Essays on macroeconomic policies after the crisis

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Essays on Macroeconomic Policies After the Crisis

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A papà

Declaration of authorship:

- Chapter 2, entitled "*Capital Flow Spillovers of U.S. Macroeconomic News: the Role of Fed's Policies*," is fruit of my own work.

- Chapter 3, entitled "*The Composition Effects of Tax-based Consolidations on Income Inequality*" is based on a joint paper with dr. E. Ernst, prof. dr. M. Giuliodori and dr. R. Merola. I (Gabriele Ciminelli) assembled the dataset, performed the empirical analysis and drafted most of the paper. This paper is forthcoming in the European Journal of Political Economy.

- Chapter 4, entitled "*Employment Protection Deregulation and Labor Shares in Advanced Economies*" is based on a joint paper with dr. R. Duval and dr. D. Furceri. I estimated the elasticities of substitution and layoff rates used in the industry-country-level analysis, performed the industry-country-level analysis and drafted large parts of the paper. This paper was issued as IMF Working Paper No. 18/186.

- Chapter 5, entitled "*Okun's Law and Demographics: Differences Across Labor Markets*" is based on a joint paper with dr. J.C. Bluedorn and Z. An. I performed the empirical analysis and drafted large parts of the paper.

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Chapter 1

Introduction

The Global Financial Crisis (GFC) of 2007-2009 and the ensuing recession marked the deepest downturn in economic history since the Great Depression. Although the response of governments was different across countries, at least in advanced economies, macroeconomic policy evolved around some common lines. This dissertation identifies three areas of intervention — (i) quantitative easing and forward guidance, (ii) fiscal austerity, and (iii) labor market deregulation — and studies the effects that each of them might have had on selected economic and financial variables.

Chapter 2 focuses on the unconventional monetary policies undertaken by the U.S. Federal Reserve (Fed) in response to the GFC. In November 2008 the Fed lowered the short-term interest rate to zero and began buying financial assets to provide liquidity to the markets (quantitative easing policy). The Fed also changed its communication, as it sought to indicate that monetary accommodation was there to stay (forward guidance policy). In a world in which capital flows across borders and where the dollar is the international reserve currency, these policies carried global spillovers. Chapter 2 studies how the Fed's 'unconventional' policies affected agents' international investment decisions in response to domestic macroeconomic news. Such news are important because they inform agents about the state of the economy and may send signals about likely future changes in monetary policy. The chapter shows that the strength of the latter crucially depends on the Fed's guidance.

While the Fed was conveying the idea that its zero-rate policy would have lasted for a long time, positive news about the U.S. labor market induced U.S. investors to seek financial exposures in emerging market economies. Conversely, when the Fed started hinting at a progressive 'normalization' of policy, the same news led investors to move capital away from emerging markets. These non-linearities are evidence of the Fed's role in shaping investors' expectations. Its reliance on time-based guidance deprived macroeconomic releases of any information about future policy. Positive news then boosted the domestic stock market and depressed risk aversion, two effects that partly explain the flow of investments towards emerging markets. By contrast, in the lead-up to policy normalization, the Fed put increased emphasis on incoming data to decide the timing of normalization. Positive news fueled expectations of firmer policy and led to a tightening of financial conditions, inducing U.S. investors to repatriate capital. The findings of Chapter 2 point to a large role of unconventional monetary policies in shaping investors' expectations.

The dissertation then moves on to fiscal policy. Fiscal policy contributed to dampening the immediate recessionary effects of the GFC. However, in later years many governments in advanced economies started to adjust their policy stance, due to increasing investors' concerns about the stability of public finances (Greece, Ireland, Italy, Portugal, and Spain) or the need to adhere to international obligations (e.g., Germany, and the Netherlands). Many commentators linked fiscal austerity to increasing economic inequality. However, the effects are not theoretically clear. As an example, a reduction in government social spending might have negative direct effects on the least well-off. On the other hand, the effects of raising taxes should depend on which groups of agents are targeted by the new policy. Indeed, while the literature seems to agree that 'spending-based' consolidations have adverse effects on income distribution, the evidence on the effects of 'tax-based' consolidations is more mixed.

Chapter 3 sheds light on these issues. It uses a multi-equation set-up to investigate the medium-term effects of tax-based consolidations (meaning episodes in which tax hikes are larger than spending cuts) on income disparity. This empirical framework allows exploring the response of potential inequality drivers, such as economic conditions and labor market outcomes. The results point to a positive effect of tax-based consolidations in improving income distribution. An analysis by the components of taxation suggests that indirect taxes are mostly responsible for this result. The estimates also show that higher indirect taxes lead to a substantial contraction in economic activity, suggesting that part of their positive effects on income distribution may be ascribed to the business cycle. However, the response of labor market variables also points to a positive labor supply channel of indirect taxes. These raise the price of the consumption basket and induce middle-aged women — who are typically more sensitive to changes in economic incentives — to increase their labor force participation.

The analysis carried out in Chapter 3 thus suggests that increases in tax revenues in the aftermath of the GFC most likely did not contribute to increasing inequality. On the contrary, they might have lowered it. Still, in designing future consolidations, policymakers should consider the equity-efficiency trade-offs of different tax instruments. Raising indirect taxes does have positive effects on income equality but also induces larger economic contractions than direct taxes.

Chapter 4 turns the focus on labor market regulation. Administrative requirements, monetary costs and other impediments on the hiring, firing and managing of workers are often cited as factors both preventing an efficient re-allocation of resources and holding up new investments. To boost productivity and regain competitiveness in the aftermath of the Crisis, many governments of Euro Area countries (e.g., France, Greece, Italy, the Netherlands, Portugal, Spain) lifted some of the restrictions faced by employers when adjusting the labor input. However, these interventions might have other unintended effects. The chapter investigates whether episodes of employment protection deregulation have had detrimental effects on the distribution of income between labor and capital.

By reducing the costs of dismissal, deregulation affects the bargaining power between workers and employers in favor of the latter. The chapter starts with two very stylized bargaining models. In the first, employers and workers bargain over the wage and employers then set the employment level taking the wage as given. Deregulation lowers wage rents and — if the implied substitution of labor for capital is not large enough (that is, the elasticity of substitution is lower than one) — leads to a decrease in the labor share of income. Under the second model, bargaining takes place over both wage and employment. The employment level is pinned down by the workers' reservation wage (e.g., the unemployment benefit). Deregulation then lowers wage rents and unambiguously decreases the labor share of income. The rest of the chapter takes these insights to the data.

The analysis uses a 'narrative' dataset of job protection deregulation episodes in a panel of advanced economies and relies upon two databases to measure labor shares, at both the country-time and the country-industry-time level. Since labor market reforms are done at the country-level, the industry-level analysis relies upon two identification strategies based on theory. Namely, it is assumed that job protection legislation reforms should have larger effects on the labor share in industries where it is more binding (that is, where employers need to adjust the workforce more frequently) and where the elasticity of substitution is lower than one. The results show that deregulation indeed had negative effects on the labor share. Using aggregate data, the adverse effect of a major liberalizing reform is found to stabilize at about half of a percentage point after five years. The industry-level analysis confirms that deregulation had larger negative effects on the labor share in industries with a lower elasticity of substitution between labor and capital and a higher 'natural' layoff rate. This chapter also contributes to the existing literature by highlighting another factor that might have contributed to the decline in labor shares observed across a broad range of advanced and emerging market economies in recent decades.

Chapter 5 takes a somewhat different perspective from the other ones. It is motivated by one of the effects of the GFC, namely the great rise in youth unemployment. Youth unemployment rates have increased to unprecedented levels — sometimes to more than fifty percent (e.g., Greece and Spain) — in the early 2010s. Although such increase mostly characterized advanced economies, high youth unemployment rates are also an issue in several emerging market economies. The chapter revisits the Okun's Law, which posits that there exists a negative relationship between a country's aggregate demand conditions and its unemployment rate, by testing its validity across different demographic groups and over two panels of countries, one of advanced and the other of emerging market and developing economies.

The analysis uncovers a large degree of heterogeneity in the strength of the Okun's Law. The difference between the unemployment rate and its equilibrium rate (the unemployment gap) for young workers is generally twice as sensitive to the cycle as it is for adults. Women's unemployment gap is significantly less sensitive to demand conditions than men's in advanced economies. The cyclical variation in the unemployment rate is also more sensitive to the cycle in advanced economies, possibly owing to a larger prominence of informal labor markets in emerging market and developing economies, which gives workers the possibility to also transition between formal and informal employment rather than just between formal employment and unemployment.

The chapter also considers some extensions to these core results. Procyclicality of labor force participation generally leads to an unemployment rate gap response that is smaller, in absolute value, than that of the employment gap (defined as the cyclical component of the employment level). Moreover, the magnitudes of labor force participation and employment sensitivities to the cycle differ widely across demographic groups, revealing even greater heterogeneity than for the unemployment gap. For example, the cyclical sensitivity of employment is about five times larger for young men than for adult women in advanced economies. The analysis also covers differences in unemployment gap sensitivities over different stages of the business cycle, with periods of negative output gaps driving stronger responses in the unemployment gap than periods of positive output gap. All in all, the results of this chapter point to the importance of demographic compositional differences across countries that may underlie aggregate cyclical sensitivities.

Chapter 2

Capital Flows Spillovers of U.S. Macroeconomic News: the Role of Fed's Policies

2.1 Introduction

In the early months of 2018, some emerging market economies (EMs) experienced rising financial stress, with Argentina even turning to the International Monetary Fund for assistance. According to data from EPFR, in May 2018 EMs-focused investment funds experienced the first monthly outflow since December 2016. Some commentators have linked these jitters to expectations of firming monetary policy rates in the United States (U.S.) (Authers, 2018; FT View, 2018).

A rich literature offers evidence that U.S. monetary policy actions do have effects on EMs asset prices (Aizenman, Binici, and Hutchison, 2016; Eichengreen and Gupta, 2015) as well as capital flows from advanced economies (Ahmed and Zlate, 2014; Fischer, 2015). However, the reassessment of expected U.S. monetary policy of early-2018 seems to have been caused by a string of positive news regarding the U.S. economic outlook rather than new announcements. This paper contends that domestic macroeconomic news are an important driver of cross-border flows and that their effect crucially depends on the stance of the Federal Reserve (Fed). Quite surprisingly, these two issues have been largely ignored by the literature.

We focus on the post-Great Financial Crisis (GFC) period and employ the local projection method to trace out the dynamic (4-week) effects of U.S. employment announcements on portfolio capital flows from the U.S. to EMs. We show that when the Fed complemented its zero rate policy with time-based forward guidance positive news induced inflows. Conversely, as the Fed progressively switched to a more data-based guidance, positive news led investors to move capital away from EMs. In a second step, we uncover the channels underlying these non-linear responses. The Fed's reliance on time-based guidance deprived macroeconomic releases of any risk-free rate information content. Instead, positive news boosted stock prices and depressed the VIX (a measure of volatility), which explains the flows of capital towards EMs. By contrast, in the lead-up to the rate liftoff, the Fed put increased emphasis on incoming data to decide the timing of normalization. Positive news fueled expectations of firmer policy and led to a tightening of financial conditions, inducing investors to repatriate capital.

Unlike most previous studies of portfolio capital flows, we do not carry out the analysis using aggregate, country level, data. Rather, we rely upon micro data at the investment fund-level as put together by EPFR. This source has been widely used in the literature. However, most contributions only employed its country flow database (Fratzscher, 2012; Fratzscher, Lo Duca, and Straub, 2018; Koepke, 2018; Li, Haan, and Scholtens, 2018). Through the EPFR's fund flow database we obtain information on investors' allocations into more than 750 investment funds — totaling about $400 billion of assets — legally domiciled in the U.S. and investing in emerging, frontier and other market economies (for simplicity we collectively refer to them as EMs).

The key advantage of the fund-level data is that it allows distinguishing between mutual and exchange-traded funds (ETFs). Through a difference-in-differences (diff-in-diff) analysis, we show that ETFs play an essential role in the international transmission of shocks. Flows in and out of ETFs account for almost all of the response to U.S. employment announcements described above. Likely, this is due to ETFs being used by more short-term oriented and less risk-averse investors, who find in them an easy way to hold liquid positions in different sets of markets. Given their rising popularity as an investment vehicle, our results suggest that ETFs might have made the financial markets in EMs more sensitive to external shocks, potentially amplifying the global financial cycle.[1]

This analysis builds upon recent advancements in the capital flows literature. IMF, 2011 highlighted a new wave of capital flows after the GFC, to which portfolio flows had contributed for about 50 percent, much more than previous historical episodes. This study inspired a burgeoning literature. Fischer, 2015 and Koepke, 2018 document the expected level of the U.S. policy rate to have adverse effects on flows. The role of financial market volatility as a global push factor driving flows in and out of EMs is emphasized in Miranda-Agrippino and Rey, 2015 and Forbes and Warnock, 2012 among others. Rey, 2015 shows the existence of a global financial cycle, which is driven by monetary policy in the U.S. and in which asset prices co-move. Li, Haan, and Scholtens, 2018 study fund flows surges and find that these depend on global factors, including U.S. equity returns and the VIX index.

Many studies analyze the effects of the Fed's unconventional policies. For instance, Ahmed and Zlate, 2014 find large positive effects of such policies on portfolio flows. Using EPFR data, Fratzscher, Lo Duca, and Straub, 2018 find that the Fed's quantitative easing policies triggered outflows from bond funds and inflows into EMs equity funds. These authors also argue that unconventional monetary policies in advanced economies have generally magnified the procyclicality of capital flows.

Only Fratzscher, 2012 touches upon the role of U.S. macroeconomic news. This author focuses on the November 2005 to October 2010 period and also finds coefficients

[1] The asset share of ETFs in the overall U.S. industry is now at over 50 percent.

of opposite sign, positive for the GFC period and negative for the rest of the sample. Fratzscher, 2012 explains this non-linearity arguing that in crisis times negative U.S. news reduce investors' risk tolerance and cause a flight-to-safety reaction, with capital fleeing EMs. Differently from Fratzscher, 2012 the non-linearities that we uncover are not typical of crises, cannot be explained by flight-to-safety behaviors, and can only be rationalized looking at changes in the Fed's stance. Non-linear effects of global push factors are also emphasized in Nier, Sedik, and Mondino, 2014 who find that the importance of the VIX in driving capital flows increases in its level.

Finally, the behavior of ETFs is only investigated by Converse, Levy-Yeyati, and Williams, 2018, who are also among the few researchers to conduct the analysis using data at the investment fund-level. These authors study the response of flows towards EMs to changes in global volatility and find that flows through ETFs respond about 1.5 times more than those through mutual funds.

This paper makes two novel contributions. First, it documents large and non-linear effects of U.S. macroeconomic news on cross-border portfolio capital flows and relates them to changes in the Fed's stance. This uncovers new specificities of the global financial cycle and highlights a further dimension through which U.S. monetary policy influences it. Second, it sheds some light on the role of ETFs in the transmission of shocks across borders. These funds have witnessed an exponential growth since their inception in 1993, and their high responsiveness to global factors might explain the increasing importance of portfolio flows as a share of total capital flows observed after the GFC (IMF, 2011).

The non-linearities uncovered in our analysis are consistent with recent papers showing that forward guidance has improved the agents' understanding of the central bank's reaction function (see Femia, Friedman, and Sack, 2013 and Engen, Laubach, and Reifschneider, 2015). Following the GFC — with policy rates at the effective lower bound (ELB) — forward guidance was meant to signal the persistence of large economic slack, which warranted a much slower policy normalization relative to previous historical episodes (Kohn, 2018). That contributed to a lower and more stable expected path of future policy rates (Swanson, 2017) and dampened the sensitivity of market interest rates to news (Feroli et al., 2017). On the other hand, the communication stance adopted by the Federal Reserve during the normalization period was more data-dependent. It was not meant to stabilize market expectations and conveyed the importance of 'under what circumstances' rather than 'when' interest rates would have moved.

The rest of paper is structured as follows: Section 2.2 briefly illustrates the rationale for central banks to adopt forward guidance policies, reviews the Fed's experience in this context and draws some implications for the current analysis. Section 2.3 presents the dataset used to carry out the empirical analysis. Section 2.4 is dedicated to the baseline fund-level analysis. It presents the methodology, discusses the results and presents some extensions. Section 2.5 zooms in on the role of ETFs in the transmission of shocks. Section 2.6 concludes by outlining some avenues for future research.

2.2 Forward Guidance: Rationale, Practice and Implications

This section is divided into three parts. The first one starts by briefly recapping the basic principles behind the usage of forward guidance as a monetary policy tool, while the second reviews how it has evolved in the U.S. after the GFC. The third part wraps things up by discussing how forward guidance might affect the response of capital flows to macroeconomic news and draws some implications for the empirical analysis.

2.2.1 Forward Guidance as a Monetary Policy Tool

Monetary theory holds that the central bank should target a certain level of the market real interest rate at which savings equal investments (defined as the equilibrium rate). Before the GFC, central banks in advanced economies used to set the policy rate to steer the market nominal rate towards a level that would be conducive to the estimated equilibrium rate.

To see that, it is useful to recall the expectation theory of the term structure of interest rates. This posits that in absence of default risks the market interest rate depends on the current and expected future policy rate according to the following formula:

$$i_{k,t} = \frac{i_t + i^e_{t+1} + i^e_{t+2} + ... + +i^e_{t+k}}{k} + l_{k,t} \tag{2.1}$$

where $i_{k,t}$ is the interest rate with maturity k prevailing at time t; i_t is the policy rate set by the central bank; i^e_{t+i} (with $i = 0,\ 1, .., n$) is the expectation at time t for its future level in period $t + i$; and $l_{k,t}$ is the term premium (that is the premium that investors require to hold long-term assets rather than rolling over short-term assets).

By changing the level of the policy rate the central bank directly influences the market interest rate through i_t. In exceptional circumstances, because either expected inflation, the equilibrium rate or both are very low, the central bank might desire to set a policy rate below zero. However, this is complicated as agents would rather hoard cash than lending at a negative rate. Hence, central banks have recently experimented with other ways to steer market interest rates lower without setting a negative interest rate. Specifically, they started sending rather explicit signals about the direction of future monetary policy (that is, acting on i^e_{t+i}), and affecting the quantity of long-term assets held by the private sector (that is, acting on $l_{k,t}$). These two 'unconventional' policies are commonly referred to as forward guidance and quantitative easing (QE) respectively.

Through QE, the central bank buys debt securities from the private sector in exchange for central bank reserves. To the extent that debt securities and reserves are not perfectly substitutable, the reduction in supply contributes to lower the liquidity premium $l_{k,t}$ that investors require to hold debt securities, thus depressing their yields (see Gagnon et al., 2011, among others, for evidence of this so-called portfolio balance channel of QE). QE might also affect the expected future short-term policy rate (i^e_{t+i}). The mere fact that the central bank decides to carry out QE may convey implicit signals about its assessment of the state of the economy and consequently about future monetary policy. In other words, QE may indicate that monetary policy will remain accommodative for a long period (see Bauer and Rudebusch, 2014 for evidence).

Forward guidance consists instead of giving explicit signals about future policy through official statements and speeches. There exist two main types of forward guidance: about the likely future path of the policy rate (policy rate forward guidance) and about the future evolution of the central bank's balance sheet (balance sheet forward guidance). Since QE partly works by lowering the expected future policy path, balance sheet forward guidance can itself have significant effects on the expected future policy rate.

The Bank of England and the Federal Reserve are two major central banks to have adopted QE. Importantly, Christensen and Rudebusch, 2012 find that the signaling channel of QE was stronger in the U.S. relative to the United Kingdom and claim that this might be due to the different forward guidance given by the two respective central banks. As it announced QE, the U.S. Federal Reserve also provided policy rate forward guidance. Market participants might have seen QE announcements as reinforcing this guidance, thus strengthening the idea of 'low rates for long.' In contrast, the Bank of England refrained from issuing rate guidance when announcing its debt purchase program. That might have dampened the strength of the QE's signaling channel. More concrete examples of the Fed's forward guidance policies are given in the next section.

2.2.2 The Experience of the Federal Reserve

Here we briefly review the evolution of forward guidance in the U.S. following the GFC. Table 2.1 provides a summary of the key statements made by the Federal Reserve, while a more in-depth review is given in Feroli et al., 2017.

In December 2008, the Federal Open Market Committee (FOMC) lowered the target range for the federal funds rate (its policy rate) to 0-0.25 percentage points, essentially its lower bound. It also introduced *open-ended* forward guidance for the first time. Precisely, it stated that "*economic conditions [were] likely to warrant exceptionally low levels of the federal funds rate for some time*" (FOMC, 2008). This open-ended guidance was modified in March 2009, as the FOMC replaced the expression 'some time' with "*an extended period*" (FOMC, 2009). The Fed reiterated this statement until July 2011.

In March 2009, the FOMC also announced an expansion of its QE program, which it had introduced in December 2008. The combination of the 'extended period' language and the announcement of QE's expansion lowered the expected path of policy rates. Conversely, the repetition of the 'extended period' language in later statements and the announcement of another round of QE (November 2010) did not seem to have had major effects on market expectations (Kohn, 2010).

Forward guidance experienced a quantum leap in the summer of 2011. Survey as well as market-based data were suggesting that market participants expected an increase of the policy rate in about a year ahead. The FOMC instead planned not to raise the policy rate for a much longer period. To realign expectations, the Fed stated that "*economic conditions [... were] likely to warrant exceptionally low levels for the federal funds rate at least through mid-2013*", thus providing an exact calendar date for the possible duration of its zero-rate policy (FOMC, 2011). The exact wording was subsequently modified in January and September 2012 to keep a guidance indicating likely low rates for about two years (see Table 2.1 below). Moessner, 2013 estimated that each of these announcements had an average depressing effect of 14 basis points on 3-year ahead future policy rates.

In September 2012 the Fed also announced the third phase of QE and committed to buying securities worth about $40 billion each month for an indeterminate period, thus reinforcing the calendar-based guidance. The open-ended nature of this round of QE led market participants to nickname it as Q-infinity, and some seemed to lose sight of the conditions for ending it (Feroli et al., 2017).[2]

In December 2012, the FOMC removed calendar-based guidance and introduced the so-called 'threshold-based' guidance. It stated that the zero-rate policy would have been appropriate at least until the unemployment rate had remained above 6.5 percentage points (1.4 points less than the actual rate). Although the Fed dropped the time-based dimension, it stated that it viewed this threshold "*as consistent with its earlier time-based guidance*" (FOMC, 2012a). The FOMC just wanted to communicate its determinacy to achieve its goals. In line with this interpretation, it announced an expansion of QE worth an additional $45 billion of monthly purchases.

It was only in May 2013 that market expectations began to change. As economic conditions continued to improve, the FOMC first stated that it was "*prepared to increase or reduce the pace of its purchases to maintain appropriate policy accommodation as the outlook for the labor market or inflation changes*" (FOMC, 2013a). That set the stage for a key speech given by Ben Bernanke later in the month (the taper tantrum speech). The then Fed's chairman declared that "*in the next few meetings, [the FOMC] could take a step down in [the] pace of [bond] purchase*" (Bernanke, 2013). In subsequent meetings, the FOMC fine-tuned its balance sheet forward guidance to clarify that QE would continue "*until the outlook for the labor market [had] improved substantially in a context of price stability*" (FOMC, 2013b). In the end, the beginning of the reduction of QE was formalized in December 2013.

2 In November 2008, the FOMC announced the purchase of $600 billion in agency mortgage-backed securities (MBS) and agency debt. That constituted the first round of QE (QE1), and it was expanded on 18 March 2009 to include additional purchases of $750 billion in agency MBS and agency debt as well as $300 billion in U.S. government bonds. QE1 ended in March 2010. In November 2010, the FOMC announced the second round of QE (QE2), worth $600 billion of longer-dated Treasury purchases.

Table 2.1: Federal Reserve's policy rate forward guidance – a timeline

	OPEN-ENDED
12/16/2008	"economic conditions are likely to warrant exceptionally low levels of the federal funds rate for **some time**" (FOMC, 2008)
03/18/2009	"economic conditions are likely to warrant exceptionally low levels of the federal funds rate for an **extended period**" (FOMC, 2009)
	CALENDAR-BASED
08/09/2011	"economic conditions [...] are likely to warrant exceptionally low levels for the federal funds rate at least **through mid-2013**" (FOMC, 2011)
01/25/2012	"economic conditions [...] are likely to warrant exceptionally low levels for the federal funds rate at least **through late 2014**" (FOMC, 2012b)
09/13/2012	"exceptionally low levels for the federal funds rate are likely to be warranted at least **through mid-2015**" (FOMC, 2012c)
	THRESHOLD-BASED
12/12/2012	"this exceptionally low range for the federal funds rate will be appropriate at least **as long as the unemployment rate remains above 6-1/2 percent**, inflation between one and two years ahead is projected to be no more than a half percentage point above the Committee's 2 percent longer-run goal" (FOMC, 2012a)
12/18/2013	"it likely will be appropriate to maintain the current target range for the federal funds rate **well past the time that the unemployment rate declines below 6-1/2 percent**" (FOMC, 2013a)
	NORMALIZATION
03/19/2014	"it likely will be appropriate to maintain the current target range for the federal funds rate for a **considerable time** after the asset purchase program ends" (FOMC, 2014)
01/28/2015	"based on its current assessment, the Committee judges that it can be **patient** in beginning to normalize the stance of monetary policy" (FOMC, 2015c)
04/29/2015	"The Committee anticipates that it will be **appropriate** to raise the target range for the federal funds rate when it has seen further improvement in the labor market" (FOMC, 2015a)
	POST-LIFTOFF
12/16/2015	"In light of the **current shortfall of inflation** from 2 percent, the Committee will carefully monitor actual and expected progress toward its inflation goal." (FOMC, 2015b)
03/15/2017	"The Committee will carefully monitor actual and expected inflation developments relative to its **symmetric inflation goal.**" (FOMC, 2017)

Notes: in the periods between older and newer sentences, the FOMC repeated older sentences.

At the same time, the FOMC changed its policy rate forward guidance to convey the idea that any increase in the policy rate would have only happened following the tapering of QE. First, it stated that it would have likely maintained the zero-rate policy "*for a considerable time*" after the end of QE (FOMC, 2014). When bond purchases were completely halted (October 2014), it stated that it would have been "*patient*" in beginning to normalize policy (FOMC, 2015c). In April 2015, it spelled out some conditions for when it would have been "*appropriate*" to raise the federal funds rate. These crucially included a reference to "*further improvement in the labor market*" (FOMC, 2015a). The first increase in the policy rate finally materialized in December 2015. At that point, the U.S. economy had created 6.5 million jobs since Bernanke's taper tantrum.

Concurrently to the first policy rate hike since the GFC, the FOMC signaled a partial shift away from the labor market in its policy reaction function. Precisely, it qualified the distance of inflation from its objective as a "*shortfall*" and added that going forward it would have "*carefully monitor[ed] actual and expected progress toward its inflation goal*" (FOMC, 2015b). The Fed increased the policy rate in five more occasions from the time of the first liftoff until the end of May 2018, one in 2016, three in 2017 and one in early 2018.

The next section discusses how forward guidance may affect the sensitivity of cross-border capital flows to macroeconomic announcements and focuses on the experience of the Federal Reserve to draw some implications for the empirical analysis.

2.2.3 Implications for the Analysis

How can forward guidance affect the response of capital flows to macroeconomic announcements? Consider a world where each country operates its independent monetary policy and investors shift capital across borders to chase the highest returns. To the extent that domestic releases reveal new information about inflation and/or output, in normal times they should affect the market expectations of future policy. News indicating higher inflation or consumer demand increase the probability that the central bank will raise the policy rate. That reduces the foreign-to-domestic rate differential and leads investors to shift their portfolios towards domestic assets. Forward guidance is relevant in that it informs investors on how the central bank will respond to fluctuations in economic activity.

Under time-based guidance, the central bank conveys the idea of low rates for long. If it is credible, market expectations for the future policy rate should not be affected by new macroeconomic data. Capital flows should then be less sensitive, or not sensitive at all, to domestic announcements. Conversely, under data-based guidance, incoming data is important to assess future policy. Signals of a stronger domestic economy should lead to a higher expected rate and repatriation of capital from foreign markets.

The Fed made extensive use of communication in the aftermath of the GFC. It started with a soft form of time-based guidance by stating that the policy rate would have remained close to zero for an undetermined period (March 2009). This was successively backed up by a quasi-promise to keep interest rates low for about two calendar years (August 2011). The FOMC started transitioning from time- to data-based guidance when it switched to threshold-based guidance (December 2012). The switch was completed when it conditioned the phasing down of QE on improvements in the labor market (September 2013).

Based on this narrative we distinguish between the following four different Fed's policy regimes:

A. the open-ended forward guidance regime (March/18/2009 to August/8/2011)
B. the calendar-based forward guidance regime (August/9/2011 to May/22/2013)
C. the normalization guidance regime (May/23/2013 to December/12/2015)
D. the post-liftoff regime (December/13/2015 to May/30/2018).[3]

A and B are both periods of time-based guidance. We analyze them separately since calendar-based guidance might have had stronger effects on market expectations. As cutoff between periods B and C, we opt for Bernanke's taper tantrum. Finally, we distinguish among a normalization and a post-liftoff regime since capital flows may display an extra sensitivity to monetary policy during liftoff periods (Ahmed, 2015).

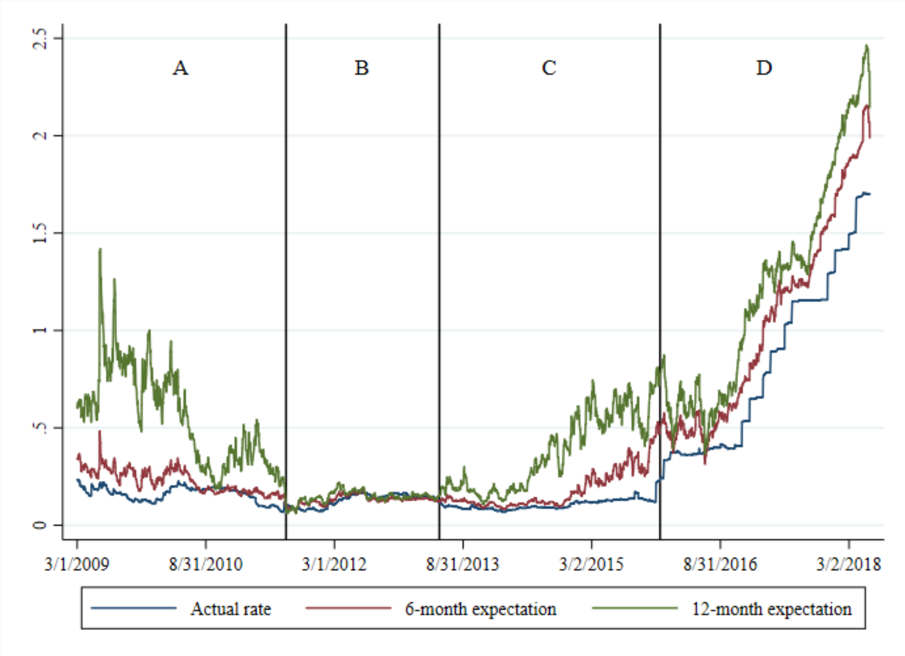
Figure 2.1 provides anecdotal evidence that the Fed did manage to affect market expectations through forward guidance. It plots the level of the federal funds rate as well as the 6- and 12-month ahead expected value during the post-GFC period. Vertical lines denote the key changes in the Fed's communication framework identified above. Strikingly, in the calendar-based period (denoted by B) market participants did not expect any increase in the policy rate for at least one year. Following Bernanke's taper tantrum, first the 12-month ahead and then also the 6-month ahead future rate started to increase relative to the actual rate. In the rest of the paper, we will investigate whether the response of capital flows to news was also affected by these events.

2.3 Dataset

We now explain the construction of the dataset. The section starts with the data used to measure capital flows. It proceeds with the macroeconomic announcement that we focus on and finishes with a description of further country-specific variables used to carry out some extensions.

[3] Although the Federal Reserve issued forward guidance already in November 2008, we choose the introduction of the extended-period language (March/18/2009) as the starting date of the analysis. In this way the sample entirely excludes the GFC period.

Figure 2.1: Evolution of the actual and future expected federal funds rate during different Fed's guidance regimes



Notes: the figure shows the federal funds rate as well as its 6-month and 12-month ahead expected future rate during the March 2009 to May 2018 period. Vertical solid lines denote key Fed's guidance changes. The first, second and third leftmost vertical lines denote the introduction of calendar-based forward guidance (August/9/2011), the taper tantrum (May/22/2013) and the first rate liftoff (December/13/2015) respectively. The windows denoted by A, B, C and D respectively indicate the open-ended guidance, calendar-based guidance, normalization guidance, and post-liftoff regimes.
Sources: Thomson Reuters Datastream

2.3.1 Cross-border Portfolio Flows

To proxy for cross-border portfolio flows, we rely on data about investors' allocations into EMs bond and equity investment funds compiled by EPFR. The two main variables of interests are fund flows ($f_{i,t}$) and initial assets ($a_{i,t}$). The former gauges the net \$ purchase of shares in fund i at time t, while the latter measures the fund's assets under management at the beginning of the period. We also collect data on funds' return, meaning the percent period-on-period change in net asset value (NAV), to be used as a control.[4]

The data are available at different frequencies (daily, weekly and monthly). We opt for the weekly frequency.[5] That permits to identify relatively well the effect of macroeconomic news, while at the same time its coverage is still fairly representative of the overall fund industry. The sample ranges from January 2007 to May 2018. The panel is unbalanced, with funds entering and leaving the sample as they are established or liquidated.

