest rates, log real investment per capita and total hours worked per capita. Since we aim to identify structural shocks in many small open economies, trade-related variables may be extremely relevant. Therefore, we add the current-account balance as our ninth variable. All data are collected from the OECD main economic indicators, the OECD world economic outlook, Bureau of Labor Statistics (BLS) and Bank for International Settlements (BIS). The annual data used in the construction of quarterly TFP come from the Penn World Table (Feenstra et al., 2015). Data and the sample periods per country are reported in Tables 4 and 5 in the Appendix.

We consider thirteen OECD countries, namely Australia, Austria, Canada, Finland, France, Germany, Italy, Japan, Sweden, South Korea, Switzerland, United Kingdom and the United States. The restriction to this set of countries is necessary, because our identification scheme, as explained later, relies on the forecast error variance decomposition up to a horizon of twenty years (eighty quarters). In an analogy to Uhlig (2003), such an identification is only convincing if estimation samples are much longer than this horizon. This is not fulfilled for the remaining OECD countries, where total hours worked are either not available or are interpolated from yearly data in the earlier part of the sample.[6] Such an interpolation is problematic since it carries over to the constructed quarterly utilization-adjusted TFP. In such a case, it distorts the identified structural shocks.

2.2. Technology measures: TFP adjusted for factor utilization and labor productivity

Internationally, there are severe limitations to the availability of official productivity measures. At a quarterly frequency, the OECD provides data on labor productivity, measured as output per employee. This measure is problematic because it disregards the cross-sectional variation as well as the downward trend in hours worked per employee. Total factor productivity is only available at a yearly frequency, which does not provide enough observations for the estimation of medium-sized VARs. Moreover, these data are not adjusted for factor utilization.

[6]Interpolation of total hours worked affects Belgium, Denmark, the Netherlands and Norway. The remaining OECD countries do not have the long data availability necessary for our identification approach.

As outlined in the introduction, we follow the most recent US-literature in our measure of productivity (technology). We construct a quarterly utilization-adjusted TFP measure as a simplified version (due to lower availability of public data in an international context) of the US-bellwether series of Fernald (2014).

We use growth accounting to compute utilization-adjusted total factor productivity, A_t, as the Solow residual from a Cobb-Douglas production function (Basu et al., 2006; Fernald, 2014):

$$\log A_t = \log Y_t - \alpha \log (U_t K_t) - \beta \log (E_t L_t) .$$

Labor input is measured as the product of total employment L_t and labor effort E_t (hours worked per employee). The capital stock K_t is similarly augmented by capital utilization U_t. Data on the real capital stock are available at a yearly frequency. As in Kamber et al. (2017), we distribute the yearly change of the capital stock on quarters using proportions of quarterly real investment. We compute quarterly capacity utilization using the deviation of the output-capital ratio from its long-run average (Imbs, 1999; Levchenko and Pandalai-Nayar, 2020):

$$U_t = \left(\frac{Y_t / K_t}{Y/K} \right)^{\frac{\delta}{\delta + r}} .$$

Long-run averages of depreciation rates, real interest rates and the output-capital ratio are denoted by δ, r and Y/K, respectively. The labor shares α is the long-run average of yearly labor shares.

An alternative productivity measure could be labor productivity as the log of real gross domestic product (GDP) per total hours worked (Ohanian and Raffo, 2012). This measure accounts for a changes in hours worked per employee in general and part-time jobs in particular, which gained relevance over time. It is therefore superior than the simpler OECD definition (log of real GDP per employee). In section 3, we argue why we should prefer utilization-adjusted TFP over these two measures of labor productivity.

We provide comparisons to two alternative data sources to show the high quality of our TFP measures. First, AMECO provides official unadjusted TFP measures at annual frequencies for all countries in our sample. Figure 7 in the Appendix compares the growth rates of AMECO TFP data to growth rates from our annualized TFP data with and without accounting for capacity utilization and labor effort. It is evident that growth rates of unadjusted TFP measures are very close. In addition, we see that controlling for factor utilization affects TFP growth rates, even on an annualized basis. This indicates that it is important to adjust for time-variation in capacity utilization and labor effort given that both fluctuate over the business cycle. Second, we compare in Figure 8 our quarterly measure of utilization-adjusted and unadjusted TFP (growth rates) for the US with the respective series computed by Fernald (2014). Our unadjusted TFP measure for the US has a correlation of 87% with Fernald's unadjusted series. A key distinction between the two is that we construct a measure for the whole economy, whereas Fernald's measure considers the US business sector only. For the adjusted series, the correlation drops to 55%. The reason is that Fernald uses much more granular data to measure capital utilization U_t than (publicly) available for other countries. However, we document later that the difference in the measure of adjusted TFP does not affect the identified technology news shocks strongly. These shocks are much closer to one another than the different adjusted TFP measures themselves. This is in line with Fernald and Wang (2016), who state that short-run variations in factor utilization do not reflect changes in technology, even if they lead to changes in measurable TFP. In other words, even if we do not control for factor utilization as well as Fernald does, the observed differences in the growth rates of both utilization-adjusted TFP series seem to be reflected in other shocks such as surprise shocks or measurement errors rather than the TFP news shocks we are interested in, see equation (1).

2.3. Stock prices and macroeconomic aggregates

Technology news shocks are observed by economic agents in advance, i.e., before they materialize in measurable increases of TFP. This induces agents to adapt their behavior in anticipation of these future developments. Therefore, we need sufficient information in our VAR (Forni and Gambetti, 2014; Forni et al., 2014; Beaudry et al., 2015). To achieve that, we include forward-looking variables that capture the contemporaneous reactions of economic agents, such as *real stock prices*, *real consumption* and *real investment* (Beaudry and Portier, 2006, 2014; Miyamoto and Nguyen, 2020). Beyond these, we add *total hours worked* and the *current account balance.*

The reaction of total hours worked to technology shocks has been hotly debated in the macroeconomic literature because the resulting impact response allows one to differentiate between the predictions of the canonical Real Business Cycle (RBC) and New Keynesian (NK) models.[7] Francis and Ramey (2005, 2009) show that it is important to control for long-run trends of total hours worked in order to correctly infer the response to technology shocks. Like the US series (which is clearly U-shaped), hours worked constructed by Ohanian and Raffo (2012) for the countries in our sample clearly exhibit trends that must be controlled for. Therefore, we detrend all total hours worked-series using a Hamilton-filter with a forecast horizon of eight quarters, thus keeping all fluctuations over the business cycle (Hamilton, 2018).[8]

The current account balance is included to capture trade and financial relations with the rest of the world, which are important for many of the (small) open economies in our sample (Beaudry and Portier, 2014; Arezki et al., 2017). Moreover, the current account is essentially forward-looking as it captures intertemporal consumption and investment decisions taken in an international setting. In alternative specifications, we replace the current account by other trade-related measures, namely the degree of trade openness, terms of trade, total trade, trade balance and the real effective exchange rate.

2.4. Inflation, monetary policy and interest-rate spread

Kurmann and Otrok (2013) document that disinflationary pressures induced by a positive technology news shock endogenously leads to an expansionary monetary policy response. That is, interest rates at the short end of the yield curve drop and the term spread increases. To capture this channel, we include inflation, three-month money market rates and the term spread between 10-year government bond yields and three-month money market rates. We use money market rates instead of policy rates because the official policy instruments vary across countries and time in our sample. Thus, money-market rates are internationally more comparable and potentially of higher relevancy than, for example, central bank policy rates.

2.5. Estimation and identification

We follow the bulk of the literature on technology news shocks in our estimation and identification procedures. That is, we estimate a separate medium-scale VAR for every country in a first step, as explained in the following. In a second step, we identify technology news shocks as the shocks that (a) have the maximum contribution to the forecast error variance of productivity at a given future horizon while (b) having no impact contemporaneously (Barsky and Sims, 2011; Barsky et al., 2014).

As far as the assumptions regarding the identification of technology news shocks are concerned, the majority of the literature models technological progress as a process with two features. First, only technology shocks have medium- to long-run effects on technological progress.

[7]For an overview of the debate initiated by Galí (1999) about the impact of technology shocks on hours worked as originally proposed by Kydland and Prescott (1982), we refer to the contributions by Christiano et al. (2003), Galí and Rabanal (2004), Uhlig (2004), Francis and Ramey (2005), Fisher (2006), Fernald (2007), Canova et al. (2010) and Francis et al. (2014).

[8]Results are robust to using an HP-filter with $\lambda = 1'600$.

Second, these shocks are characterized by having anticipated and unanticipated properties. That is, we can model technological progress A_t as a stochastic process that is driven by past technology *news* shocks u_t^a and contemporaneous technology *surprise* shocks u_t^s (Barsky and Sims, 2011):

$$\log A_t = \log A_{t-1} + u_t^s + u_{t-j}^a + v_t \tag{1}$$

Such a simplified model of technological progress abstracts away from other sources of short-run fluctuations, which are captured by a measurement error v_t in equation (1). However, we follow the literature and assume that such short-run fluctuations do not have permanent effects (Barsky and Sims, 2011). Put differently, we assume that technology news and surprise shocks are the overwhelming drivers of technology in the medium- to long-run. This assumption has implications for the identification and interpretation of the two different shocks. First, news shocks (which only have an effect at later horizons) are unaffected by short-run fluctuations.[9] Second, surprise shocks likely capture measurement errors, and are therefore not economically interpretable.

The full structural model is

$$\mathbf{A}\mathbf{y}_t = \mathbf{B}\mathbf{x}_{t-1} + \mathbf{u}_t, \quad \mathbf{u}_t \sim \mathcal{N}(0, \mathbf{I}). \tag{2}$$

The $(n \times 1)$ vector $\mathbf{y}_t$ contains contemporaneous variables, while $\mathbf{x}_{t-1} = [\mathbf{y}_{t-1} \cdots \mathbf{y}_{t-p} 1]'$ is a $(k \times 1)$ vector of lagged observations (up to lag $p = 6$) and a constant. Endogenous variables are measured in the transformation given in Table 4. The matrices $\mathbf{A}$ and $\mathbf{B}$ contain the structural contemporaneous and lag coefficients of the model. Structural errors $\mathbf{u}_t$ are independent and standard-normally distributed.

Among the structural errors, we are only interested in a technology news shock. We order this shock first, i.e., we denote it as $u_{1,t}$. That is, we need to identify the first row of $\mathbf{A}$ or, equivalently, the first column of $\mathbf{A}^{-1} := \tilde{\mathbf{a}}_1$. In order to identify technology news shocks, we proceed in two steps. First, we make use of the fact that we can estimate the corresponding reduced-form model of the VAR using standard Minnesota priors as in Hamilton (1994).We assume that our variables follow an AR process with autocorrelation parameter one on the first lag and zero on all later lags for non-stationary series (technology measures, consumption, investment, stock prices, total trade and real openness). All other series have a prior of zero on all lags. Parameters for the prior standard deviation are standard. The reduced-form model (3), including its link to the structural model in equation (2), is:

$$\mathbf{y}_t = \mathbf{\Phi}\mathbf{x}_{t-1} + \varepsilon_t, \quad \mathbf{u}_t \sim \mathcal{N}(0, \mathbf{\Sigma}) \tag{3}$$

$$\mathbf{\Phi} = \mathbf{A}^{-1}\mathbf{B}$$

$$\varepsilon_t = \mathbf{A}^{-1}\mathbf{u}_t$$

$$\mathbf{\Sigma} = \mathbf{\Sigma}_{tr}\left(\mathbf{\Sigma}_{tr}\right)' = \mathbf{\Sigma}_{tr}\left(\mathbf{Q}\mathbf{Q}'\right)\left(\mathbf{\Sigma}_{tr}\right)' = \mathbf{A}^{-1}\left(\mathbf{A}^{-1}\right)'$$

$$\tilde{\mathbf{a}}_1 = \mathbf{\Sigma}_{tr}\mathbf{q}_1. \tag{4}$$

The last equation (4) states that identification of $\tilde{\mathbf{a}}_1$ (the impact effect of technology news shocks) is equivalent to identifying a rotation vector $\mathbf{q}_1$. That is, we search for a rotation vector $\mathbf{q}_1$ that transforms reduced-form errors ε_t into economically meaningful structural shocks $u_{1,t}$. As said above, we have two identification assumptions: First, we assume that technology news

[9] In order for this statement to be correct, it is crucial to identify technology news shocks *at a specific horizon in the future* (Kurmann and Sims, 2020; Francis et al., 2014), instead of the initialy proposed truncation horizon, e.g., an "interval" between 0 and 40 quarters (Barsky and Sims, 2011). For a more detailed discussion of this issue, we refer to the work by Dieppe et al. (2019).