[4] The net asset value change excludes asset changes due to new inflows.
[5] The week is defined as to start on Thursday at the beginning of the U.S. trading day.

Although funds are not required to report to EPFR, the platform has rather good coverage. Open-ended investment funds managed more than $49 trillion in assets at the end of 2017, which was about 23 percent of all worldwide debt and equity markets (ICI)). The funds reporting to EPFR had a total of $30.7 trillion of assets. EPFR's coverage of US-domiciled funds is even larger, with assets managed by reporting funds being 92 percent of the total. Not all funds report at the weekly frequency, however, but those that do still account for a sizable share of the overall industry's assets (about 42.5 percent).

Using the EPFR dataset has other advantages. Being it at the fund-level, it allows observing important fund-specific characteristics such as the geographical investment destination as well as the legal domicile. The latter is particularly important since by selecting only funds that are domiciled in the U.S. we obtain a good proxy of bilateral (gross) capital flows from the U.S. to EMs. Another useful information concerns the type of funds, whether they invest in equity or debt and whether they are exchange-traded or mutual funds (the main difference being that the former are traded continuously throughout the day on secondary markets, which make them more liquid).

We clean the data following standard procedures. We exclude funds that have life of less than one year. We also drop funds with less than $10 million assets under management on average.[6] Finally, we exclude observations with abnormal jumps, defined as having flows larger or smaller than one-third of assets. After cleaning, we are left with a panel comprising 753 US-domiciled funds and about 250 thousand observations. Appendix A.1 discusses relevant descriptive statistics and provides some stylized facts.

A concern with using EPFR data to measure portfolio capital flows is that they only capture flows through investment funds. For a sensitivity analysis, we also collect weekly data on all types of cross-border portfolio capital flows to EMs as compiled by the IIF. Another methodological difference relative to EPFR is that, while fund flows are a gross measure of cross-border flows, the IIF estimates net flows (that is, gross flows from country A to country B minus gross flows from B to A). The main limitation relates to country coverage. IIF provides overall, rather than bilateral, capital flow data. Moreover, destination countries are just a few large EMs.

6 For all the funds that enter the panel after the starting date of the sample, we exclude the first four observations. New funds typically raise capital for a period lasting from some weeks to a few months before starting to invest. Hence, when a fund enters the panel, EPFR records assets as being equal to 0 and flows equal to the capital raised. This causes unreasonably large outliers in our variable of interest (which is flows as a share of the beginning of period assets). Similarly, when a fund leaves the panel, EPFR records negative flows equal to the assets under management at the end of the preceding period. Therefore, we also exclude the last four observations of the funds exiting the sample.

2.3.2 Macroeconomic Releases and Other Data

To study the reaction of flows to macroeconomic news, we focus on one specific announcement: the net change in non-farm payroll (NFP) employment, released the first Friday of every month by the BLS. Existing literature has found this to be the most important macroeconomic announcement for financial markets worldwide. The reason is that NFP employment figures are closely watched by the FOMC when making monetary policy decisions. This is epitomized by a famous quote by former Chairman Alan Greenspan: "*Everything we've looked at suggests that it's the payroll data which are the series which you have to follow*" (2004). Below we discuss why this is the case.

The Federal Reserve has three main objectives, maximum employment, stable prices, and moderate long-term interest rates. Therefore, the NFP release provides direct information on how far the Federal Reserve is from reaching the employment-related part of its mandate and gives hints about the likelihood of future changes in monetary policy. Moreover, the less is the degree of labor market slack the more workers are likely to demand higher wages, which should ultimately lead to higher inflation. Hence, NFP data can also be useful to gauge the strength of domestic price pressures.[7]

NFP releases are relevant also for other reasons. First, the NFP series moves very close to the overall economy (at this respect see the evolution of NFP over the January 2011 to May 2018 period depicted in Figure A.5 in Appendix A.2). Thus, its importance goes beyond the labor market, and indeed the NFP series is one of the key indicators used by the NBER to determine whether the economy is in an expansion or a recession. Second, as they are released with just a few days lag relative to the end of the month, NFP announcements are very timely. As shown in Gilbert et al., 2017 this property is crucial to determine the financial market relevance of a macroeconomic announcement. These authors find that NFP data explain about 25 percent of the variation in U.S. government bond yields in days they are released, whereas other macroeconomic news have barely any effect. Their importance is also confirmed in earlier empirical studies (Beber and Brandt, 2009; Faust et al., 2007; Swanson, 2017).

[7] The unemployment rate is another important labor market-related release. Crucially, however, NFP data are considered by Federal Reserve's officials to provide a more accurate picture of the state of the labor market. The unemployment rate is derived from a 60 thousand households survey, which counts the number of employed individuals, whereas NFP data come from a 400 thousand business establishments payroll survey and count the net number of jobs created. The difference between employed individuals and jobs is important as the former includes self-employment and unpaid family workers. In most cases, these are low-paying alternatives to wage and salary work and therefore tend to behave counter-cyclically. That is, they increase during recessions as the prospects of finding a job is lower, and they decrease during expansions as the same likelihood goes up. Hence, changes in the number of employed individuals might send mixed signals about the labor market strength (for a more in-depth review see Wu, 2004).

Since market participants form expectations about upcoming announcements, we follow standard practice in the literature and identify the 'surprise' component by relying on Bloomberg, which surveys economic analysts about their forecast. We then subtract the median response from the actual release to obtain a measure of the unexpected component. In practice, we construct the following variable:

$$news_t = \frac{(r_t - E[r_t])}{\sigma_R}$$

with r_t being the actual release; $E[r_t]$ the median survey response; and σ_x the unconditional standard deviation of the surprise component $r_t - E[r_t]$.

Since the analysts surveyed by Bloomberg can update their answer until the day of the announcement, it is unlikely that they receive new information between the moment in which they report (update) their forecast to the moment in which the release takes place. Confirming this, the NFP surprise series (plotted in Figure A.6 in Appendix A.1) behaves as white noise, suggesting that it effectively captures the unanticipated component. We formally verify that this is the case by regressing it on the Bloomberg's median expectation ($E[r_t]$). The result from this simple test indicates that the forecast error is indeed orthogonal to the analysts' information set.[8] Another potential concern is that macroeconomic announcements tend to have less impact on asset prices when analysts' disagreement is higher (Pericoli and Veronese, 2015). We therefore collect data on the forecast standard deviation for a sensitivity analysis.

Besides NFP, we also collect data on other U.S. as well as foreign macro releases. For the U.S., we consider the ISM PMI manufacturing index, the unemployment rate, the consumer price index, and retail sales. As for foreign releases, we focus on those from other largest advanced economies, namely the U.K., Japan, Germany, and the Euro Area. Table A.3 in Appendix A.2 contains a list of all the macroeconomic news we consider, together with relevant descriptive statistics and data sources.

Macroeconomic announcements may affect attitude towards risk, for instance, by altering perceptions about the state of the economy. Since the literature has identified risk appetite as an important capital flows driver (Miranda-Agrippino and Rey, 2015 and Forbes and Warnock, 2012), we collect additional data on the VIX (a measure of volatility) to investigate this potential channel. For similar reasons, we source data on the value of the U.S. stock market and the 6-month ahead expected future federal fund rate. All series are retrieved from Datastream.

[8] In practice, we estimate the following regression: $news_t = \alpha + \beta E[r_t] + \varepsilon$. The F-test statistics is 1.27, thus rejecting the null hypothesis that $\alpha = \beta = 0$.

2.4 Baseline Analysis

We carry out the empirical analysis in two stages. We start by estimating how flows into the average investment fund respond to U.S. employment news. We then zoom in on the role of ETFs through a difference-in-differences (diff-in-diff) analysis.

2.4.1 Methodology

For the econometric analysis, we rely on the local projection method (Jordà, 2005). This is similar in spirit to the event study approach (usually used to investigate the static effects of macroeconomic announcements on financial variables). But it allows exploring dynamic effects in a rather compact format. The horizon considered is a 4-week window, including the week of the release and the following three. Another advantage of the local projection method is that it suits very well the estimation of non-linearities. We will exploit this feature in the next section.

Employing local projections entails regressing the cumulative flows over the $t+k$ horizon onto the surprise series at time t. Specifically, for each $k = 0, 1, 2, 3$, we estimate the following equation:

$$100 * \frac{\sum_{j=0}^{k} f_{i,t+j}}{a_{i,t}} = \sum_{r=1}^{4} \beta_k^r d_t^r news_t + A_k D_t + B_k Z_{i,t} + \gamma_i + \varepsilon_{i,t} \tag{2.2}$$

where $f_{i,t}$ denotes investors' \$ allocations into fund i at time t; $a_{i,t}$ is the volume of assets under management by fund i at beginning of period t (also in \$); d_t^r, with $r = 1, .., 4$, are four dummy variables each taking value 1 during a different Fed's guidance regime and 0 otherwise; $news_t$ is the NFP variable, taking value equal to the surprise component in weeks in which there is a release and 0 otherwise; D_t is a vector with the four d_t^r dummies (to allow for regime-specific intercepts); $Z_{i,t}$ is a vector comprising twelve lagged values of the one-period flows and net asset value change, both in percentage of fund's assets; γ_i are investment fund fixed effects; and $\varepsilon_{i,t}$ is the error term, assumed to be uncorrelated to the regressors.

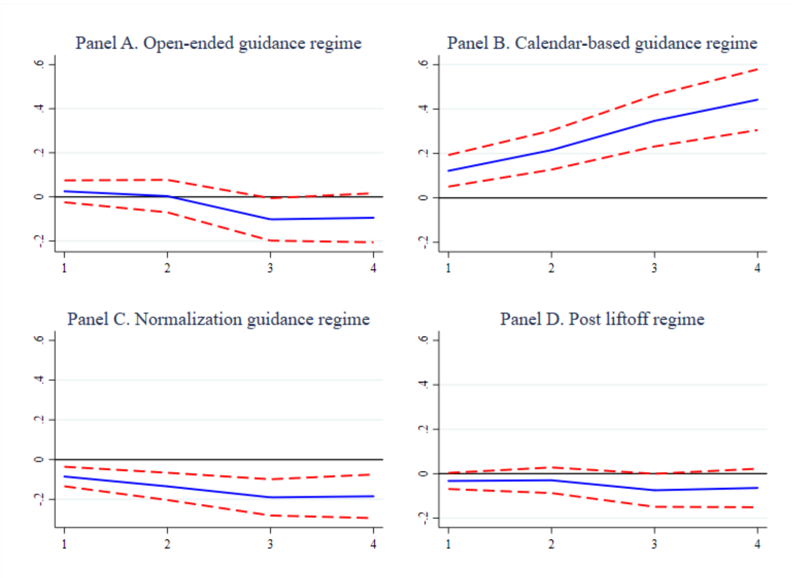
The β_k^rs are the coefficients to be estimated. Each of them measures the average response of fund flows over the $t+k$ horizon to a standard deviation NFP surprise in regime r. The estimation is done through OLS. Standard errors are clustered at the fund-level to control for heteroskedasticity and potential autocorrelation within each cross-section. To show the results, we plot impulse response functions (IRFs) using the $\hat{\beta}_k^r$ coefficients for the point estimates and their standard errors to derive confidence bands.

2.4.2 Results

Figure 2.2 below shows IRFs obtained estimating Equation 2.2. Blue solid lines report the estimated responses. Red dotted lines are 90 percent confidence intervals. Flows are measured as a percentage of funds' assets. Each panel reports the IRF corresponding to a different regime. Panels A and B contain results for the open-ended and calendar-based guidance regimes respectively, while Panels C and D focus on the normalization and post-liftoff periods (refer to Section 2.2.3 for an exact definition). The same abbreviations will be followed in the rest of the analysis.

The results point to stark non-linearities. Whereas during the calendar-based regime positive news caused U.S. investors to pour money into EMs-focused funds, the same announcements had the opposite effect during the normalization regime. In the former period, the effect of one standard deviation surprise is estimated to be 0.1 percent at impact (week 1) and to get stronger over the time, until reaching about 0.4 percent in the fourth week (still statistically significant). Conversely, during the normalization regime, the sensitivity is estimated to be slightly more than -0.1 percent at impact and more than twice as negative in the second week. The effect levels off at about -0.2 percent in the third week (still statistically significant).[9]

Figure 2.2: Fund flows sensitivity to employment announcements during different Fed's guidance regimes



Notes: the Figure shows the estimated responses to U.S. employment announcements of allocations into investment funds. The funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as defined by MSCI). Sensitivities are measured in percentage of beginning of period assets. The y-axis denotes the cumulated effect of a one standard deviation surprise in the U.S. non-farm payroll data release. The x-axis denotes the horizon of the response (in weeks), with 1 being the week of the announcement. The blue solid line shows the β_k^r coefficients obtained estimating Equation 2.2. Red dotted lines are 90 percent confidence bands obtained using respective standard errors, clustered at the fund-level. The open-ended guidance, calendar-based guidance, normalization guidance, and post-liftoff regimes range, respectively, from March/18/2009 to August/8/2011, from August/9/2011 to May/22/2013, from May/23/2013 to December/12/2015 and from December/13/2015 to May/30/2018.
Sources: Bloomberg, Emerging Portfolio Fund Research and own calculations.

The results for the open-ended guidance and post-liftoff regimes are similar to those for the normalization period but smaller in absolute value and statistically

9 The results are robust to using a different lag structure and, as expected since the surprise series behaves as white noise, also to including forward surprises ala Teulings and Zubanov, 2014.

significant only in week 3. When the Federal Reserve was issuing open-ended guidance, a positive surprise had adverse lagged effects on flows, equal to about a tenth of a percentage point. Flows were still negative following good news during the post-liftoff regime, but the estimates are even smaller.

When the horizon is extended further, the response of flows during the calendar-based regime keeps increasing until the sixth week to then finally level off at about 0.6 percent (results available upon request). Importantly, neither the effect estimated for this period neither that for the normalization regime is found to reverse over a longer horizon.

What can explain the positive response of flows during the calendar-based regime? We argue that the strong time-based guidance provided by the Fed stabilized the market expectation for the future policy rate, thus making interest rates insensitive to economic releases. Absent implications for future monetary policy, positive news bolstered the investment outlook and decreased risk aversion, as agents gained confidence that the economic recovery was gaining strength. Given that the U.S. is the largest economy in the world, these factors had positive spillovers for the rest of the global economy, including EMs.[10] On the other hand, after the Fed hinted at the beginning of normalization employment announcements recovered their risk-free rate information content. The prospect of a stronger labor market brought forward the expected exit from unconventional monetary policies, which itself had contributed to decreasing volatility (Bekaert, Hoerova, and Duca, 2013). That would explain the outflows from EMs observed following positive news.[11]

We next test whether these explanations are correct. We first carry out an event study to analyze how U.S. monetary policy expectations (proxied by the 6-month future federal funds rate), equity prices and risk aversion (proxied by the VIX) reacted to employment announcements during different forward guidance regimes. Second, we include these variables in the baseline specification (Equation 2.2) and check how the results are affected.

For the event study, we consider both a 1-day and a 1-week window. The latter is useful to check whether the effects of employment releases persisted over a relatively long horizon. The estimation is done by OLS. Standard errors are heteroskedasticity (white) robust. Results are shown in Table 2.2 below. Each column reports estimates for a different forward guidance regime. Bold numbers indicate statistical significance at the 90 percent confidence level.

[10] Notice that positive effects for capital flows to EMs do not exclude that positive news also led to more investment in US-dedicated funds.

[11] The link between U.S. macro-financial conditions and growth in EMs is likely to be heterogeneous among countries, but for a typical open EM it may be substantial. As an example, Solmaz, 2015 study the impact of external shocks, proxied by the oil price, U.S. industrial production, and U.S. credit spreads, on Turkey through a Bayesan vector autoregression model and find that about 60 percent of the variance of output can be explained by external factors. Moreover, some commodity exporters-EMs would arguably benefit from better U.S. growth prospects through higher commodity prices (see Reinhart and Reinhart, 2008 for evidence on the relationship between commodity prices and capital flows to EMs).

Table 2.2: Sensitivity of key U.S. financial variables to employment announcements during different Fed's guidance regimes

		(A)	(B)	(C)	(D)
1-day	6-month fed fund future	**1.79**	0.09	**1.19**	**1.17**
	MSCI U.S. stock index	**0.37**	**0.75**	-0.02	0.25
	CBOE VIX volatility index	-0.16	**-3.55**	-0.39	-1.80
1-week	6-month fed fund future	0.36	0.17	**0.68**	1.13
	MSCI U.S. stock index	0.35	**1.07**	**-0.77**	-0.14
	CBOE VIX volatility index	-0.27	-5.16	**7.13**	-1.76

Notes: the Table shows the estimated responses to a one standard deviation surprise in the U.S. non-farm payroll data release of (i) the 6-month ahead federal fund future rate, (ii) the growth rate of the MSCI U.S. stock index, and (iii) the growth rate of the CBOE VIX U.S. equity volatility index. The response of (i) is measured in basis points, that of (ii) and (iii) in percentage points. The leftmost column reports the horizon considered. "1-day" refers to daily responses. "1-week" refers to weekly (5-trading-day) responses. Columns denoted by (A), (B), (C), and (D) report estimates for different Fed's guidance regimes: respectively the open-ended (March/18/2009 to August/8/2011), calendar-based (August/9/2011 to May/22/2013), normalization (May/23/2013 to December/12/2015) and post-liftoff (December/13/2015 to May/30/2018). Estimates are obtained from estimating the following specification: $100 * (y_{t+n} - y_{t-1}) = \sum_{r=1}^{4} \left(\alpha_k^r d_t^r + \kappa_k^r d_t^r news_t \right) + \varepsilon_t$, where y_t is either the federal funds 6-month future rate, the log of the MSCI U.S. stock index, or the log of the VIX index; t denotes time (in days); ε_t is an error term, assumed not to be correlated with the regressors; and the rest of the notation is as in Equation 2.2. Bold numbers indicate statistical significance at the 90 percent confidence level, based on heteroskedasticity (white) robust standard errors.
Sources: Bloomberg, Thomson Reuters Datastream and own calculations.

The estimates confirm that the effect of NFP announcements on the 6-month ahead fed fund rate was highly conditional on the Fed's communication stance. During the open-ended guidance regime (Column A), a one standard deviation surprise increased it by 1.8 basis points at impact. When the FOMC switched to calendar-based guidance, employment releases had no effect (Column B). This changed again after the taper tantrum, as the fed fund future rate was on average 1.2 basis point higher following a one standard deviation surprise (Column C). A similar effect is also found for the post-liftoff period (Column D). Looking at the other variables, U.S. stock prices rose and expected volatility fell at impact after better than anticipated employment data during the calendar-based guidance regime. After the taper tantrum, these effects disappeared.[12]

[12] These results are reminiscent of those in Boyd, Hu, and Jagannathan, 2005, who study how U.S. government bond and stock prices respond to unemployment announcements in different stages of the business cycle. These authors find that bonds normally do not react in contractions, which indicates that labor market announcements do not carry relevant information on the risk-free rate in that stage of the cycle. On the other hand, they find equity prices to respond negatively to news of rising unemployment, thus suggesting that these signal either deteriorating earning prospects, increasing risk premia or both. Boyd, Hu, and Jagannathan, 2005 also show that both equity and bond prices rise following higher than expected unemployment during expansion. To the extent that equity valuations depend on the risk-free rate through the discount rate, their increase following bad news indicate that new information on the policy rate dominates that on earnings and risk premia, as the latter should push equity prices in the opposite direction.

Focusing on the larger window reveals other interesting patterns. The response of the federal fund future rate was statistically different from zero only during the normalization regime. The reaction of the MSCI and VIX indexes was respectively equal to -0.8 and +7.1 percent during the same period (both significant). These effects suggest that stock market investors, many of whom are retail, might need some time to digest all the implications of the employment report fully. In line with this observation, the 5-day effect of NFP announcements on both equity prices and volatility during the calendar-based regime was larger than the 1-day estimate (although just borderline significant for the latter, with p-value of 0.11).[13]

Next, we formally investigate whether the observed sensitivity of portfolio flows to NFP releases can be explained by the effects that these had on U.S. domestic financial conditions. We complement Equation 2.2 including the U.S. stock market and the VIX indexes (both in log-differences), as well as the federal fund 6-month future rate, all interacted with the forward guidance dummies. These variables are introduced first separately one at a time and then jointly in the same regression. Table 2.3 below reports the results.

The estimates for the calendar-based guidance regime (Column B) are qualitatively in line but quantitatively different from the baseline. Conditioning on the same stock market growth rate decreases the magnitude of the estimated coefficients by about 30 percent. The inclusion of the VIX does have qualitatively similar albeit smaller effects. Instead, not surprisingly as this did not react to employment releases during this period, including the fed funds future rate does not affect the results. These findings indicate that when the Fed issued calendar-based guidance positive domestic news propagated abroad through, at least partly, improved equity prospects and decreased risk aversion.

Turning to the normalization period (Column C), including either the fed fund future rate, the stock market, or the VIX has similar dampening effects on the baseline estimate. The coefficients are reduced by about 25 percent at impact (week

[13] The effects of employment announcements on the U.S. stock market during the calendar-based regime and on the expected federal fund rate during the normalization regime survive even when considering a larger, 4-week, window (results available upon request).

Table 2.3: Fund flows sensitivity to employment announcements – conditioning on key U.S. financial variables

		(A)	(B)	(C)	(D)
1-week ($k = 0$)	Unconditional (baseline)	0.03	**0.12**	**-0.09**	-0.03
	6-month fed fund future	0.02	**0.12**	**-0.07**	-0.02
	MSCI U.S. stock index	0.02	**0.10**	**-0.06**	-0.03
	CBOE VIX volatility index	0.02	**0.11**	**-0.07**	**-0.04**
	All	0.01	**0.09**	**-0.05**	-0.03
2-week ($k = 1$)	Unconditional (baseline)	0.00	**0.22**	**-0.13**	-0.03
	6-month fed fund future	-0.02	**0.21**	**-0.10**	-0.01
	MSCI U.S. stock indexh	-0.05	**0.15**	**-0.09**	-0.03
	CBOE VIX volatility index	-0.02	**0.18**	**-0.08**	-0.02
	All	-0.06	**0.14**	-0.05	0.02
3-week ($k = 2$)	Unconditional (baseline)	**-0.10**	**0.35**	**-0.19**	**-0.07**
	6-month fed fund future	**-0.14**	**0.34**	**-0.13**	-0.05
	MSCI U.S. stock index	**-0.18**	**0.27**	**-0.12**	**-0.08**
	CBOE VIX volatility index	**-0.15**	**0.3**	**-0.12**	-0.06
	All	**-0.21**	**0.25**	-0.07	-0.01
4-week ($k = 3$)	Unconditional (baseline)	-0.09	**0.44**	**-0.18**	-0.06
	6-month fed fund future	**-0.15**	**0.44**	-0.1	-0.04
	MSCI U.S. stock index	**-0.19**	**0.34**	-0.09	-0.07
	CBOE VIX volatility index	**-0.15**	**0.38**	-0.11	-0.04
	All	**-0.23**	**0.31**	-0.03	0.02

Notes: the Table shows the estimated responses of allocations into investment funds to a one standard deviation surprise in the U.S. non-farm payroll data release, conditioning for key U.S. financial variables. The funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as defined by MSCI). Sensitivities are measured in percentage of beginning of period assets. Estimates are obtained from Equation 2.2. The leftmost column reports the horizon considered (k). Rows denoted by "Unconditional" reports coefficients from the baseline regression (Figure 2.2). Rows denoted by "6-month fed fund future" report coefficients estimated including the 6-month ahead federal fund future rate. Rows denoted by "MSCI U.S. stock index" report coefficients estimated including the growth rate of the U.S. stock price index. Rows denoted by "CBOE VIX volatility index" report coefficients estimated including the growth rate of the VIX index. Rows denoted by "All" report coefficients estimated including the 6-month ahead federal fund future rate as well as the growth rates of the U.S. stock index and the VIX index. Columns denoted by (A), (B), (C), and (D) report estimates for different Fed's guidance regimes: respectively the open-ended (March/18/2009 to August/8/2011), calendar-based (August/9/2011 to May/22/2013), normalization (May/23/2013 to December/12/2015) and post-liftoff (December/13/2015 to May/30/2018). Bold numbers indicate statistical significance at the 90 percent confidence level, based on clustered standard errors (at the fund-level).
Sources: Bloomberg, Datastream, EPFR and own calculations.

1) and up to 50 percent over the full horizon. When the three variables enter the regression jointly, the effect of employment announcements is negligible and never statistically significant, except in week 1. These results constitute evidence that announcements of a healthier than anticipated U.S. labor market led to portfolio

capital outflows from EMs due to their tightening effects on Fed's policy expectations. Such news also induced selling in the stock market and contributed to increasing risk aversion, which in turn had further adverse effects on fund flows. These channels are consistent with the finding in Miranda-Agrippino and Rey, 2015, Forbes and Warnock, 2012 and others that increases in the VIX are associated with capital outflows from EMs.

In Appendix A.3 we report and discuss extensively some robustness checks performed on the baseline analysis (Figure 2.2). Summarizing, we verify that the results are not driven by employment release outliers, that they are robust to giving a lower weight to the releases in which forecaster uncertainty was higher (see Pericoli and Veronese, 2015), and that they do not depend on the exact cutoff date chosen to distinguish between the calendar-based and normalization regimes. We also show that the main dynamics survive when using a broader measure of portfolio capital flows (sourced by the IIF) and when considering other macroeconomic announcements. Finally, we check that the results for the calendar-based guidance regime do not capture flight-to-safety effects (that is, they are not driven by negative surprises).

Could the link envisaged so far between the Fed's policies and the flow sensitivity to U.S. employment news be spurious? It might be that investors correctly understand the FOMC's reaction function. Then the almost null response of the federal funds future rate to employment releases during the calendar-based period could be due to the market participants correctly assessing that the large negative output and inflation gaps warranted low rates for a long time. However, this argument does not seem to be valid since these gaps were less negative when the FOMC introduced calendar-based guidance than they had ever been since the start of the GFC.[14] Hence, the same patterns uncovered for the calendar-based guidance regime should have also been observed for the open-ended guidance period.

To recap, this section has provided evidence that the stance of the Federal Reserve is crucial to explain how shocks in the U.S. propagated abroad through portfolio capital flows in the aftermath of the GFC. Announcements of higher than expected employment growth led to flows of capital towards EMs when the Fed flagged extremely low policy rates going forward, as investors digested the positive implications for the global economy. On the other hand, when the Fed signaled upcoming normalization, the same announcements pushed investors to repatriate capital in the U.S., as the tightening of financial conditions induced by the expectation of higher policy rates more than counterbalanced the real economy spillovers of a stronger U.S. labor market.

[14] The output gap was equal to -4.7 percentage points on average between April 2009 and September 2011, while it was -3.1 percentage point when the Federal Reserve introduced calendar-based forward guidance and -1.4 on average between October/2011 and June/2013 (Fred).

2.5 The Role of Exchange-traded Funds

Do flows to all funds respond to macroeconomic announcements in the same way? This section explores potential heterogeneities. The primary focus is on the differential flow sensitivity to news of exchange-traded-funds (ETFs) relative to mutual funds.

2.5.1 Background

ETFs experienced exponential growth since they were first established in 1993. Their asset share in the overall U.S. fund industry rose from 5 to 18 percent between 2007 and 2017 (ICI). Among the equity funds considered in the analysis, ETFs increased their share from 46 to 53 percent over the March 2009 to May 2018 period. The rise of bond ETFs was even larger, going from less than 5 to more than 40 percent in the same period (see Figures A.3 and A.4 in Appendix A.1).

Why have ETFs become so popular? The short answer is that they offer a cheap way to acquire liquid positions in a broader set of markets. Liquidity depends on the fact that ETFs, like stocks, are traded continuously throughout the day on secondary markets. That is different from mutual funds, which trade exclusively on the primary market, and for which transactions only take place at the end of the trading day (with prices also being determined only then). As ETFs usually track an index, its inverse, or their multiples, investors can quickly gain exposure or bet against an overall market. Their passive nature, meaning that managers do not actively select companies, also permits to charge lower fees relative to mutual funds.

These characteristics make ETFs likely to attract a different pool of investors, more short-term oriented and more reactive to changes in broader market sentiment, relative to mutual funds. Backing this intuition, 52 percent of ETF-holding U.S. households were willing to take substantial or above-average risk for substantial or above-average gain in 2017, compared to just 34 percent of mutual fund-holding households (ICI). We therefore analyze whether the flow responses to employment releases observed above were more relevant for ETFs than mutual funds. The rest of this section describes the empirical methodology, presents the baseline results and finally carries out some extensions to disentangle further heterogeneities within ETFs, concerning their asset size and type of investment (whether equity or debt).

2.5.2 Methodology

To investigate the differential response of flows to ETFs relative to mutual funds, we carry out a difference-in-differences (diff-in-diff) analysis. In practice, we create a dummy variable d_i^E, taking value 1 for ETFs and 0 otherwise, and estimate the following regression:

$$100 * \frac{\sum_{j=0}^{k} f_{i,t+j}}{a_{i,t}} = \sum_{r=1}^{4} \lambda_k^r d_t^r d_i^E news_t + B_k Z_{i,t} + \gamma_i + \tau_{f,t} + \varepsilon_{i,t} \tag{2.3}$$

where $\tau_{f,t}$ denotes investment destination-specific time fixed effects; d_i^E is the ETF dummy and the rest of the notation is as in Equation 2.2. Note that the time effects absorb any common variation in ETFs and mutual funds flows. Therefore the λ_k^r coefficients measure the differential (extra) effect of employment releases on flows to ETFs relative to mutual funds.

This specification has a further advantage in that it accounts for the two sets of capital flow drivers identified the literature, namely global push, and local pull factors. The former are global developments affecting investors' risk appetite and are beyond the scope of influence of individual EMs. The latter are country-specific characteristics driving capital towards some countries rather than others. The inclusion of time fixed effects at the investment destination means to account for local pull factors (allowed to be time-varying).[15] U.S. employment surprises are instead thought as a global push factor. The estimation is done through OLS. Standard errors are clustered at the fund-level.

2.5.3 Results

Figure 2.3 below shows IRFs of the differential effect of employment announcements on flows to ETFs relative to mutual funds, obtained plotting the $\hat{\lambda}_k^r$ coefficients estimated from Equation 2.3 and the respective confidence bands. In line with the baseline analysis, we do not find significant differential responses during the open-ended guidance and post-liftoff regimes (Panels A and D). Instead, IRFs for the calendar-based and normalization guidance periods (Panels B and C respectively) are statistically significant and qualitatively similar to those presented in Figure 2.2. These estimates — derived from a richer specification including time-by-investment-destination fixed effects — adds robustness to the previous results and confirms the intuition that ETFs attract a pool of investors that respond more strongly to global shocks than mutual funds.

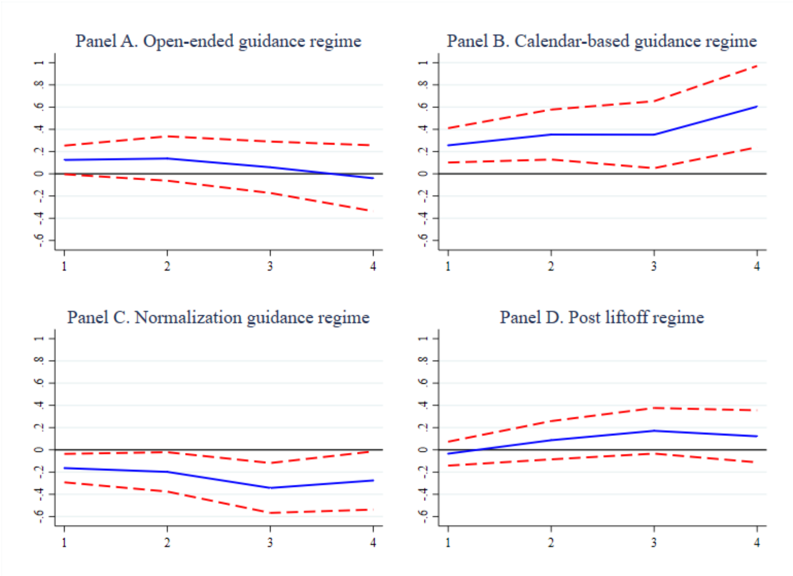
The 4-week response of ETFs to a standard deviation surprise was about +0.6 (-0.3) percentage points higher (lower) than that of mutual funds during the calendar-based (normalization) regime. Using these coefficients, those obtained from Equation 2.2, and the share of ETFs, we calculate the 4-week cumulated response of flows into mutual funds to have been approximately 0.3 and -0.1 percent during the calendar-based and normalization regimes respectively. Accordingly, those into ETFs were about 0.9 and -0.4 percent in the same periods. This analysis suggests that ETFs and mutual funds exhibited similar responses to NFP releases, but the sensitivity of ETFs was three to four times larger. This is qualitatively in line to Converse, Levy-Yeyati, and Williams, 2018, who study whether ETFs respond more than mutual funds to changes in risk aversion and find that that is indeed the case. However, they estimate ETFs to be only up to 50 percent more sensitive.

[15] Saying that the destination-by-time fixed effects account for local pull factors assumes that ETFs and mutual funds respond to them in the same way (that is, ETFs are not more sensitive). Converse, Levy-Yeyati, and Williams, 2018 show that this is indeed the case.

Appendix A.4 reports some sensitivity analyses showing that the results presented here are robust to the inclusion of other U.S. as well as foreign macroeconomic news as controls. The rest of the section explores the presence of heterogeneities within different ETFs. In the interest of brevity, the focus will be on the calendar-based and normalization regimes only.

We start by exploring heterogeneities in size. The dataset used in the analysis comprises 753 funds, and their asset distribution is highly skewed to the left (see Figure A.1 in Appendix A.1). Funds in the upper quartile of the ETFs (mutual funds) distribution make up 92 (85) percent of assets. Above we showed that flows to ETFs react more to employment releases than mutual funds. If the reason is that ETFs can be traded continuously and are used as instruments for short-run investment strategies, then larger ETFs should be expected to be even more sensitive as they are more liquid.

Figure 2.3: Differential sensitivity of ETFs relative to mutual funds



Notes: the Figure shows the estimated differential responses to U.S. employment announcements of allocations into exchange-traded relative to mutual funds. The funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as defined by MSCI). Differential sensitivities are measured as a percentage of beginning of period assets. The y-axis denotes the cumulated differential effect of a one standard deviation surprise in the U.S. non-farm payroll data release. The x-axis denotes the horizon of the response (in weeks), with 1 being the week of the announcement. The blue solid line shows the λ_k^r coefficients obtained estimating Equation 2.3. Red dotted lines are 90 percent confidence bands obtained using respective standard errors, clustered at the fund-level. The open-ended guidance, calendar-based guidance, normalization guidance and post-liftoff regimes range, respectively, from March/18/2009 to August/8/2011, from August/9/2011 to May/22/2013, from May/23/2013 to December/12/2015 and from December/13/2015 to May/30/2018.
Sources: Bloomberg, EPFR and own calculations

We check whether the higher responsiveness exhibited by ETFs relative to mutual funds is driven by large ETFs. We divide funds into three categories: small medium and large. The former are those in the lower quartile of the asset distribution (less than \$29.8 million in assets), while the latter are in the upper quartile (more than \$358.5 million in assets). Medium-sized funds are all the rest. We then extend

Equation 2.3 to allow for different λ^r coefficients based on size. Results for the calendar-based and normalization regimes are reported in Table 2.4 below, together with those obtained from Equation 2.3.[16] The new estimates should be interpreted as the mean differential response of small/medium/large ETFs relative to the typical mutual fund. Stars indicate that the coefficient is statistically different from at least one of the two others (as informed by a Wald test).

Table 2.4: Differential sensitivity of ETFs relative to mutual funds - distinguishing by funds' size

		(B)	(C)
1-week	All ETFs (baseline)	**0.26**	**-0.16**
	Small ETFs	0.19	0.17*
	Medium ETFs	0.12	-0.01*
	Large ETFs	**0.45**	**-0.51***
2-week	All ETFs (baseline)	**0.35**	**-0.2**
	Small ETFs	0.09*	0.31*
	Medium ETFs	0.15*	-0.01*
	Large ETFs	**0.71***	**-0.65***
3-week	All ETFs (baseline)	**0.35**	**-0.34**
	Small ETFs	-0.16	0.11
	Medium ETFs	0.18	-0.25
	Large ETFs	**0.77**	**-0.64**
4-week	All ETFs (baseline)	**0.6**	**-0.28**
	Small ETFs	-0.09*	0.25*
	Medium ETFs	0.44	-0.26
	Large ETFs	**1.07***	**-0.48***

Notes: the Table shows the estimated differential responses to U.S. employment announcements of allocations into exchange-traded relative to mutual funds, distinguishing between small, medium and large exchange-traded funds. The funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as defined by MSCI). Differential sensitivities are measured in percentage of beginning of period assets. The leftmost column reports the horizon considered (k). Rows denoted by "All ETFs (baseline)" report the $\hat{\lambda}^r_k$ coefficients estimated from Equation 2.3. Rows denoted by "Small ETFs", "Medium ETFs" and "Large ETFs" report respectively the $\hat{\lambda}^{s,r}_k$, $\hat{\lambda}^{m,r}_k$ and $\hat{\lambda}^{l,r}_k$ coefficients estimated from the following Equation $100 * \frac{\sum_{j=0}^{k} f_{i,t+j}}{a_{i,t}} = \sum_{r=1}^{4}\left(\lambda^{s,r}_k d^s_i d^r_t + \lambda^{m,r}_k d^m_i + \lambda^{l,r}_k d^l_i d^r_t\right) d^r_t d^E_i news_t + B_k Z_{i,t} + \gamma_i + \tau_{f,t} + \varepsilon_{i,t}$, where d^s_i, d^m_i, and d^l_i are dummy variables for respectively small, medium and large funds, and the rest of the notation is as in Equation 2.3. A * next to the coefficient indicates that this is statistically different from at least one of the other two at the 90 percent confidence level, according to a Wald test for equal coefficients. Columns denoted by (B) and (C) report estimates for different Fed's guidance regimes: respectively the calendar-based (August/9/2011 to May/22/2013) and normalization (May/23/2013 to December/12/2015). Bold numbers indicate statistical significance at the 90 percent confidence level, based on clustered standard errors (at the fund-level).
Sources: Bloomberg, EPFR and own calculations.

16 Results for the open-ended and post-liftoff regimes are available upon request

Focusing on the calendar-based regime and the 4-week horizon, we find that the response of flows into large ETFs following a one standard deviation surprise was 1.1 percentage point higher than that of the average mutual fund. This is significantly more than the differential response of small ETFs (which instead is not statistically different from zero). Similarly, the response to the same surprise during the normalization regime was about -0.5 percentage points lower for large ETFs. This coefficient is statistically different from the one estimated for small ETFs, which is not significant. These results could be a byproduct of large funds in general (not only large ETFs) reacting more to global factors. We verify that this is not the case by estimating an extension of Equation 2.2 allowing for size-dependent effects. The results are shown in Table A.8 in Appendix A.4. The null hypothesis of equal coefficients cannot be rejected, thus indicating that the higher responsiveness of large funds is only a prerogative of ETFs.

Miranda-Agrippino and Rey, 2015 study how U.S. monetary policy affects the joint dynamics of international financial variables and find that when the Fed tightens policy, global risky asset prices tend to go down, while cross-border credit is significantly reduced. They point to global banks as the main channel of transmission (see also Bruno and Shin, 2014 and Correa et al., 2016). The results presented in this section suggest that (large) ETFs are another important vehicle through which shocks in the U.S. are transmitted to EMs. Next, we will extend the analysis to distinguish between equity and bond funds.

We argued above that during the calendar-based regime fund flows responded positively to better than expected employment news due to improved prospects for the U.S. and the global economy. As equity is more dependent on growth than debt, equity flows should have been more substantial during that period. Turning to the normalization regime, the expectation of tighter Fed's policies following positive news should have had larger adverse effects on bond flows. On the other hand, the results presented in Tables 2.2 and 2.3 suggest that the sensitivity of flows to employment releases partly depended also on the response of the VIX, which should have similar effects for both bond and equity funds.

As before, we again estimate an extension of Equation 2.3 allowing for different λ^r coefficients based on size. This time the estimation is carried out separately for the restricted equity and bond fund samples. Table 2.5 shows the results.

Focusing on the calendar-based guidance regime, the estimates for equity ETFs are qualitatively and quantitatively similar to those reported in Table 2.4. The 4-week extra response of flows to large ETFs was 1.3 percentage point larger relative to the typical equity mutual fund. Except in one case, the coefficients for the bond sample are not statistically different from zero. Turning to the normalization period, both large equity and large bond ETFs generally experienced significantly higher outflows than the respective typical mutual fund. The extra sensitivity estimated for large ETFs range between -0.5 and -0.9 percentage points depending on the horizon and sample considered. For the bond sample, medium-sized ETFs also experienced significant outflows (quantitatively similar to large ETFs).

Table 2.5: Differential sensitivity of ETFs relative to mutual funds – distinguishing by funds' size and type of investment

		Equity sample		Bond sample	
		(B)	(C)	(B)	(C)
1-week	All ETFs (baseline)	**0.29**	-0.11	0.08	**-0.44**
	Small ETFs	0.24	0.22*	-0.01	0.02*
	Medium ETFs	0.16	0.06*	-0.25	**-0.44**
	Large ETFs	**0.48**	**-0.48***	0.32	**-0.55***
2-week	All ETFs (baseline)	**0.39**	-0.1	0.03	**-0.65**
	Small ETFs	0.14*	0.36*	-0.01	0.19*
	Medium ETFs	0.12*	0.1*	0.22	**-0.6***
	Large ETFs	**0.85***	**-0.57***	-0.15	**-0.89***
3-week	All ETFs (baseline)	**0.37**	**-0.28**	(0.11)	**-0.59**
	Small ETFs	-0.15	0.14	-0.06	0.16*
	Medium ETFs	0.09	-0.16	0.71	**-0.59**
	Large ETFs	**0.97***	**-0.59**	**-0.37**	**-0.69**
4-week	All ETFs (baseline)	**0.64**	-0.28	0.31	-0.15
	Small ETFs	-0.03*	0.14*	-0.16	**1.00***
	Medium ETFs	0.38*	-0.26	1.05	-0.17*
	Large ETFs	**1.30***	**-0.46***	-0.22	-0.45*

Notes: the Table shows the estimated differential responses to U.S. employment announcements of allocations into exchange-traded relative to mutual funds, distinguishing between small, medium and large exchange-traded funds. The funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as defined by MSCI). Differential sensitivities are measured in percentage of beginning of period assets. The leftmost column reports the horizon considered (k). Rows denoted by "All ETFs (Equation 2.3)" report the $\hat{\lambda}_k^r$ coefficients estimated from Equation 2.3. Rows denoted by "Small ETFs", "Medium ETFs" and "Large ETFs" report respectively the $\hat{\lambda}_k^{s,r}$, $\hat{\lambda}_k^{m,r}$ and $\hat{\lambda}_k^{l,r}$ coefficients estimated from the following Equation $100 * \frac{\sum_{j=0}^{k} f_{i,t+j}}{a_{i,t}} = \sum_{r=1}^{4} \left(\lambda_k^{s,r} d_i^s d_t^r + \lambda_k^{m,r} d_i^m + \lambda_k^{l,r} d_i^l d_t^r \right) d_t^r d_i^E news_t + B_k Z_{i,t} + \gamma_i + \tau_{f,t} + \varepsilon_{i,t}$, where d_i^s, d_i^m, and d_i^l are dummy variables for respectively small, medium and large funds, and the rest of the notation is as in Equation 2.3. A * next to the coefficient indicates that this is statistically different from at least one of the other two at the 90 percent confidence level, according to a Wald test for equal coefficients. Columns denoted by (B) and (C) report estimates for different Fed's guidance regimes: respectively the calendar-based (August/9/2011 to May/22/2013) and normalization (May/23/2013 to December/12/2015). Bold numbers indicate statistical significance at the 90 percent confidence level, based on clustered standard errors (at the fund-level).
Sources: Bloomberg, EPFR and own calculations.

This section started off noticing that the popularity of ETFs among U.S. investors in EMs has increased considerably since their inception (just looking at the period of the analysis, the ETFs asset share rose from about 42 to over 51 percent). It then argued that due to their higher liquidity, lower fees, and capacity to grant exposure to a broad set of markets, ETFs are likely to be used by more short-term oriented and less risk-averse investors. The analysis carried out afterward showed (large) ETFs to be significantly more responsive to U.S. employment announcements than mutual funds, suggesting that their rising popularity might have contributed to making the global financial system more synchronized. In line with expectations, the higher sensitivity of ETFs was driven by equity funds during the calendar-based regime, while bond and equity flows were equally important during the normalization regime.

2.6 Conclusions

This paper has analyzed the impact of U.S. employment announcements on portfolio capital flows from the U.S. to EMs in the post-GFC period. Employment news are important because they reveal new information about the health of the U.S. economy, the largest in the world, and might inform about future U.S. monetary policy. The latter has global importance given that the $ is the world's reserve currency.

When the Federal Reserve issued time-based forward guidance to convey the idea of low rates for long, employment announcements did not affect monetary policy expectations. Instead, positive news boosted the domestic stock market and lowered risk aversion, thus leading to positive (equity) flows to EMs. Conversely, when the Federal Reserve hinted at forthcoming normalization, good news increased the expected future policy rate, depressed equity prices, and increased risk aversion. All these factors pushed U.S. investors to withdraw capital from EMs.

The second part of this paper focused on the role of exchange-traded funds (ETFs). These have an important role in the international transmission of U.S. news. Flows in and out of large EMs-dedicated ETFs displayed a substantial extra sensitivity to developments in the US. Likely, this is due to ETFs being used by more short-term oriented and risk-seeking investors who find in ETFs a cheap way to gain exposure to different sets of markets. Given their rising popularity as an investment vehicle, the dynamics documented in this paper suggest that ETFs might have made EMs more synchronized with the global financial cycle.

Future work should aim at assessing the quantitative importance of the dynamics uncovered in the current analysis for recipient countries. Preliminary analyses — carried out in this respect and reported in Appendix A.5 — suggest that there exists a great heterogeneity in the magnitude of flows across countries. More work should aim at assessing potential determinants of such cross-country differences in magnitudes of flows.

Chapter 3

The Composition Effects of Tax-Based Consolidations on Income Inequality

3.1 Introduction

Following the build-up of large public debt stocks in the wake of the global financial crisis of 2007-2008 and the ensuing recession, consolidating public finances has become a priority in several advanced economies. As an example, in the Euro Area, the structural balance grew from -4.8% of potential GDP in 2009 to -0.9% in 2015. Fiscal policy in the United Kingdom and the United States followed a similar path, with the structural balance ratio being increased respectively from -8.5% to -4.1% and from -7.7% to -3.4% during the same period (IMF World Economic Outlook).

Many commentators have raised concerns that this wave of fiscal consolidations may exacerbate high and rising income and wealth disparities, with adverse consequences for long-term economic growth. Indeed, a vast strand of the literature argues that inequality may compromise economic growth through many channels. First, it creates political instability, which may discourage investments (Alesina and Perotti, 1996; Berg and Ostry, 2011). Second, in a highly unequal society, the majority of citizens are not in a condition to save or to invest in education, which reduces investments and the accumulation of human capital (Aghion, Caroli, and Garcia-Penalosa, 1999; OECD; OECD; Galor and Moav, 2004). Third, inequality may also create financial instability. Indeed, some studies maintain that inequality played a major role in the global financial crisis of 2007-2008 by contributing to the debt accumulation by lower- and middle-income class agents (Fitoussi and Saraceno, 2009; Kumhof, Romain, and Winan, 2015; Rajan, 2010). In this light, policy-makers have become more concerned about the consequences for inequality of their policy actions.

An obvious way in which fiscal consolidation may influence income inequality is through changes in the amount of government redistribution. For instance, lowering government transfers reduces the disposable income of low-income agents, thereby raising inequality. Conversely, increasing the top marginal income tax rate penalizes richer agents and therefore should decrease disparities. Fiscal restraint may also induce behavioral responses by agents. As an example, a higher labor tax lowers the net wage and may induce agents to either substitute away labor for leisure (substitution effect) or supply more labor to maintain a similar level of consumption (income effect). Hence, depending on which effect dominates — and assuming agents' heterogeneity in either the type of utility function or labor supply elasticities — the inequality effects of higher taxes through the labor supply channel may differ.

Further, fiscal consolidations may affect inequality through its general equilibrium effects. Ball et al., 2013 argue that fiscal adjustments reduce output and increase unemployment. That decreases the wage share, which in turn tends to increase inequality due to the relatively higher share of wage income in lower-income groups. Moreover, Bastagli, Coady, and Gupta, 2012 suggest that the tendency of employers to hoard high-skilled workers, who usually have higher income levels, could be another factor potentially raising inequality at times of fiscal restraint. On the other hand, in countries with rigid labor markets, it might be more difficult for firms to shed off labor, thus limiting the scope of these channels. To sum up, a theoretical prediction on the impact of fiscal consolidations on inequality depends on both the specific policy measures used and the assumptions underlying the economic structure.

In this context, the aim of our analysis is twofold. First, we empirically assess the effects of tax-based consolidations (i.e., consolidations in which tax hikes are larger than spending cuts) on income inequality, output, and labor market conditions. Second, we establish some stylized facts about which particular tax instruments are typically used during tax-based consolidations and investigate the composition effects, distinguishing between direct and indirect taxes and their main sub-components. Although we are aware of the centrality of both income and wealth inequality in the debate, we focus on income inequality due to limited time series data availability on wealth inequality. Moreover, as income directly impacts living standards, rising income inequality has likely played a more prominent role in fueling the recent wave of social discontent that policy-makers are now trying to address (Rajan, 2010).

Our primary focus is on disposable income inequality since ultimately this is what matters for the relation between inequality and growth. However, to assess the direction and strength of the government's redistribution channel, we also evaluate the impact of fiscal adjustments on market income inequality. Further, we investigate the effects of tax-based consolidations not only on inequality but also on economic activity and labor market outcomes. We do so to disentangle the other channels through which consolidations affect inequality. We only analyze tax-based consolidations since, as we will see below, this is the area with the most disagreement in the literature.

The empirical literature on the effects of fiscal consolidations on income inequality has been limited in scope and has provided inconclusive evidence. Ball et al., 2013, Agnello and Sousa, 2014, and Woo et al., 2017 all start from the same action-based consolidations dataset of Devries et al., 2011, which contains information about spending cuts and tax hikes during fiscal consolidation episodes in 17 OECD countries between 1978 and 2009, but employ different methodologies: Ball et al., 2013 use local projections, Agnello and Sousa, 2014 adopt seemingly unrelated regressions (henceforth SUR), and Woo et al., 2017 use SUR and fixed effects. All these contributions distinguish between tax-based and spending-based consolidation episodes, where tax-based consolidations are defined as having tax hikes larger than spending cuts, and *vice versa* for spending-based consolidations. While they all conclude that spending-based consolidations increase income inequality, Ball et al., 2013, Woo et al., 2017, and Agnello and Sousa, 2014 find, respectively, significant positive, negative but insignificant and significant negative effects of tax-based consolidations on income inequality.

These different findings appear to be due to the way in which tax-based consolidations are accounted for and to the horizon considered. Agnello and Sousa, 2014 focus on the impact effects and employ both a dummy variable to denote all years of tax-based consolidations and the original variable constructed by Devries et al., 2011, which accounts for the differences in the size of the fiscal packages.[1] When employing the dummy variable, Agnello and Sousa, 2014 find negative but statistically insignificant effects on inequality. On the other hand, they find statistically significant negative effects (lower inequality) when accounting for the size of the consolidation. Woo et al., 2017 use the original Devries et al., 2011 variable to analyze the 1-year response of inequality to tax-based consolidations and find negative but not significant effects. Finally, Ball et al., 2013 employ the local projection method to derive impulse responses over an 8-year horizon and find positive effects. This result needs two qualifications. Firstly, the authors use a dummy variable taking value 1 in the first year of the tax-based consolidation cycle and 0 otherwise. As shown in Agnello and Sousa, 2014, this identification strategy might lead to estimates that are biased upward. Secondly, they report one-standard-error confidence bands for their impulse responses. Hence, their results are only significant at the 69% confidence level.

Our analysis contributes to the existing literature in several aspects. First, the studies reviewed above estimate the effects of fiscal consolidations in a single-equation setup.[2] To the extent that fiscal consolidations are not random assignments and that they also impact economic activity (see Guajardo, Leigh, and Pescatori, 2014; and Alesina, Favero, and Giavazzi, 2015), we argue that this approach might forgive potential feedback effects from economic activity to inequality. We opt instead for a multi-equation setup that takes into account both the direct effects of changes in fiscal policy on inequality, output and labor market variables and the indirect (feedback) effects among them. Second, we note that economic inequality is typically slow-moving. Hence, improving on Agnello and Sousa, 2014, and Woo et al., 2017, we study the dynamic (long-term) effects of tax-based consolidations on inequality.

[1] The difference between these two alternative approaches (accounting or not accounting for the size of the consolidation) seems particularly relevant since the tax-based consolidations identified in Devries et al., 2011 range from being as small as 0.3% of GDP (Denmark 1995) to being as large as 3.7% of GDP (Canada 1984-1990).

[2] An exception is Mourelo and Escudero, 2016 who consider the impact of fiscal consolidations on both employment and growth (but not inequality) for 32 countries during the Great Recession.

Differently from Ball et al., 2013, we account for the size of the consolidation. Third, we borrow from a recent and growing literature which documents how real-time fiscal measures tend to overstate the actual *ex-post* impact of fiscal consolidations (see Castro, Pérez, and Rodríguez-Vives, 2013, Frankel and Schreger, 2013 and Beetsma, Furtuna, and Giuliodori, 2017 among others). Hence, we use *ex-post* realized data as opposed to *ex-ante* real-time data (as instead done in the existing literature) to measure the true extent of a consolidation effort and avoid potential measurement errors.[3]

We also contribute to the literature in other aspects. The studies reviewed above limit the analysis to changes in overall tax revenues and neglects potential composition effects. An attempt to empirically study the impact of tax policy changes on inequality in a multivariate framework is carried out by Martínez-Vázquez, Vulovic, and Dodson, 2012 and Muinelo-Gallo and Roca-Sagalés, 2013. However, these contributions examine the effects of budget-neutral tax changes rather than of consolidation episodes. Moreover, they do not address the potential endogeneity between fiscal variables and economic activity. Our paper fills this gap in the literature by analyzing the effects of specific tax policy measures on income inequality in the context of fiscal adjustments that are not driven by output stabilization considerations.

Our sample includes 16 OECD countries during the period 1978-2012. To identify episodes of fiscal consolidations, we use the action-based datasets compiled by Devries et al., 2011 and Alesina, Favero, and Giavazzi, 2015. These two databases exclusively consider consolidation episodes aiming solely at reducing the government deficit, and not at stabilizing economic activity. This allows us to limit the potential endogeneity between tax-based consolidations and GDP. Next, we make use of the OECD Revenue Statistics Database (**OECD:Revenu**) to measure the actual extent of the tax consolidation effort. The OECD Revenue Statistics Database also allows us to quantify changes in specific tax instruments, and therefore pin down which particular instrument was most used during each consolidation year and analyze its effects. As measures of income inequality, we use both the market and the disposable income Gini indexes, as well as data on top income shares and income ratios. For the estimation, we rely on a panel vector autoregressive (PVAR) methodology. Although our analysis is at the macro level, we explore potential heterogeneities in the labor market outcomes of fiscal consolidations for groups of agents of different sex and age.

We begin by establishing some stylized facts about the design of tax-based consolidations. We show that governments normally rely on either direct or indirect taxes (rather than a combination of them) to consolidate the budget, which further highlights the relevance of our research question. We also show that personal taxes are by far the most preferred instrument, with general consumption taxes (such as

[3] Importantly, we still make use of narrative datasets to identify episodes of fiscal consolidations that do not depend on contemporaneous changes in output growth. Our analysis is different from previous ones insofar as we rescale the consolidation episodes by the *ex-post* realized changes in tax revenues rather than *ex-ante* real time estimates (see Section 3.2.1 for more details).

value added and sale) being a distant second. Turning to the empirical analysis, our results point to statistically significant and economically meaningful positive effects of tax-based consolidations in reducing income inequality. However, this comes at the cost of a contraction in economic activity. Next, we find that using different tax instruments matters; that is, indirect taxes reduce income inequality by more than direct taxes. Looking at the specific tax instruments, personal taxes seem to be the most suited to reduce inequality while at the same time minimizing the equity-efficiency trade-off. General consumption taxes also have substantial positive short-run effects on the labor force participation rate, which dampens their recessionary effects. Finally, we do not find corporate taxes to have any impact on income inequality.

Our findings point to the existence of a positive labor supply channel of indirect taxes. Higher indirect taxes decrease the number of goods that households can buy given a certain income and create incentives for agents voluntarily out of the labor force to start searching for a job. That is, the income effect dominates the substitution effect. This, in turn, promotes labor force participation, especially of middle-aged women, and reduces income inequality. Instead, we do not find evidence backing the hypothesis of a negative labor demand channel through which fiscal consolidations jeopardize income equality.[4] Lastly, we find little evidence pointing to a positive government redistribution channel of tax-based consolidations.

The remainder of the paper is structured as follows. Section 3.2 presents the dataset and explains the empirical methodology. Section 3.3 contains our baseline results on the overall effects of tax-based consolidations. Section 3.4 focuses on the composition of tax-based consolidations and disentangles the specific effects of each single tax instrument. Section 3.5 concludes. We report extensive robustness checks in Appendix B.

3.2 Dataset and Methodology

3.2.1 Dataset and Stylized Facts

Our empirical analysis covers 16 OECD countries between 1978 and 2012 at the annual frequency. To identify exogenous fiscal consolidation shocks, we follow a narrative approach and start from the action-based dataset compiled by Devries et al., 2011. Devries et al., 2011 make use of official policy records to gather real-time data on estimated changes in tax revenues and public spending resulting from consolidation measures decided in 17 OECD countries during the period 1978-2009. The peculiarity of this dataset is that it only selects those consolidation episodes that have the sole objective of reducing the budget deficit and are not driven by output stabilization, labor market conditions or income inequality developments. Examples of such episodes may include consolidations that are caused by the operation of fiscal

[4] Although some tax instruments, particularly specific consumption taxes, do cause the unemployment rate to increases, this is not accompanied by an increase in income inequality.

rules, by the presence of a ceiling on public debt or by commitments to reduce the public debt taken by governments in a supranational context. Next, we use another action-based dataset compiled by Alesina et al., 2015, which identifies additional fiscal consolidation episodes in 11 countries between 2010 and 2013, employing the same method of Devries et al., 2011.[5]

After merging the two action-based datasets of Devries et al., 2011 and Alesina et al., 2015, we have an unbalanced panel with data on consolidation episodes occurred in Austria, Belgium, Denmark, France, Germany, Ireland, Italy, Portugal, Spain, the United Kingdom, and the United States over the period 1978-2013, and Australia, Canada, Finland, Japan, the Netherlands and Sweden over the period 1978-2009. In carrying out the analysis, we restrict the sample to the period 1978-2012 due to the availability of data on the Gini index. Following Alesina, Favero, and Giavazzi, 2015, we exclude the Netherlands from the sample, as for this country the consolidation episodes identified by Devries et al., 2011 are endogenous to output growth and cannot be used as fiscal policy instruments.

The consolidation episodes in our dataset differ significantly in their size and nature. We classify them into three categories: (i) any consolidation episode, (ii) spending-based consolidations (i.e., when spending cuts are larger than tax hikes), (iii) tax-based consolidations (i.e., when tax hikes are larger than spending cuts). We identify 188 consolidation episodes, of which 112 are spending-based, 73 tax-based, and three featured tax hikes exactly equal to spending cuts (hence, we classify them as neither spending- nor tax-based consolidations). Since tax hikes and spending cuts are likely to affect income inequality differently, including episodes of spending-based consolidations in the analysis may confound the results on the effects of tax

[5] An alternative approach to identify episodes of fiscal adjustments consists of simply considering periods of large changes in tax revenues and government spending. We refer to it as the statistical approach. The narrative approach and the statistical approach may yield very different results when analyzing the effects of fiscal policy. For instance, Guajardo, Leigh, and Pescatori, 2014 analyze the growth impact of fiscal consolidations using first the dataset compiled by Devries et al., 2011, based on the narrative approach, and then a dataset compiled by Alesina and Ardagna, 2013, based instead on the statistical approach. Estimates based on the former suggest that fiscal consolidation has Keynesian contractionary effects. On the contrary, estimates based on the latter find neoclassical expansionary effects (for similar findings see also Afonso and Jalles, 2014). Potential reasons behind these opposite results may be that the statistical approach (i) tends to classify periods in which the budget balance improved simply due to favorable economic conditions as periods of fiscal consolidation, and (ii) gives less weight to unsuccessful episodes of fiscal consolidation (i.e., when the government does not succeed in consolidating the budget). We acknowledge that the narrative approach may also have some drawbacks. First, it largely relies on subjective judgments to identify exogenous fiscal consolidations and quantify their magnitude. Second, it may not eliminate the endogeneity between fiscal policy and growth completely, if the debt level and changes in output are correlated. As it will be clear later, we try to address the first point by combining data from the action-based datasets with *ex-post* fiscal data. Overall, we believe that the narrative approach, although not immune from criticisms, is the most suited approach to address the endogeneity problem.

changes. Therefore, we mainly focus on tax-based consolidations. We use data on spending-based consolidations for a robustness check.

Besides distinguishing between spending cuts and tax hikes, the action-based dataset of Devries et al., 2011 does not always provide additional information about the composition of each consolidation episode. To gather data about the different tax instruments used by governments, we rely on the (OECD Revenue Statistics Database). This provides information on revenues generated by different tax instruments, measured as a share of GDP. Hence, our identification strategy, summarized in Appendix B.1, consists in analyzing *ex-post* changes in tax revenues in years of tax-based consolidations as identified through the narrative approach.

Our focus on *ex-post* changes of tax revenues also addresses a concern that the real-time information about the size of fiscal consolidations contained in narrative datasets are imprecise estimates of the actual fiscal policy changes implemented by governments. Indeed, a large literature has shown that fiscal plans and real-time fiscal measures tend to overstate the actual (*ex-post*) impact of fiscal consolidations (Beetsma, Furtuna, and Giuliodori, 2017; Beetsma, Giuliodori, and Wierts, 2009; Castro, Pérez, and Rodríguez-Vives, 2013; Frankel and Schreger, 2013; Gupta et al., 2017).[6] Of particular relevance for our analysis, Beetsma, Furtuna, and Giuliodori, 2017 compare the *ex-post* realizations of the consolidation episodes identified by Devries et al., 2011 and find a systematic shortfall of *ex-post* realized revenues relative to the narrative measure (of 0.15 percentage points of GDP for tax revenues). By rescaling the narratively identified tax-based consolidation episodes with the *ex-post* realized changes in tax revenues we mitigate the concern that our tax variable is an oversized representation of the real consolidation effort. Admittedly, this rescaling exercise might introduce a source of endogeneity in our shock variable, as tax revenues arguably respond to GDP. Following the methodology used in Guajardo, Leigh, and Pescatori, 2014, we show that this potential source of endogeneity is not important in practice (see Appendix B.1 for more details).

Turning to the role of specific tax instruments, we focus on two broad ones: direct and indirect taxes. Property and wealth taxes show only small changes relative to direct and indirect taxes during tax-based consolidations. Hence, we do not consider them in the analysis. Direct taxes include (i) personal taxes, (ii) corporate taxes, (iii) social security contributions (henceforth SSC) and (iv) payroll taxes. Indirect taxes include: (i) general consumption taxes (henceforth GT), such as value added and sale taxes, (ii) taxes on specific goods and services (henceforth SGS), such as for instance excises and fiscal monopolies, and (iii) taxes on the use of goods and services (henceforth UGS), such as taxes on vehicles. For illustrative purposes, a flow-chart of the composition of direct and indirect taxes is depicted in Appendix

[6] For instance, Beetsma, Giuliodori, and Wierts, 2009 and Castro, Pérez, and Rodríguez-Vives, 2013 explore how fiscal data revisions in the EU gradually develop as the time distance from the original fiscal plan increases and suggest that governments try to systematically exploit the margins of acceptable reporting, but are subsequently corrected by Eurostat.

B.2. Due to the marginal change of payroll and UGS tax revenues during tax-based consolidations, we exclude them from the analysis.

Table 3.1 shows the mean values of tax hikes and spending cuts identified through the narrative approach and the corresponding *ex-post* changes in tax revenues for the three different categories of consolidation episodes discussed above. The average *ex-post* change in tax revenues during tax-based consolidations (0.57% of GDP) was lower than the government's real-time estimate (0.72% of GDP). This figure is perfectly in line with the finding of Beetsma, Furtuna, and Giuliodori, 2017 and reinforces the case for rescaling the narratively identified consolidation episodes by the actual *ex-post* realized changes in tax revenues to correctly quantify the real consolidation effort and thus limit the potential for measurement errors. We will come back to this issue when performing robustness checks on our baseline specification.

Table 3.1: Mean values of consolidation episodes (% of GDP) – 1978-2012

	Narrative approach		Realization	Obs.
	Tax	Spending	Tax	
Any consolidation	0.45	0.64	0.45	188
Spending-based	0.28	0.91	0.37	112
Tax-based	0.72	0.24	0.57	73

Notes: Narrative approach refers to the real-time consolidation episodes identified by Devries et al., 2011 for the 1978-2009 period and Alesina et al., 2015 for the 2010-2013 period. Realization refers to tax data provided by the OECD Revenue Statistics Database. Tax and spending refer respectively to changes in total tax revenues and general government spending. The spending-based sample comprises episodes in which spending cuts, as identified through the narrative approach, were larger than tax hikes, and *vice versa* for the tax-based sample. All numbers are expressed as averages. The column obs. denotes the number of observations in the sample.
Source: Devries et al., 2011, Alesina et al., 2015, OECD Revenue Statistics Database and own calculations.