shocks explain a majority of productivity movements at a medium- to long horizon h. In our empirical application, we will work with $h = 80$ quarters and – in a robustness check – with $h = 40$ quarters. This horizon is chosen such that non-permanent shocks should have little or no effect anymore. Second, we assume that technology news shocks do not have an immediate impact on our productivity measure.[10]

These two identifying assumptions can be operationalized using the decomposition of the forecast-error variance of productivity (which we order first in the VAR) at horizon h, $\Omega_1(h) := (y_{1,t+h} - E_t(y_{1,t+h}))^2$. The contribution of the technology news shock to $\Omega_1(h)$ is given by

$$\Omega_{1,1}(h) = \mathbf{e}_1' \left(\sum_{l=0}^{h-1} \mathbf{\Psi}_l \mathbf{\Sigma}_{tr} \mathbf{q}_1 \mathbf{q}_1' \mathbf{\Sigma}_{tr}' \mathbf{\Psi}_l' \right) \mathbf{e}_1, \tag{5}$$

where $\mathbf{\Psi}_l$ is the response matrix of $\mathbf{y}_{t+l}$ to reduced-form errors ε_t, and $\mathbf{e}_1$ is a $n \times 1$ vector of zeros with one in the first position. The second assumption implies that the first element of $\mathbf{q}_1$ is zero (Barsky and Sims, 2011). The first assumption implies that we search for the vector $\mathbf{q}_1$ that maximizes $\Omega_{1,1}(h)$ in equation (5). For normalization and comparability across countries, we further scale technology news such that they increase productivity by 1 percentage point at horizon $h = 40$.[11]

The literature proposes at least three alternative identification strategies. First, as an alternative to choosing a fixed horizon h, one could maximize the joint forecast error variance contribution of two structural shocks between horizons $\underline{h} = 0$ and $\bar{h} = 40$ (Barsky and Sims, 2011; Kurmann and Otrok, 2013). Both shocks are orthogonalized is such a way that surprise technology shocks are allowed to impact productivity contemporaneously, while technology news shocks materialize only with a delay. Nevertheless, variations at shorter horizons may be affected by other shocks than technology news and surprise shocks, sowing doubts in the interpretability of technology "surprise" shocks. This argument has in particular been made with respect to transitory measurement errors in TFP (Kurmann and Sims, 2020), but it is also relevant for labor productivity (Benati, 2007).[12] In our case, we see from a comparison of different utilization-adjusted TFP measures in the US that the series themselves have a much lower correlation than technology news shocks identified from these series. This indicates to us that our TFP measure allows only to identify technology news shocks. A second alternative identification strategy originating from the early literature on technology shocks implies long-run zero restrictions under the assumption that technology shocks are the only shocks exerting a permanent impact on productivity in the long-run (Galí, 1999). However, this identification suffers from (a) estimation bias existing in finite samples (Faust and Leeper, 1997; Uhlig, 2003) and (b) an unknown number of cointegrating relationships (Beaudry and Lucke, 2010; Fisher, 2010). Medium-run restrictions like the ones employed in this paper do not suffer from these deficiencies. A third alternative would be to use external instruments for shock identification. Examples are publications of scientific books or patents (Alexopoulos, 2011; Miranda-Agrippino et al., 2020). However, this strategy does not provide a good instrument for country-specific technology news shocks, as book and patent markets cannot be considered "closed" in the small, open economies of our sample.

[10] Kurmann and Sims (2020) document that this assumption is not needed to get meaningful results for US data. However, as discussed below, data from other countries require this additional identifying assumption.

[11] We choose 40 quarters for normalization – and limit our plots to this horizon – despite the fact that we use a 80-quarter horizon for identification in our baseline model. This choice allows a better visual comparison of our results to the majority of the existing literature.

[12] For a more detailed discussion on this issue, we refer to the work by Nam (2016).

3. Results

Our baseline results come from a medium-sized VAR with nine variables, namely a productivity measure, real consumption per capita, inflation computed as the log-difference of the GDP deflator, real stock prices, three-month interest rates, the term-spread between ten-year bond yields and the three-month interest rates, the current account (% GDP), real investment per capita and total hours per capita. For every country, we use the longest possible time series as reported in Table 5. Table 6 shows the variable selection for two submodels we use for comparisons (the baseline specifications of Kurmann and Otrok (2013) and Kurmann and Sims (2020), respectively. In addition, we perform robustness analyses where we replace the current account by alternative trade-related variables.

3.1. Comparison of different technology measures in the US

In this section we aim to show that our productivity measure for the US comes close to the series by Fernald (2014) in terms of identified technology news shocks. To do this, we compare identified technology news shocks from a model with Fernald's TFP measure to four alternative models with the following technology measures: First, adjusted total factor productivity as described in section 2.2. Second, an unadjusted measure of total factor productivity based on the same growth accounting framework, but without the removal of labor effort and capacity utilization. Third, labor productivity computed as output per total hours worked (OR) as in Ohanian and Raffo (2012). Fourth, labor productivity computed as output per employee, following the OECD definition (OECD).

Table 1 shows the correlation of the four data series in the US, and the correlation of productivity news shocks identified from separate VAR estimations. In the data, our core measure (utilization-adjusted TFP), has the second highest correlation to Fernald's measure. For our paper, the more interesting result comes from the next three columns. There we show the correlation of productivity news shocks identified in the three different VAR models described above, using the alternative productivity measures to replace Fernald's utilization-adjusted TFP series. The correlation of news shocks is in general much higher than the correlation of the underlying data. Moreover, technology news shocks based utilization-adjusted TFP have the highest correlations (nearly 90%) with technology news processes based on the Fernald data. Last, we see correlations drop in VAR models with more variables. Notable, the decrease in correlation is least strong for the utilization-adjusted TFP measure. As we need larger models needed in an international context to avoid problems of insufficient information (see the following subsection), this speaks again in favor of utilization-adjusted TFP.

Table 1: Correlation of US productivity news shocks

	Data	VAR (KO)	VAR (KS)	VAR (Baseline)
Labour prod (OR)	62.63%	87.31%	88.60%	83.71%
Labour prod (OECD)	38.26%	77.61%	80.67%	72.34%
TFP unadj	34.42%	87.40%	87.66%	80.95%
TFP adj	55.49%	88.91%	88.88%	87.97%
TFP adj (Fernald)	100%	100%	100%	100%

Note: The numbers show the correlation of different productivity measures (rows) to the **last** vintage of utilization-adjusted TFP by Fernald (2014). In columns we report the correlation of quarter-on-quarter growth rates (Data) as well as news shocks from the smaller VAR models based on Kurmann and Otrok (2013), Kurmann and Sims (2020) and a medium-sized VAR with nine variables.

Thus, the results show that the labor productivity measure of the OECD and an unadjusted TFP measure are not suitable for our analysis. As to the other two measures, it seems that the adjusted TFP measure is more robustly related to the measure by Fernald, at least for the purpose of identifying productivity news shocks. There are two further arguments for using adjusted TFP over the labor productivity by OR. First, TFP is theoretically more appealing because it encompasses by construction neutral technology and is therefore not affected by investment-specific technology shocks. Second, we can base our identification on a zero impact reaction of productivity to a news shock (Barsky and Sims, 2011). While it may not be necessary in the US, if identification is based on a longer horizon (Kurmann and Sims, 2020), we need the zero restriction in other countries, as documented in subsection 3.2.3 below. However, a zero impact reaction to news shocks is only reasonable for (utilization-adjusted) TFP, while GDP and hours worked – the ingredients of the labor productivity measure – both could (and potentially should) react on impact. With respect to the comparison of adjusted and unadjusted TFP, Jaimovich and Rebelo (2009) show theoretically that capacity utilization reacts to technology news. Therefore, the use of a technology measure not adjusted by factor utilization invalidates the zero restriction as well.

3.2. International technology news shocks

3.2.1. Results for the baseline VAR

For the US, our baseline VAR produces results that are similar to those in the literature, see the impulse-response functions in Figure 1. Utilization-adjusted TFP increases to a permanently higher steady state after the shock. Real consumption increases to a new permanent level, but also shows a small positive impact effect (in line with the permanent-income hypothesis). There is a positive medium-run effect on total hours worked as well as a potentially small negative impact effect (Kurmann and Sims, 2020) due to the trade-off between the marginal utility of consumption and the marginal disutility of labor. Real investment drops on impact (probably due to expectations of better technologies), but then increases to a permanently higher level as capital becomes more productive. Real stock prices increase on impact in expectations of future higher profits, and decline afterwards in line with the efficient markets hypothesis. Prices fall on impact, potentially due to price-setting frictions. This leads to an endogenous reaction of monetary policy, pushing down short-run interest rates to counteract the disinflationary impulse and therefore increasing the term spread. The current account does not react on impact, but then falls to a permanently lower level. This may be due to the response of consumption, which is stronger than in most other countries.

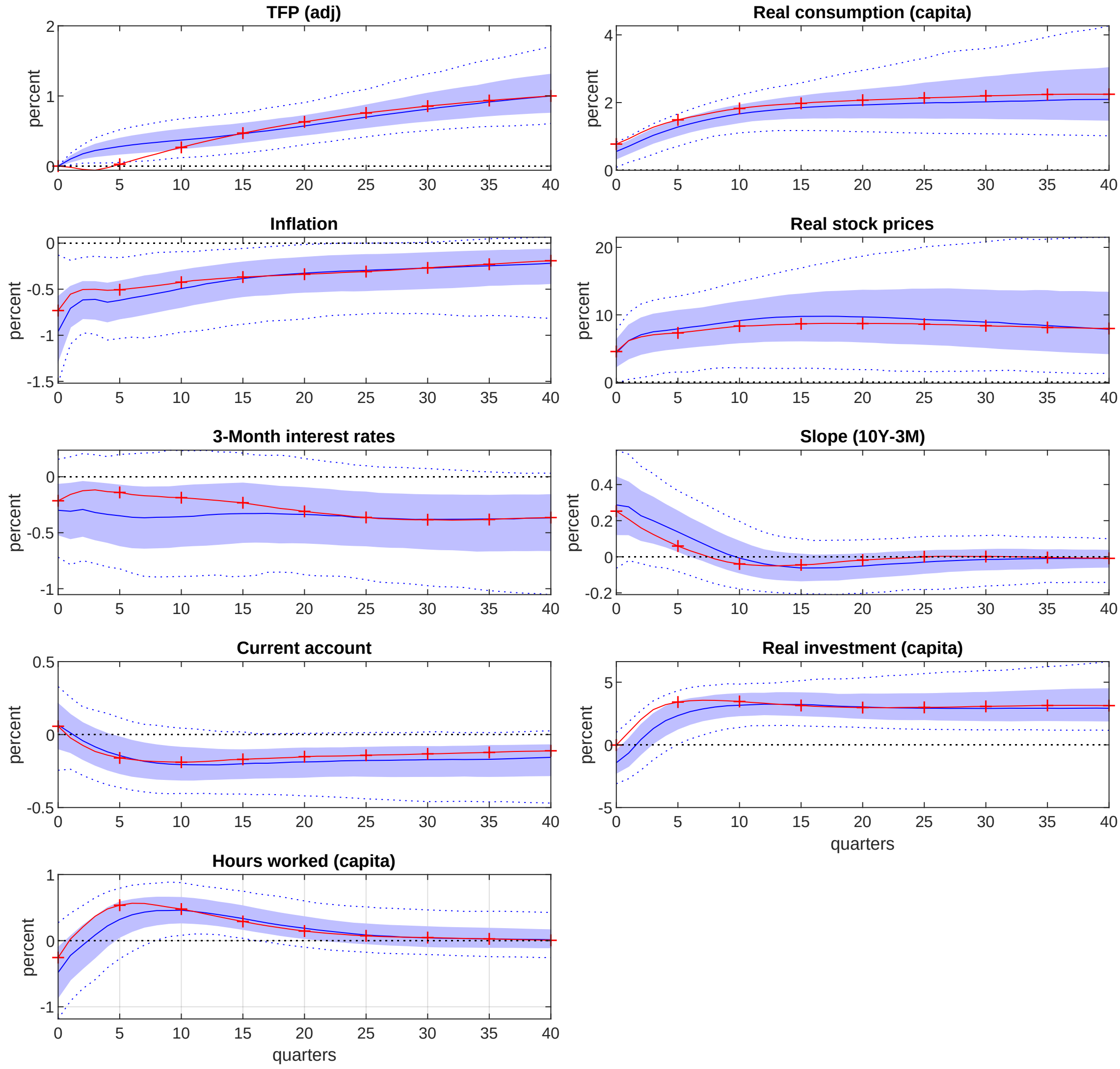


Figure 1: IRF of baseline VAR in the US, utilization-adjusted TFP and Fernald measure

Note: Blue solid line: median impulse response function to technology news shocks on utilization-adjusted TFP, scaled to have a 1% impact on productivity after 40 quarters. Blue areas/blue dotted lines: 67%/95% confidence sets. Red solid line: median IRF to technology news shock on Fernald (2014) measure.