Table 3.2 presents descriptive statistics of the change in the different tax instruments we focus on during (i) the full and (ii) the tax-based consolidation samples. Governments typically relied the most on direct taxes, and particularly personal taxes, to consolidate the budget. Among indirect taxes, GT were by far the most used instrument.

Next, we notice that the correlation between the changes in direct and indirect tax revenues during tax-based consolidations years is close to 0. That suggests that in most cases governments resorted either to direct or indirect taxes, rather than to a combination of them, to consolidate the budget. Table 3.3 further shows the correlation coefficients among the different direct and indirect tax instruments during tax-based consolidations. Personal and corporate taxes and SSC display very low correlations among each other. In this light, our aim to assess the composition effects of tax-based consolidations gains even more relevance.

Along with the series of tax shocks, we also include other variables in our dataset. As a measure of inequality, we rely on the Gini index, calculated both from market and

Table 3.2: Mean changes of different tax instruments (% of GDP) – 1978-2012

	Full sample	Tax-based
Total	0.13	0.57
Direct	0.09	0.41
Indirect	0.02	0.15
Personal	0.01	0.22
Corporate	0.02	0.07
SSC	0.05	0.09
GT	0.05	0.12
SGS	-0.04	0.02

Notes: The tax-based sample comprises consolidation episodes in which tax hikes, as identified through the narrative approach, were larger than spending cuts. SSC, GT and SGS stand for, respectively, social security contributions, general taxes and specific goods and services. For a precise definition of the different tax categories refer to Appendix B.2.
Source: OECD Revenue Statistics database and own calculations.

disposable incomes.[7] This measure has two main advantages: it is Lorenz-consistent, and its estimates are widely available, both over time and across countries.[8] We collect the Gini indexes from the Standardized World Income Inequality Database (SWIID, Version 4.1), compiled by Solt, 2016. The advantage of using the SWIID is that it provides the most comparable series across countries.

For data on the labor force participation and the unemployment rates (both overall and by sex and age), as well as for real per capita GDP, we rely on information contained in the OECD Economic Outlook. The labor force participation rate is defined as the percentage of the population aged between 15 and 64 years which is either employed or unemployed. Similarly, the unemployment rate is the share of

7 The Gini index measures the extent to which the distribution of income among individuals deviates from a perfectly equal distribution. It ranges between 0 (perfect equality) and 100 (perfect inequality). It is usually estimated from survey data, and it can be based on both market and disposable income. The Gini index of market income (or gross Gini index) is calculated on income before taxes and transfers. The Gini index of disposable income (or net Gini index) is calculated on income after taxes and transfers.

8 A measure of inequality is said to be Lorenz-consistent if it satisfies the following four criteria: (i) the anonymity principle (i.e. it does not matter who is earning the income), (ii) the population principle (i.e., the population size does not matter), (iii) the relative income (i.e. only relative income matters), and (iv) the Dalton transfer principle (i.e. if an income distribution can be achieved from another by constructing a sequence of regressive transfers, then the newly created distribution must be deemed more unequal than the original one).

Table 3.3: Correlation of changes in tax revenues during tax-based consolidation years

	Personal	Corporate	SSC	GT	SGS
Personal	1.00	0.04	0.04	-0.07	-0.14
Corporate		1.00	-0.05	-0.10	0.12
SSC			1.00	0.11	0.06
GT				1.00	-0.34
SGS					1.00

Notes: SSC, GT and SGS stand for, respectively, social security contributions, general taxes and taxes on specific goods and services.
Source: OECD Revenue Statistics database and own calculations.

jobless people in the labor force between 15 and 64 years who are available to work and are actively seeking employment.[9]

To carry out some extensions and robustness checks, we collect additional data. As alternative inequality measures, we employ the share of income belonging to the richest 0.01%, 0.01-1%, and 1-10% individuals as well as the ratios of income of the individuals at the 90^{th}, 50^{th} and 10^{th} percentiles of the income distribution.[10] Both top income shares and the ratios of income account for market income inequality and are measured in percentage points. We obtain top income share data from the World Wealth and Income Database (WID, 2015). Income ratios are taken from the OECD Economic Outlook. Due to some missing values, we linearly interpolate these alternative inequality series.

To construct alternative tax shock variables, we retrieve data on the standard rate of the general consumption tax for all the countries in our sample except the United States, where consumption taxes are set by the states rather than the federal government. For European countries, we use information contained in EC, 2016. For Australia, Canada and Japan we use information available on their respective government's websites.

To identify episodes of systemic banking crises, we use information contained in Laeven and Valencia, 2012. Moreover, from the OECD Economic Outlook we collect: (i) government consumption as a percentage of GDP, (ii) the consumer price inflation rate, (iii) the employment rate, defined as employed people as share of total population aged between 15 and 64 years, (iv) average hours worked per employed

9 The only exception is Austria, for which data for the 15-64 age group is not available before 1994. For this country, we use labor force participation and unemployment rates among all age groups.

10 Top income shares are estimated from tax filing data and are based on market incomes. They are used as proxies for the concentration of incomes in the right tail of the income distribution. The ratios of income are estimated from survey data.

individual, (v) GDP per hour worked. Finally, we collect data on (i) imports and exports as a percentage of GDP, (ii) the trade balance as a percentage of GDP, and (iii) gross savings as a percentage of GDP from WB, 2015.

3.2.2 Methodology

For the econometric analysis, we make use of PVAR models. By adopting a multi-equation methodology, we account for potential interactions among the endogenous variables that might be otherwise overlooked within a single-equation framework.[11]

Given that our dataset is at the annual frequency, we estimate the VAR model in a panel format by pooling together observations for all the countries considered. This approach implies imposing cross-country homogeneity on the relationships among the endogenous variables. To take into account cross-country heterogeneity, we follow Beetsma and Giuliodori, 2011 and include in the regressions country-fixed effects and country-specific linear time trends.[12] Additionally, we include time-fixed effects to control for unobserved common factors. In Section B.3 of Appendix B, we show that our main results are robust to the inclusion of alternative deterministic components.

Our PVAR takes the following standard form:

$$y_{i,t} = A_0 + A_1 y_{i,t-1} + A_2 y_{i,t-2} + \alpha_i + \delta_t + \tau_{it} + \epsilon_{i,t} \tag{3.1}$$

where the sub-indexes (i, t) refer respectively to country and time, $y_{i,t}$ is the vector of endogenous variables, the As are the coefficient matrices, and α_i, δ_t and τ_{it} denote respectively country-fixed effects, time-fixed effects and country-specific linear time trends. Finally, $\epsilon_{i,t}$ is a vector of error terms, which are assumed to be serially uncorrelated. The baseline PVAR model includes five variables, namely the tax shock (as a percentage of GDP), the real per capita GDP (in logs), the disposable Gini index (in units), the unemployment rate and the labor force participation rate (both in percentage points).

Following Sims, 1980, the endogenous variables enter the PVAR in levels. This allows us to model possible cointegrating relationships among them. In line with standard practice in the VAR literature on the macro effects of fiscal policy at the yearly frequency and consistently with the Akaike and Schwarz information criteria, we opt for a baseline specification containing two lags of the endogenous variables. In Section B.3 of Appendix B we show that our results are robust to different lag specifications and to using first differences rather than levels. After adjustments, and due to some missing data, we have a total of 479 observations.

[11] Several contributions in the literature employ the VAR methodology to estimate the macroeconomic effects of fiscal policy shocks and identify these through the narrative approach. For references, see Ramey, 2011, Guajardo, Leigh, and Pescatori, 2014, and Alesina, Favero, and Giavazzi, 2015.

[12] We include linear trends since the real GDP, the Gini index and the labor force participation all display a trending behavior.

To construct our tax-based consolidation shock variable, we create a dummy d_t^1 taking value 1 in years where governments implement a tax-based consolidation and 0 otherwise. We then interact this dummy variable with the first difference of total tax revenues as a percentage of GDP. Next, we define direct (indirect) tax-based consolidations as those episodes in which (i) governments implement a tax-based consolidation and (ii) the change in direct (indirect) tax revenues is larger than that of indirect (direct), property and wealth tax revenues. We then create a dummy variable d_t^2 (d_t^3) for direct (indirect) tax-based consolidations and we interact it with the change in direct (indirect) tax revenues. We further distinguish between personal, corporate, SSC, GT and SGS tax-based consolidations. To create the respective shock variables we proceed in a fashion similar to what described above.

To sum up, our shock variables are constructed according to the following formula:

$$X_{i,t}^j = d_{i,t}^j \Delta t_{i,t}^j \tag{3.2}$$

where $j = 1$ stands for overall tax-based consolidation, and $j = 2, .., 8$ for direct, indirect, personal, corporate, SSC, GT and SGS tax-based consolidation; $t_{i,t}^j$ denote revenues stemming from tax instrument j and Δ is the first difference operator.[13] We report the number of observations for each tax-based consolidation sample and its mean value in Table 3.4.

Table 3.4: Mean value and frequency of tax-based consolidation shocks

	Overall	Direct	Indirect	PIT	CIT	SSC	GT	SGS
Mean	0.57	0.66	0.51	0.52	0.52	0.41	0.61	0.27
Obs.	73	43	23	28	9	6	13	10

Notes: Mean refers to the mean value of tax-based consolidation shock variables (in percentage of GDP). Obs. refers to the number of each tax-based consolidation shock. PIT, CIT, SSC, GT and SGS stand for, respectively, personal income taxes, corporate income taxes, social security contributions, general taxes and specific goods and services. Direct and indirect tax-based consolidations do not sum up to the number of overall tax-based consolidations since in 7 instances the change in property and wealth taxes was higher than that of direct and indirect taxes.
Source: OECD Revenue Statistics Database and own calculations.

Our approach to construct the tax shocks using tax revenues has the advantage of capturing the effects of policy interventions on both the tax rate and the tax base. Indeed, changes to the tax base are fairly common. Tax credits, exemptions, or deductions are often introduced or removed. Even not indexing the nominal threshold defining the different brackets of the personal income tax to the price level amounts to a change in the base. Ideally, we would like to use a proxy for changes

[13] We also estimate the model using alternative shock variables, which we construct by interacting the change of revenues stemming from each tax instrument with the dummy variable $d_{i,t}^1$ taking value 1 in years where governments implement a tax-based consolidation and 0 otherwise. The results are qualitatively in line with those obtained using our standard shock variables.

in the tax rate and one for the tax base. However, due to lack of a quantifiable measure for the tax base, using tax revenues is the most suited approach to address our research question.

Given the characteristics of the action-based datasets, which identify consolidation episodes that were motivated by the sole objective of reducing the budget deficit, the most natural way to identify the PVAR in Equation (3.1) is to use a Cholesky decomposition. This strategy is particularly convenient when one of the variables is exogenous to the others, as in our case. By ordering our tax variable first, we impose this to be contemporaneously unaffected by GDP, the Gini index, the unemployment rate or the labor force participation rate. On the other hand, we allow these variables to be contemporaneously affected by the tax shock and by each other, thus capturing all potential feedback effects. Moreover, an important advantage of using the Cholesky decomposition is that the order of the variables after the shock does not matter (see Christiano, Eichenbaum, and Evans, 1999 for a theoretical explanation).

In the following sections, we estimate impulse response functions (hereafter IRFs) to tax shocks over a 10-year horizon and construct confidence intervals as ± 1.645 standard errors (equivalent to a 90% confidence level) around the mean response.[14] To compute standard errors we use Monte Carlo methods with 1,000 replications. The GDP response is measured in percentage change, while the response of the Gini index is in units and the response of both the unemployment and the labor force participation rates are in percentage points.

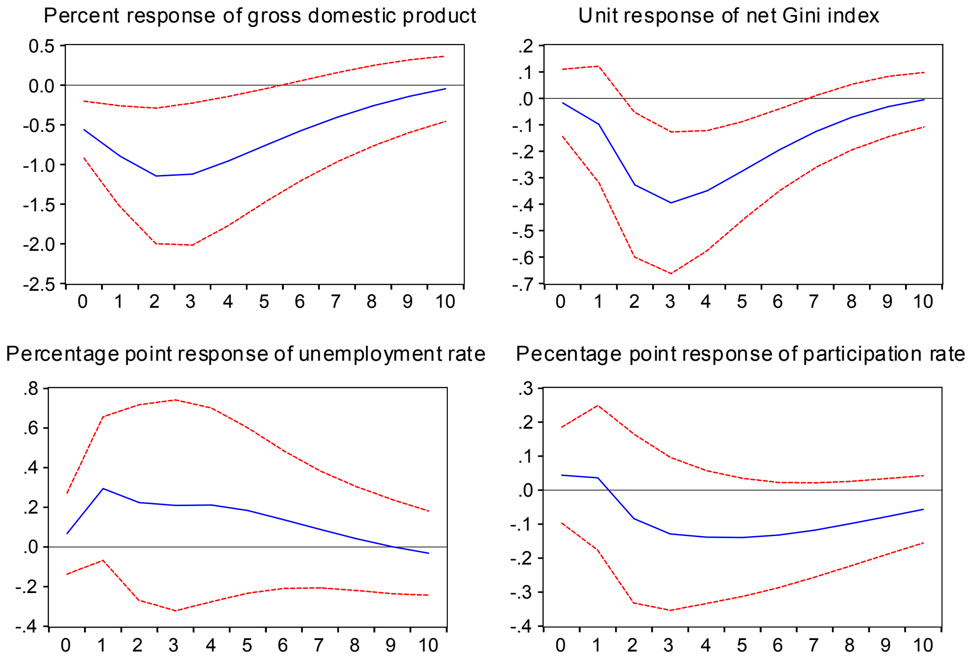
3.3 Overall Effects of Tax-based Consolidations

In this section, we present our baseline results. Figure 3.1 shows the response of real GDP, the disposable income Gini index, the unemployment rate, and the labor force participation rate to a 1% of GDP increase in total tax revenues during tax-based consolidation episodes. The response of the Gini index is not statistically different from 0 on impact, but it turns negative and significant two years after the shock (in the order of -0.3 percentage points), and it remains significant up to 6 years after the shock. In absolute value, the 0.4 drop in the Gini index after three years (the peak response) is equal to about a tenth of its sample standard deviation. Hence, in line with Agnello and Sousa, 2014, our estimates suggest that tax-based consolidations have statistically significant and economically meaningful effects in decreasing disposable income inequality. These effects disappear in the longer-run.

Consistently with the previous empirical literature (see, among others, Guajardo, Leigh, and Pescatori, 2014, Alesina, Favero, and Giavazzi, 2015, and Jordà and Taylor, 2016), a 1% of GDP increase in tax revenues has significant negative short to medium-run effects on real GDP. Output decreases by 0.6% on impact, by 0.9% after

[14] Alternative approaches to construct confidence intervals are also accepted in the literature (for instance, Ball et al., 2013 use ± 1 standard errors), but we opt for a more conservative level of statistical significance.

Figure 3.1: The effects of a 1% of GDP tax-based consolidation shock



Note: The central solid blue line represents the response to a 1% tax shock, the solid red lines represent the 90% confidence intervals.

1 year, 1.1% after 3 years, and 0.8% after 5 years. The effect becomes insignificant in the long run. The responses of the unemployment rate and the labor force participation rate are respectively positive and negative in the short to medium term, which can be interpreted as a consequence of the economic contraction. However, neither of them is statistically different from 0. In general, the dynamics of the macroeconomic effects that we estimate, which vanish within a 5-year horizon after the shock are consistent with the model-based evidence provided by Coenen et al., 2012.

We now investigate to what extent changes in government redistribution may be driving our result that tax-based consolidations decrease income inequality. To do so, we estimate a 6-variable PVAR featuring both the market and the disposable income Gini indexes. The difference between them can be interpreted as a measure of the reduction of inequality that is achieved through taxes and transfers, with higher values indicating more redistribution. If the two variables exhibit similar responses, then we could hypothesize that changes in disposable income inequality are mostly driven by changes in the market income distribution. Conversely, if the responses are different, changes in the amount of government redistribution could (also) be driving the response of disposable income inequality. Table 3.5 reports relevant results. The estimates for the market and the disposable Gini indexes are very similar. That suggests that the reduction in inequality observed in our baseline specification is achieved mainly through a reduction in market income disparities.

Table 3.5: Augmented specification with both gross and net income Gini indexes

	Impact	1y	3y	5y	10y
GDP	**-0.64**	**-1.00**	**-1.15**	-0.65	0.10
Disposable Gini	-0.04	-0.13	**-0.41**	**-0.26**	0.02
Market Gini	-0.12	-0.18	**-0.57**	**-0.30**	0.03
Unemployment	0.06	0.29	0.19	0.12	-0.09
Participation	0.06	0.04	-0.14	-0.16	-0.04

Notes: The table reports the response to a 1% of GDP tax-based consolidation shock. Bold numbers indicate significance at the 10% confidence level.

To verify the validity of our results, we carry out a number of robustness checks on our baseline model. We report relevant results in Section B.3 of Appendix B. We first estimate responses to a 1% increase in total taxes during both any consolidation year and a spending-based consolidation year. The estimated coefficients suggest that the contemporaneous presence of spending cuts and tax hikes might confound the results on the effects of tax hikes. That is why we exclusively focus on tax-based consolidations. Next, we show that our baseline results are not biased by (i) anticipation effects, (ii) reverse causality issues, or (iii) episodes of consolidations during which revenues actually decrease. Although in Appendix B.1 we already checked that our tax shock variable is orthogonal to contemporaneous changes in output growth, we also show that our results are robust to using an alternative shock variable employing cyclically adjusted taxes. Finally, we estimate the model employing the original real-time tax estimates compiled by Devries et al., 2011 and Alesina et al., 2015 and show that using such measure the results are qualitatively in line but quantitatively weaker than under our baseline, thus highlighting the importance of using *ex-post* as opposed to *ex-ante* measures of the fiscal consolidation effort.

As additional robustness checks, we repeat the estimation (i) including different deterministic components, (ii) using different lags specifications, and (iii) relying on the local projection method. Moreover, we show that our results are not driven by (i) particular countries, (ii) time periods, and (iii) shock outliers. We also estimate the model including a set of control variables commonly used in the literature. Finally, we use alternative measures of inequality, such as income ratios and the top income shares. Overall, these robustness checks confirm the validity of our baseline results.

3.4 Composition Effects

3.4.1 Direct and Indirect Tax-based Consolidations

In the previous section, we have analyzed the effects of changes in overall taxes during fiscal consolidation episodes. In what follows, we disentangle the effects of specific tax instruments. As a first step, we estimate IRFs to a 1% of GDP in direct and indirect tax-based consolidation shocks. Results are presented in Table 3.6.

Table 3.6: Composition effects of tax-based consolidations

	Impact	1y	3y	5y	10y
a) Tax-based consolidation (baseline)					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) Direct tax-based					
GDP	**-0.59**	-0.72	-1.13	-0.80	-0.06
Disposable Gini	0.03	0.13	-0.14	-0.17	0.00
Unemployment	0.06	0.19	-0.06	-0.04	-0.10
Participation	-0.06	0.06	-0.08	-0.08	0.00
c) Indirect tax-based					
GDP	**-1.79**	**-4.10**	**-4.90**	**-3.86**	-0.74
Disposable Gini	-0.32	**-0.79**	**-1.25**	**-0.88**	-0.17
Unemployment	**0.75**	**1.90**	**2.43**	**1.82**	0.10
Participation	**0.59**	0.22	-0.26	**-0.56**	-0.32

Notes: The table reports the response to a 1% of GDP overall, direct and indirect tax-based consolidation shock respectively. Bold numbers indicate significance at the 10% confidence level.

A typical direct tax-based consolidation does not have significant positive effects in reducing disposable income inequality, while an indirect-based one does. The Gini index decreases on average by 0.8 percentage points the year after an indirect tax-based consolidation shock. After three years the decline is equal to 1.3, while after five years it is 0.9. Regardless of the instrument used, a tax shock always has a negative and significant effect on real GDP on impact. However, this effect is stronger and more persistent in the case of indirect tax-based consolidations. Consistently with the large and persistent decline in output, the unemployment rate significantly increases both on impact and in the short to medium term in the case of indirect taxes. Instead, the labor force participation rate increases by 0.6 percentage points on impact after an indirect tax shock. The reaction of the labor force participation rate decreases and even turns negative and significant after five years, which we interpret as a lagged response to the contraction in economic activity. In contrast, neither the unemployment rate nor the participation rate displays significant reactions to a direct tax-based consolidation.

Concerning the estimated response of real GDP, our results are supported by recent theoretical work by Gehrke, 2018. In analyzing fiscal policy rules in a new Keynesian model with labor market frictions, Gehrke, 2018 finds that multipliers are larger for consumption than labor taxes. This result hinges on the presence of wage bargaining between firms and employers and on the fact that the path of labor taxes is time-varying. Under Nash bargaining, the wage is a weighted average of the intertemporal firms' profits and the workers' reservation wage (the unemployment benefit) scaled by the labor tax. The latter drives a wedge between the value of working and that of not working. If labor taxes were constant, the wage would be as in typical Nash bargaining model. However, expected changes in labor taxes determine how the workers value the share of future firm profits. A decrease (increase) in the labor tax depresses (increases) wage demands by workers, as it increases (decreases) the value of working. Hence, the after-tax wage barely changes and so does labor supply. Changes in consumption taxes, instead, directly affect aggregate demand, with larger effects on output. This result also holds in a model without labor market frictions. For instance, Forni, Monteforte, and Sessa, 2009 estimate a DSGE model for the Euro area and find relatively strong multipliers for consumption taxes. This effect can be explained by monetary policy being tightened following an increase in consumption taxes (due to the increase in inflation). Higher taxes and tighter monetary policy induce agents to decrease consumption and firms to scale back production, with negative effects on employment.

Instead, our estimates contrast those of Muinelo-Gallo and Roca-Sagalés, 2013, who find that an increase in direct taxes has a negative impact on both net inequality and growth whereas increasing indirect taxes does not have significant effects. However, these authors analyze the effects of budget-neutral changes in fiscal policy rather than of fiscal consolidations. Moreover, their model neglects potential dynamic effects, and it assumes market income inequality to be exogenous, whereas it may well be endogenous to both growth and fiscal policy.

We now narrow the focus on the effects of indirect taxation on inequality. Most of the literature assumes indirect taxes to be regressive, since low-income agents normally spend a larger fraction of their income on consumption goods relative to high-income households. However, following this argument, to find a direct positive effect of indirect taxes on inequality, a measure of consumption-based inequality should be used. Unfortunately, limited cross-country data prevents us from investigating further the validity of this line of thought.

On the other hand, using data on income inequality, we find that indirect taxes improve equity. This outcome may be partly explained by the operation of a positive labor supply channel. Our results suggest that indirect taxes create incentives for agents to participate more actively in the labor market. That may be due to a negative income effect, since indirect taxes raise the price of the consumption basket. Although we cannot estimate it due to lack of data on labor market outcomes by income groups, the extent of the labor supply channel is likely to be stronger for lower-income agents. The reason for this is that they tend to spend a more substantial fraction of their income on consumption goods. Hence, they should be relatively more affected by an increase in indirect taxes.

The mechanisms described above are supported by the theoretical analysis developed by Coenen, Mohr, and Straub, 2008. These authors develop a DSGE model with two types of agents: unconstrained (Ricardian) agents and liquidity constrained (hand-to-mouth) agents, who can be considered as a proxy for, respectively, high- and low-income agents. Coenen, Mohr, and Straub, 2008 show that an increase in the consumption tax dampens the consumption of both types of agents. However, while unconstrained/high-income agents reduce their labor supply, constrained/low-income agents tend to work even more, since they rely mainly on adjusting the supply of labor services to smooth consumption. Conversely, an increase in the labor income tax reduces both consumption and hours worked. Hence, similar to our analysis, in Coenen, Mohr, and Straub, 2008 the positive labor-supply channel is at work when indirect taxes increase, but not when the personal income tax increases. Other papers stressing the role of labor-supply channels in the analysis of the distributional effects of taxation are Buscher et al., 2001, Böhringer, Boeters, and Feil, 2005, and Pestel and Sommer, 2017. Crucially, in line with our results, these papers show that the labor-supply channel is more relevant for low-income agents.

The hypothesis that the marginal propensity to work may vary with income or wealth has also been theoretically investigated by Athreya, Owens, and Schwartzman, 2016. In their analysis, a wealth-based redistribution program from high-income to low-income households can decrease the labor supply of low-income agents by more than it raises that of high-income agents, if at least some of the former are borrowing-constrained. This is because borrowing-constrained agents will not save any portion of the transfer, thus increasing their consumption and leisure time.

Additionally, Blundell, Pistaferri, and Saporta-Eksten, 2016 have further highlighted another form of labor supply heterogeneity, namely within household heterogeneity. In their model, female labor supply serves as an important household insurance mechanism and their findings evidence how spouses react differently to wage changes. In this light, we next investigate the role of female labor supply in the context of our analysis.

We extend our baseline specification on the effects of an indirect tax-based consolidation by adding more variables. Results are reported in Table 3.7. First, we add the inflation rate (Panel (b)). This goes up by 1.9 and 2.3 percentage points respectively on impact and after one year, thus confirming that the price

Table 3.7: Additional results on indirect tax-based consolidations

	Impact	**1y**	**3y**	**5y**	**10y**
a) Indirect-tax based					
GDP	**-1.79**	**-4.10**	**-4.90**	**-3.86**	-0.74
Disposable Gini	-0.32	**-0.79**	**-1.25**	**-0.88**	-0.17
Unemployment	**0.75**	**1.90**	**2.43**	**1.82**	0.10
Participation	**0.59**	0.22	-0.26	**-0.56**	-0.32
b) Inflation rate					
GDP	**-1.64**	**-3.81**	**-4.56**	**-3.70**	-0.90
Disposable Gini	-0.29	**-0.72**	**-1.17**	**-0.85**	-0.17
Unemployment	**0.67**	**1.72**	**2.23**	**1.70**	0.17
Participation	**0.53**	0.13	-0.21	**-0.51**	**-0.34**
Inflation rate	**1.88**	**2.34**	-0.41	**-0.48**	-0.13
c) Men and women participation					
GDP	**-1.76**	**-3.93**	**-4.48**	**-3.41**	-0.64
Disposable Gini	-0.26	**-0.67**	**-1.06**	**-0.75**	-0.16
Unemployment	**0.70**	**1.77**	**2.16**	**1.55**	0.05
Men participation	0.06	0.19	-0.24	-0.41	-0.20
Women participation	**1.08**	0.21	-0.22	**-0.54**	-0.30
d) Men and women participation and employment					
GDP	**-1.85**	**-4.10**	**-4.75**	**-3.68**	-0.77
Disposable Gini	-0.27	**-0.66**	**-1.04**	**-0.76**	-0.18
Men employment	-0.64	**-1.70**	**-2.16**	**-1.72**	-0.10
Women employment	**0.64**	-0.56	**-1.48**	**-1.55**	-0.49
Men participation	0.08	0.21	-0.20	-0.37	-0.22
Women participation	**1.13**	0.24	-0.17	-0.48	-0.35
e) Men and women participation and employment, 45 to 54 year age group					
GDP	**-1.89**	**-5.27**	**-6.86**	**-4.82**	-1.11
Disposable Gini	-0.39	**-0.86**	**-1.30**	**-0.97**	-0.36
Men employment, 45 to 54	-0.55	**-1.90**	**-2.75**	**-2.21**	-0.41
Women employment, 45 to 54	**1.35**	-0.01	**-1.34**	**-1.53**	-0.85
Men participation, 45 to 54	-0.16	-0.51	-0.54	**-0.45**	-0.12
Women participation, 45 to 54	**1.59**	0.66	0.01	-0.34	-0.56

Notes: Panel (a)-Panel (e) report the response to a 1% of GDP indirect tax-based consolidation under alternative PVAR specifications. Bold numbers indicate significance at the 10% confidence level.

of the consumption basket does increase following an indirect tax-based consolidation. Second, we distinguish between women and men labor force participation rates (Panel (c)). In accordance with the several contributions emphasizing higher participation elasticities for women (see for instance Blundell, Pistaferri, and Saporta-Eksten, 2016 and Bargain, Orsini, and Peichl, 2011), we find a significant response of 1.1 percentage points in female labor force participation, while the response of male participation is not significant. Third, to check that higher participation is reflected in higher employment, we estimate the model including employment rates as percentages of the population instead of the unemployment rates (Panel (d)). Women employment increases by 0.6 percentage points on impact, whereas the change in men employment is negative (-0.6), but not significant. In the medium term, as the depth of the recession gets larger, both male and female employment rates significantly decrease. However, the magnitude of the declines is smaller for women than for men.

Finally, we estimate the model including both female and male participation and employment rates for different age groups (15 to 24, 25 to 34, 35 to 44, 45 to 54 and 55 to 64). In line with the findings of Blundell, Pistaferri, and Saporta-Eksten, 2016, we hypothesize that second earner spouses should display larger labor supply elasticities. In turn, these are more likely to be prime age women. Hence, we expect stronger responses for women in the 35 to 44 and the 45 to 54 age groups. In Table 3.7 we only report results for the 45 to 54 age group (Panel (e)). This, together with the 35-44 age group is the only one to have significant coefficients. For both groups, the impact responses of female participation and employment are positive and significant, at 1.6 and 1.4 percentage points respectively, whereas those of men are not.

These findings confirm our earlier hypothesis about the existence of a positive labor supply channel of indirect taxes and further point to important gender and age heterogeneities in the agents' labor supply responses. Our results further suggest that, by boosting women participation and employment, higher indirect taxes might also reduce gender inequality.[15] Unfortunately, scarce data availability prevents us to also explore income as another source of heterogeneity in the agents' response to changes in indirect taxes, which could determine the negative effects of indirect tax-based consolidations on equality.[16]

Before narrowing the analysis further down, we perform a number of robustness checks. First, we check whether our results remain valid once including both direct and indirect tax-based consolidation shocks simultaneously rather than one at a time. Second, we check whether our results are driven by a particular country. To

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15 We also estimated the model including both female and male participation by age group for direct tax-based consolidations and found again positive, although not significant, responses for middle-aged women, and negative responses for men.

16 An alternative, but not mutually excluding, channel may be working through the effects of indirect taxes on inequality via capital income and labor demand. The fall in GDP and the rise in unemployment might affect high-skilled workers and capital owners particularly strongly, thereby lowering their market incomes more than for low-income households. This would also lead to a reduction in measured inequality.

this purpose, we estimate the model excluding one country at a time. Finally, we estimate the model using our alternative measures of income inequality: the top income shares and the income ratios. We report and discuss in greater detail all robustness checks in Section B.4 of Appendix B. Overall, our main results are robust to these variations.

3.4.2 Composition Effects of Direct Tax-based Consolidations

In this section, we examine the effects of specific direct tax instruments. In particular, we focus on personal, corporate and SSC tax-based consolidations. Our analysis so far has suggested that direct tax-based consolidations only have a significant negative impact effect on GDP. However, it may be that different instruments have different effects. For instance, personal taxes are generally deemed to be more progressive — and therefore more redistributive — than SSC. This is all the truer in those countries where SSC are directly used to finance the future pensions or where governments call for a cap on the maximum taxable income for SSC. Corporate taxes, instead, may have ambiguous effects on income inequality, as capital income owners may shift the tax burden on wage earners (see Bastagli, Coady, and Gupta, 2012 for a discussion).

Table 3.8 shows the estimates to a 1% of GDP personal, corporate and SSC tax-based consolidation shock.[17] To ease the comparison, in Panel (a) we also display the baseline results for direct tax-based consolidations. The estimates point out that personal tax-based consolidations have a significant effect in reducing income inequality. Following a 1% of GDP shock, the Gini index decreases by 0.7 percentage points after three years and by 0.6 percentage points after five years. Real activity also drops in the short to medium term, with GDP declining by 0.9%, 1.3%, 2.1%, and 1.7% respectively on impact and after one, three, and five years. Instead, neither the unemployment rate nor the labor force participation rate is significantly affected, although the estimates have the expected positive and negative signs.

A corporate tax-based consolidation does have negative, although not statistically significant, effects on GDP. The effects on the other variables are negligible. Since capital profit earners usually have higher incomes than wage earners, in principle, we would expect that corporate taxes should reduce inequality. However, recent empirical evidence suggests that in advanced economies capital profit earners manage to shift between 45% and 75% of the corporate tax burden to the employees' wages (Bastagli, Coady, and Gupta, 2012). This would explain the muted response in the Gini index. Our estimates of an SSC tax-based consolidation show a significant positive short-term response of the labor force participation. However, this result is driven by one particular country and does not survive when this country is excluded

[17] These results are robust to different PVAR specifications in which several shocks enter at the same time. Estimates are available upon request. This comes as no surprise given the small correlation coefficients between SSC, personal and corporate taxes (see Table 3.3).