Figure 2 combines the median impulse-response functions from the models for all countries. By and large, the US-results are representative in an international context. In other countries, productivity seems to reach the higher level faster than in the US. In Italy and Japan, we observe some overshooting, which indicates that the identified technology news shock still contains traces of other (non-permanent) shocks.[13] We will return to this issue further below when we discuss alternative, but inferior, identification approaches. For both real investment and total hours worked, the reaction on impact is negative, but (marginally) insignificant, which is also in-line with US results.[14] The reaction of real consumption is broadly similar to the US case, but shows

[13]Italy also shows an initially negative response of stock prices, a huge drop in inflation and a very strong interest rate response.

[14]For the US, there is a debate in the literature if technology news shocks induce business cycle effects through

a higher degree of cross-country variation than other IRFs, especially in the long run. The only variable where the US seems to be an outlier is the current account, which reacts (permanently) positive for other countries.

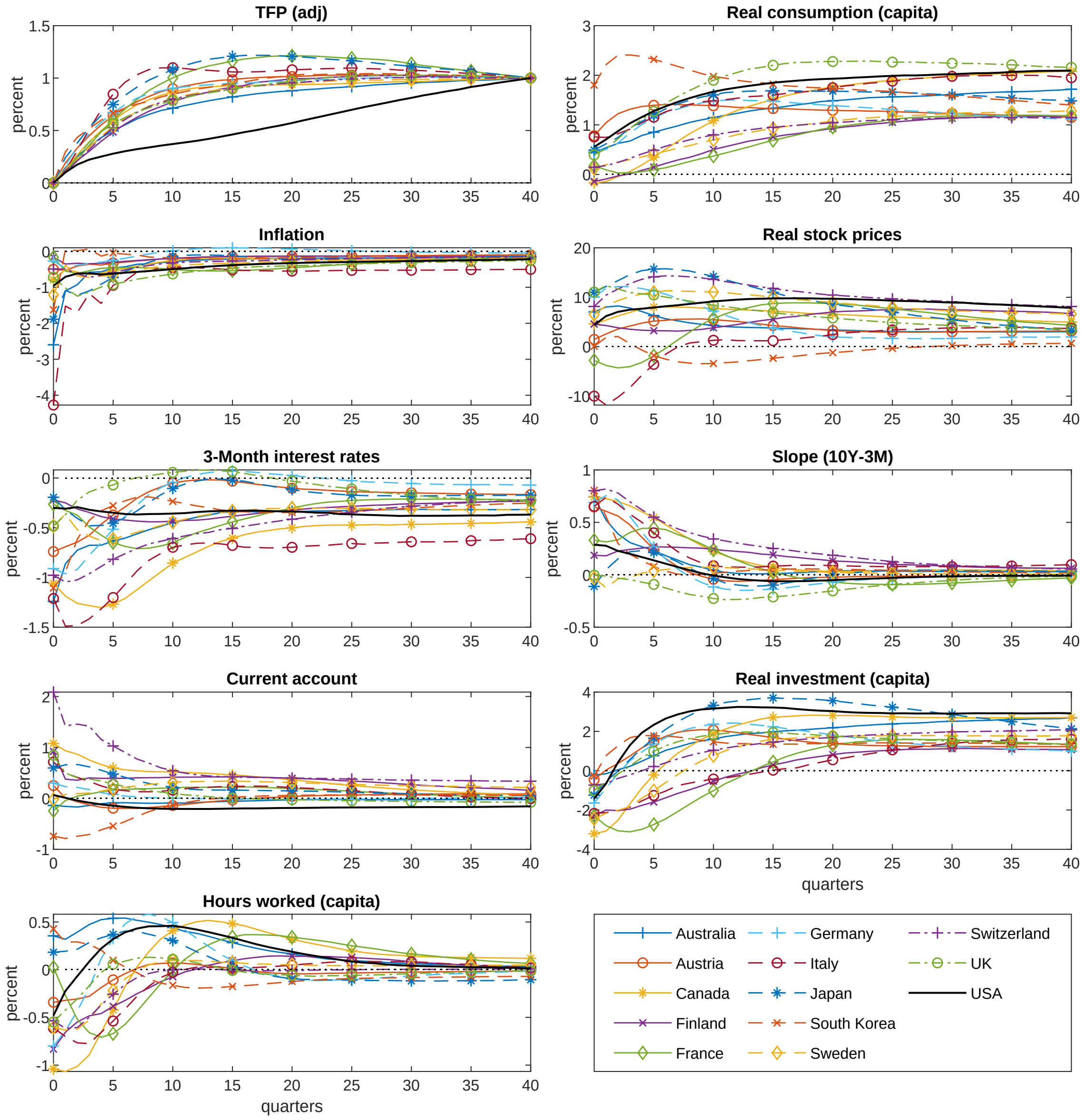


Figure 2: IRF in baseline VAR, news shock to adjusted TFP, all countries

Note: Shocks are scaled to have a 1% impact on productivity after 40 quarters.

The technology news shocks contribute in all countries strongly to permanent developments (like increases in consumption, investment and stock prices), but rather little to business-cycle fluctuations (like variations in hours worked or inflation), see Figure 3. For utilization-adjusted TFP, the contribution after 10 years is between 40% and 70%. Again, the US is representative for our sample of countries. The only exception is inflation: unexpected (future) changes in inflation can be much better explained by productivity news than in other countries.

a cyclical reaction of total hours worked (Beaudry and Portier, 2006, 2014) or if they have mostly long-run effects (Barsky and Sims, 2011).

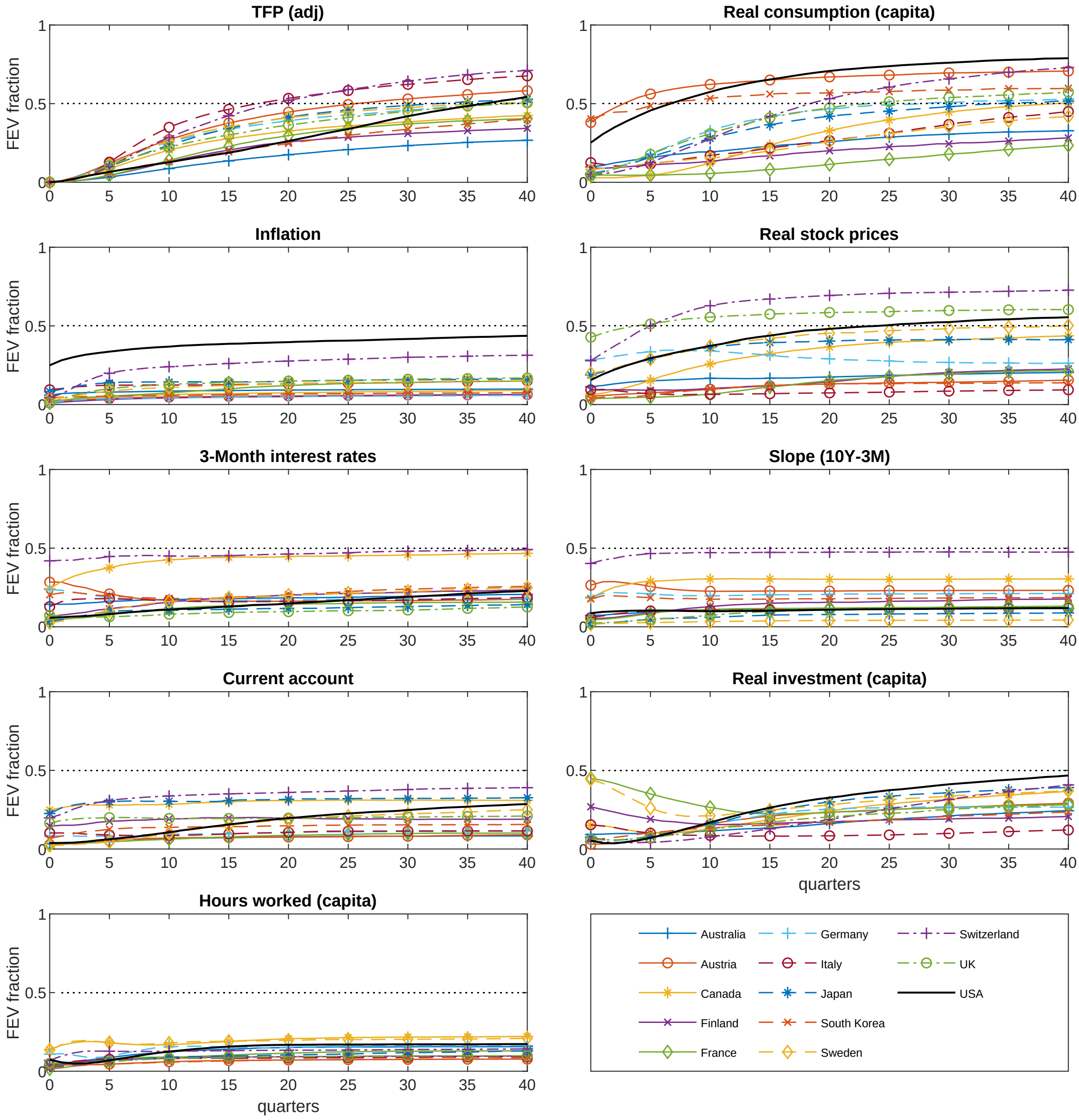


Figure 3: FEVD in baseline VAR, news shock to adjusted TFP, all countries

3.2.2. Results for alternative model specifications

What happens if we estimate smaller models based on the baseline specifications in Kurmann and Otrok (2013) and Kurmann and Sims (2020)? First, the impulse-response functions are very similar to the larger system, see Figures 9 and 10 in the Appendix. In particular, we still find an unclear picture for the impact reaction of hours worked, and an endogenous reaction of monetary policy to lower inflationary pressure. However, the contribution of the productivity news shock to the forecast error variance of utilization-adjusted TFP is markedly smaller than in the larger VAR, see Figure 11 in the Appendix. This is the case in all countries but the US. The effect is also much stronger in the four-variable model based on Kurmann and Sims (2020). One problem with that model is that the selected variables (TFP, consumption, inflation and hours worked) do not contain a strongly forward-looking variable like stock prices or the term spread, which are included in the two other VARs. That is, the results show the importance of including forward-looking variables to avoid the problem of insufficient information in VAR models (Forni and Gambetti, 2014).

In a series of robustness checks, we replace the current account by an alternative measure of openness. These include *trade openness, terms of trade, trade balance, total trade* and the *real effective exchange rate*. For identification and the reaction of other variables to technology news shocks, an important aspect is to control for openness – it does not matter which specific openness measures is included.[15] However, we see some interesting differences for the openness variables themselves, which we show in Figure 12 in the Appendix. First and in line with the results for the current account, countries become more open, total trade increases and the trade balance tends to improve. Terms of trade and the REER are largely unaffected. In terms of the forecast error variance decomposition in Figure 13 in the Appendix, we see that technology news shocks contribute strongly to total trade and trade openness (which are the sum of exports and imports, expressed either in per capita or in real GDP units). For both variables, they explain on average 50% of the variation after 10 years, with cross-country contributions ranging from 20% to 90%. This indicates that technology news shocks are disseminated through total trade. For all other openness measures, the contribution to the medium-run forecast error variance is mostly below 50%.

3.2.3. Results for alternative identification schemes

We identify a technology news shock as the one shock that maximizes the FEV of TFP at $h = 80$ quarters and impose an additional restriction that there should be no impact reaction of TFP. Thus, we combine two standard approaches from the literature. The first has long been standard in the technology news shock literature and consists of the combination of a medium-run horizon $h = 40$ and the additional zero restriction (Nam, 2016). The second comes from a recent paper by Kurmann and Sims (2020). The authors argue for the US that the zero impact restriction can be dropped if the maximization horizon is extended to $h = 80$. Table 2 shows the correlations between identified technology news shocks from our baseline identification to the alternative, for the medium-scale VAR model and the two smaller alternative models. For all cases, we report the correlation for the US individually as well as the average, minimum and maximum across all other countries.

We find that our identification is very similar to the one with a maximization of the FEVD at $h = 40$ quarters. For the US, the correlation ranges from 94% to 97%, depending on the size of the model, while it is even larger for the other countries. However, we find the zero restriction on impact is absolutely necessary to differentiate between technology news shocks and other shocks inducing long-lasting effects. If we drop the zero restriction, the US is nearly the only country where identified shocks have a high correlation of around 90% in the medium-scale baseline VAR. The other countries have on average much lower correlation rates (at 69%). Australia and Finland have even correlation rates below 50%. These correlations go down even further for the two smaller VARs. The impulse-response functions of utilization-adjusted TFP is often hump-shaped, and in many countries the impact reacton is stronger than after 40 quarters, see Figure 4. The problem is more severe for smaller VAR specifications with fewer forward-looking variables. This is a strong sign that the identified "structural" shock in the model without zero restriction is a mixture of a technology news shock with other shocks. As an intermediate case, we also tested an approach where we cleanse the identified shock from short-run variations until $h = 4$ quarters (Belke et al., 2020). This does not strictly impose a zero restriction, but pushes down the short-run effects of news shocks on TFP. However, when we apply such an approach, short-run impulse-responses of TFP are negative, which is highly unplausible.

[15]We opted for the current account due to its forward-lookingness and prominence in the literature (Arezki et al., 2017).

Table 2: Correlation of technology news shocks across countries, baseline identification to alternative identification schemes

Alternative model	Country	Baseline	VAR (KO)	VAR (KS)
	US	88.86%	75.46%	90.19%
h=80, no zero restriction	mean (other countries)	69.17%	53.57%	38.02%
	minimum/maximum	[42%;96%]	[20%;85%]	[13%;52%]
	US	93.73%	96.71%	96.19%
h=40, zero restriction	mean (other countries)	99.56%	99.26%	99.15%
	minimum/maximum	[99%;100%]	[97%;100%]	[96%;100%]

Note: The numbers show the correlation of news shocks obtained through different identification schemes (rows) across VAR specifications (columns) to the news shocks using FEV-maximization at $h = 80$ and a zero impact reaction.

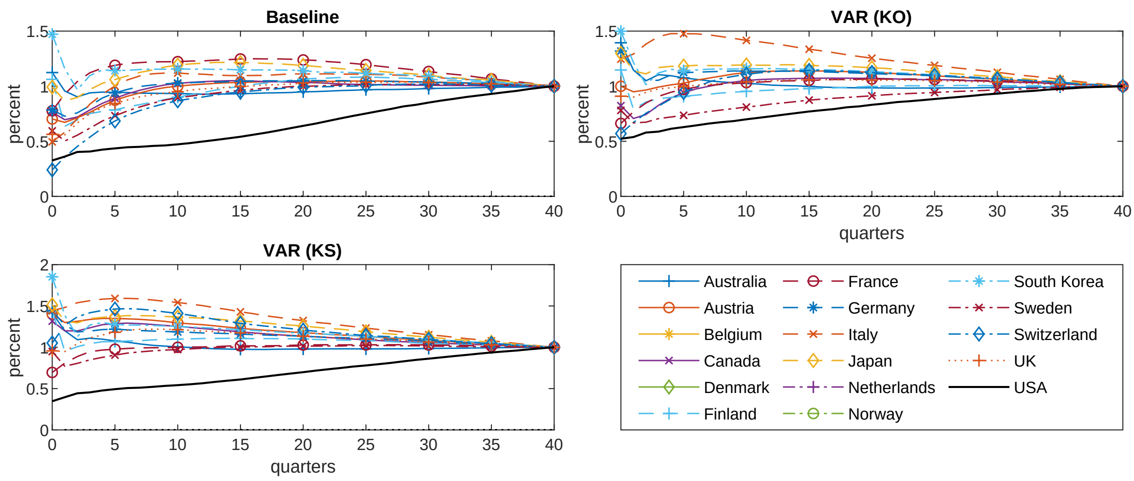


Figure 4: IRF of TFP to productivity news without zero restriction, different VAR specifications

Note: Shocks are scaled to have a 1% impact on productivity after 40 quarters.

3.3. International correlation of technology news shocks

The country-specific baseline VAR models deliver technology news shock processes that are significantly positively correlated across countries, see Figure 5.[16] Germany is the country with the highest correlations (up to 40%), while the US is the country with the largest number of significant correlation links to 10 countries. Results are very similar when the current account is replaced by other openness variables. Correlations are weaker in the small model based on the variables from Kurmann and Sims (2020), and stronger in the model based on (Kurmann and Otrok, 2013) which includes more forward-looking variables, but abstracts from international linkages.

[16]South Korean shocks are negatively correlated with shocks from the UK model, and not significantly correlated to any other shock.

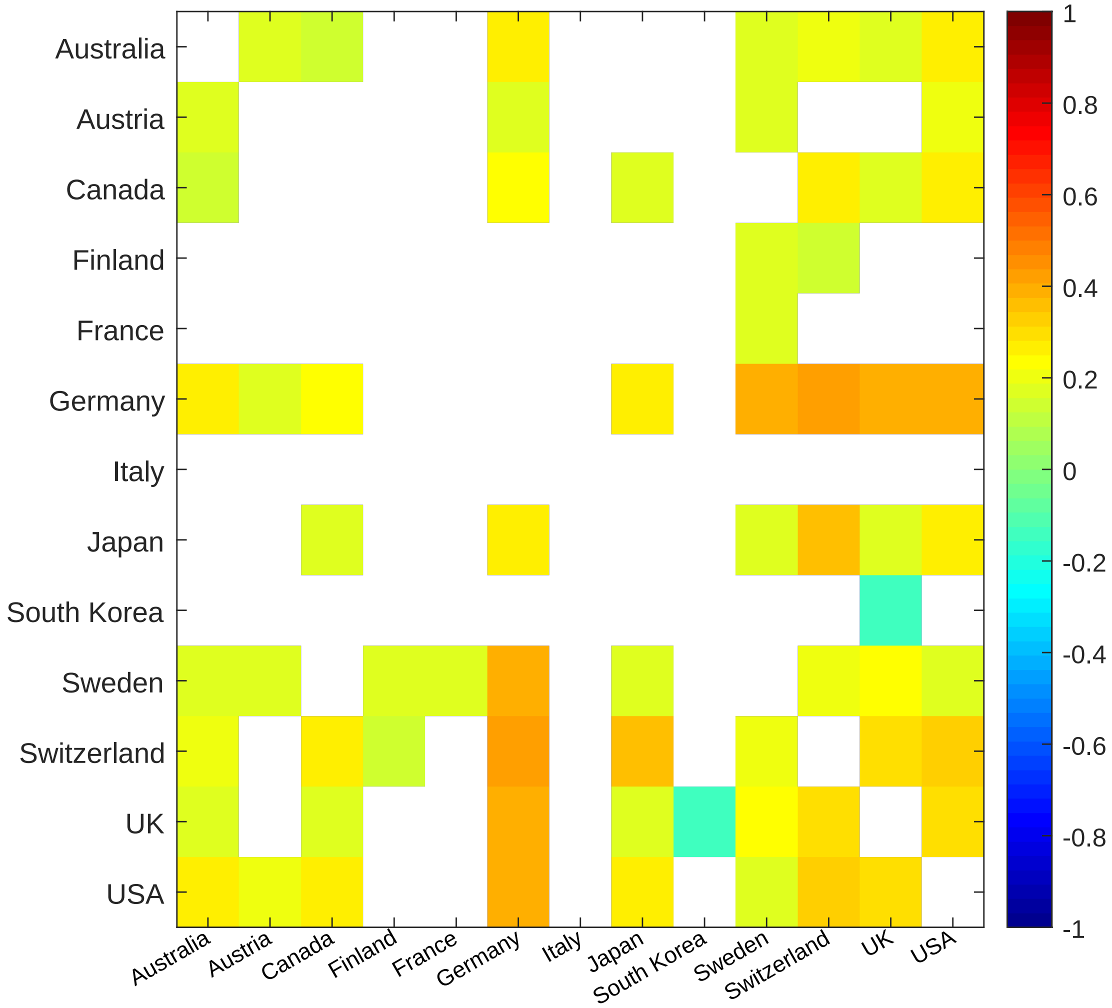


Figure 5: Correlation of TFP news shocks across countries, baseline

Note: Correlations are only shown if they are significant at the 10% level.

These documented positive correlation links are natural because we estimate our VAR models country-by-country and do not control for foreign variables other than our openness variables. Thus, it is very likely that our identified TFP news shock are in fact a mixture of domestic TFP news and news about technology originating abroad. Consider a US TFP news shock: This shock will not only diffuse slowly through the US economy, but it will be transmitted to foreign economies which may also apply this new technology. In particular, open economies and countries relying strongly on the adoption of US technologies should be more affected by such foreign technology news shocks. The same argument naturally holds for other countries of origin. Indeed, a principal component analysis of TFP news shocks shows that there is no common global (or US) factor driving the news shocks in the 13 countries of our baseline estimation. Table 7 shows that four (seven/ten) principal components are needed to explain 50% (75%/90%) of the variance of TFP news shocks.

This fact has implications for the impulse-response functions we should expect. On the one hand, whenever domestic variables respond (qualitatively) similar to foreign and domestic TFP news shocks, we expect impulse-response functions to our mixture of shocks to be qualitatively similar across countries. Put differently, all variables that exhibit international comovement in reaction to single-country TFP news shocks should have similar IRFs in reaction to our mixture of shocks. On the other hand, whenever foreign and domestic shocks have differential effects, the reaction to our identified TFP news shocks would be unclear.

Based on the existing literature, we would expect most variables to comove. For example, Levchenko and Pandalai-Nayar (2020) show that key Canadian macroeconomic variables like real GDP, consumption and hours comove with their respective US counterparts after US TFP news shocks.[17] Though this is not the subject of their study, this should also imply a comovement in stock prices. Similarly, an increase in foreign and domestic aggregate supply should push down inflation in both sides of the border, leading to endogenous expansionary monetary policy reactions. Regarding openness variables, Nam and Wang (2015) show that exports and imports both increase, which implies that total trade should increase in reaction to both domestic and foreign shocks. The reaction of other openness variables is less obvious.

Our results above confirm the hypothesis of positive spillovers, i.e., an international comovement of most of our variables. However, we can do more to investigate this issue. We can – like a large body of the economic literature – use US developments as a proxy for global technological progress, and assume that US developments are exogenous to the rest of the world. The exogeneity assumption is reasonable because the US economy is relatively closed, and it is one of the technologically most advanced economies in the world. Therefore, it is likely that US TFP is driven almost exclusively by domestic shocks. Controlling for US technology news shocks therefore would allow to differentiate between domestic and foreign shocks in country i if US productivity news shocks are well identified and if they are the only foreign productivity news that diffuse to country i. The first condition is fulfilled, as our identification for the US is very robust across different models. The second condition may be fulfilled for a country like Canada (Nam and Wang, 2015), but is questionable for a small open economy with strong trade ties to direct neighbors, like Switzerland. The IRFs to technology news shocks after controlling for US shocks are presented in Figure 14. We see that the impulse-response functions are very similar to the baseline result in Figure 2. This indicates that (given the significant correlation of news shocks) spillovers are not solely driven by US shocks.[18]

An alternative to approach this problem is to control for US productivity instead of US news shocks. Kamber et al. (2017) use the log difference of productivity in country i and the US as a measure of "global" productivity. This approach has one advantage and one serious disadvantage. The advantage is that we would not need to assume that US news shocks are well identified. The disadvantage is that this approach assumes that US productivity news shocks fully materialize abroad. Put differently, this approach fixes the coefficient on the exogenous US productivity news shock in a structural equation on productivity movement to one. Plotting the impulse-response functions from this approach, shown in Figure 15, against the previous alternative and the baseline model, shows how unreasonable the assumption of full diffusion is. Impulse-response functions are quite unrelated to what we would expect. For example, the long-run reaction of consumption to a 1% increase in productivity ranges from -5% in Canada to +3% in Finland, UK and South Korea. IRFs of all other variables are similarly incoherent.

3.3.1. Exogeneity of technology news shocks

It could – for example – be that knowledge on technology news shocks originating in the US (or any other country) take time to be transmitted to other countries. Therefore, we perform an information sufficiency test along the lines of Forni and Gambetti (2014) to know whether the technology news shocks in one country from our baseline VAR cannot be predicted through past

[17] A similar finding is provided by Miyamoto and Nguyen (2017), who identify US technology shocks as in Galí (1999) and assess their impact on Canadian aggregates.