Table 3.8: Composition effects of direct tax-based consolidations

	Impact	1y	3y	5y	10y
a) Direct tax-based					
GDP	**-0.59**	-0.72	-1.13	-0.80	-0.06
Disposable Gini	0.03	0.13	-0.14	-0.17	0.00
Unemployment	0.06	0.19	-0.06	-0.04	-0.10
Participation	-0.06	0.06	-0.08	-0.08	0.00
b) Personal tax-based					
GDP	**-0.87**	**-1.34**	**-2.10**	**-1.72**	-0.38
Disposable Gini	-0.08	-0.09	**-0.66**	**-0.58**	-0.05
Unemployment	0.31	0.50	0.52	0.47	0.04
Participation	-0.04	0.06	-0.21	-0.30	-0.16
c) Corporate tax-based					
GDP	-1.23	-1.59	-2.14	-1.45	-0.22
Disposable Gini	0.25	0.42	0.10	-0.04	-0.03
Unemployment	0.15	0.23	0.17	0.08	-0.20
Participation	-0.10	-0.14	-0.14	-0.10	0.03
d) SSC tax-based					
GDP	1.87	1.91	-3.40	-3.67	-1.88
Disposable Gini	-0.29	0.00	0.77	0.28	-0.23
Unemployment	-0.89	-1.27	-2.92	-0.62	0.05
Participation	**1.24**	**2.63**	1.27	0.82	0.26

Notes: The table reports the response to a 1% of GDP direct, personal, corporate and SSC tax-based consolidation shock. SSC stands for social security contributions. Bold numbers indicate significance at the 10% level.

from the sample (see the country stability robustness check in Section B.3 of Appendix B).

Our results are partly in line with those of Martínez-Vázquez, Vulovic, and Dodson, 2012, who analyze how changes in tax revenues affect inequality in a panel of 150 countries over the 1970-2009 period using the Generalized Method of Moments estimation. They find that personal income taxes have a significant adverse effect on income inequality. The effects of corporate taxes in reducing inequality, instead, are estimated to be weaker in more open economies. Differently from our results, SSC are found to be positively associated with income inequality.

3.4.3 Composition Effects of Indirect Tax-based Consolidations

Indirect taxes comprise several instruments. We focus on general taxes (GT) and specific goods and services (SGS) taxes, as revenues stemming from other instruments only show marginal changes during tax-based consolidations. Concerning the potential impact of GT and SGS taxes on income inequality, we do not have a particular prior. Both of them are expected to increase consumption inequality. For what concerns income inequality, instead, the potential effects are more ambiguous and likely to depend on the contemporaneous responses of real economic activity and labor market variables. On the one hand, higher GT and SGS taxes decrease the marginal return of labor. Hence, agents might respond by substituting away labor for more leisure time (i.e., substitution effect). On the other hand, since agents' real income decreases, they could respond by supplying more labor (i.e., income effect). Our previous findings suggest that overall the income effect may dominate the substitution effect. We now investigate whether these results hold for both the sub-components of indirect taxes.

Estimates for GT and SGS tax-based consolidations are presented in Table 3.9 (Panels (b) and (c)). In the medium term, a GT shock significantly lowers inequality, with the Gini index decreasing 0.6 percentage points after five years. At the same time, GT produce a statistically significant decline in real economic activity, with GDP declining by 3.6 and 3.3 percentage points respectively after three and five years. The response of labor participation is positive and significant, at 0.5 percentage points, on impact and remains positive up to three years after the shock, although it loses significance. Therefore, the income effect seems to dominate the substitution effect.

To provide further empirical evidence backing our results on GT, we estimate responses to a one percentage point increase in either the standard VAT rate or the goods and service tax (GST) rate during both tax-based and GT-based consolidation years.[18] We report IRFs in Panel (a) and (b) of Table 3.10 below. The estimates confirm what already emerged above. A one percentage point increase in the standard GT rate raises the labor force participation rate by 0.2 percentage points both on impact and one year after the shock. The response of the disposable Gini index is negative throughout all the horizon, although not statistically significant.

[18] Several countries introduced a GT tax only after the beginning of our sample. This is the case for Australia (in 2000), Canada (in 1991), Finland (in 1994), Japan (in 1989), Portugal (in 1986) and Spain (in 1986). Moreover, in the United States, GST rates are fixed by local (State) governments and not by the federal government. Hence, we exclude the United States from this analysis. In total, we count 15 instances of tax-based consolidations that resulted in a change in the standard GT rate in the 15 countries of our sample excluding the United States. Of these 15 episodes, we exclude one episode, namely Ireland in 1984, since in that occasion the government drastically overhauled the VAT system. More precisely, it decreased the standard rate by 12 percentage points, but it also cut the number of reduced rates from 5 to 2, which could confound the effects of the change in the standard rate.

Table 3.9: Composition effects of indirect tax-based consolidations

	Impact	1y	3y	5y	10y
a) Indirect tax-based					
GDP	**-1.79**	**-4.10**	**-4.90**	**-3.86**	-0.74
Disposable Gini	-0.32	**-0.79**	**-1.25**	**-0.88**	-0.17
Unemployment	**0.75**	**1.90**	**2.43**	**1.82**	0.10
Participation	**0.59**	0.22	-0.26	**-0.56**	-0.32
b) GT tax-based (sales and value added tax)					
GDP	-1.02	-1.89	**-3.61**	**-3.27**	-1.10
Disposable Gini	-0.14	-0.43	-0.72	**-0.61**	-0.22
Unemployment	0.59	0.91	1.62	**1.45**	0.26
Participation	**0.53**	0.35	0.28	-0.19	-0.23
c) SGS tax-based (excises, monopoly, customs and others)					
GDP	**-3.63**	**-10.75**	**-11.36**	**-8.10**	-0.50
Disposable Gini	**-0.97**	**-2.06**	**-3.06**	**-2.06**	-0.16
Unemployment	1.37	**4.95**	**5.86**	**4.10**	-0.16
Participation	1.08	0.05	**-1.66**	**-1.84**	-0.77

Notes: The table reports the response to a 1% of GDP indirect, GT and SGS tax-based consolidation shock. GT and SGS stand for, respectively, general taxes and specific goods and services. Bold numbers indicate significance at the 10% confidence level.

As an attempt to disentangle which income group is more likely to benefit during a GT-based consolidation, we also report estimates using the income ratios of the agents in the 90^{th}, 50^{th} and 10^{th} percentile of the income distribution (Panel (c), (d) and (e) of Table 3.10). These results suggest that both low and middle-income households gain relative to rich households, but low-income ones gain more.

To summarize, the underlying mechanism we have in mind works as follows: a hike in GT pushes up inflation and hence decreases households' real income. The income loss creates incentives for agents voluntarily out of the labor force to search for a job and for those working part-time to increase working hours. In turn, as agents join the labor force, their probability of becoming employed increases. We believe this labor supply channel to be particularly strong for female second-earners in low- and middle-income households.

Turning to SGS taxes (Panel (c) of Table 3.9), the estimates we obtain are more difficult to rationalize. An SGS tax-based consolidation displays extremely large negative multipliers. However, this result is driven by a single country in the sample, namely Portugal (see the country stability robustness check in Section B.3 of

Table 3.10: Additional results on GT tax-based consolidations

	Impact	1y	3y	5y	10y
a) Any tax-based consolidation year					
GDP	**-0.45**	**-0.68**	**-0.91**	**-0.63**	-0.10
Disposable Gini	-0.03	-0.01	-0.12	-0.11	-0.06
Unemployment	0.06	0.11	0.16	0.19	0.04
Participation	**0.18**	0.18	0.00	-0.04	-0.04
b) GT-based consolidation year					
GDP	-0.30	-0.48	-0.79	-0.57	-0.10
Disposable Gini	-0.01	-0.03	-0.16	-0.13	-0.05
Unemployment	0.03	0.08	0.05	0.13	0.04
Participation	**0.18**	0.16	0.00	-0.04	-0.04
c) P90/P10 Income ratio					
GDP	-1.02	**-2.67**	**-4.57**	**-3.32**	**-1.45**
P90/P10	**-16.11**	**-11.43**	-4.03	-1.35	0.46
Unemployment	**1.06**	**1.82**	**2.35**	1.04	0.18
Participation	0.29	0.26	0.09	-0.17	-0.05
d) P90/P50 Income ratio					
GDP	-1.02	**-2.65**	**-4.52**	**-3.31**	-1.45
P90/P50	**-6.56**	-3.33	-0.56	-0.69	0.13
Unemployment	**1.05**	**1.81**	**2.32**	1.04	0.18
Participation	0.28	0.26	0.09	-0.16	-0.05
e) P50/P10 Income ratio					
GDP	-1.10	**-2.73**	**-4.58**	**-3.26**	-1.40
P50/P10	-2.67	-2.79	-2.31	-0.17	0.28
Unemployment	**1.09**	**1.87**	**2.36**	1.01	0.17
Participation	0.29	0.25	0.08	-0.18	-0.05

Notes: Panels (a) and (b) report the response to a 1 percentage point increase in the standard general tax rate. Panels (c), (d) and (e) report the response to a 1% of GDP shock in GT tax-based consolidations, using income ratios as measure of inequality. Bold numbers indicate significance at the 10% confidence level.

Appendix B). When Portugal is excluded, the response of GDP is still negative, but much smaller in absolute value. SGS tax-based consolidations have larger and more immediate effects in reducing the Gini index relative to GT-based consolidations, and this result is robust to the exclusion of Portugal. The Gini index decreases by one percentage points on impact, and it keeps declining over a five-year horizon. In parallel with the economic contraction, the unemployment rate increases in the short run, while the labor force participation rate decreases after three and five years. We expect that these two dynamics should exacerbate inequality. Hence, the only possible channel explaining the observed decline in the Gini index is the contraction in real economic activity. That could be explained by deep recessions hitting high-income agents more strongly than low-income agents, thereby causing a decrease in disposable income inequality.

3.5 Conclusions and Further Extensions

In this paper, we use PVAR models to estimate the composition effects of tax-based consolidations on income inequality, real output, and labor market variables in 16 OECD countries during the 1978-2012 period. The results suggest that tax-based consolidations reduce both market and disposable income inequality, but at the cost of a decrease in output in the short to medium run.

Looking at the effects of specific instruments, our results point to a positive labor supply channel through which general consumption taxes decrease income inequality. By causing an increase in the price of the consumption basket, hikes in general indirect taxes affect low-income households particularly strongly, since they generally have a higher marginal propensity to consume. The increase in prices induces those agents who, before the tax hike, were voluntarily inactive to start searching for a job. We observe this positive labor supply channel to be particularly relevant for middle-aged women in low- and middle-income households. Higher participation rates increase the probability of being employed and ultimately reduce income inequality. Among direct taxes, only personal income taxes increase equity, without having significant negative effects on labor force participation.

In general, our results suggest that incentives to labor market participation represent an important channel through which different tax instruments may affect income inequality. Instead, we do not find much evidence supporting the hypothesis of a government redistribution channel through which tax-based consolidations decrease inequality.

A possible criticism of our analysis is that we only look at agents' aggregate behaviors. We acknowledge that different population groups may react heterogeneously to taxation. We partially address this issue when we analyze the response of labor force participation for men and women separately. However, agents' heterogeneity should be further taken into account and, provided that disaggregated data are available for a large number of countries, a useful extension of this paper would be to disentangle the effects of taxation for different groups of agents. Another interesting avenue for future research would also be to set-up a theoretical macroeconomic model that could more formally explain the different channels emphasized in our empirical analysis, as well their propagation in the short, medium and long run.

Chapter 4

Employment Protection Deregulation and Labor Shares in Advanced Economies

4.1 Introduction

Since the 1980s labor shares have trended downwards in many countries around the world (Karabarbounis and Neiman, 2013). This trend accelerated in the 1990s, and it has been particularly pronounced in advanced economies (IMF, 2017; OECD, 2012). Such a decline flies in the face of the predominant view in macroeconomics, since Kaldor (1957, 1961), that the labor share tends to be stable over the long run. This has triggered renewed interest in the drivers of labor shares, with particular focus on the roles of technological progress in equipment goods and implied substitution of capital for routine labor tasks (Acemoglu and Restrepo, 2016; Alvarez-Cuadrado, Van Long, and Poschke, 2018; Dao et al., 2017; Eden and Gaggl, 2015; Karabarbounis and Neiman, 2013), rising concentration and pricing power across markets (Autor et al., 2017; Barkai, 2017), globalization of trade, finance and production (Boehm, Flaaen, and Pandalai-Nayar, 2017; Dao et al., 2017; Elsby, Hobijn, and Şahin, 2013; Furceri, Loungani, and Ostry, 2018), and measurement issues (Bridgman, 2017; Koh, Santaeulalia-Llopis, and Zheng, 2015; Rognlie, 2016). This paper contends that, alongside these (non-mutually-exclusive) drivers, changes in institutions that weakened worker bargaining power have also played a role. While the point is general, for identification purposes, we focus narrowly on job protection deregulation aimed at enhancing the functioning of labor markets. We empirically show that such deregulation contributed to part of the observed decline in labor shares in many advanced economies — possibly a little over a tenth overall, keeping in mind that this estimate abstracts from other policy and non-policy changes that may also have weakened worker bargaining power in recent decades.

The analysis covers 26 advanced economies over the period 1970-2015. To capture labor market deregulation, we make use of a unique 'narrative' cross-country dataset of major reforms of employment protection legislation (EPL) for regular workers, compiled in Duval et al., 2018. Strikingly, in the five years after major reforms, the aggregate labor share declined by more than seven-tenths of a percentage point in reforming countries, on average, compared to status quo countries.

We test empirically for this stylized fact applying the local projection method (Jordà, 2005) to trace out the response of the labor share to our reform events. To gauge the macroeconomic effects of EPL reforms on the labor share we first carry out the analysis at the country-time level. Then, to understand the underlying channels, we focus on the country-industry-time level. For the latter, we apply a difference-in-differences identification strategy ala Rajan and Zingales, 1996, using two alternative identifying assumptions that are grounded in theory. First, following Micco and Pages, 2006 and Bassanini, Nunziata, and Venn, 2009, stringent dismissal regulations are more binding, and therefore should have a more substantial impact, in industries where firms have a higher 'natural' propensity to regularly adjust their workforce — that is, a higher 'natural' layoff rate. Second, following Blanchard and Giavazzi, 2003 and Bentolila and Saint-Paul, 2003, insofar as EPL affects workers' bargaining power and wage bargaining conforms at least in part to a Right-to-Manage model, deregulation lowers wage rents and triggers substitution of labor for capital, with an impact on the labor share that depends on the elasticity of substitution between these factors. The upshot is that deregulation is more likely to reduce the labor share in industries characterized by a lower degree of substitutability between capital and labor.

There are two further advantages of having a three-dimensional (i industries, j countries and t time periods) dataset:

- First, it allows us to control for country- and industry-specific time-varying unobserved factors, such as macroeconomic shocks, as well as country-industry time-invariant characteristics by including country-time (j, t), industry-time (i, t) and country-industry (j, i) fixed effects. The inclusion of the country-time fixed effects is particularly important as it absorbs any unobserved cross-country heterogeneity in macroeconomic conditions and policies that affect labor shares in a similar way across industries. In a pure cross-country time-series analysis, this would not be possible, leaving open the possibility that the impact attributed to EPL reforms could be due to other unobserved factors. Similarly, the inclusion of industry-time (i, t) fixed effects absorbs any unobserved industry-specific developments that may affect industry labor shares in a similar way across countries, such as the adoption of new technologies.

- Second, it mitigates concerns about reverse causality. While it is typically difficult to identify causal effects using cross-country time-series data, it is much more likely that EPL reforms affect cross-industry differences in labor shares than the other way around. Since we control for country-time fixed effects — and therefore for aggregate labor shares — reverse causality in our set-up would imply that differences in labor shares across industries influence the probability of reforms at the aggregate level. Moreover, our primary independent variable is the interaction between job protection reforms and industry-specific factors (natural layoff rates and/or elasticities of substitution); this makes it even less plausible that causality runs from the industry-level labor share to these composite variables.

To further strengthen the causal interpretation of our results, we verify their robustness to the inclusion of several additional controls whose omission could bias our estimates, including past and expected values of GDP growth and proxies for the other labor share drivers identified in the literature, such as technological progress and international trade.

Our key finding is that job protection deregulation reduces labor shares. In the country-level analysis, we find a major reform that liberalizes EPL to reduce the aggregate labor share by 0.6 to 0.8 percentage point, on average, over the medium term. In the country-industry-level analysis, the effect of that same reform is about 0.9 percentage point higher in high layoff-rate industries (defined as those in the 75th percentile of the cross-industry distribution of layoff rates in the United States) compared with their low layoff-rate counterparts (those in the 25th percentile).[1] The medium-term differential effect between industries with a low and high elasticity of substitution between capital and labor (defined as those in the 25th and 75th percentiles of the cross-industry distribution of elasticities) is more substantial, at 1.5 percentage point. We also find these effects to be mainly driven by a decline in the real wage; this further supports our interpretation that weaker bargaining power has been the principal channel through which EPL deregulation has lowered labor shares in reforming advanced economies.

Using both our country-level and industry-level estimates, we perform an illustrative back-of-the-envelope calculation of the impact of all past EPL reforms, both liberalizing and tightening ones, on the labor share. This exercise suggests a non-trivial impact; job protection deregulation may have contributed about 15 percent to the overall labor share decline. That reflects primarily the deregulation wave of the 1990s and 2000s, which is also the period over which labor shares declined the most in advanced economies.

Our paper relates to the extensive empirical literature on the drivers of labor shares which, somewhat surprisingly, has touched very little on the role of labor market regulation. Some papers study the impact of other drivers of labor shares, notably international trade and offshoring, via their effect on workers' bargaining power (see e.g. Kramarz, 2008, and the recent review by Hummels, Munch, and Xiang, 2016). Instead, our focus is on the direct role of labor market institutions. Blanchard, Nordhaus, and Phelps, 1997 and Blanchard and Giavazzi, 2003 provide theoretical support for a link between labor market deregulation, weaker bargaining power, and lower labor shares, and argue that such link is consistent with the decline observed across European countries during the 1990s. They also make a distinction

[1] Following Bassanini, Nunziata, and Venn, 2009, we use industry layoff rates computed from U.S. data to proxy for 'natural' layoff rates as in the U.S. contracts can typically be terminated at will. Hence, this country is the closest to a frictionless economy. For more details, see Section 4.3.2.

between the short- and long-term effects. They do not provide any formal evidence, however.

The few empirical studies that attempt to quantify the impact of labor market institutions on the labor share have typically failed to find any significant effect. Using cross-country industry-level data, Bentolila and Saint-Paul, 2003 explore a range of labor share drivers, including the frequency of labor conflicts, which they take as a proxy for workers' bargaining power. They find this variable to be insignificant, in a simple OLS regression without fixed effects. Elsby, Hobijn, and Şahin, 2013 exploit variation in the rate of unionization across US industries but do not find a significant association with the labor share. Checchi and García-Peñalosa, 2008 explore the impact on labor shares of several labor market institutions in a cross-country time-series set-up covering 16 OECD countries over the 1960-2000 period, but they do not consider EPL. (Deakin, Malmberg, and Sarkar, 2014) analyze the impact of EPL in an error correction framework for six OECD countries over 1970-2010 and do not find any statistically significant effect. Our sharper identification strategy — using a three-dimensional set-up with a rich set of fixed effects and two identification assumptions ala Rajan and Zingales, 1996 drawn from theory — and reliance on a new dataset of major job protection reforms is what radically distinguishes our analysis from these earlier contributions.

Our paper also relates to the extensive empirical literature on the macroeconomic effects of job protection legislation on economic outcomes, which has primarily focused on productivity and employment. While not entirely settled, the bulk of the evidence suggests that stringent regulation lowers productivity by distorting job turnover, and may also lower employment (for a comprehensive review, see e.g. OECD, 2013). However, except for the few studies mentioned earlier, this literature has not explored the impact of job protection on labor shares. Our paper fills this gap, thereby complementing recent research that has documented the macroeconomic effects of these and other labor market reforms.

The remainder of this paper is organized as follows. Section 4.2 discusses two stylized wage bargaining models to help guide our empirical strategy. Section 4.3 presents our new dataset of major employment legislation reforms, it illustrates the derivation of the layoff rates and elasticities of substitution that are used for the identification, and it provides some stylized facts concerning the decline of labor shares around EPL reform episodes. Section 4.4 sets up the econometric framework. In Section 4.5, we present the main regression results and perform several sensitivity analyses. Section 4.6 contains some extensions to decompose the channels driving our results. Section 4.7 concludes.

4.2 Theoretical Framework

To illustrate the theoretical impact of employment protection deregulation on the labor share and motivate our empirical approach, we use two stylized wage bargaining models, the Right-to-Manage and the Efficient Bargaining models (see e.g. Blanchard and Fischer, 1989). For ease of exposure, and following others, such as Blanchard, Nordhaus, and Phelps, 1997, we assume that EPL deregulation directly weakens workers' bargaining power. For the rest, our theoretical analysis mostly follows Bentolila and Saint-Paul, 2003.

4.2.1 Competitive Labor Market

As a start, let's consider the case of a fully competitive labor market where labor is paid its marginal product. We assume that real output Y is produced using a constant elasticity of substitution (CES) production function with constant returns to scale:

$$Y = F(K, AL) = (\alpha(K)^{\varepsilon} + (1-\alpha)(AL)^{\varepsilon})^{(1/\varepsilon)} \tag{4.1}$$

where K, L and A denote capital, labor and labor-augmenting technical change, respectively, while the parameter ε relates to the elasticity of substitution, σ, according to: $\sigma = 1/(1-\varepsilon)$. Defining the labor-to-capital ratio in effective units as $l \equiv \frac{AL}{K}$, rewriting $F(K, AL) = Kf(\frac{AL}{K})$, and using that in competitive markets labor is paid its marginal product — such that $\frac{w}{p} = Af'(l)$, where w is the nominal wage and p the price level — we can write the labor share as:

$$LS = \frac{wL}{pY} = l\frac{f'(l)}{f(l)} = \frac{(1-\alpha)(AL)^{\varepsilon}}{\alpha(K)^{\varepsilon} + (1-\alpha)(AL)^{\varepsilon}} \tag{4.2}$$

For reasons that will become clear below, we want to express the labor share in terms of the capital-to-output ratio k which is $k = \frac{K}{\alpha(K)^{\varepsilon}+(1-\alpha)(AL)^{\varepsilon})^{1/\varepsilon}}$. After simple manipulations, we can rewrite Equation 4.2 as:

$$LS = 1 - \alpha k^{\varepsilon} \tag{4.3}$$

The key insight from Equation 4.3 is that when labor is paid its marginal product, any change in factor prices and/or quantities affects the labor share only through its effects on the capital-to-output ratio k.

4.2.2 Bargaining Under the Right-to-Manage model

To study the effects of EPL reforms on the labor share, we now introduce labor market frictions in the form of bargaining between employers and workers. We start by assuming that employers and workers first bargain over the wage, with employers then setting employment taking the wage as given. In this case, it remains optimal for employers to set employment such that labor is paid its marginal product (that is, $\frac{w}{p} = Af'(l)$). Equation 4.3 still holds.

What happens when easing EPL? Lower protection reduces workers' bargaining power, which in turn results in a lower wage. Employers respond by substituting labor for capital, and therefore the capital-to-output ratio decreases. This drives a change in the labor share, whose sign depends on whether capital and labor are complements ($\varepsilon < 0$) or substitutes ($\varepsilon > 0$). To see this formally, take the derivative of the labor share expression in Equation 4.3 with respect to workers' bargaining power θ:

$$\frac{\partial LS}{\partial \theta} = -\alpha\varepsilon k^{\varepsilon-1}\frac{\partial k}{\partial \theta} \Rightarrow \begin{cases} > 0 \text{ if } \varepsilon < 0 \\ < 0 \text{ if } \varepsilon > 0 \end{cases} \tag{4.4}$$

where the inequalities follow from the fact that $\frac{\partial k}{\partial \theta} > 0$. Equation 4.4 shows that under the Right-to-Manage model, EPL deregulation that reduces workers' bargaining power ($\theta \downarrow$) will lower the labor share if capital and labor are relative complements ($\varepsilon < 0$) but increase it if they are substitutes ($\varepsilon > 0$).

4.2.3 Efficient Bargaining

Under efficient bargaining, firms and workers instead bargain over both employment and wages. They set employment efficiently by equalizing the marginal product of labor to its opportunity cost, which is the workers' reservation wage. The wage itself is a weighted average of the average and marginal products of labor, with the weight on the former reflecting the bargaining power of workers. Formally, under Nash bargaining the real wage follows:

$$\frac{w}{p} = \theta A\frac{f(l)}{l} + (1-\theta)Af'(l) \tag{4.5}$$

In this setting labor is paid more than its marginal product and Equation 4.3 does not longer hold. Recalling the definitions of l and k, it can be easily shown that:

$$LS = 1 - \alpha(1-\theta)k^{\varepsilon} \tag{4.6}$$

EPL deregulation then reduces workers' bargaining power and the real wage, but employment does not change since it is pinned down by the efficient bargaining condition that links the marginal product of labor to the reservation wage. Therefore, the labor share unambiguously declines, regardless of the elasticity of substitution between labor and capital. Formally:

$$\frac{\partial LS}{\partial \theta} = -\varepsilon\alpha(1-\theta)k^{\varepsilon-1}\frac{\partial k}{\partial \theta} + \alpha k^{\varepsilon} = \alpha k^{\varepsilon} > 0 \tag{4.7}$$

using $\frac{\partial k}{\partial \theta} = 0$, which in turn reflects the fact that changes in workers' bargaining leave unchanged the capital-to-output ratio due to the efficient bargaining condition.

4.2.4 Implications for the Empirical Analysis

Some of the insights from these two stylized models are similar. For example, in both cases, lower workers bargaining power unambiguously reduces the labor share if labor and capital are relative complements. Other predictions vary, particularly regarding whether lower bargaining power always reduces the labor share.

In practice, actual bargaining may combine elements of both models. The Right-to-Manage model has been regarded as describing rather well the actual functioning of labor markets in most European countries (see for instance Layard et al., 2005). At the same time, it has been argued that unions still play a part in determining the employment level, such that actual bargaining mixes up elements of both the right-to-manage and efficient bargaining models (see, among others, the theoretical contribution of Manning, 1987). This leads Cahuc, Carcillo, and Zylberberg, 2014, for example, to conclude that the right-to-manage and efficient bargaining models may ultimately "*represent limit cases of the same model*" (page 441).

Insofar as EPL increases worker bargaining power, the key implication for our empirical analysis is that deregulation is more likely to lower the labor share in countries and/or industries where capital and labor are less substitutable. In the next sections, we take these insights to the data.

4.3 Dataset

In this section, we describe the data used in the empirical analysis. We start by illustrating the dataset of EPL reforms episodes that are the focus of the analysis. Next, we discuss the derivation of the layoff rates and the estimation of the elasticities of substitution. The section proceeds presenting the labor share and remaining data and it concludes providing some key stylized facts regarding the evolution of the labor share over the 1970-2015 period, with major emphasis on its behavior around EPL reform episodes.

4.3.1 Employment Protection Legislation Reforms

Major reforms to EPL are identified by examining legislative and regulatory actions reported in all available editions of the OECD Economic Surveys, as well as additional country-specific sources, for 26 advanced economies over the 1970-2013 period (for details, see Duval et al., 2018).[2] This methodology is related to the 'narrative' approach used by Romer and Romer (1989, 2004, 2010, 2017) and Devries et al., 2011 to identify monetary and fiscal shocks and periods of high financial distress.

[2] The 26 countries covered are: Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Korea, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Slovak Republic, Spain, Sweden, Switzerland, United Kingdom and United States.

In a first step, over 100 legislative and regulatory actions related to EPL are identified. In a second step, such actions are classified as major liberalizing or tightening reforms if one of the following three alternative criteria is met: (i) the OECD Economic Surveys uses strong normative language to define the action; (ii) the policy action is mentioned repeatedly across different editions of the Survey; or (iii) the OECD EPL indicator is in the 5th percentile of the distribution of the absolute changes in the indicator — or it would be if the OECD's scoring system were applied, but no OECD EPL indicator score is available for the country and year considered. When only the third condition is met, an extensive search through other available domestic and national sources is performed to identify the precise policy action underpinning the change in the indicator. Following this process, a variable is constructed that, for each country, takes value 0 in non-reform years, 1 in liberalizing reform years, and -1 in tightening reform years. Table C.1 in Appendix C.1 lists all reforms and tightening reforms identified in this way, while Figure C.1 shows the distribution of reforms over time.

An essential advantage of this approach vis-à-vis other existing databases is that it identifies major legislative reforms as opposed to just a long list of actions that in some cases would be expected to have little or no bearing on macroeconomic outcomes. Likewise, compared with an alternative approach that would infer major reforms from large changes in the OECD EPL indicators, we have a more extended time-series coverage — starting in 1970 rather than 1988 — and document precisely the timing of each action. These features are particularly useful for our empirical analysis that seeks to identify the dynamic effects of reforms.

4.3.2 Layoff Rates

To identify the effect of reforms at the industry level, we derive industry-specific measures of layoff rates. To compute those, we strictly follow the approach of Bassanini, Nunziata, and Venn, 2009, which in turn builds on Micco and Pages, 2006, and define them as the percentage ratio of laid-off workers over total wage and salary employment. Differently from Bassanini, Nunziata, and Venn, 2009, our rates are computed to match the ISIC Rev. 4 industry classification (the one used in the 2017 EU KLEMS database). To this purpose, we use data contained in the 2014 Displaced Workers Survey (DWS), conducted in the context of the more comprehensive IPUMS-CPS (see Flood et al. (2017)).[3] We use U.S. data given that employment protection legislation is virtually non-existent there. Hence, the U.S. is the closest empirical example of a frictionless economy in which employers can freely adjust the workforce in response to operational needs. Appendix C.2 describes in detail the construction of US layoff rates. Table C.2 lists the layoff rate for each

[3] Bassanini, Nunziata, and Venn, 2009 construct U.S. layoff rates using data contained in the 2004 CPS Displaced Workers Supplement for 22 industries classified to match the classification used in the EU KLEMS 2007 database (ISIC Rev. 3 classification).

industry in the sample. The industry with the highest layoff rate is Electrical and Optical Equipment, while that with the lowest is Coke and Refined Petroleum.

4.3.3 Elasticities of Substitution

While several papers provide estimates of the elasticities of substitution (EOS) for the aggregate economy, fewer focus on the industry level, and to our knowledge none does so for the ISIC Rev 4 industry classification, which is the one we use (see below). Therefore, we derive industry-specific EOS following standard practice in the literature, estimating the structural parameter directly from the solution to the firm's profit maximization problem (see, among others, Berndt, 1976, and Antras, 2004). In particular, we infer industry-specific EOS by estimating the following equation:

$$ln\left(\frac{P^K_{j,t}}{P_{j,t}}\right) = ln(\alpha_j) = \frac{1}{\sigma}ln\left(\frac{F_{j,t}(K_{j,t}, L_{j,t})}{K_{j,t}}\right) + \epsilon_{j,t} \tag{4.8}$$

Where P^K is the price of capital services; P is the price of the aggregate output $F(K, L)$; K and L, are capital and labor services, and σ is the elasticity of substitutions. Appendix C.3 provides details on the estimation, as well as an alternative measure of EOS that will be used as robustness checks. Table C.3 lists the EOS for each industry in the sample. The average is about 0.7, with the EOS varying between 0.3 (Construction) and 1.5 (Telecommunications). Overall, our estimates are in line with those of Antras, 2004, Oberfield and Raval, 2014 and Lawrence, 2015. Using different methodologies and data, these authors found the average EOS in the U.S. to be well below unity.

4.3.4 Labor Share and Other Data

Country-level time series of labor shares are taken from OECD Analytical Database. These data cover an unbalanced set of 26 advanced economies from 1970 to 2015. To derive industry-country labor shares, we use harmonized data on value added and labor compensation as contained in the EU KLEMS databases. To maximize the country-coverage, we use data from both the 2017 release (Jäger, 2017) and the 2012 release (O'Mahony and Timmer, 2009).[4] Overall, for the country-industry-time level analysis, we have an unbalanced panel comprising 32 industries in 22 advanced economies from 1970 to 2015.[5] Mean values of labor shares and value-added shares,

[4] The EU KLEMS database provides data on added value and labor compensation in 34 industries, classified according to the ISIC Rev. 4 classification. Next, we define the labor share as the percentage ratio of labor compensation relative to added value. We drop two industries from the sample, namely activity of households as employers and activities of extraterritorial organizations and bodies, as for most countries labor compensation and/or added value data are not available.

[5] The countries for which industry-level data are not available are Iceland, New Zealand, Norway, and Switzerland.

together with the layoff rates and estimated EOS used for the baseline analyses, are reported in Table C.4 of Appendix C.4.

Whereas below we present stylized facts for all the 32 industries, our baseline empirical analysis does not cover those that typically belong to the public sector, and it also excludes the agriculture and the construction industries. To motivate this choice, we observe that special EPL provisions typically apply to civil and public servants as well as seafarers.[6] Moreover, EPL generally does not apply to seasonal workers, who account for a sizable share of overall employment in the agriculture and construction industries. We also exclude from the analysis the (i) Coke, Refined Petroleum and Nuclear Fuel, and the (ii) Other Manufacturing industries due to potential issues in the measurement of added value. In a sensitivity analysis, we show that our baseline results do not hinge on the exclusion of these two industries.

To verify that our results are consistent with a reform-driven decline in worker bargaining power and real wages, we construct a measure of the real wage using data on average hourly earnings and hours worked (Jäger, 2017) as well as on the price level (IMF World Economic Outlook).