[18] We ran a similar analysis controlling for German productivity news shocks, with similar results. Note that in this case it is even harder to argue that German developments are truly exogenous.

information from all other countries. In particular, we calculate the first k principal components of all available data and test, if the first four lags of these principal components can predict the identified technology news shock.[19] Following the recommendation of Forni and Gambetti (2014), we start with a low number of factors ($k = 2$). The results are reported in Table 3: For our baseline VAR, news shocks in no country can be predicted at the 1% level, and only Australia is predictable at the 5% level (a level which may be too restrictive due to the multiplicity of tests we run here). The smaller VAR specifications, in comparison, have many more countries where insufficient information could be a problem. In particular smaller countries with strong dependence on international business and financial cycles (like Finland and Switzerland) seem to be affected.

Table 3: Information Sufficiency test with $k = 2$ common factors

Country	Baseline	VAR (KO)	VAR (KS)
Australia	0.02**	0.04**	0.49
Austria	0.51	0.12	0.50
Canada	0.32	0.11	0.33
Finland	0.41	0.01**	0.01**
France	0.22	0.91	0.04**
Germany	0.93	0.04	0.05*
Italy	0.06*	0.43	0.10*
Japan	0.58	0.65	0.34
South Korea	0.33	0.01**	0.02**
Sweden	0.24	0.90	0.01***
Switzerland	0.26	0.01***	0.36
UK	0.07*	0.74	0.04**
USA	0.84	0.60	0.04**

In total, we conclude that our baseline VAR is likely informationally sufficient to identify technology news shocks. For the two alternative model specifications, the test also indicates informational sufficiency. However, there are some countries (Finland as an example), where the information in the smaller VAR models seems to be insufficient to identify technology news shocks. At least for our baseline VAR model, the test therefore shows that technology news shocks are not transmitted between countries with a time lag. Instead, they are immediately observed in other countries, as also indicated by the substantial internationl correlation of shocks.

3.3.2. Technology news shocks and international business cycle synchronization

We also investigate the implications of technology news shocks behind the synchronization of international business cycles. To this end, we generate simulated data from each country-specific VAR model based only on the identified technology news shocks. Moreover, we compute business cycle synchronization measures based on actual and simulated data along the lines of Cesa-Bianchi et al. (2019). Figure 6 reports the synchronization measures generated based on actual and simulated data for real consumption per capita. Synchronization of simulated data is less nagative than the one present in actual data, indicating a higher degree of international synchronization. This leads us to conclude that the identified technology news shock processes go beyond synchronized business cycles as they represent something more fundamental, e.g., technology.

[19] Simple principal components need to account for differences in data availability, which leaves the test with 123 observations common to all countries. Accounting for missing data (mostly at the beginning of the sample) using the EM-algorithm allows to include the full time-series of shocks in the predictability test. However, this does not change results substantially.

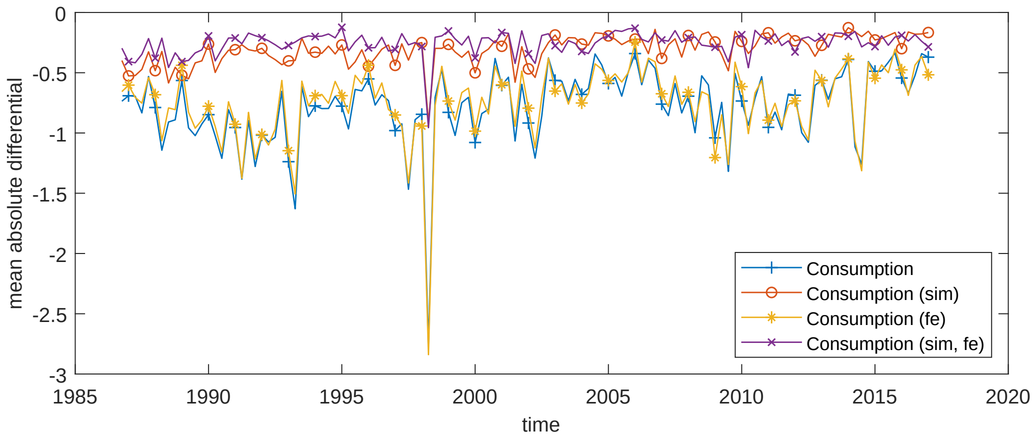


Figure 6: Comparison of synchronization measures based on actual and simulated data for real consumption per capita

Note: Synchronization measures are the average absolute difference between growth rates of consumption (observed series and simulated data conditional only on TFP news shocks). Removing country-fixed effects ("fe") before the calculation of absolute differences does not change the synchronization measures.

4. Conclusion

So far the literature on technology news shocks has focused on the US as the major hub of technological innovations worldwide. In addition, most of the existing analyses investigate technology spillover-effects based on country-pair relationships with the US being the dominant and exogenous source. In this paper we take a global perspective on the international dissemination of technology. To this end, we construct productivity measures for several industrialized economies and identify the underlying technology news shock processes in the respective country-specific data. We document a robust response pattern of macroeconomic variables internationally. This finding goes in line with the fact that the industrial structure of many advanced economies is to a degree similar. therefore, we should also expect similar technology processes driving the respective macroeconomic variables. Furthermore, we also show that technology news shocks propagate through financial and trade-related variables and therefore serve as possible channels for the dissemination (either informationally or physically) of such shocks. Moreover, we show that technology diffusion processes are significantly correlated across countries. In particular, we find that the US are not the only source of technology diffusion.

Acknowledgements

We thank Christiane Baumeister, Fabio Canova, Danilo Cascaldi-Garcia, Günter Coenen, Jean-Marie Dufour, Neville Francis, Hashmat Khan, Mathias Klein, Winfried Koeniger, Samad Sarferaz, Eric Sims, Peter Tillmann, Mathias Trabandt, and seminar participants at the IWH, University of Leipzig, KOF, University of Gießen, the HenU/INFER Workshop on Applied Macroeconomics (Henan), the IWH-CIREQ-GW Macroeconometric Workshop, the CGDE Workshop (Leipzig), the CEF Annual Meeting (Ottawa), and the IAAE Annual Meeting (Nicosia) for valuable comments and insightful discussions. All errors are our own.

References

Alexopoulos, M., 2011. Read All about It!! What Happens Following a Technology Shock? American Economic Review 101, 1144–1179.

Arezki, R., Ramey, V., Sheng, L., 2017. News Shocks in Open Economies: Evidence from Giant Oil Discoveries. The Quarterly Journal of Economics 132, 103–155.

Barsky, R., Sims, E., 2011. News Shocks and Business Cycles. Journal of Monetary Economics 58, 273–289.

Barsky, R.B., Basu, S., Lee, K., 2014. Whither News Shocks? NBER Macroeconomics Annual 29, 225–264.

Basu, S., Fernald, J.G., Kimball, M.S., 2006. Are Technology Improvements Contractionary? American Economic Review 96, 1418–1448.

Beaudry, P., Dupaigne, M., Portier, F., 2011. The International Propagation of News Shocks. Review of Economic Dynamics 14, 72–91.

Beaudry, P., Guay, P.F.A., Portier, F., 2015. When is Nonfundamentalness in VARs a Real Problem? An Application to News Shocks. NBER Working Paper, No. 21466. National Bureau of Economic Research.

Beaudry, P., Lucke, B., 2010. Letting Different Views about Business Cycles Compete. NBER Macroeconomics Annual 2009 24, 413 – 455.

Beaudry, P., Portier, F., 2006. Stock Prices, News, and Economic Fluctuations. American Economic Review 96, 1293–1307.

Beaudry, P., Portier, F., 2014. News-Driven Business Cycles: Insights and Challenges. Journal of Economic Perspectives 52, 993–1074.

Belke, A., Elstner, S., Rujin, S., 2020. Growth Prospects and the Trade Balance in Advanced Economies. Ruhr Economic Papers 827. RWI - Leibniz Institute for Economic Research.

Ben Zeev, N., Khan, H., 2015. Investment-Specific News Shocks and U.S. Business Cycles. Journal of Money, Credit and Banking 47, 1443–1464.

Benati, L., 2007. Drifts and Breaks in Labor Productivity. Journal of Economic Dynamics and Control 31, 2847–2877.

Canova, F., Lopez-Salido, D., Michelacci, C., 2010. The Effects of Technology Shocks on Hours and Output: A Robustness Anaylsis. Journal of Applied Econometrics 25, 755–773.

Cascaldi-Garcia, D., 2017. News Shocks and the Slope of the Term Structure of Interest Rates: Comment. American Economic Review 107, 3243–49.

Cesa-Bianchi, A., imbs, J., Saleheen, J., 2019. Finance and Synchronization. Journal of International Economics 116, 74–87.

Chang, Y., Hong, J.H., 2006. Do Technological Improvements in the Manufacturing Sector Raise or Lower Employment? American Economic Review 96, 352 – 368.

Christiano, L., Eichenbaum, M., Vigfusson, R., 2003. What Happens After a Technology Shock? NBER Working Paper, No. 9819. National Bureau of Economic Research.

Dieppe, A., Francis, N., Kindberg-Hanlon, G., 2019. New Approaches to the Identification of Low-Frequency Drives: An Application to Technology. Policy Research Working Paper, No. 9047. World Bank.

Eaton, J., Kortum, S., 1996. Trade in Ideas: Patenting and Productivity in the OECD. Journal of International Economics 40, 251 – 278.

Faust, J., Leeper, E., 1997. When Do Long-Run Identifying Restrictions Give Reliable Results? Journal of Business and Economic Statistics 15, 345–353.

Feenstra, R.C., Inklaar, R., Timmer, M.P., 2015. The Next Generation of the Penn World Table. American Economic Review 105, 3150–3182.

Fernald, J., 2007. Trend Breaks, Long-Run Restrictions, and Contractionary Technology Improvements. Journal of Monetary Economics 54, 2467–2485.

Fernald, J., 2014. A Quarterly, Utilization-Adjusted Series on Total Factor Productivity. Working Paper Series 2012-19. Federal Reserve Bank of San Francisco.

Fernald, J.G., Wang, J.C., 2016. Why has the Cyclicality of Productivity Changed? What Does It Mean? Annual Review of Economics 8, 465–496.

Fisher, J.D., 2006. The Dynamic Effects of Neutral and Investment-Specifc Technology Shocks. Journal of Political Economy 114, 413–451.

Fisher, J.D., 2010. Discussion of: Letting Different Views About Business Cycles Compete. NBER Macroeconomics Annual 24, 457–474.

Forni, M., Gambetti, L., 2014. Sufficient Information in Structural VARs. Journal of Monetary Economics 66, 124–136.

Forni, M., Gambetti, L., Sala, L., 2014. No News in Business Cycles. Economic Journal 124, 1168–1191.

Francis, N., Owyang, M.T., Roush, J.E., DiCecio, R., 2014. A Flexible Finite-Horizon Alternative to Long-Run Restrictions with an Application to Technology Shocks. Review of Economics and Statistics 96, 638–647.

Francis, N., Ramey, V.A., 2005. Is the Technology-Driven Real Business Cycle Hypothesis Dead? Shocks and Aggregate Fluctuations Revisited. Journal of Monetary Economics 52, 1379–1399.

Francis, N., Ramey, V.A., 2009. Measures of per Capita Hours and Their Implications for the Technology-Hours Debate. Journal of Money, Credit and Banking 41, 1071–1097.

Galí, J., 1999. Technology, Employment, and the Business Cycle: Do Technology Shocks Explain Aggregate Fluctuations? American Economic Review 89, 249–271.

Galí, J., Rabanal, P., 2004. Technology Shocks and Aggregate Fluctuations: How Well Does the Real Business Cycle Model Fit Postwar U.S. Data? NBER Macroeconomics Annual 19, 225–318.

Görtz, C., Tsoukalas, J.D., Zanetti, F., 2020. News Shocks under Financial Frictions. CAMA Working Papers 2020-94. Centre for Applied Macroeconomic Analysis, Crawford School of Public Policy, The Australian National University.

Hamilton, J., 2018. Why You Should Never Use the Hodrick-Prescott Filter. Review of Economics and Statistics 100, 831–843.

Hamilton, J.D., 1994. Time Series Analysis. Princeton: Princeton University Press.

Imbs, J.M., 1999. Technology, Growth and the Business Cycle. Journal of Monetary Economics 44, 65–80.

Jaimovich, N., Rebelo, S., 2009. Can News About the Future Drive the Business Cycle? American Economic Review 99, 1097–1118.

Kamber, G., Theodoridis, K., Thoenissen, C., 2017. News-Driven Business Cycles in Small Open Economies. Journal of International Economics 105, 77–89.

Kosaka, M.S., 2013. News-Driven International Business Cycles. B.E. Journal of Macroeconomics 13, 751–793.

Kurmann, A., Otrok, C., 2013. News Shocks and the Slope of the Term Structure of Interest Rates. American Economic Review 103, 2612–2632.

Kurmann, A., Otrok, C., 2017. News Shocks and the Term Structure of Interest Rates: Reply. American Economic Review 107, 3250–3256.

Kurmann, A., Sims, E., 2020. Revisions in Utilization-Adjusted TFP and Robust Identification of News Shocks. Review of Economics and Statistics , 1–49.

Kydland, F., Prescott, E., 1982. Time to Build and Aggregate Fluctuations. Econometrica 50, 1345–1370.

Levchenko, A.A., Pandalai-Nayar, N., 2020. TFP, News, and "Sentiments:" The International Transmission of Business Cycles. Journal of the European Economic Association 18, 302–341.

Mansfield, E., 1989. The Diffusion of Industrial Robots in Japan and the United States. Research Policy 18, 183–192.

Miranda-Agrippino, S., Hoke, S.H., Bluwstein, K., 2020. When Creativity Strikes: News Shocks and Business Cycle Fluctuations. CEPR Discussion Papers, No. 15062. Center for Economic and Policy Research.

Miyamoto, W., Nguyen, T.L., 2017. Understanding the Cross-Country Effects of U.S. Technology Shocks. Journal of International Economics 106, 143–164.

Miyamoto, W., Nguyen, T.L., 2020. The Expectational Effects of News in Business Cycles: Evidence from Forecast Data. Journal of Monetary Economics 116, 184–200.

Nam, D., 2016. News Shocks and News-Driven Business Cycle Hypothesis: Identification Strategy versus Factor-Utilization-Adjusted TFP Series. Mimeo.

Nam, D., Wang, J., 2014. Are Predictable Improvements in TFP Contractionary or Expansionary: Implications from Sectoral TFP? Economics Letters 124, 171–175.

Nam, D., Wang, J., 2015. The Effects of Surprise and Anticipated Technology Changes on International Relative Prices and Trade. Journal of International Economics 97, 162–177.

Nam, D., Wang, J., 2018. Understanding the Effect of Productivity Changes on International Relative Prices: The Role of News Shocks. Pacific Economic Review 3, 490–516.

Ohanian, L., Raffo, A., 2012. Aggregate Hours Worked in OECD Countries: New Measurement and Implications for Business Cycles. Journal of Monetary Economics 59, 40–56.

Rotemberg, J.J., 2003. Stochastic Technical Progress, Smooth Trends, and Nearly Distinct Business Cycles. American Economic Review 93, 1543–1559.

Uhlig, H., 2003. What Drives GNP? Working Paper. Humboldt University.

Uhlig, H., 2004. Do Technology Shocks Lead to a Fall in Total Hours Worked? Journal of the European Economic Association 22, 361–371.

5. Appendix

Figures

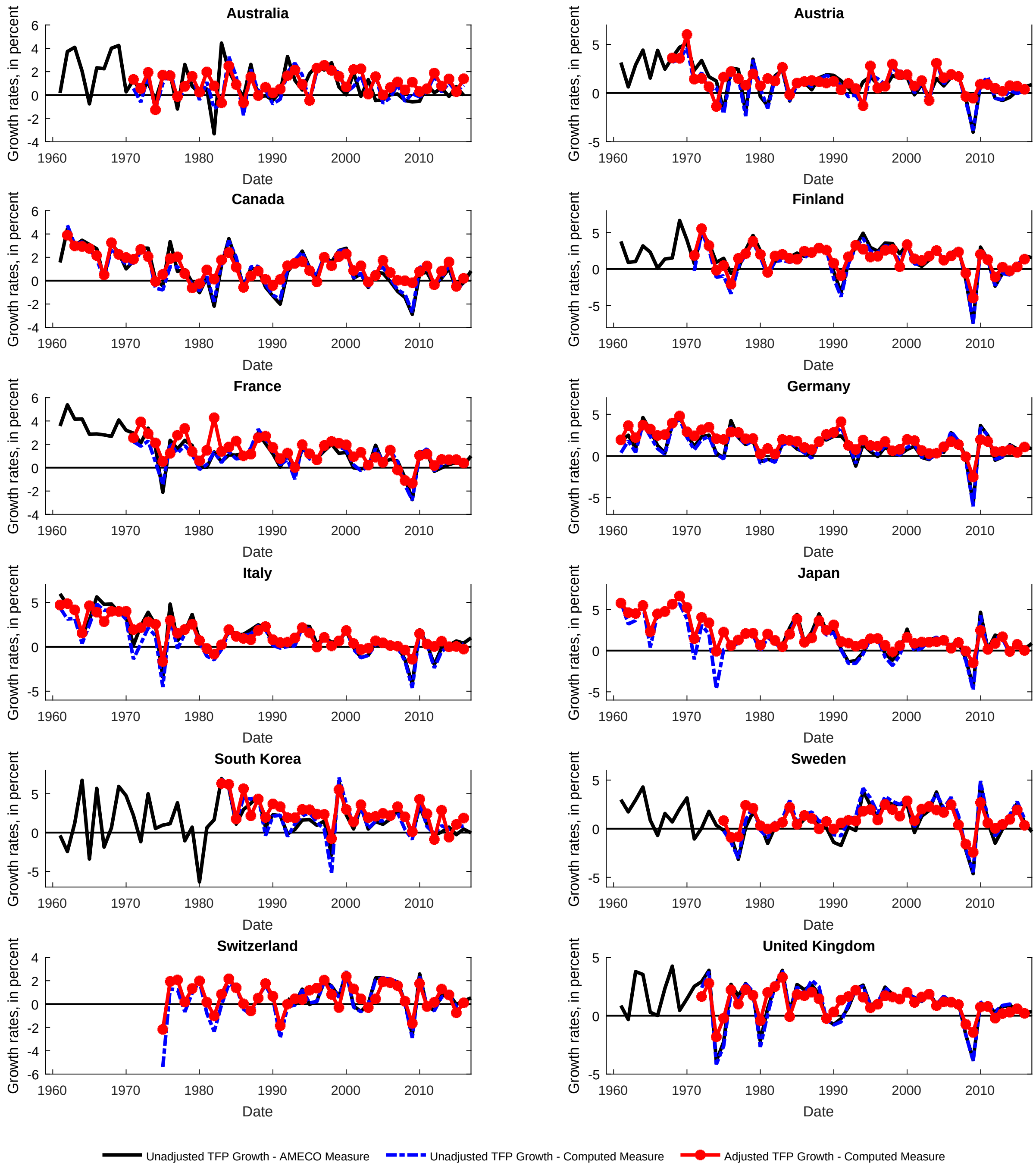
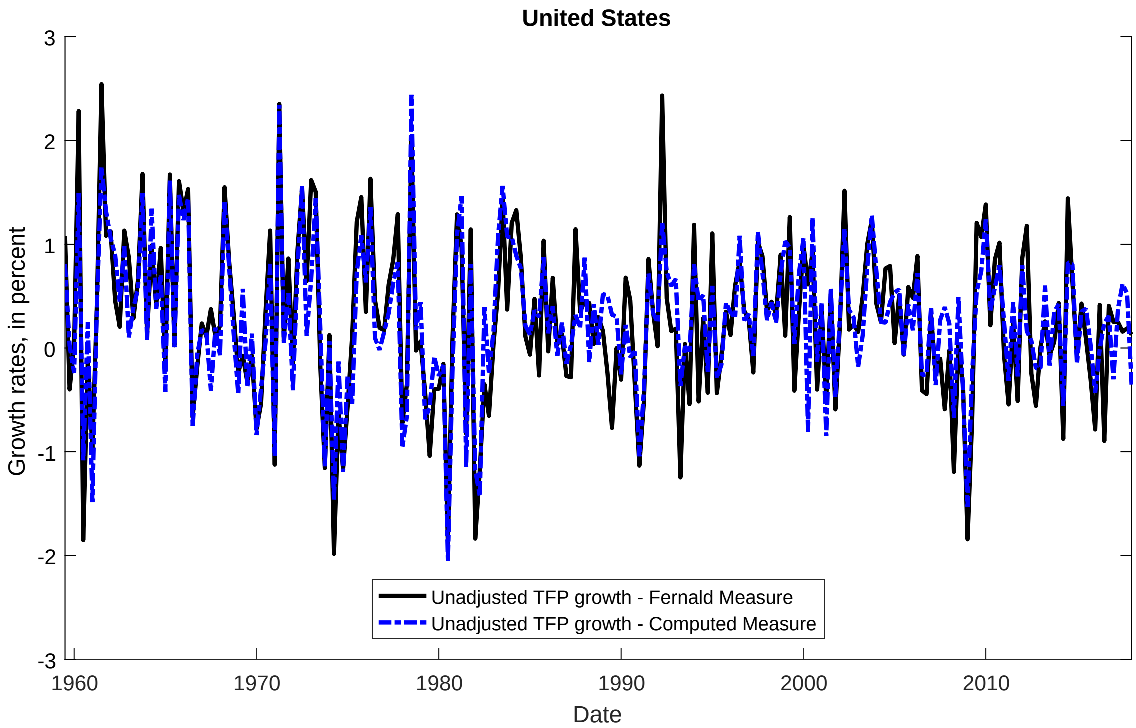


Figure 7: Unadjusted and adjusted TFP Growth Rates



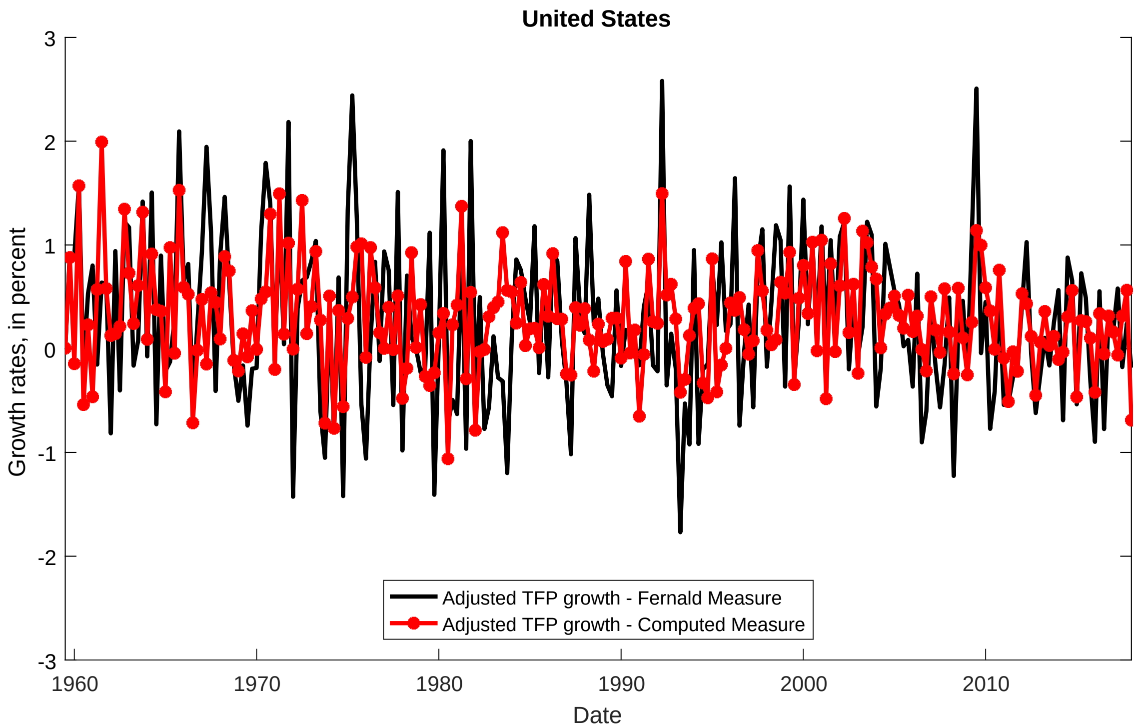


Figure 8: Comparison of Unadjusted and Adjusted US TFP Growth Rates. Correlations of unadjusted and adjusted series are 87% and 55%, respectively