All specifications control for major reforms of EPL for temporary workers, which are identified following strictly the same approach used to construct the dataset of major reforms of EPL for regular workers (for details, see Duval et al., 2018). For robustness checks, we collect further data to be used as additional controls. Two variables capture the roles of technological change and globalization, which feature prominently in the recent literature on labor share drivers. Specifically, we proxy for openness to trade and technological change using respectively the ratio of imports and exports to GDP and the price of investment goods relative to output (both sourced from the Penn World Tables, version 9.0, see Feenstra, Inklaar, and Timmer, 2015). Moreover, since current and expected GDP growth rates could correlate with both EPL reforms and labor shares, we also control for them, using data from the OECD Economic Outlook. Finally, we control for trade union density, which we take from the ICTWSS database (Visser, 2016).

4.3.5 Stylized Facts

Appendix C.5 discusses some stylized facts about the evolution of labor shares over the period 1970-2015.[7] Three stand out. First, labor shares have generally been on a declining trend since the mid-1970s, with the decline accelerating in the 1990s. Second, there exist significant heterogeneities both across countries and industries.

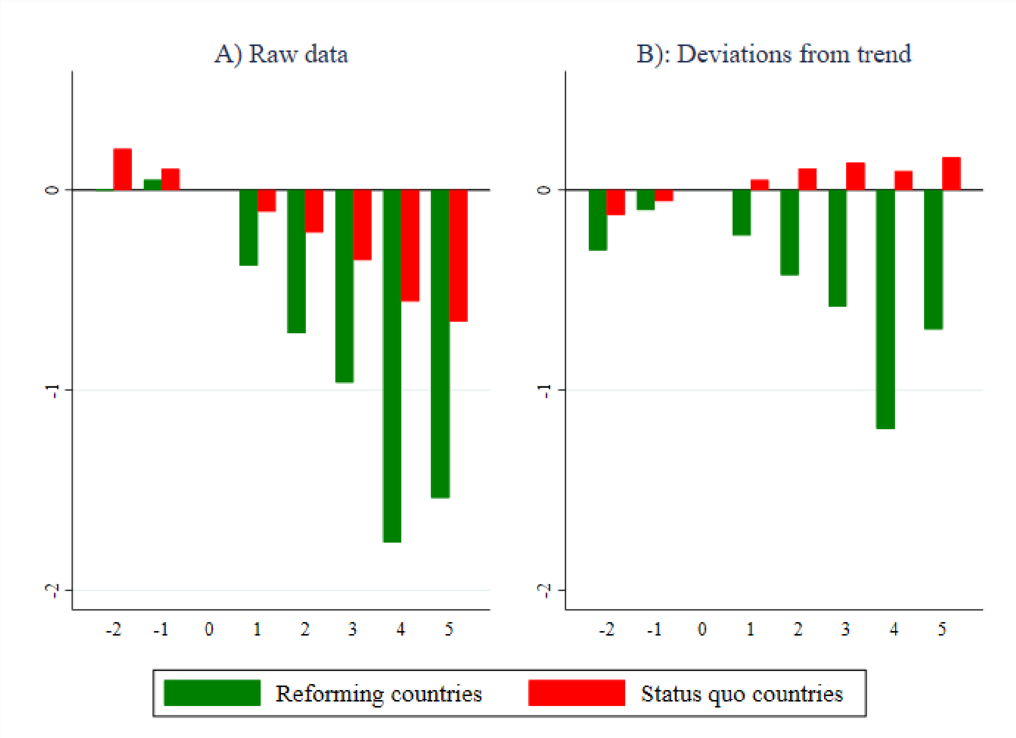
[6] Among the countries covered in our analysis, EPL for public and civil servants is governed by special laws in the following ones: Austria, Belgium, Czech Republic, Denmark, Finland, France, Germany, Greece, Italy, Japan, Korea, Luxembourg, Portugal, Slovakia, Spain, and the United States. For more information, see the ILO EPLex database.

[7] Since most of our stylized facts rely on data at the country-industry-level, for consistency, this section focuses on the 22-country sample for which such data are available. Country-level stylized facts for our full sample of 26 countries are available upon request.

Third, about 70 percent of the decline in country-level labor shares can be accounted for by within-industry changes.

Most importantly in the context of this paper, the decline in the labor share has been typically larger in periods following EPL reforms. To document this, we start by noting that liberalizing reforms were predominantly implemented during the 1990s and the 2000s, which is also the period over which the labor share declined the most (see Figure C.2 in Appendix C.5). Then, in Figure 4.1 we compare the mean cumulative change in country labor shares in the years before and after any EPL reform in reforming countries (solid green bars) versus non-reforming countries (red bars). Before EPL reforms, labor shares had typically been on a declining trend whose slope was similar between reforming and non-reforming countries. Crucially, the decline accelerated following EPL deregulation, while it did not in non-reforming countries.

Figure 4.1: Cumulative changes in country labor shares around reform years



Notes: the figure compares the mean cumulative change in country labor shares relative to reform years in (i) reforming countries (green bars), and (ii) status quo countries (red bars). Panel A reports changes in the raw data. Panel B reports changes in de-meaned and de-trended data. The y-axis measures the size of the mean cumulative change (in percentage points). The x-axis represents the number of years before (negative numbers) and after (positive numbers) the base year (denoted by 0).
Sources: Jäger, 2017, Duval et al., 2018 and own calculations.

The fact that the labor share was declining at a similar pace in reforming and non-reforming countries before reform years suggests that our reform episodes were orthogonal to labor share trends. We also formally verify this by running a simple multinomial logit regression. Particularly, we regress our reform variable $R_{j,t}$ and regress it onto the contemporeaneous as well two lags of the labor share change, plus its own two lags and the variable capturing major reform to temporary contracts (similar to our baseline specification). The results (available upon request) do not suggest that endogeneity is an issue.

To check whether the decline in the labor share in the aftermath of EPL reforms displayed some heterogeneity across industries, we repeat the same analysis for within-industry labor shares by splitting the sample according to industry characteristics (Figure 4.2). Panel A (B) of Figure 4.2 shows the mean cumulative change in the labor share before and after EPL reforms for industries in the lower (upper) quartile of the distribution of US layoff rates. Panel C (D) shows the same statistics, but for industries in the lower (upper) quartile of the distribution of EOS. In line with priors, the decline in labor shares following EPL reforms observed at the macro level appears to be driven by industries with higher layoff rates and higher relative complementarity between capital and labor. This gives us comfort about the identification strategy that we adopt to establish the causal effects of labor market deregulation on labor shares, which we explain more in detail in the next section.

4.4 Econometric Framework

To estimate the dynamic response of labor shares to EPL reforms, we employ the local projection method proposed by Jordà, 2005 to derive impulse-response functions (IRFs). This approach has been advocated by Auerbach and Gorodnichenko, 2012 and Romer and Romer, 2017, among others, as a flexible alternative to vector autoregression (autoregressive distributed lag) specifications since it does not impose dynamic restrictions and it is better suited to estimate nonlinearities in the dynamic response.

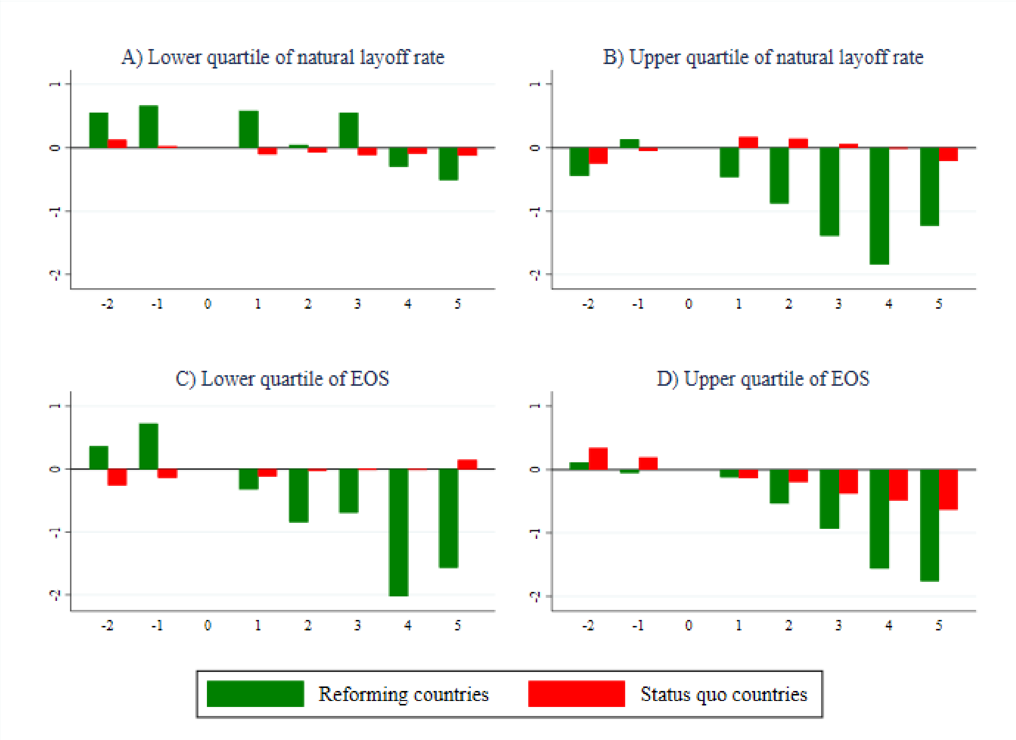
Starting with the country analysis, we estimate the following equation for each response horizon $k = 0, .., 5$:

$$\begin{gathered} y_{j,t+k} - y_{j,t-1} = \alpha_j + \tau_t + \beta_k R_{j,t} + \theta X_{j,t} + \\ + \sum_{h=1}^{k} \varphi_h R_{j,t+h} + \sum_{l=1}^{L} (\delta_l \Delta y_{j,t-l} + \gamma_l R_{j,t-l}) + \epsilon_{j,t} \end{gathered} \tag{4.9}$$

in which j and t denote country and time; y is the labor share of income; β_k denotes its response at each horizon (year) k after the reform; α_j are country fixed effects, included to take account of differences in countries' invariant characteristics; τ_t are time fixed effects, included to take account of global shocks; $R_{j,t}$ is our EPL reform variable, which takes value 0 in non-reform years, 1 in liberalizing reform years and -1 in tightening reform years; $X_{j,t}$ is a set of control variables; and Δ denotes the first difference operator.

Equation 4.9 includes forward reform dummies ($\sum_{h=1}^{k} R_j t + h$). This is to control for reforms that happen within the response horizon $t + k$ (for $k > 1$) that are not

Figure 4.2: Cumulative changes in industry labor shares around reform years by industry



Notes: the figure compares the mean cumulative change in country-industry labor shares relative to years of EPL reforms in (i) reforming countries (green bars), and (ii) status quo countries (red bars), and for industries in the lower (Panel A) and upper (Panel B) quartiles of the layoff rates as well as those in the lower (Panel C) and upper (Panel D) quartiles of the elasticities of substitution. The y-axis measures the size of the labor share change (in percentage points). The x-axis represents the number of years before (negative numbers) and after (positive numbers) the base year (denoted by 0).
Sources: Jäger, 2017, Duval et al., 2018 and own calculations.

captured by $R_{j,t}$. As shown by Teulings and Zubanov, 2014, not doing so would leave the model misspecified and bias our estimates. In our context, this is particularly important since EPL reforms are sometimes adopted in sequence or reversed after some years.

We also include recession dummies and dummies capturing reforms to temporary contracts. The former aim to address possible omitted variable bias that could stem from the fact that economic conditions may shape the likelihood of reform, as suggested by the 'crisis-induces-reform' hypothesis (Drazen and Easterly, 2001; Tommasi and Velasco, 1996), while the latter attempt to control for potential contemporaneous reforms that may also influence the labor share. In a sensitivity analysis, we add further controls, including, among others, trade openness and the relative price of investment goods, which have been put forward as prominent drivers of labor share trends in advanced economies.

(Elsby, Hobijn, and Şahin, 2013; IMF, 2017; Karabarbounis and Neiman, 2013). We find our results to be unaffected, reflecting that major EPL reforms are not correlated with these drivers. The empirical specification is completed by two lags of the 1-period labor share change and of the EPL reform dummy.[8]

Equation 4.9 is estimated using OLS. IRFs are obtained by plotting the β_k coefficients for $k = 0, .., 5$, with 90 percent confidence bands computed using the associated standard deviations, based on clustered robust standard errors.

Next, to minimize any endogeneity concerns and explore the channels through which EPL reforms affect the labor share of income, we turn to country-industry-level analysis, using a difference-in-differences identification strategy in the spirit of Rajan and Zingales, 1996. Specifically, we estimate the following equation:

$$\begin{aligned} y_{i,j,t+k} - y_{i,j,t-1} &= \tau_{j,t} + \alpha_{j,t} + \mu_{j,t} + \beta_k \vartheta_i R_{j,t} + \theta X_{i,j,t} + \\ &+ \sum_{h=1}^{k} (\varphi_h \vartheta_i R_{j,t+h}) + \sum_{l=1}^{L} (\delta_l \Delta y_{i,j,t-l} \gamma_l \vartheta_l R_{j,t-1}) + \epsilon_{i,j,t} \end{aligned} \tag{4.10}$$

in which $y_{i,j,t+k}$ is the labor share in industry i of country j in period $t+k$; $\tau_{j,t}$ are country-time fixed effects, which control for any variation that is common to all industries of a country's economy, such as country-wide macroeconomic shocks and reforms in other (non-EPL) areas; $\alpha_{i,j}$ are country-industry fixed effects, included to take account of cross-sectional differences in average changes in country-industry labor shares; $\mu_{i,t}$ are industry-time fixed effects that control for different labor share changes across industries $R_{j,t}$ our EPL reform variable; ϑ_i industry-specific characteristics, discussed below, which we use to identify the causal effects of EPL reform on the (country-industry-level) labor share; $X_{i,j,t}$ is a set of control variables including a temporary contracts reform dummy plus, in a sensitivity analysis, other labor share drivers. All controls are interacted with industry-specific characteristics (ϑ_i). As in the country-level analysis, we include forward reform dummies (see Teulings and Zubanov, 2014) as well as two lags of the first-difference of $y_{i,j,t+k}$ and of $R_{j,t}$.

This difference-in-differences specification relies on two alternative identification assumptions. The first is that stringent dismissal regulations are more binding, and therefore raise workers' bargaining power more, in industries characterized by a higher 'natural' propensity to adjust their workforce (that is, a higher 'natural' layoff rate). The second identifying assumption follows from our theoretical framework and suggests that job protection deregulation is likely to reduce the labor share more in industries where capital and labor are less substitutable. Hence, we estimate Equation 4.10 using three alternative industry-specific characteristics, ϑ_i: (i) the 'natural' layoff rate; (ii) $1-\varepsilon$ the inverse of the EOS ($1-\varepsilon = 1/\sigma$); and (iii) the interaction between these two, because the lower the EOS is, the more deregulation should reduce the labor share in industries where EPL is more binding. Since we include country-year dummies, which control for aggregate effects, our results should be interpreted as the cross-industry differential effects.

8 As shown below, the results are robust to different lag specifications.

Equation 4.10 is estimated with OLS for each $k = 0, .., 5$. Similar to the country-level analysis, IRFs and the associated confidence bands are computed using the coefficients β_k and the respective standard errors. These are clustered at the country-industry-level and, for the identifications relying on (i) the EOS and (ii) the interaction between EOS and the layoff rates, they are obtained through bootstrapping.[9] The inclusion of the rich set of fixed effects and controls should largely address endogeneity concerns related to omitted variable bias. Besides, reverse causality is unlikely to be a concern in our set-up. First, the natural propensity to layoff in the U.S. is arguably orthogonal to industry-level labor share changes in other countries. A similar argument holds for the EOS between capital and labor. Second, it is highly unlikely that industry-level labor share patterns can influence EPL reform. Movements in the labor share at the aggregate level may well do so, but this potential source of reverse causality is addressed through the inclusion of country-time fixed effects. In other words, claiming reverse causality would mean arguing that differences in labor share changes across industries lead to economy-wide EPL reforms. This, we argue, is implausible.

4.5 Baseline Results and Robustness Checks

4.5.1 Country-level Analysis

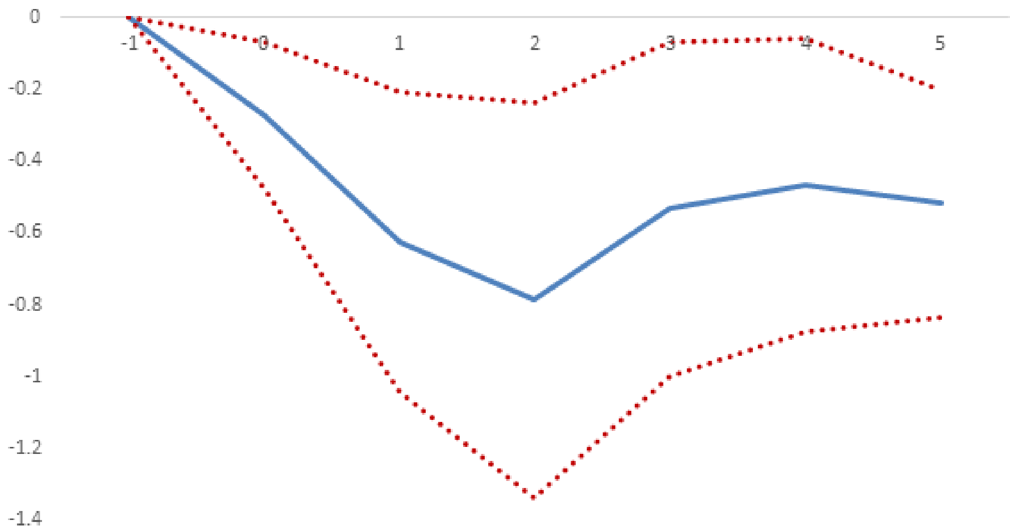
Figure 4.3 shows the estimated dynamic response of the labor share to a liberalizing EPL reform over the five-year period following implementation, together with the 90 percent confidence interval around the point estimate. Major deregulation episodes have a statistically significant and persistent detrimental effect on the labor share. This effect reaches 0.8 percentage point two years after the reform, before declining marginally to 0.6 percentage point. It eventually levels off at this level eight years after the reform.[10]

Next, we perform a few back-of-the-envelope calculations to get a rough sense of the share of the overall decline in labor shares that may be ascribed to EPL deregulation, based on our estimates. We found a major EPL liberalizing reform to cause the labor share to decline by about 0.6 percentage point over the four years following the reform. By calculating the net number of liberalizing reforms over the period considered for each country, we can compute an illustrative estimate of the overall impact of EPL deregulation on the change in the labor share. Taking the average of these estimates across countries, we find that deregulation may

[9] When we estimate Equation 4.10 using the EOS and the interaction between EOS and layoff rates as identification variables the regressor are derived from estimated variables themselves and standard t-statistics may be biased upwards. Hence, we compute standard errors via bootstrapping method (with 500 replications). However, our results are robust to not using bootstrapped standard errors.

[10] We also separately estimated the effect of liberalizing and tightening EPL reforms. As expected, the magnitude of the estimated response is similar (although of opposite sign). This indicates that our results are not driven by tightening reform episodes.

Figure 4.3: Country-level analysis – baseline results



Notes: estimates based on Equation 4.9. Solid line denotes the percentage point response of labor share to EPL reforms. Dotted lines indicate 90 percent confidence interval based on clustered standard errors. The X-axis reports the horizon, with 0 indicating the reform year. The Y-axis reports the magnitude of the estimated coefficients (in percentage points).
Sources: OECD Analytical Database, Duval et al., 2018 and own calculations.

have accounted for about 14 percent of the overall labor share decline in advanced economies over 1970-2015.

Our figures implicitly assume that the labor share decline estimated over the four years following the reform persists in the long run. Indeed, this is what our analysis suggests if we extend its horizon beyond four years; in our baseline regression, the effect of EPL reforms is found to stabilize at about -0.8 percentage point after eight years. Given that the magnitude of the trend decline in the labor share depends on the period considered, including on whether the end year falls within a recession or an expansion period (Kehrig and Vincent, 2017), we also perform the same calculation over the periods 1970 to 2007 — thereby excluding the Great Recession — and from 1990 to 2015, when the trend decline in the labor share was steepest. Remarkably in line with the overall estimate above, we find that changes in EPL contributed about 14 percent and 15 percent to the overall labor share decline over the 1970-2007 and 1990-2010 periods, respectively.

Robustness checks

To check the sensitivity of these results to potential sources of endogeneity, we estimate two additional specifications with a richer set of control variables. First, we control for factors that have been put forward as fundamental forces behind the trend labor share decline in advanced economies, namely technological progress and international trade, as well as other potential drivers such as changes in trade union density. Second, we also estimate a specification including past GDP growth as well as expected future GDP growth between periods t and $t+k$ — the horizon over which the impulse response functions are computed — at time $t-1$. Table 4.1 summarizes the results from these two robustness checks; they turn out to be very similar to, and not statistically different from, our baseline, suggesting that the potential sources of endogeneity listed above are not empirically important in practice.

Table 4.1: Country-level analysis – robustness checks

	Impact	1y	2y	3y	4y	5y
Baseline	**-0.27**	**-0.63**	**-0.78**	**-0.53**	**-0.47**	**-0.55**
Other labor share drivers	**-0.26**	**-0.58**	**-0.85**	**-0.69**	**-0.62**	**-0.69**
(Exp.) GDP	**-0.28**	**-0.61**	**-0.73**	**-0.47**	**-0.47**	**-0.49**

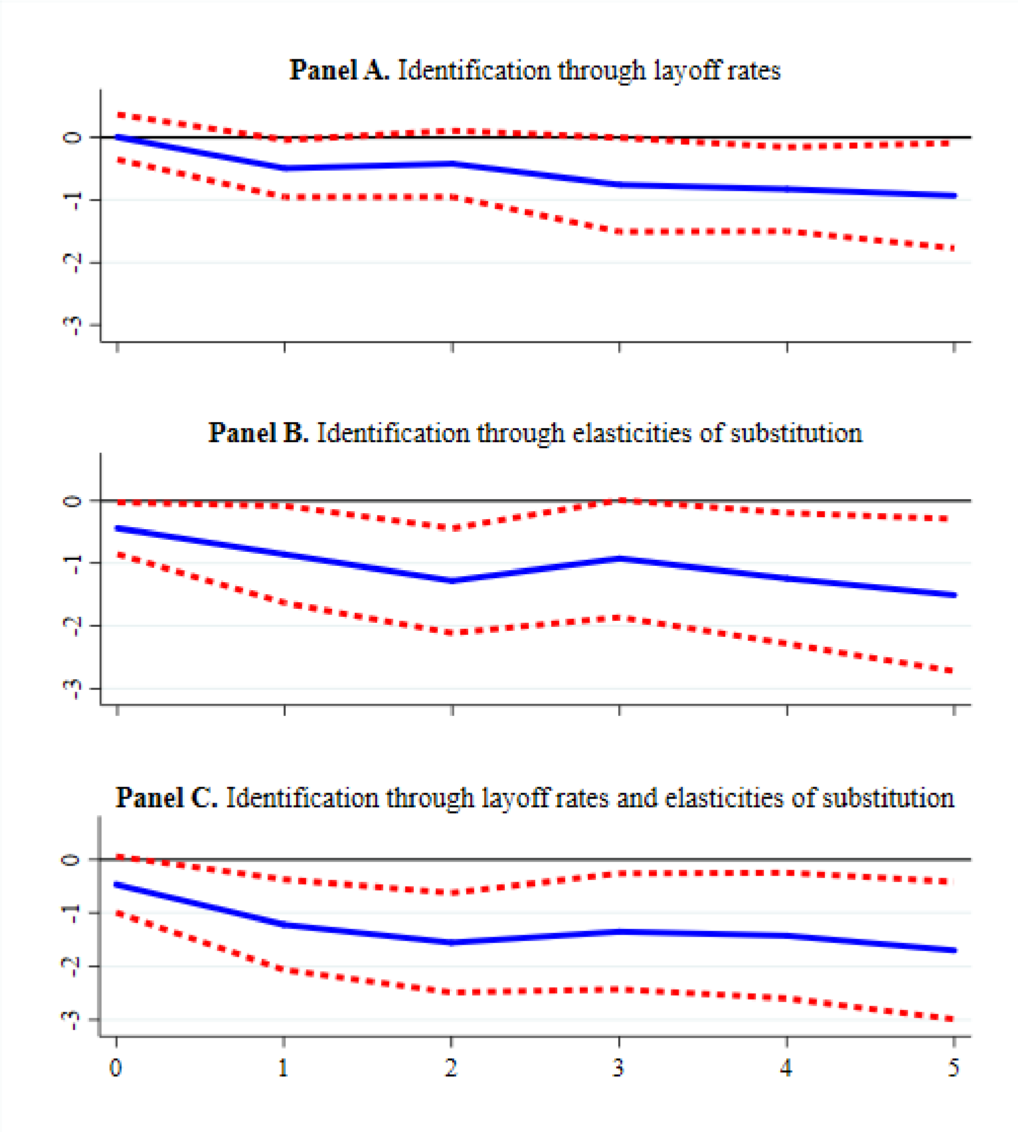
Notes: estimates based on Equation 4.9. Coefficients are in percentage points. Bold numbers indicate significance at the 90 percent confidence interval, based on clustered standard errors. The row "Other labor share drivers" reports estimates from a regression including the change in union density, the relative investment price, the trade openness as controls. The row "(Exp. GDP)" reports estimates based on a regression including current, past and expected future GDP growth as controls.
Sources: OECD Analytical Database, Duval et al., 2018, Feenstra, Inklaar, and Timmer, 2015, Visser, 2016, and own calculations.

4.5.2 Country-industry-level Analysis

Figure 4.4 presents the results from the country-industry analysis, that is, from estimating Equation 4.10. Panels A, B and C show the IRFs when the effect of the reform is identified, respectively, through the layoff rates, the EOS and the interaction between these two. The estimated coefficients are rescaled by interacting them with the difference between the values of the 75th and 25th percentiles of the relevant industry characteristics. Therefore, the IRFs show the estimated differential effect of the reform between industries in these percentiles. The same approach is applied to construct the confidence bands. The results are qualitatively similar across all specifications, indicating a relative decline in the labor share in high layoff rates and low substitutability industries. They are also quantitatively larger when we identify the reform using both the layoff rates and the EOS, which is our preferred specification.

Panel A shows that over the medium term — five years after the reform takes place — job protection deregulation tends to reduce the labor share in industries with a high layoff rate relative to those with a low-layoff-rate. The differential medium-term reduction in the labor share following an EPL reform between an industry with a relatively high natural layoff rate (at the 75th percentile of the cross-industry distribution of layoff rates in the U.S) and one with a relatively low natural layoff rate (at the 25th percentile of the distribution) is about 0.9 percentage point.

Figure 4.4: Country-industry-level analysis – baseline results



Notes: estimates based on Equation 4.10. Solid lines denote the estimated average differential labor share effect of EPL reforms between industries in the 75th percentile and 25th percentile of the layoff rates distribution (Panel A), in the 25th percentile and 75th percentile of the distribution of the elasticities of substitution (Panel B) and in the 25th percentile and 75th percentile of the distribution of the interaction between the two (Panel C). Dotted lines indicate 90 percent confidence interval based on standard errors clustered at the country-industry-level. For Panels B and C standard errors are obtained through bootstrapping (500 replications. The Y-axis reports the magnitude of the estimated coefficients (in percentage points), while the X-axis reports the response horizon (in years).
Sources: Jäger, 2017, Duval et al., 2018, Flood et al. (2017) and own calculations.

In line with theory, the results also suggest that the effect of EPL reforms on the labor share tends to be larger in industries with a lower EOS between capital and labor (Panel B). The medium-term differential reduction in the labor share between an industry with a relatively low EOS (at the 25th percentile of the ε's distribution) and one with a relatively high EOS (at the 75th percentile of the ε's distribution) is about 1.5 percentage point.

Finally, and as expected, the identification through the interaction between the natural layoff rate and the EOS yields the largest and sharpest estimate of the differential impact of EPL reforms across industries (Panel C). In short, the effect tends to be larger in industries with a higher natural layoff rate and a lower elasticity of substitution. Quantitatively, the joint effect of moving from the 25th to the 75th percentile of the layoff rate and from the 75th to the 25th percentile of the EOS' distribution is about -1.7 percentage point 5 years following a liberalizing EPL reform. Except upon impact, the effect is statistically significant over the entire horizon considered.

Under a number of simplifying assumptions, discussed in Appendix C.7, we perform a back-of-the envelope calculation similar to the one based on our country-level estimates. Reassuringly, we find that EPL deregulation might have accounted for about 15 percent of the overall labor share decline in the average advanced economy, which is very similar to what we calculated using our country-level estimates.

Robustness checks

We now check the sensitivity of our results to several different specifications. Relevant results are reported in Appendix C.6. We start verifying that our findings are not driven by any given country or industry. To do so, we estimate Equation 4.10 excluding first one country and then one industry at a time. Figures C.10 and C.11 report the corresponding impulse responses, together with the baseline estimates and relative confidence bands. All the newly obtained impulse responses lie close to the baseline and always fall within its confidence bands.

Since the data we used for the labor share comes from two different vintages of the EU KLEMS database (2012 and 2017), there might be statistical differences across them. Therefore, we verify that our results hold when only the latest version is used (Table C.5). In Table C.5 we also show that the baseline estimates do not depend on the exclusion of the Coke, Refined Petroleum and Nuclear Fuel, and the Other Manufacturing industries. Finally, exploiting that the EPL reforms we analyze generally do not apply to the public sector, agriculture and construction, we estimate an alternative specification in which these industries are used as a control group by setting $\vartheta_i R_{j,t}$ to 0 for them. In line with our expectation, the results point to even larger differential effects across industries (Table C.5). The distribution of industries according to natural layoff rates relies on US layoff rates that might be imprecisely estimated. To address this potential concern, we rerun the specifications that rely on the layoff rates for the identification using an alternative measure. Specifically, we divide industries into two categories depending on whether their layoff rates were above the median in all the three years covered by the 2014 Displaced Workers Survey (see Table C.2), and construct a dummy variable that takes value 1 (0) for industries in which EPL is (is not) binding. According to this rather conservative classification, EPL only binds in seven industries, and in an equal manner across them. In another robustness check, we use the layoff rates calculated for the year 2013 instead of the average over 2011-2013. The impulse responses obtained using these alternative measures, reported in Table C.6, are qualitatively similar to the baseline results. When the dummy variable is used, the estimated coefficients of the reform variable are quantitatively lower than in our baseline, but they cannot be readily compared. Importantly, when our preferred identification strategy based on the interaction between layoff rates and EOS is used, the negative impact of EPL reforms on the labor share is statistically significant at least form the third year onward in all cases.

We also run a sensitivity analysis on our measure of the EOS between labor and capital, re-estimating our specifications using the alternative sets of EOS discussed in Appendix C.3. We employ in turn the EOS estimated (i) using data on the real capital stock as a proxy for capital services, (ii) using the nominal capital stock divided by capital services to proxy for the rental rate of capital, and (iii) relaxing the assumption of Hicks-neutral technical change. The results, presented in Table C.7, are very similar to our baseline results. Again, they are most statistically significant when using our preferred identification strategy based on the interaction between layoff rates and EOS.

Another possible concern with an OLS estimation of Equation 4.10 might be that the results could be biased due to the omission of other macroeconomic developments that may affect industry-level labor shares through their interaction with industry-specific natural layoff rates or/and the elasticities of substitution, and that may at the same time correlate with EPL reforms. A candidate is the change in union density, whose trend decline may have reduced workers' bargaining power and affected the labor share through the same channels as EPL reforms. While changes in union density are not correlated with EPL reforms — the correlation is only -0.01 — we nonetheless check the robustness of our results by adding to Equation 4.10 an interaction term between the change in union density and the industry-specific natural layoff rate (or/and the elasticity of substitution).

Likewise, while the effects of technological progress — proxied by the relative price of investment — and trade openness on labor shares are controlled for through country-time fixed effects, they could still be a source of omitted variable bias if (i) they were correlated with EPL reforms, and (ii) their impact varied with industry-specific characteristics. Therefore, we check the robustness of our results by also adding to Equation 4.10 the interaction of these variables with industry-specific characteristics. Table C.8 shows the results from these sensitivity analyses. The effects of EPL reforms on country-industry labor shares when controlling for the additional factors described above are very close to, and not statistically different from, our baseline estimates.

4.6 Extensions

What factors drive the negative response of the labor share to job protection deregulation? Wage bargaining models of the type we presented in Section 4.2 imply that, insofar as EPL reform reduces worker bargaining power, it should lower the real wage, all else equal. Implications for the capital-to-output ratio and the employment level are more ambiguous a priori, as they depend on whether bargaining takes place only over the wage or also over employment.

As a cross-validation exercise, we test whether EPL reforms are also associated with a decline in the real wage. To this end, we re-estimate Equation 4.10 using the change of the (log) hourly real wage as the dependent variable. Since there are no theoretical reasons to expect that the effect of deregulation on the real wage should depend on the elasticity of substitution, the identification relies exclusively on the layoff rate. We also apply the same approach to estimate the effect of deregulation on the employment level and the capital-to-output ratio.

Table 4.2 shows the estimated coefficients. In line with theoretical priors, EPL deregulation leads to a relative fall in the real wage in industries with a high natural layoff rate — where EPL is more binding, and deregulation thus has a greater impact on worker bargaining power — relative to those with a low rate. This negative differential effect between industries at the 75th and 25th percentiles of the layoff rate distribution gets larger over time, reaching about -1.5 percent four years after the reform. Consistent with this finding, employment growth instead shows a positive differential response, which becomes significant two years following the reform. The capital-output ratio displays a negative medium-term response, although this is not significant at conventional confidence levels. Overall, these results are supportive of a significant role of bargaining power in driving the impact of EPL deregulation on the labor share, in line with our illustrative theoretical framework.

Table 4.2: Country-industry-level analysis – extension on labor share drivers

	Impact	1y	2y	3y	4y	5y
Identification through layoff rates						
Labor share	0.01	**-0.5**	-0.42	-0.76	**-0.83**	**-0.93**
Real wage	0.22	**-0.96**	**-1.22**	**-1.38**	**-1.47**	**-1.3**
Employment	0.11	**0.41**	0.42	**0.83**	0.66	0.19
Capital-to-output ratio	1.45	2.84	0.49	-2.3	-4.66	-3.87

Notes: estimates based on Equation 4.10 and using layoff rates for the identification. The rows "Labor share", "Real wage", "Employment" and "Capital-to-output ratio" report estimates obtained using, respectively, the labor share, the log hourly wage deflated by the price index, the log of engaged individuals and the ratio of the nominal capital stock to value added as dependent variables. Bold numbers indicate significance at the 90 percent confidence interval, based on clustered standard errors at the country-industry-level.
Sources: Jäger, 2017, Duval et al., 2018, Flood et al. (2017) and own calculations

Next, we perform another extension to check whether the direction of the effect of EPL reforms depend on whether labor and capital are complement or substitute. Our baseline specification does not allow for any switch in the sign of the impact of EPL reforms on the labor share depending on whether the EOS is above or instead below 1. This is because we did not want to tie our empirical strategy to the Right-to-Manage model — or any other specific wage bargaining model — since actual wage bargaining is likely to be more involved and combine elements from various models. Yet, if bargaining took place only over wages following the Right-to-Manage model, the sign of the impact of EPL deregulation on the labor share should depend strictly on whether capital and labor are relative complements or instead substitutes. To test this formally, we split the sample in two according to whether the EOS is above or below 1 and run Equation 4.10 on the two restricted samples. For the identification, we rely on the natural layoff rates. Results are presented in Table 4.3.

Table 4.3: Extension on sample split according to the elasticity of substitution

	Impact	**1y**	**2y**	**3y**	**4y**	**5y**
Identification through layoff rates						
Full sample	0.01	**-0.56**	-0.48	-0.85	**-0.93**	**-1.05**
Elasticity above 1	**0.96**	**1.3**	**1.51**	**1.83**	1.39	0.86
Elasticity below 1	0.01	**-0.61**	-0.47	**-0.92**	**-0.92**	**-0.98**

Notes: estimates based on Equation 4.10 and using layoff rates for the identification. The rows "Full sample", “Elasticity above 1”, “Elasticity below 1” report estimates obtained using, respectively, the full sample, the restricted sample of industries with elasticity of substitution above 1 and the restricted sample of industries with elasticity of substitution below 1. Bold numbers indicate significance at the 90 percent confidence interval, based on clustered standard errors at the country-industry-level.
Sources: Jäger, 2017, Duval et al., 2018, Flood et al. (2017) and own calculations

The new estimates are broadly in line with the Right-to-Manage model’s predictions: relative to industries with a low natural layoff rate, those with a high layoff rate experience an increase in the labor share following a liberalizing EPL reform when the EOS above 1 (substitutability), and a drop if the EOS is below 1 (complementarity).

Quantitatively, the effect of moving from the 25th to the 75th percentile of the layoff rate distribution among industries characterized by substitutability is significant already upon impact and reaches about +1.8 percentage point after 3 years, whereas the corresponding effect is negative and significant, at about -0.9, in the sub-sample of industries with EOS below 1. At longer horizons, the effect becomes statistically insignificant in the former group of industries (possibly owing to loss of statistical power due to the small sample), while it remains significant in the latter.

4.7 Conclusions

This paper explored the impact of job protection deregulation on labor shares using both country-time-level and country-industry-time-level data and a new dataset of major reforms of regular contracts covering 26 advanced economies over the past four decades. We applied the local projection method to estimate the dynamic response of labor shares at both the country- and country-industry-level. For the latter analysis, we used two alternative identifying assumptions ala Rajan and Zingales, 1996 derived from theory, namely that job protection reforms should have more substantial effects in industries characterized by a high 'natural' propensity to regularly adjust their workforce and a low elasticity of substitution between capital and labor.

Unlike previous literature, we found a statistically and economically significant adverse effect of weaker job protection on labor shares. In line with theory, this effect is concentrated in industries with a higher propensity to regularly adjust the workforce and a lower elasticity of substitution between capital and labor, and it is likely driven by a reduction in wage rents. To account for country-specific macroeconomic shocks and other aggregate drivers of labor shares, as well as for industry-specific developments, our country-industry-level analysis included country-time and industry-time fixed effects, and country-industry fixed effects as well. Our findings are also robust to a variety of alternative specifications controlling for potential omitted variable bias and reverse causality as well as including different deterministic components.

Our results call for more research on the role of labor market deregulation, alongside those of technology and globalization, in the extensive literature on the drivers of the decline in labor shares. On the policy front, they also point to the need for assessing the effects of labor market reform plans on a wide range of macroeconomic outcomes — including productivity, employment, and output, but also wages and labor shares — and for addressing trade-offs between efficiency and equity when designing such reforms.

Chapter 5

Okun's Law and Demographics: Differences Across Labor Markets

5.1 Introduction

Starting with Okun, 1963 study of the United States, a rich empirical literature has documented the existence of a negative and stable relationship between an economy's aggregate demand conditions and its overall unemployment. This empirical regularity, known as Okun's law, is expressed as a negative linear association between the cyclical component of the unemployment rate (henceforth unemployment gap), and the output gap. This law has been found to hold across a broad set of economies, but more strongly in advanced economies (AEs) than emerging market and developing economies (EMDEs; see Ball, Leigh, and Loungani, 2017, An, Ghazi, and Prieto, 2017, Ball et al., 2016).

But does this negative relationship between unemployment and demand conditions vary across demographic groups within a country? Are some groups more sensitive to demand conditions? Motivated by the spikes in youth unemployment seen in many European countries in the wake of the Great Recession, Hutengs and Stadtmann, 2013a, Banerji et al., 2014, Banerji, Lin, and Saksonovs, 2015 examined the cyclical sensitivity of youth unemployment for samples of advanced European countries, finding it to be about twice as large as that of adults, reflecting youth's relatively more fragile attachment to employment. Hutengs and Stadtmann, 2013b looked at the relationship for a small sample consisting mostly of emerging European economies and similarly found that younger cohorts' cyclical unemployment is much more sensitive than older cohorts'.

Recently, there has been work to further unpack the Okun relationship, by both age and gender. Dixon, Lim, and Ours, 2017 estimated Okun coefficients (coefficients from a linear regression of the unemployment gap on the output gap) for a sample of OECD economies by age and gender, replicating the earlier findings for age but also finding that women's Okun coefficients tended to be lower than men's. Evans, 2018 investigated Okun coefficients by age and gender in Australia using an unobserved components model, finding similar results to Dixon, Lim, and Ours, 2017. In this paper, we expand upon these analyses, looking at the relationship between demand conditions and cyclical unemployment by demographic group (age and gender) for large samples of 38 AEs and 58 EMDEs.

Our baseline results indicate that there is a large degree of heterogeneity in the cyclical sensitivities of unemployment across demographic and economy groups. EMDE adult men's unemployment gap rises about 0.14 percentage points for a one percentage point decline in the output gap, while AE adult men's gap rises about 0.30. Women's unemployment gap is significantly less sensitive to demand conditions than men's in AEs, at only about 80 percent the magnitude for both youth and adults. By contrast, EMDE adult women's cyclical sensitivity of unemployment is exactly equal that of EMDE adult men's, while that of young women is about 80 percent the size of young men's, although the two are not statistically different from each other. These findings are robust to alternative regression specifications and estimation procedures.

We also consider several extensions to these core results, enabling us to elaborate upon the possible channels by which demand conditions influence aggregate labor outcomes by demographic group. First, we decompose the cyclical unemployment rate response into employment and participation margins. The results indicate that, for all groups, procyclicality of labor force participation leads to an unemployment rate gap response that is smaller, in absolute value, than that of the employment gap (defined as the cyclical component of the employment level). Moreover, the magnitudes of labor force participation and employment sensitivities to the cycle differ widely across demographic groups, revealing even greater heterogeneity than the unemployment gap responses across demographics. For example, the cyclical sensitivity of employment is about five times larger for young men than for adult women in AEs. Within the sample of EMDEs, young men also exhibit the largest cyclical employment sensitivity, but their sensitivity is still less than half that of young men in AEs. Adult women, by contrast, have roughly the same sensitivity in AEs and EMDEs. Aggregating across demographic groups, the overall population's employment cyclical sensitivities in AEs and EMDEs are not as different. The cyclical component of employment is estimated to be 0.30 and 0.43 percentage points higher for each percentage point increase in the output gap in EMDEs and AEs, respectively. That points to the importance of demographic compositional differences across countries underlying aggregate cyclical sensitivities — despite their generally lower labor market cyclical sensitivity, EMDEs are characterized by larger populations of more sensitive demographic groups (such as young men) than AEs, attenuating the difference in aggregate sensitivities between AEs and EMDEs.

Second, we study whether the cyclical sensitivity of unemployment depends on the stage of the business cycle — are there differences in responsiveness across periods of positive and negative output gaps? Our estimates suggest that cyclical unemployment is more sensitive in upturns than downturns. This finding is again sharper for young men. Focusing again on employment, we find that its cyclical component decreases by 1.03 percentage points for each percentage point decrease in the output gap for young men in EMDEs, while its response during periods of positive output gap is not statistically different from 0. Such large asymmetry is not evident when looking at the unemployment gap sensitivity, as it is masked by a sizeable asymmetric response of the cyclical component of the labor force participation rate. That decreases by 0.53 percentage points for each percentage point reduction in the output gap during bad economic times. Its response is instead not statistically different from 0 during good economic times.

The rest of the paper is structured as follows: Sections 5.2 and 5.3 respectively discuss the econometric methodology and the dataset; Section 5.4 presents the baseline empirical results on Okun's Law and some robustness checks; Section 5.5 discusses the different extensions; and Section 5.6 concludes.

5.2 Econometric Methodology

We start by analyzing the validity of Okun's Law across both demographic groups (adult men, adult women, young men, and young women) and country groups (AEs and EMDEs). Next, we explore the channels determining the unemployment response. First, we analyze to what extent the cyclical behavior of the labor force participation reduces or amplifies the strength of Okun's Law. Second, we study what margin drives Okun's Law, whether short- or long-term unemployment rate. We then conclude the analysis by investigating whether Okun's Law is stronger during good or bad economic times. The rest of this section explains the empirical methodology in greater detail.

5.2.1 Baseline Methodology

We assume that output hovers around a long-run, potential, level which may or may not grow over time and that it similarly exists a long-run, natural, level of the unemployment rate. Okun's Law is then a short-run relationship between the deviation of output from its potential level and that of unemployment from its natural rate. Okun, 1963 originally interpreted this relationship as the result of fluctuations in aggregate demand. These generate movements in output, to which employers respond by adjusting the employment level.[1] If labor force participation were relatively stable, the change in employment would, in turn, result in a similar movement in the unemployment rate. Defining the deviations of output from its potential level and those of unemployment from the natural rate as, respectively, the output gap and the unemployment gap, we estimate Okun's Law through the following gaps specification (similar to Ball, Leigh, and Loungani, 2017, among others):

[1] Others see Okun's Law as resulting from the production function, in which it is the level of employment that determines the level of output. For instance, this is the interpretation of Daly et al., 2012. Empirically, it may well be possible that causation runs both ways. We do not take a stance in this debate and rather see the relationship between deviations from potential output and the natural rate as a pure stylized fact.

$$u_{i,t} - u^*_{i,t} = \mu_i + \beta[ln(y_{i,t}) - ln(y_{i,t})^*] + \epsilon_{i,t} \tag{5.1}$$

In Equation 5.1 above, $u_{i,t}$ indicates the unemployment rate of country i in year t, $y_{i,t}$ is real GDP, and * indicates their long-run levels. μ_i are country fixed effects, included to account for potential cross-country differences in time-invariant characteristics. $\epsilon_{i,t}$ is an error term, capturing shifts in the output-employment relationship, and it is assumed to have zero mean. The β coefficient, also defined as the Okun's coefficient, measures the short-run responsiveness of the unemployment gap to the output gap.

Unlike Ball, Leigh, and Loungani, 2017, who estimate the Okun's coefficient on a country-by-country basis, we estimate it through panel regression with country-fixed effects. That allows us to overcome the limited availability of output and unemployment data in some EMDEs, while it restricts us to assume the same coefficient within country groups. The estimation is done through ordinary least squares. Standard errors are clustered at the country level to control for potential correlation in the unobserved components of the unemployment gap within country.

Concerning our priors, we expect the Okun's coefficient to be negative. However, it is difficult to formulate a hypothesis regarding its exact magnitude as this is likely to depend on several factors, which are difficult to quantify. If employers could adjust labor freely (the case of friction-less labor markets), the Okun's coefficient should depend on the (inverse) elasticity of output to employment and the sensitivity of labor force participation to output fluctuations. Ball, Leigh, and Loungani, 2017 argue that, in friction-less labor markets and with a constant labor force participation, the Okun's coefficient would be around -1.5. Their estimates are much higher (around -0.4 on average for the AEs they consider) due to the facts that (i) in practice employers do face costs in adjusting the headcount, and (ii) participation might respond to cyclical movements in output in a way that dampens the response of the unemployment rate (i.e. it increases when output increases). For similar reasons, we expect the Okun coefficient to always be above -1.5.

As mentioned above, our analysis distinguishes between AEs and EMDEs. We motivate this choice by noticing that less developed countries tend to have more informal labor markets. In turn, we expect informality to provide some buffer to the impact of overall business conditions on unemployment rates, as it offers individuals the outside option of self- (informal) employment. Hence, this weakens the link between employers' labor demand and the level of employment which lies at the basis of Okun's Law. In this light, we expect the group of EMDEs to display a lower Okun's coefficient than AEs.

Besides differentiating broadly between countries of different income levels, we do not consider further cross-country heterogeneity. Our aim is rather to explore whether the Okun's coefficient differs across demographic groups. Heterogeneities in this respect could indicate segmentations in the labor market in either the demand or the supply of labor (or both), but they could also arise as a result of policy and/or institutional factors.

We estimate Equation 5.1 separately for the overall working age population and then for adult women, adult men, young women, and young men. We generally expect output fluctuations to generate larger variations in unemployment for the youth than the adult, reflecting that the factors increasing labor adjustment costs, such as, for instance, employment protection regulations and social considerations, are typically lower for the youth. Indeed, this is what has been found already by Banerji, Lin, and Saksonovs, 2015 for AEs.

Concerning gender differences, we consider women to generally have a more fragile employment condition than men. Hence, our prior is for women unemployment to respond more to cyclical movements in output than men. However, women might also be less attached to the labor force. Hence, the importance of flows from employment directly to nonparticipation during bad economic times and the other way around during good economic times (Elsby, Hobijn, and Şahin, 2015) might be such that the estimated Okun's coefficient is smaller in absolute value than for men.

5.2.2 Channels and Other Extensions

Next, we decompose the unemployment response into an employment and participation margin. Verifying how much (cyclical) participation responds to the output gap is important to understand how much of the Okun's coefficient is driven by the employment margin. To see this formally, we can write the unemployment rate as 1 minus the employment rate: $U_{i,t}/L_{i,t} = 1 - E_{i,t}/L_{i,t}$ where $E_{i,t}$ and $L_{i,t}$ respectively indicate the levels of employment and participation. Re-arranging and taking logs, we obtain the following:

$$ln(E_{i,t}) - ln(L_{i,t}) = ln(1 - u_{i,t}) \approx -u_{i,t}$$

The expression above means that, for low levels of the unemployment rate, this can be approximated by the difference between the log-levels of the labor force participation and employment. We then estimate the sensitivity of both cyclical employment and participation by replacing $u_{i,t}$ with either $ln(E_{i,t})$ or $ln(L_{i,t})$ in Equation 5.1:

$$ln(E_{i,t}) - ln(E_{i,t})^* = \mu_i + \delta[ln(y_{i,t}) - ln(y_{i,t})^*] + \epsilon_{i,t} \tag{5.2}$$

$$ln(L_{i,t}) - ln(L_{i,t})^* = \mu_i + \theta[ln(y_{i,t}) - ln(y_{i,t})^*] + \epsilon_{i,t} \tag{5.3}$$

where we define $ln(E_{i,t}) - ln(E_{i,t})^*$ and $ln(L_{i,t}) - ln(L_{i,t})^*$ as the employment and the labor force participation gap respectively, δ and θ are the parameters to be estimated and the rest of the notation is as in Equation 5.1.

We also extend the baseline model to allow for non-linearities in the relationship according to the stage of the business cycle. In other words, we analyze whether Okun's Law is stronger during good or bad economic times. We create a dummy variable ($d_{i,t}$) taking value 1 for periods in which the output gap is positive and 0 otherwise and estimate the following extended specification:

$$\begin{aligned} z_{i,t} - z^*_{i,t} = d_{i,t}\mu_i + (1 - d_{i,t})\mu_i + \rho^z\{d_{i,t}[ln(y_{i,t}) - ln(y_{i,t})^*]\}+ \\ +\sigma^z\{(1 - d_{i,t})[ln(y_{i,t}) - ln(y_{i,t})^*]\} + \epsilon_{i,t} \end{aligned} \tag{5.4}$$

where $z_{i,t}$ is, in turn, the unemployment rate, the log-level of employment and that of the labor force participation, ρ^z and σ^z are the parameters to be estimated, and the remaining notation is as in Equation 5.1.

5.3 Dataset

Our analysis is carried out both for the overall working age population (including individuals with age ranging from 15 to 64 inclusive) and for four different demographic groups: adult men, adult women, young men, and young women. Adult and youth are defined as to include the population with age ranging from, respectively, 25 to 64 and 15 to 24 (all inclusive). The sample spans the years from 1990 to 2015 and covers 38 AEs and 58 EMDEs, classified according to the definition contained in the IMF World Economic Outlook. We provide a list of the countries covered in the Appendix D. Due to data availability issues, the panel is unbalanced.

Data on the working age and youth unemployment and labor force participation rates (for all genders) come from the ILOSTAT of the International Labour Organization (ILO). We source population data from the United Nations statistics. To calculate the adult unemployment rate, we proceed in the following manner. We first calculate the unemployment level of the youth and the working age population, according to the following expression:

$$U^{a,g}_{i,t} = \frac{u^{a,g}_{i,t} l^{a,g}_{i,t}}{P^{a,g}_{i,t}}$$

where U and L indicate the level of, respectively, unemployment and labor force participation, and P indicates population; the superscript a indicates the age cohort (either Y for the youth or WAP for the working age population), the superscript g indicate gender (either W for women, M for men and B for both) the subscripts i and t denote country and time; capital and small letters indicate levels and rates respectively. Similarly, we calculate the level of the youth and the adult labor force participation as:

$$L_{i,t}^{a,g} = \frac{l_{i,t}^{a,g}}{P_{i,t}^{a,g}}$$

Finally, we compute the adult unemployment rate as:

$$u_{i,t}^{a,g} = \frac{(U_{i,t}^{WAP,g} - U_{i,t}^{Y,g})}{(L_{i,t}^{WAP,g} - L_{i,t}^{Y,g})}$$

The employment level is computed, for each demographic group, using the labor force and participation level according to $E_{i,t}^{a,g} = L_{i,t}^{a,g} - U_{i,t}^{a,g}$

The analysis of the sensitivity of the unemployment rate and the employment and participation is constrained to the sample for which both the adult and the youth unemployment and participation rates data are available so to have a constant sample for all the estimations.

To estimate the potential level component of our dependent variables in Equations 5.1 to 5.4, we adopt the following algorithm. First, we linearly interpolate the underlying original series when missing observations occur in some country. Second, since the empirical analysis has a time-series dimension, we exclude from the sample all countries with less than five observations. Third, we apply the Hodrick-Prescott filter to the interpolated series and estimate its potential level for each country. The smooth parameter is set to 100 for the yearly data. Finally, we treat all observations that are either — preceded and followed by three or more missing observations — or for which the original data is not available as missing.

Data on real GDP comes from the IMF World Economic Outlook and does not have the issue of missing values. To estimate the output gap $(y_{i,t} - y^*_{i,t})$ we use the log of real GDP and apply the Hodrick-Prescott filter with smooth parameter 100. We also collect data on per capita and potential GDP from the WEO for sensitivity analyses.

5.4 Okun's Law Across Demographic Groups

5.4.1 Baseline Results

Table 5.1 shows the estimates from Equation 5.1. In line with existing empirical evidence, the unemployment gap in AEs for the overall working age population is estimated to be 0.31 percentage points lower for each one percentage point rise in the output gap, while it is about half that amount lower in EMDEs (0.17 percentage points). The lower cyclical sensitivity of unemployment in EMDEs is as we expected. Lower income countries tend to have more informal labor markets, which might dampen the sensitivity of the unemployment gap to the business cycle as workers can easily transition between formal employment and self- (informal) employment, rather than between employment and unemployment (or nonparticipation) in absence of informality. Confirming this intuition is also the much lower fit of the Okun's Law in EMDEs relative to AEs, with the explanatory power of the regression being more than three times smaller (the R^2 is 0.14 for EMDEs and 0.47 for AEs).

Table 5.1: Okun's Law Across Demographic Groups

	AEs			**EMDEs**		
	β	**s.e.**	R^2	β	**s.e.**	R^2
All working age	-0.31**	-0.05	0.47	-0.17**	-0.03	0.14
Adult women	-0.22**	-0.04	0.35	-0.14**	-0.03	0.08
Adult men	-0.30**	-0.05	0.43	-0.14**	-0.03	0.13
Young women	-0.53**	-0.09	0.36	-0.25**	-0.06	0.07
Young men	-0.67**	-0.11	0.44	-0.32**	-0.06	0.13

Notes: the table presents estimates from Equation (1). Standard errors, clustered at the country level, are in parenthesis. *, and ** denote significance at the 90 percent, and 1 percent confidence level, respectively. AEs and EMDEs stand respectively for advanced economies and emerging markets and developing economies. The sample of AEs comprises 38 countries and 908 observations. The sample of EMDEs comprises 57 countries and 751 observations.
Sources: Authors' estimation based on ILO Key Indicators of the Labour Market and IMF World Economic Outlook.

Looking at different demographic groups, we observe further heterogeneity. The relationship between the unemployment gap and business conditions is generally stronger for men than women and youth than adult. That is valid in both AEs and EMDEs, although the differences are starker in AEs. There, young men (for which Okun's Law is strongest) display an Okun coefficient that is about three times larger in absolute value relative to that of adult women (the group for which Okun's Law is the least relevant).

Considering both genders, the sensitivity of the youth unemployment gap is about twice as large as that of the adults in EMDEs and somewhat more than twice in AEs. The estimated coefficient is -0.67 and -0.32 for young men in AEs and EMDEs respectively, as opposed to just -0.30 and -0.14 for adult men. The differences between men and women are smaller in terms of coefficients, but larger for what concerns the explanatory power of the regression. This is evident when looking at adults in EMDEs: the Okun coefficient is the same for men and women, at -0.14, while the R^2 is 0.13 for men and only 0.08 for women, indicating that other factors are more important in explaining the cyclical fluctuations of the unemployment gap for women than for men. Finally, the finding that the responsiveness of the unemployment gap to business conditions is about half in EMDEs relative to AEs survives across all different demographic groups.

Recent analyses focusing on AEs had already found the Okun's Law to be more important for the youth than the adult (Banerji et al., 2014). Our estimates extend this result to EMDEs. What could explain the larger sensitivity of the youth unemployment gap? Some potential explanations relate to labor market policies. For starters, the youth are typically more likely to be employed under temporary contracts, which tend to have lower hiring and firing costs. Moreover, employment protection regulations often constrain the freedom of employers to choose which employees to dismiss and tend to protect more senior workers or workers with family responsibilities. Even when legal norms are less stringent – a more likely case for emerging economies – it is more socially acceptable for the employer to first lay-off younger workers during bad economic times.

Perhaps more surprising is the finding that women display a lower unemployment gap sensitivity than men. One potential explanation is that, being more marginally attached to the labor force than men, cyclical flows between employment and nonparticipation, which dampen the observed sensitivity of unemployment to the business cycle, are more important for women than for men. Indeed, Elsby, Hobijn, and Şahin, 2015 observed that such flows are relevant for women whereas they are much smaller for men in the U.S. Another, somewhat related, possible explanation relates to the behavior of the labor force participation. If women's participation were to be more procyclical than men's, the estimated sensitivity of the women's unemployment gap would be lower (see the discussion in the earlier Subsection 5.2.2). We will delve deeper on these explanations in the next section.

5.4.2 Robustness Checks

Before proceeding further, we conduct several robustness checks regarding the variables used, the sample considered, and the assumptions made. As a first robustness check, we supplement Equation 5.1 with the inclusion of time fixed in effects to account for possible common movements in the unemployment gap that are unrelated to output. We also verify that our results do not depend on the classification of countries between AEs and EMDEs and we estimate Equation 5.1 excluding from the sample a set of countries that may be classified either as advanced or emerging, depending on the classification rules used, or that have graduated from emerging during the sample period.

Third, we check that our results are robust to different techniques to estimate the output gap: we then estimate Equation 5.1 using both a measure of the output gap obtained applying the HP filter on per capita GDP and the level of potential output as estimated in the IMF World Economic Outlook. Finally, we assume that both the natural rate of unemployment and the potential GDP growth rate are constant over time. That allows us to first-difference Equation 5.1 and derive an alternative, first difference, specification that does not require us to obtain measures of the potential level of output and the natural rate. In practice, we estimate the following specification:

$$\Delta u_{i,t} = \mu_i + \beta[\Delta ln(y_{i,t})] + \epsilon_{i,t}$$

Results from these robustness checks are reported in Tables D.1 to D.5 in Appendix D. All estimates are similar to those obtained from the baseline regressions, which reassure us about the robustness of our results. In carrying out the rest of the analysis, we will use the baseline specification.

5.5 Channels and Other Extensions

5.5.1 Decomposition Between the Employment and Participation Margins

As discussed in Section 5.2.2, the Okun coefficient is determined by the sensitivities of both the labor force participation and the employment gaps to changes in the business cycle, and it can be approximated as the difference between the two. Here we decompose the unemployment response into its employment and participation channels. The conventional wisdom is that procyclical, but small, movements in the labor force tend to slightly dampen the response of the unemployment rate to the business cycle (that is, the unemployment gap response is below but close that of the employment gap in absolute value). Our results, shown in Table 5.2 below, suggest that this intuition is indeed valid for the overall working age population and the adults in AEs, but not so much for EMDEs and the youth in AEs. We discuss our results more in detail below.

As expected, both the participation and the employment gaps display positive coefficients across all demographic groups, indicating that these two variables are procyclical. The ratio, in absolute value, of the estimated coefficient for the employment gap (Equation 5.2) relative to that of the unemployment gap is lowest for adult women in AEs (just 1.13), reflecting their low and not statistically significant labor force gap response. For both the overall working age population and adult men the same ratio is somewhat higher, but still below 1.4. On the other hand, the employment gap responds almost twice as much as the unemployment gap for young women and young men in AEs (the ratio is 1.78 and 1.87 respectively). These results are driven by much higher participation sensitivities for the youth relative to adults in AEs, which can be explained by considering that younger agents are more likely to have the option between study and work.

Turning to EMDEs, we note that the ratio between the employment and the unemployment gaps response is comprised between 1.64, for adult women, and 1.78, for young men. The tighter range relative to AEs reflects lower and higher (in relative terms) participation responses for the youth and adults respectively. The former result can be explained by noticing that, due to less developed educational systems in EMDEs, the schooling option is less present in EMDEs than in AEs for the youth.

Table 5.2: Cyclical sensitivity of employment and labor force participation rates

	AEs			EMDEs		
Panel A. Log employment						
	δ	s.e.	R^2	δ	s.e.	R^2
All working age	0.43**	-0.06	0.28	0.3**	-0.07	0.13
Adult women	0.25**	-0.04	0.09	0.26**	-0.08	0.03
Adult men	0.39**	-0.06	0.29	0.23**	-0.05	0.05
Young women	0.93**	-0.16	0.2	0.44**	-0.16	0.04
Young men	1.25**	-0.19	0.32	0.57**	-0.13	0.07
Panel B. Log labor force participation						
	θ	s.e.	R^2	θ	s.e.	R^2
All working age	0.09**	-0.02	0.06	0.11*	-0.06	0.12
Adult women	0.02	-0.03	0.01	0.08	-0.07	0.02
Adult men	0.07**	-0.02	0.03	0.08*	-0.04	0.03
Young women	0.23*	-0.09	0.02	0.11	-0.11	0.03
Young men	0.38**	-0.08	0.07	0.17*	-0.09	0.03

Notes: Panels A and B respectively present estimates from Equations 5.2 and 5.3. Standard errors, clustered at the country level, are in parenthesis. *, and ** denote significance at the 90 percent, and 1 percent confidence level, respectively. AEs and EMDEs stand respectively for advanced economies and emerging markets and developing economies. The sample of AEs comprises 38 countries and 908 observations. The sample of EMDEs comprises 57 countries and 751 observations.
Sources: Authors' estimation based on ILO Key Indicators of the Labour Market and IMF World Economic Outlook.

Focusing only on employment gap sensitivities, there are few results that are worth highlighting. For the overall working age population, AEs have a sensitivity that is only about 1.5 times larger than EMDEs, rather than about two times as it was the case for the unemployment gap. For one specific demographic group, adult women, the sensitivity is about the same in AEs and EMDEs (0.25 and 0.26 respectively). Moreover, looking at the response of the employment gap reveal even greater heterogeneities among demographic groups in AEs. Young men, with a sensitivity of 1.25, have an employment response that is five times as large as that of adult women.

It is worth noticing also the larger participation sensitivities for men relative to women. What could be the reason for this apparently counterintuitive result? The incidence of discouraged workers might display more cyclical variation for men than for women. That might be the case if, for instance, men were employed more in cyclical sectors, such as construction.

The results illustrated here are also useful to interpret the lower unemployment gap sensitivities displayed by women, which were reported in the previous Section, particularly for AEs. The two explanations that we put forward, namely the larger importance of flows between employment and nonparticipation and the stronger sensitivity of the labor force participation gap for women, do not seem to have an empirical backing. Indeed, it emerges that the smaller magnitude of the Okun's coefficient is driven by a lower employment gap response for women than for men. We will come back on this result in a later extension when we look at the differences in the Okun's coefficient across the business cycle.

5.5.2 The Importance of the Business Cycle

In this section, we investigate whether the strength of Okun's Law varies according to the stage of the business cycle. Specifically, we differentiate through good and bad economic times, defined as periods of positive and negative output gap respectively (for more details refer to Equation 5.4 in Section 5.2.2). Table 5.3 below shows the estimated coefficients. The negative relationship between unemployment and the output gap is stronger during bad times. That is true in general, although the estimated coefficients are only statistically different from each other in AEs, and just for the overall working age population and both youth and adult men.

What could drive this result? To shed more light on this issue, we extend this business cycle analysis to the employment and labor force participation margins. The results, shown in Table 5.4 below, are intriguing. Except in one case, the labor force participation does not exhibit significant non-linearities. The employment gap instead does. Again, the non-linearities are driven by men. In AEs, the adult men employment gap is 0.32 (0.49) percent higher (lower) for each percentage point increase (decrease) in the output gap. Young men employment displays similar (relative) sensitives, with the cyclical component increasing 1.06 percent during upturns and decreasing 1.52 during downturns. These differences are statistically

Table 5.3: Okun's Law in good and bad states

	AEs				EMDEs			
	ρ/σ	**s.e.**	**Wald**	R^2	ρ/σ	**s.e.**	**Wald**	R^2
All working age	-0.25** -0.39**	-0.04 -0.07	0.01	0.48	-0.15** -0.20**	-0.06 -0.05	0.5	0.14
Adult women	-0.20** -0.24**	-0.03 -0.05	0.17	0.35	-0.13** -0.17**	-0.05 -0.05	0.57	0.08
Adult men	-0.23** -0.39**	-0.04 -0.08	0.02	0.44	-0.11* -0.18**	-0.04 -0.04	0.25	0.13
Young women	-0.48** -0.59**	-0.09 -0.11	0.17	0.35	-0.16* -0.36**	-0.09 -0.1	0.57	0.08
Young men	-0.54** -0.86**	-0.11 -0.15	0.02	0.45	-0.25** -0.40**	-0.09 -0.09	0.31	0.13

Notes: the Table presents estimates from Equation 5.4, using the unemployment rate gap as dependent variable. In each row the first/second line refers to the sensitivity in the good/bad state. Standard errors, clustered at the country level, are in parenthesis. *, and ** denote significance at the 90 percent, and 99 percent confidence level, respectively. The columns 'Wald' report the p-value from a Wald test for equal coefficients ($H_0 : \rho = \sigma$). AEs and EMDEs stand respectively for advanced economies and emerging markets and developing economies. The sample of AEs comprises 38 countries and 908 observations. The sample of EMDEs comprises 57 countries and 751 observations.
Sources: Authors' estimation based on ILO Key Indicators of the Labour Market and IMF World Economic Outlook.

significant at the 95 percent confidence level. Instead, we note that (i) women employment does not exhibit statistically significant differences during good and bad times and, (ii) the coefficients in good and bad times are just slightly lower than those of men during good times. The bottom line of this analysis is that periods of negative output gap are especially detrimental for men, and particularly young men, in AEs.