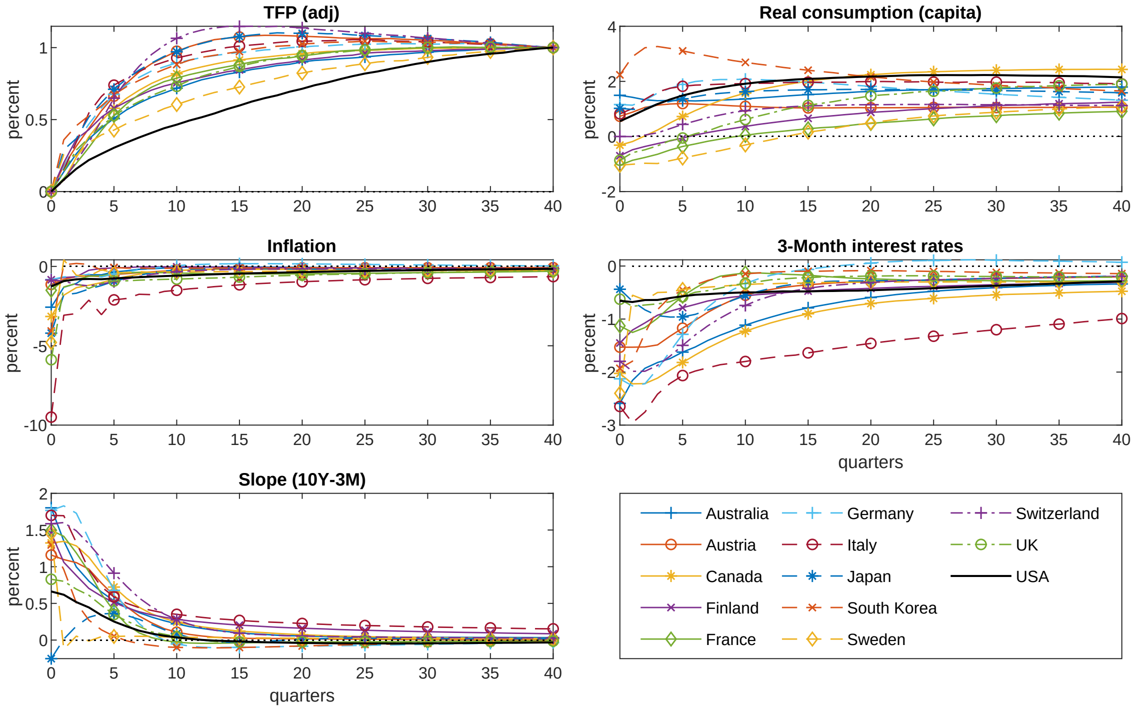


Figure 9: IRF in VAR (KO), news shock to adjusted TFP, all countries

Note: Shocks are scaled to have a 1% impact on productivity after 40 quarters.

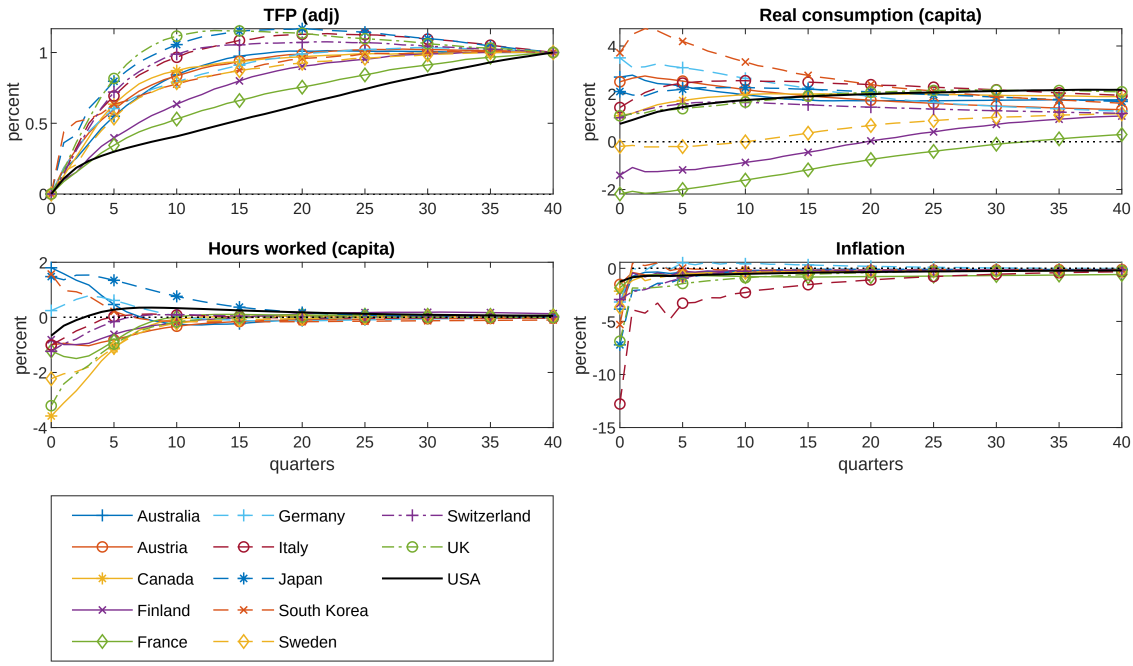


Figure 10: IRF in VAR (KS), news shock to adjusted TFP, all countries

Note: Shocks are scaled to have a 1% impact on productivity after 40 quarters.

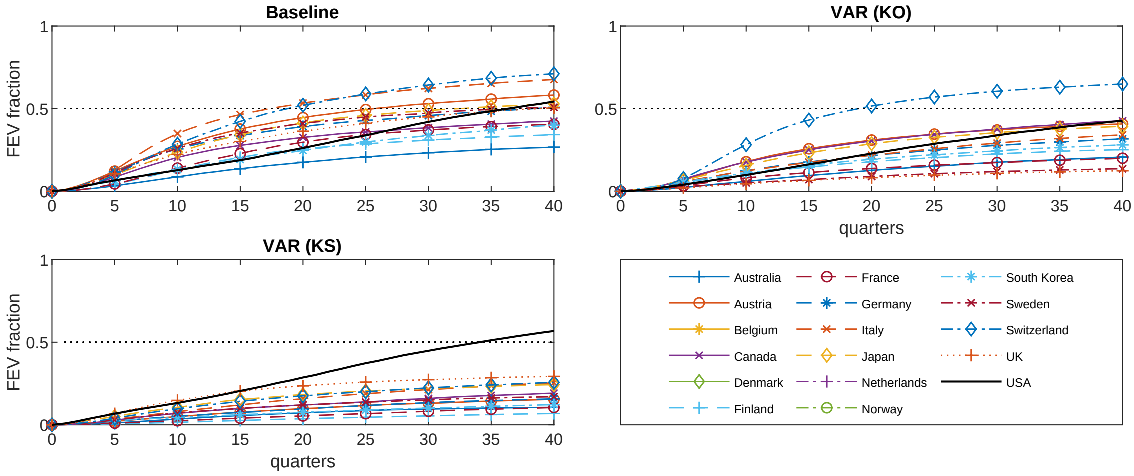


Figure 11: FEVD of TFP across different VARs, news shock to adjusted TFP, all countries

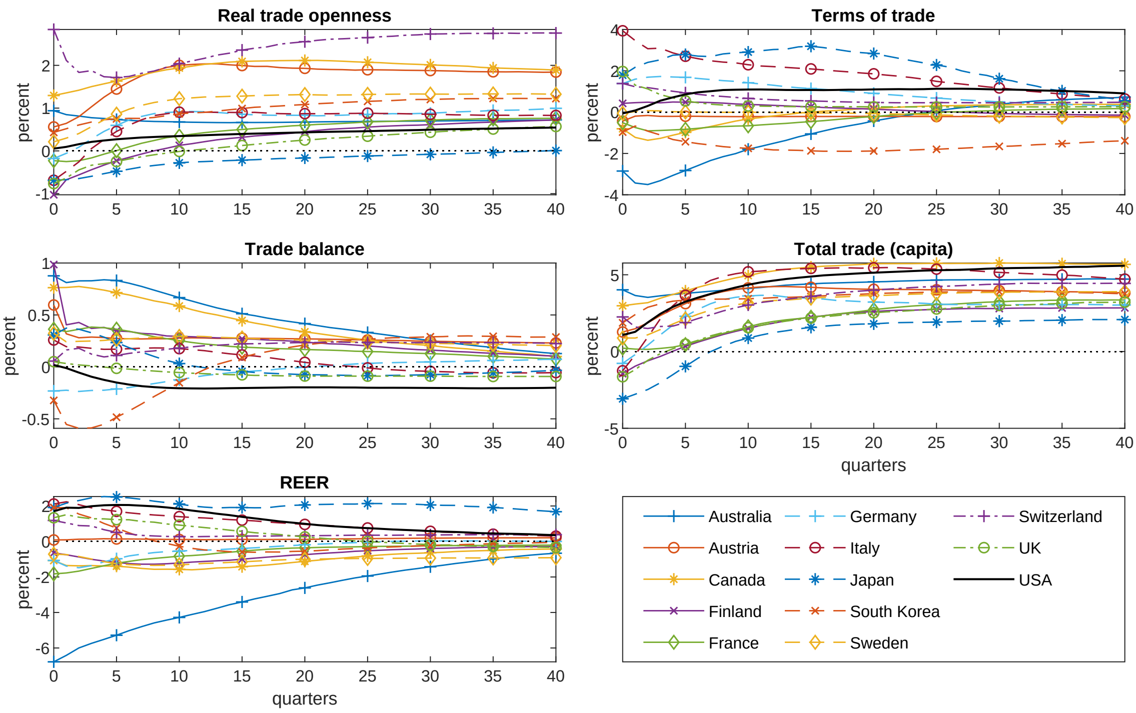


Figure 12: IRF of different openness measures, news shock to adjusted TFP, all countries

Note: Shocks are scaled to have a 1% impact on productivity after 40 quarters.

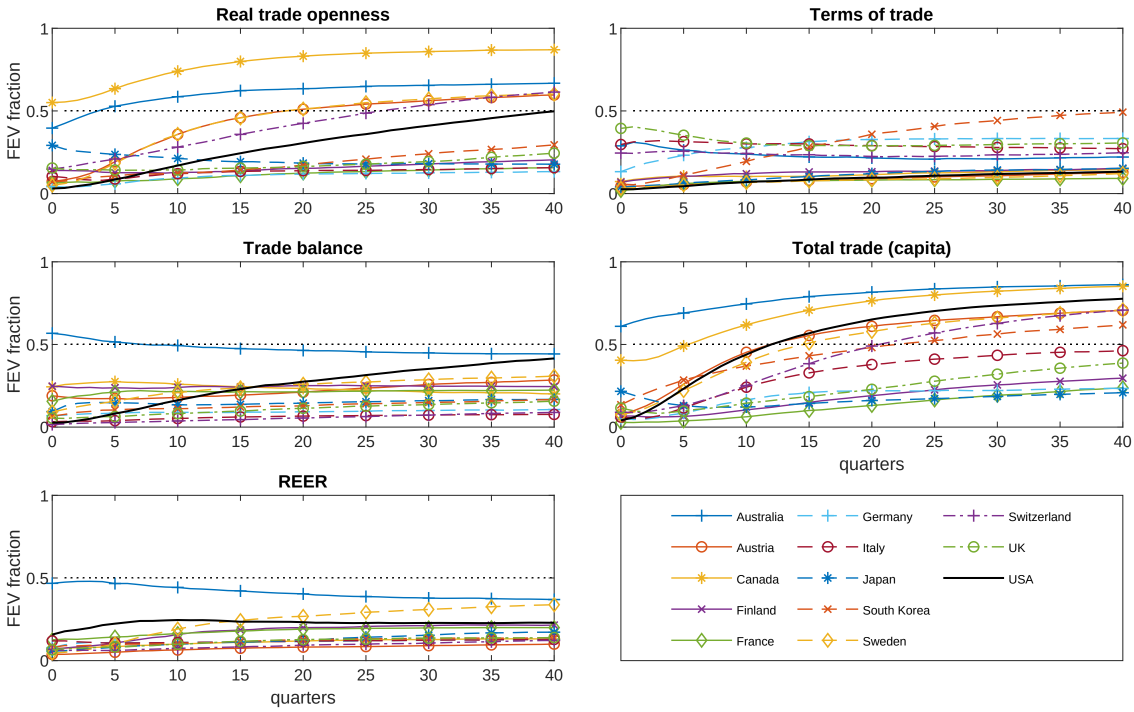


Figure 13: FEVD of different openness measures, news shock to adjusted TFP, all countries

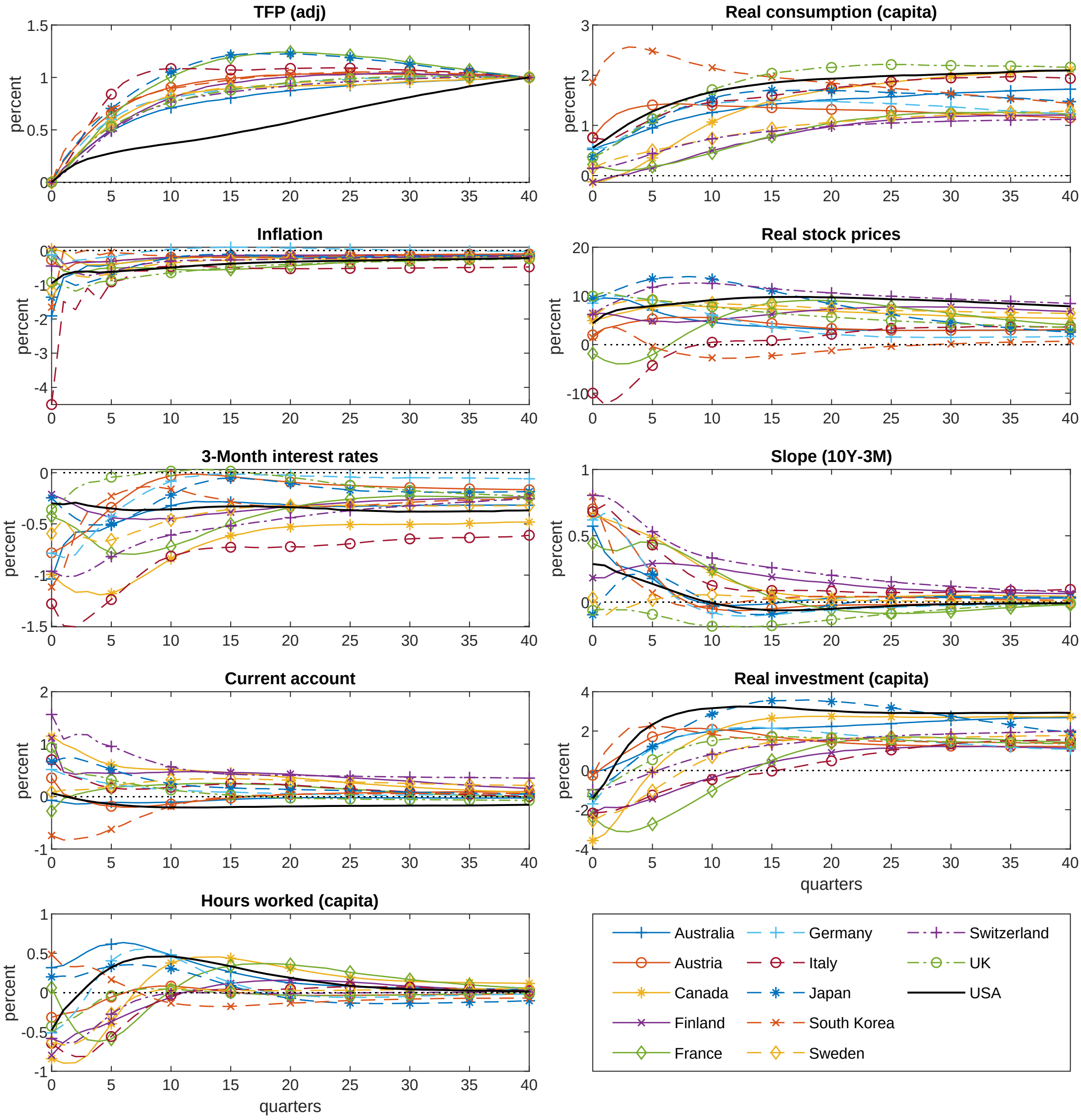


Figure 14: IRF from models with baseline US productivity news shocks as exogenous variable

Note: Baseline models for all countries but the US are extended by US productivity news shocks from baseline model. US results reported for comparison. Shocks are scaled to have a 1% impact on productivity after 40 quarters.

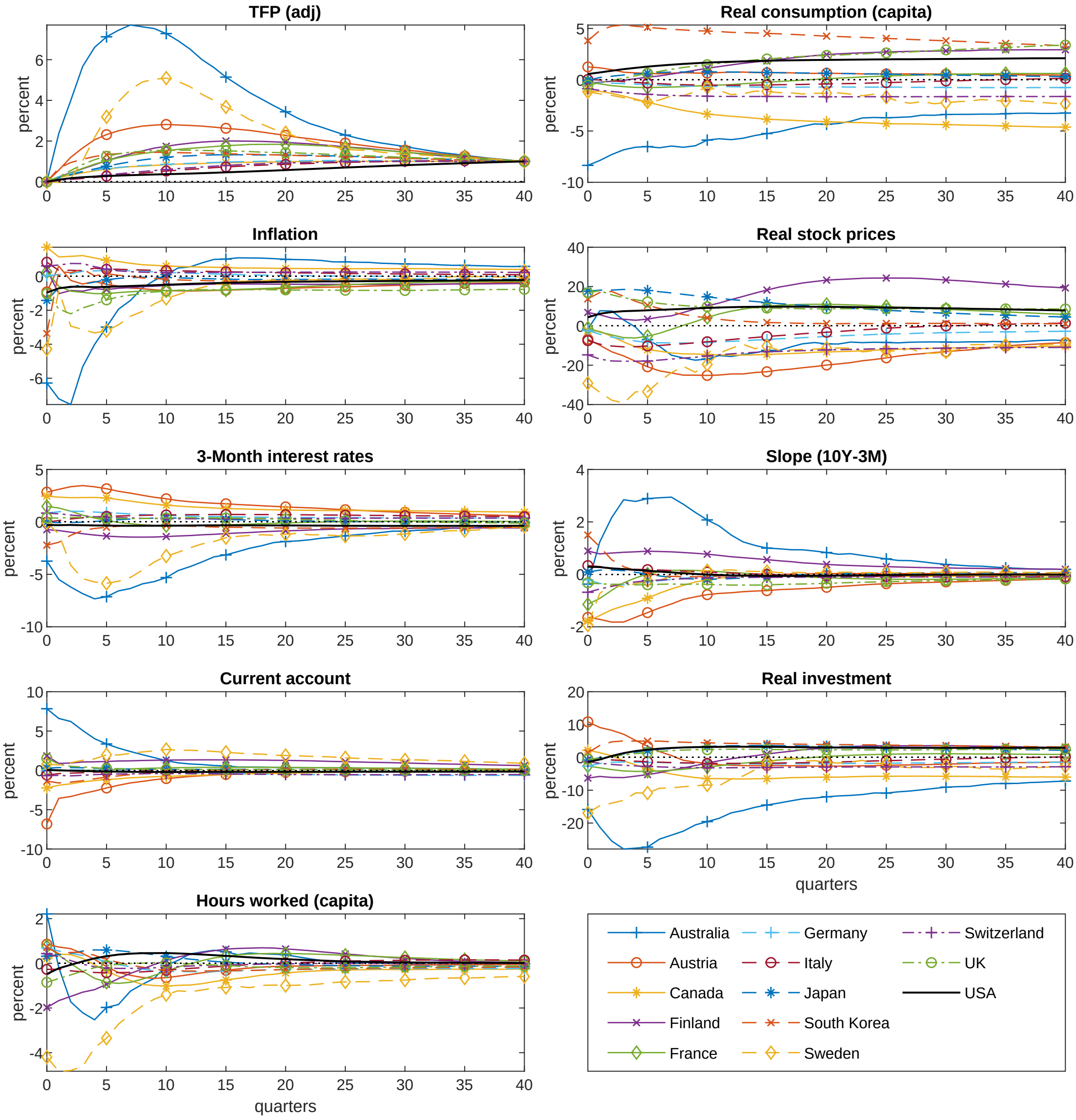


Figure 15: IRF from models corrected for US productivity

Note: Baseline models for all countries but the US are corrected for US productivity developments. US results reported for comparison. Shocks are scaled to have a 1% impact on productivity after 40 quarters.

Tables

Table 4: Definition of Variables

Variables	Description	Source	Transformation
Variables in baseline model			
Total Factor Productivity	Utilization-adjusted Total Factor Productivity (TFP)	Fernald (2014)	Cumsum (TFP*400)
Labor Productivity	Output (rGDP) per hours worked by all employed persons (Total_Hours)	OECD, Bureau of Labor Statistics, Ohanian and Raffo (2012)	log(rGDP/Total_Hours)*100
Real Consumption	Real private consumption (rC) per capita (Pop)	OECD	log(rC/Pop)*100
Real Stock Prices	Share price index (SP) deflated by the GDP Deflator (GDP_Defl)	OECD	(log(SP)-log(GDP_Defl))*100
Inflation	Annualized percentage change of the GDP deflator (GDP_Defl)	OECD	(log(GDP_Defl)-log(GDP_Defl$_{-1}$)*400
3-Month interest rate	Short-term interest rate (3-month)	OECD	-
Slope of the Term Structure of Interest Rates	10-year bond yields minus 3-month interest rate	OECD	-
Current account balance	Current account balance as share of GDP	OECD	-
Real Investment	Real private fixed-capital formation (rI) per capita (Pop)	OECD	log(rI/Pop)*100
Hours worked per capita	Total hours worked (Total_Hours) per capita (Pop)	OECD	log(Total_Hours/Pop)*100
Variables in robustness checks			
Total Trade	Sum of real exports and imports of goods and services per capita (Pop)	OECD	log((rExports+rImports)/Pop)*100
Trade balance	Real exports minus imports of goods and services over real GDP	OECD	(rExports-rImports)/rGDP
Trade Openness	Degree of real trade openness	OECD	(rExports+rImports)/rGDP
Real Effective Exchange Rate	Real Effective Exchange Rate (REER)	BIS	log(REER)*100
Variables used for computation of TFP			
Capital stock	Net capital stock	Penn World Table	-
Depreciation rate	Average depreciation rate of the capital stock	Penn World Table	-
Labor share	Share of labour compensation in GDP at Current Prices	Penn World Table	-
Variables used for computation of other variables			
Real GDP (rGDP)	Real gross domestic product	OECD	-
Population (Pop)	Working age population	OECD	-
Real exports (rExports)	Real exports	OECD	-
Real imports (rImports)	Real imports	OECD	-

Table 5: Countries and sample periods

Country:	Australia	Austria	Canada	Finland	France	Germany	Italy
Sample period:	1967Q1-2016Q4	1967Q1-2016Q4	1961Q2-2016Q4	1970Q1-2016Q4	1970Q1-2017Q4	1962Q1-2016Q4	1960Q2-2016Q4
Country:	Japan	South Korea	Sweden	Switzerland	UK	USA	
Sample period:	1970Q1-2016Q4	1976Q3-2016Q4	1974Q1-2016Q4	1974Q1-2016Q4	1971Q1-2016Q4	1959Q2-2018Q2	

Table 6: Variable selection in submodels compared to baseline model

Baseline model	Kurmann and Otrok (2013)	Kurmann and Sims (2020)
Productivity Measure	Utilization-adjusted TFP	Utilization-adjusted TFP
Real consumption (capita)	Real consumption (capita)	Real consumption (capita)
Inflation (GDP Deflator)	Inflation (GDP Deflator)	
Real stock prices		
Short-term interest rates	Short-term interest rates	Short-term interest rates
Spread (10Y-3M)	Spread (10Y-3M)	
Current Account		
Real investment (capita)		
Hours worked (capita)		Hours worked (capita)

Table 7: Cumulative contribution of principal components to variance of TFP news shocks

Component	Cumulative variance contribution
PC 1	23%
PC 2	35%
PC 3	45%
PC 4	54%
PC 5	62%
PC 6	69%
PC 7	75%
PC 8	81%
PC 9	86%
PC10	90%
PC11	94%
PC12	97%
PC13	100%

Halle Institute for Economic Research –
Member of the Leibniz Association

Kleine Maerkerstrasse 8

D-06108 Halle (Saale), Germany

Postal Adress: P.O. Box 11 03 61

D-06017 Halle (Saale), Germany

Tel +49 345 7753 60

Fax +49 345 7753 820

www.iwh-halle.de

ISSN 2194-2188

Leibniz
Association