5.6 Conclusions

Starting with Okun, 1963, a rich empirical literature has documented the existence of a negative and stable relationship between an economy's aggregate demand conditions and its overall unemployment. We show that there is a large degree of heterogeneity in the cyclical sensitivities of unemployment across demographic and economy groups. EMDE adult men's unemployment gap rises about 0.14 percentage points for a one percentage point decline in the output gap, while AE adult men's gap rises about 0.30. Women's unemployment gap is significantly less sensitive to demand conditions than men's in AEs. By contrast, EMDE adult women's cyclical sensitivity of unemployment is exactly equal that of EMDE adult men's. The youth unemployment gap is generally twice as sensitive as that of adults. These findings are robust to alternative regression specifications and estimation procedures.

Table 5.4: Cyclical sensitivity of employment and labor force participation in good and bad states

	AEs				EMDEs			
Panel A. Log employment								
	ρ/σ	**s.e.**	**Wald**	**$\hat{R2}$**	**ρ/σ**	**s.e.**	**Wald**	**$\hat{R2}$**
All working age	0.36** 0.52**	-0.07 -0.09	0.09	0.28	0.31** 0.29*	-0.1 -0.13	0.91	0.13
Adult women	0.23** 0.29**	-0.06 -0.08	0.6	0.1	0.29* 0.21	-0.12 -0.14	0.69	0.03
Adult men	0.32** 0.49**	-0.06 -0.1	0.09	0.3	0.25** 0.21*	-0.09 -0.09	0.81	0.05
Young women	0.91** 0.95**	-0.21 -0.22	0.6	0.1	0.26 0.67*	-0.25 -0.27	0.69	0.03
Young men	1.06** 1.52**	-0.23 -0.19	0.04	0.32	0.22 1.03**	-0.2 -0.25	0.03	0.08
Panel B. Log labor force participation								
	ρ/σ	**s.e.**	**Wald**	**$\hat{R2}$**	**ρ/σ**	**s.e.**	**Wald**	**$\hat{R2}$**
All working age	0.09* 0.08	-0.04 -0.05	0.91	0.06	0.14* 0.06	-0.07 -0.1	0.54	0.12
Adult women	0.02 0.01	-0.05 -0.07	0.92	0.01	0.15 -0.01	-0.09 -0.13	0.33	0.02
Adult men	0.08* 0.05	-0.04 -0.03	0.69	0.03	0.14* 0	-0.06 -0.08	0.21	0.03
Young women	0.30* 0.14	-0.14 -0.18	0.92	0.01	0.03 0.21	-0.17 -0.19	0.33	0.02
Young men	0.39* 0.37*	-0.16 -0.14	0.96	0.07	-0.12 0.53**	-0.13 -0.19	0.02	0.04

Notes: Panels A and B present estimates from Equation (6), using the log employment gap and the log labor force participation gap, respectively, as dependent variables. In each row, the first/second line refers to the sensitivity in the good/bad state. Standard errors, clustered at the country level, are in parenthesis. *, and ** denote significance at the 90 percent, and 1 percent confidence level, respectively. The columns 'Wald' report the p-value from a Wald test for equal coefficients ($H_0 : \rho = \sigma$). AEs and EMDEs stand respectively for advanced economies and emerging markets and developing economies. The sample of AEs comprises 38 countries and 908 observations. The sample of EMDEs comprises 57 countries and 751 observations.
Sources: Authors' estimation based on ILO Key Indicators of the Labour Market and IMF World Economic Outlook.

We also consider several extensions to these core results. First, we decompose the cyclical unemployment rate response into employment and participation margins. The results indicate that, for all groups, procyclicality of labor force participation leads to an unemployment rate gap response that is smaller, in absolute value, than that of the employment gap (defined as the cyclical component of the employment level). Moreover, the magnitudes of labor force participation and employment sensitivities to the cycle differ widely across demographic groups, revealing even greater heterogeneity than the unemployment gap responses across demographics.

Second, we study whether the cyclical sensitivity of unemployment depends on the stage of the business cycle. Our estimates suggest that cyclical unemployment is more sensitive in upturns than downturns. This finding is again sharper for young men. Focusing again on employment, we find that its cyclical component decreases by 1.03 percentage points for each percentage point decrease in the output gap for young men in EMDEs, while its response during periods of positive output gap is not statistically different from 0. Such large asymmetry is not evident when looking at the unemployment gap sensitivity, as it is masked by a large asymmetric response of the cyclical component of the labor force participation rate. Indeed, this decreases by 0.53 percentage points for each percentage point reduction in the output gap during bad economic times, while its response is not statistically different from 0 during good economic times.

The findings provided in this paper argue against the 'one size fits all' rule. Heterogeneities across demographic groups are important. Future work should aim at exploring differences across countries and their determinants.

Appendix A

Appendix to Chapter 2

A.1 Dataset Descriptive Statistics — Cross-border Portfolio Flows

Here we present some descriptive statistics of the EPFR investment fund flow database. Table A.1 reports the number, average size and share of equity as well as of exchange-traded funds by investment mandate. Global EM funds are by far the most numerous (about 67 percent of all funds). Asian and Latin American regional funds constitute another 10 percent, while, except for the BRIC countries (Brazil, Russia, India, China and South Africa), country-specific funds are relatively few. Figure A.1 shows the distribution of funds by assets. This is highly skewed to the right, with funds in the upper quartile capturing about 90 percent of all industry assets.

Table A.2 shows mean and standard deviation of fund flows and NAV changes during the different Fed's guidance regimes identified in Section 2.2.3. These statistics are reported for the entire fund sample as well as for both the bond versus equity and ETFs versus mutual funds sample splits. On average, funds recorded both the highest flows and NAV returns in the open-ended regime, while they displayed negative returns only in the normalization regime. The average ETF experienced more inflows than the average mutual fund throughout the entire sample except for the normalization period. This is confirmed also at the aggregate level. Figure A.2 depicts the evolution of total industry assets as well as the ETFs share. The latter increased more than 8 percentage points through the analysis period, from about 43 percent in March 2009 to a little over 51 percent in May 2018. The average ETF asset share was also higher than the ETF share of the total number of funds, indicating that ETFs were larger on average. The rising popularity of ETFs was much stronger among bond than equity funds (see Figures A.3 and A.4). Across the former, their share increased from less than 5 to more than 40 percent during the March 2009 to May 2018 period.

Table A.1: Number, mean size and other characteristics by investment mandate

Investment mandate	funds	Composition % equity	Composition % ETF	Assets mean	Assets s.d.
Africa	2	100.0	50.0	58.3	35.5
Argentina	2	100.0	100.0	47.2	63.1
Asia ex-Japan	54	88.9	25.9	639.5	1149.9
BRIC	4	100.0	75.0	419.2	301.8
Brazil	11	90.9	81.8	1028.7	2639.1
Chile	2	100.0	50.0	291.8	224.8
China	47	89.4	63.8	368.5	1173.2
Colombia	2	100.0	100.0	72.9	60.0
Egypt	1	100.0	100.0	45.1	21.2
Emerging Europe	8	100.0	25.0	187.4	195.7
Global Emerging Markets	502	71.5	15.3	933.2	3893.3
Greater China	27	100.0	/	194.5	299.5
India	22	100.0	50.0	424.4	723.1
Indonesia	3	100.0	66.7	253.3	191.1
Israel	6	100.0	50.0	54.6	43.4
Korea (South)	7	100.0	42.9	805.6	1388.5
Latin America	18	94.4	33.3	311.4	598.7
Malaysia	2	100.0	50.0	489.4	336.4
Mexico	3	100.0	33.3	778.5	850.2
Middle East & Africa	3	100.0	66.7	87.9	67.7
Middle East	2	100.0	100.0	18.4	12.1
Nigeria	1	100.0	100.0	29.7	22.4
Pakistan	1	100.0	100.0	25.0	22.0
Peru	1	100.0	100.0	277.6	135.3
Philippines	1	100.0	100.0	229.5	129.3
Poland	2	100.0	100.0	117.7	112.5
Qatar	1	100.0	100.0	44.6	7.6
Russia	10	100.0	50.0	343.2	642.5
Saudi Arabia	1	100.0	100.0	22.4	44.5
South Africa	1	100.0	100.0	478.2	96.4
Taiwan	3	100.0	33.3	1174.2	1406.4
Thailand	3	100.0	33.3	272.6	226.2
Turkey	2	100.0	50.0	275.2	229.1
United Arab Emirates	1	100.0	100.0	39.8	8.6
Vietnam	1	100.0	100.0	331.0	126.4
Total	753	79.3	25.5	745.4	3166.3

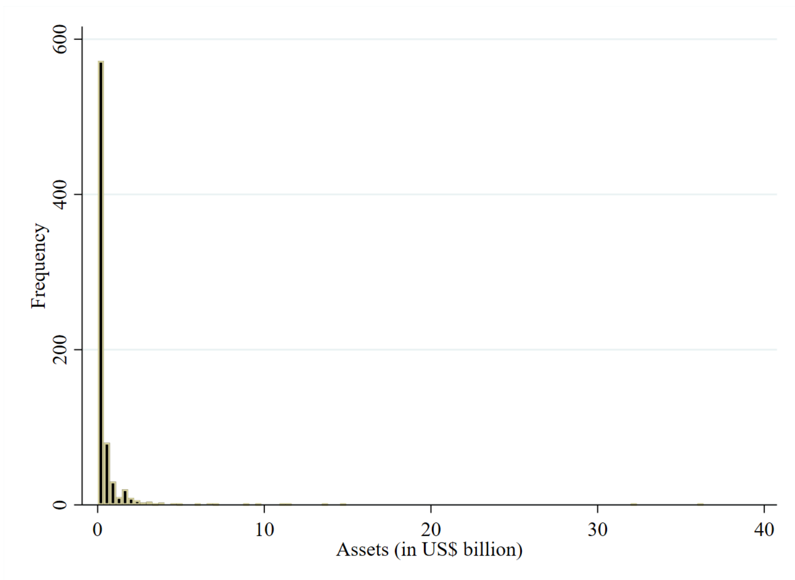
Notes: the Table reports descriptive statistics of investment funds that are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as defined by MSCI). The period considered goes from March 2009 to May 2018. The first column reports the country or region where funds invest (investment mandate). The second column reports the number of funds. The third and fourth columns report the percentage of all equity and exchange-traded-funds respectively. The fifth and sixth columns report the mean and standard deviation of funds' assets respectively (in U.S. $ million).
Sources: EPFR and own calculations.

Table A.2: Flows and NAV returns during different Fed's guidance regimes

			(A)		(B)		(C)		(D)	
			m.	**s.d.**	**m.**	**s.d.**	**m.**	**s.d.**	**m.**	**s.d.**
All funds	equity & bond	% flow	0.5	3.3	0.3	3.1	0.1	3.1	0.1	2.9
		% nav	0.5	3.3	0.1	3.1	-0.1	2.6	0.3	2.2
	equity	% flow	0.4	3.2	0.2	3.1	0.1	3.2	0.1	2.9
		% nav	0.5	3.5	0.1	3.4	-0.1	2.9	0.3	2.5
	bond	% flow	1.0	3.6	0.7	3.3	-0.1	2.9	0.1	2.8
		% nav	0.3	1.1	0.1	1.0	-0.1	1.1	0.1	0.9
Mutual funds	equity & bond	% flow	0.3	2.7	0.2	2.9	0.1	2.8	0.0	2.6
		% nav	0.5	3.1	0.1	2.8	-0.1	2.1	0.2	1.8
	equity	% flow	0.2	2.5	0.1	2.8	0.1	2.8	0.0	2.6
		% nav	0.5	3.4	0.1	3.1	-0.1	2.4	0.3	2.1
	bond	% flow	0.8	3.5	0.6	3.1	-0.1	2.8	0.0	2.6
		% nav	0.3	1.2	0.1	1.1	-0.1	1.1	0.1	0.9
ETFs	equity & bond	% flow	1.0	4.6	0.3	3.8	0.1	3.9	0.3	3.5
		% nav	0.4	3.7	0.0	3.9	-0.1	3.6	0.3	3.0
	equity	% flow	0.9	4.6	0.3	3.7	0.1	3.9	0.3	3.5
		% nav	0.4	3.8	0.0	4.1	-0.2	3.8	0.3	3.2
	bond	% flow	2.4	4.1	1.2	4.2	-0.1	3.4	0.3	3.6
		% nav	0.3	0.9	0.1	0.9	-0.1	1.0	0.1	0.9

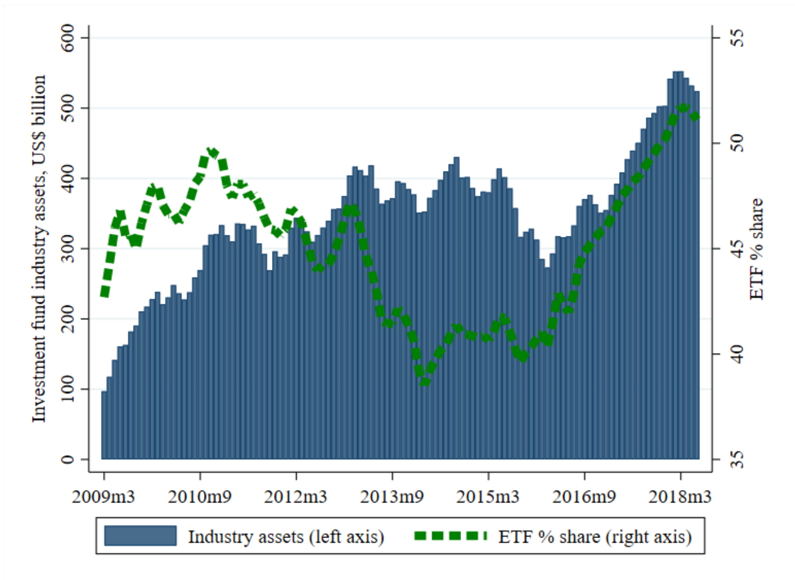
Notes: the Table reports the mean (m.) and standard deviation (s.d.) of weekly flows and net asset value changes, both measured in percent of beginning of period assets. Descriptive statistics are reported for different categories of funds (either exchange-traded funds (ETFs), mutual funds) and different investment focus (equity or bond). All the investment funds considered are legally domiciled in the U.S. and that invest in emerging, frontier and other market economies (as defined by MSCI). Columns denoted by "(A)", "(B)", "(C)", and "(D)" report statistics for different Fed's guidance regimes: respectively the open-ended (March/18/2009 to August/8/2011), calendar-based (August/9/2011 to May/22/2013), normalization (May/23/2013 to December/12/2015) and post-liftoff (December/13/2015 to May/30/2018).
Sources: EPFR and own calculations.

Figure A.1: Mean asset distribution



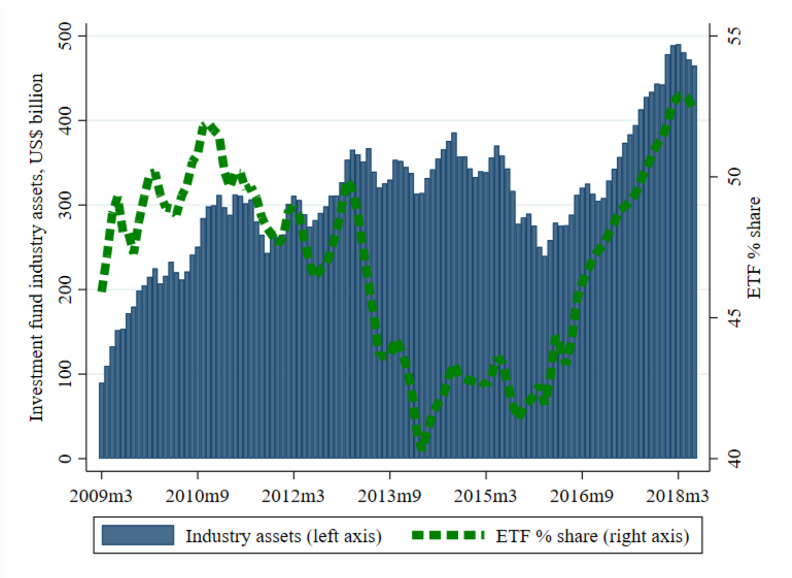
Notes: the Figure shows the distribution of funds by the average $ amount of assets held during the period March 2009 to May 2018. The funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as classified by MSCI).
Sources: EPFR and own calculations

Figure A.2: Total industry assets and ETF share - March 2009 to May 2018



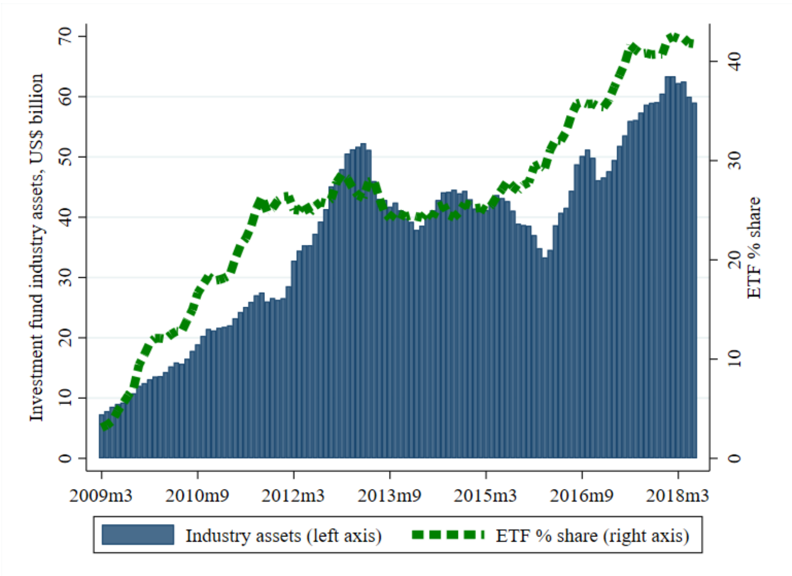
Notes: the Figure depicts the amount of assets held by all funds domiciled in the U.S. and investing in emerging, frontier and other market economies (as classified by MSCI), as well as the respective share of exchange-traded funds.
Sources: EPFR and own calculations

Figure A.3: Total industry equity assets and ETF share - March 2009 to May 2018



Notes: the Figure depicts the amount of equity assets held by all funds domiciled in the U.S. and investing in emerging, frontier and other market economies (as classified by MSCI), as well as the respective share of exchange-traded funds.
Sources: EPFR and own calculations.

Figure A.4: Total industry bond assets and ETF share - March 2009 to May 2018



Notes: the Figure depicts the amount of bond assets held by all funds domiciled in the U.S. and investing in emerging, frontier and other market economies (as classified by MSCI), as well as the respective share of exchange-traded funds.
Sources: EPFR and own calculations.

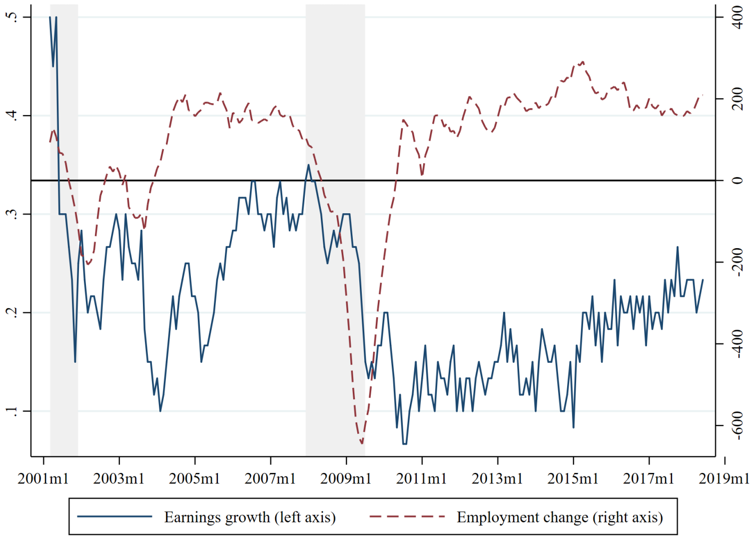
A.2 Dataset Descriptive Statistics — Macro News

Table A.3: Macroeconomic news

		m.	**s.d**	**min**	**max**	**source**
US	Non-farm payroll	-2.5	62.4	-123.0	175.0	Bloomberg
	Core CPI (MoM %)	0.0	0.1	-0.3	0.1	Datastream
	Unemployment rate	0.0	0.1	-0.5	0.3	Boomberg
	Retail sales	0.0	0.4	-1.5	1.8	Datastream
	ISM pmi index	0.4	1.7	-4.7	3.5	Datastream
U.K.	Markit pmi	0.3	1.8	-3.8	4.3	Investing
	Policy rate	0.0	0.0	0.0	0.3	Boomberg
	Jobless claims change	-3.8	13.5	-42.9	53.7	Boomberg
	GPD (YoY %)	-0.1	0.2	-0.6	0.4	Boomberg
Japan	Retail trade	0.1	1.1	-2.6	2.8	Boomberg
	Industrial production	-0.4	1.4	-4.7	3.8	Boomberg
	CPI (YoY %)	0.0	0.1	-0.2	0.3	Boomberg
	Jobless rate	0.0	0.2	-0.5	0.3	Boomberg
Germany	Markit pmi	0.1	0.5	-1.1	3.1	Investing
	Unemployment change	-5.1	17.2	-62.5	38.9	Boomberg
	Ifo business climate	0.3	1.1	-2.5	4.7	Boomberg
	Zew current situation	1.3	6.5	-31.5	20.3	Boomberg
	Zew expectations	0.3	7.4	-19.2	27.8	Boomberg
Eurozone	GPD (YoY %)	0.0	0.1	-0.2	0.2	Boomberg
	CPI (YoY %)	0.0	0.0	-0.1	0.1	Boomberg
	Consumer confidence	0.0	0.9	-4.1	5.0	Boomberg
	Markit pmi	0.0	0.2	-0.7	0.5	Investing

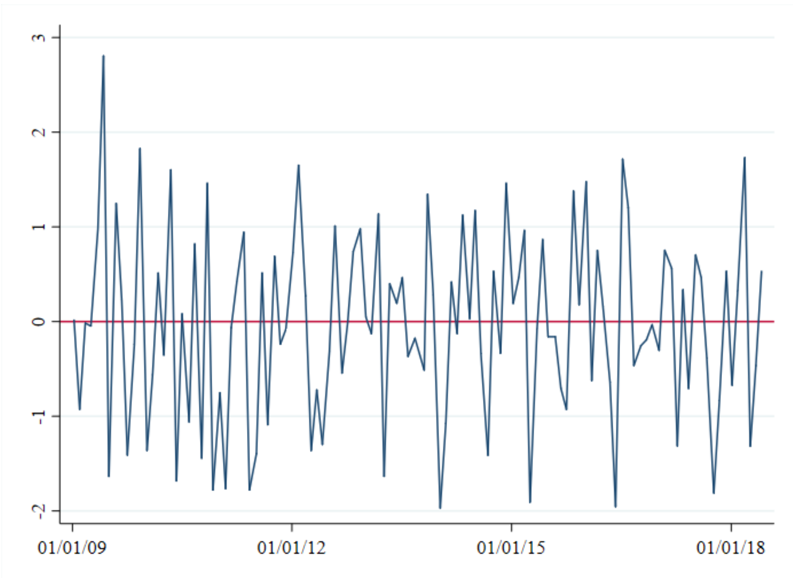
Notes: the Table reports descriptive statistics of macroeconomic announcement used for the analysis. The leftmost column reports the country or currency union concerned. The second leftmost column lists the name of the announcements. Column denoted by "m." reports the mean surprise (with a surprise being defined as the difference between the actual release and median forecast). Column denoted by "s.d." reports the surprise standard deviation. Columns denoted by "min" and "max" report the minimum and maximum values of the surprise. All statistics refer to the period March 2009 to May 2018. Column "source" reports the source from which data on actual release and news forecast are taken.
Sources: Bloomberg, Datastream, Investing.com and own calculations.

Figure A.5: U.S. non-farm payroll employment change and average hourly earnings growth - 2001 to 2018



Notes: The Figure shows the net change in payroll employment and average hourly earnings growth in the U.S. over the period March 2011 to May 2018. The left (right) y-axis measures earnings growth (employment change). The x-axis reports dates. Shaded areas are periods of recessions as defined by the National Bureau of Economic Research.
Sources: Bloomberg, Datastream and National Bureau of Economic Research.

Figure A.6: Non-farm payroll surprise series - March/2009 to May/2018



Notes: the Figure shows the standardized non-farm payroll surprise series. A surprise is defined as the difference between a macroeconomic news actual release and the median of analysts' forecast. To obtain a standardized measure, the surprise is divided by its standard deviation.
Sources: Bloomberg and own calculations.

A.3 Robustness Checks and Other Extensions on the Baseline Analysis

In this appendix we verify the robustness of the baseline results (those reported in Figure 2.2) to some alternative specifications. First, we check that our findings are not driven by NFP release outliers. We do so by re-estimating Equation 2.2 constraining the sample so as to exclude all observations associated with a specific NFP release. We repeat this process for each release and show the resulting estimates in Figure A.7, using as confidence bands those of the baseline analysis. Particularly for the calendar-based and normalization regimes, all IRFs obtained in this way are clustered around the baseline response and always fall within the original confidence bands, thus indicating that the results are not driven by outliers.

Second, we check that our findings do not depend on the way in which the NFP series is constructed. Results in Figure 2.2 are obtained weighting equally all releases. However, Pericoli and Veronese, 2015 show that macroeconomic announcements have smaller effects in instances in which forecaster disagreement is higher. We then construct two additional series. In the first one, observations below (above) the 25th (75th) percentile of the distribution of forecaster disagreement (measured by the forecast standard deviation) receive a weight equal to 1.5 (0.5), while all other observations have unit weight. In the second one, we give weight equal to 0.5 to observations above the 66th percentile (the same threshold used in Pericoli and Veronese, 2015), and weight equal to 1 to all the others.

These robustness check estimates are shown in Table A.4. The new IRFs are very similar to the baseline. The most notable differences occur when using the first alternative variable and, as expected due to the higher economic uncertainty, during the open-ended guidance period. The 4-week cumulated effect is estimated to be similar to that observed for the normalization period, and statistically different from 0. The new coefficients are also significant for the post-liftoff regime (at impact and in the second week after the release), but much smaller in absolute value. This exercise confirms the robustness of the baseline results and further indicates that the open-ended period was more similar to the normalization than the calendar-based guidance regime.

Next, we use alternative data sourced from the Institute of Internal Finance (IIF) to measure capital flows. The advantage of this database is that it covers all types of cross-border portfolio capital flows, as opposed to only those happening through allocations in investment funds. However, recipient countries are just a few EMs and countries of origin are not confined to the U.S. The other main limitation is that flows cannot be normalized to some reference value and are thus expressed in \$. Bearing in mind these differences, we again rely on the local projection method and estimate the following specification:

$$\sum_{j=0}^{k} f_{t+j} = \sum_{r=1}^{4} \left(\alpha_k^r d_t^r + \kappa_k^r d_t^r x_t\right) + \sum_{l=1}^{12} \varphi_k^l f_{i,t-l} + \varepsilon_t \tag{A.1}$$

where in this case f_t are net portfolio capital flows at week t; V_t is a vector containing twelve lagged values of f_t; and the rest of the notation follows from that of Equation 2.2.

The estimation is carried out through OLS. Standard errors are heteroskedasticity (white) robust. IRFs are shown in Figure A.8. The results are qualitatively very similar to the baseline analysis. This is especially true for the calendar-based and normalization guidance regimes. For the former period, the estimated 4-week cumulated response to a one standard deviation NFP surprise is equal to about $4 billion. For the normalization guidance regime, the response is smaller in absolute value and significant only 1 and 2 weeks after the release. However, considering that IIF data measure global flows, as opposed to flows stemming from the U.S., these results are in line with the baseline and suggest that fund flow data can be used as a relatively good proxy for overall net cross-border portfolio flows.

We also verify that the results do not depend on the particular date chosen to distinguish between the calendar-based and normalization guidance regimes. For the baseline, we used as cutoff the day in which Bernanke first hinted at the possibility of scaling down QE during his famous taper tantrum speech (May/22/2013). As alternatives, we employ the days in which (i) the FOMC switched between calendar- and threshold-based guidances (December/12/2012), and (ii) it officially announced the reduction of QE (December/8/2013).[1] The results are shown in Table A.5 in Appendix A.3 and are similar to the baseline. If anything, when relying on alternative (i), the estimated sensitivity for the calendar-based regime is about 25 percent larger. This highlights that portfolio capital flows behaved in a very peculiar way (relative to the other periods) exactly when the FOMC explicitly signaled low rates for a 2-to-3-year period, in what was the quintessence of time-based guidance.

We also show that the non-linear response of flows to macroeconomic announcements are not confined to employment news. We consider another major macroeconomic release, namely the monthly growth in retail sales and estimate Equation 2.2 using this instead of the NFP. Results are shown in Figure A.9. The responses for the open-ended, calendar-based and normalization guidance regimes are in line with those observed for NFP announcements. Furthermore, positive retail sales releases also induced outflows during the post-liftoff regime. This can be reconciled noticing a shift in the Fed's guidance happened after raising rates for the first time. The U.S. economic recovery in the post-GFC period was characterized by solid job growth but

[1] For a review of the different forward guidance statements refer to Section 2.2.2.

stagnant wages, the latter being in contrast to previous historical episodes.[2] In this context, the FOMC signaled that its focus was shifting from employment to inflation (see Table 2.1 for the exact statement). This might have diminished the importance attached to employment data by market participants, while leaving unaffected that of retail sales, which is an indicator of consumer spending and thus a potential signal of inflationary pressures.

Among the existing literature, the paper by Fratzscher, 2012 is the only other one analyzing the sensitivity of capital flows to U.S. macroeconomic announcements.[3] This author focuses on the November 2005 to October 2010 period and also estimates coefficients of opposite sign, positive for the GFC period (August/2007 to March/2009) and negative for the rest of the sample. Fratzscher, 2012 explains this non-linearity arguing that in crisis times negative U.S. news reduce investors' risk tolerance and cause a flight-to-safety reaction, with capital fleeing EMs.

Although unlikely, since the August/2011 to May/2013 period does not coincide with a financial crisis in the U.S., we check whether the dynamic described above can also explain our results. If flight-to-safety behaviors indeed mattered, then negative surprises should be driving the result. We twist Equation 2.2 to allow for non-linear effects depending on the sign of the surprise by interacting the NFP series (x_t) with two dummy variables (d_t^p and d_t^n) for positive and negative surprises, and estimate the relevant coefficients.

The new IRFs are reported in Table A.6. A * indicates that positive and negative surprises are estimated to have statistically different effects. During the calendar-based regime (Column B) the impact response of flows is significantly stronger for positive than negative surprises. The latter have almost null effects, confirming the unsuitability of flight-to-safety explanations. On the other hand, estimates reported in Column C of Table A.6 (Appendix A.3) show that the response of fund flows to positive and negative surprises had roughly the same effects, which suggests that the dynamics just described also worked in reverse. That is, negative surprises delayed the expected timing of normalization, thus leading to positive inflows. In this sense, investors perceived the Federal Reserve to be 'data-dependent'. This interpretation is also consistent with the frequent references by the FOMC to labor market conditions as a crucial factor to decide the timing of normalization (see the discussion in Section 2.2.2).

[2] See Blanchard, Cerutti, and Summers, 2015 and Blanchard, 2016 for a discussion of the Phillips curve relationship in recent decades and refer to Figure A.5 in Appendix A.2 for a visual representation of employment changes and earnings growth in the 2001-2018 period).

[3] Differently from the current analysis, Fratzscher, 2012 does not focus only on NFP announcements but rather constructs a variable made of a weighted average of major U.S. releases. Instead of fund-level data he uses data aggregated at the country level. Finally, he studies flows to EMs stemming from all funds, as opposed to those only domiciled in the US.

Table A.4: Employment surprise variable

		(A)	(B)	(C)	(D)
1-week	Equal weights (baseline)	0.03	**0.12**	**-0.09**	-0.03
	High/low weigth for low/high disag.	0.00	**0.12**	**-0.08**	**-0.04**
	Low weigth for high disag.	0.03	**0.14**	**-0.08**	**-0.04**
2-week	Equal weights (baseline)	0.00	**0.22**	**-0.13**	-0.03
	High/low weigth for low/high disag.	-0.05	**0.20**	**-0.12**	-0.04
	Low weigth for high disag.	-0.03	**0.23**	**-0.13**	-0.04
3-week	Equal weights (baseline)	**-0.10**	**0.35**	**-0.19**	**-0.07**
	High/low weigth for low/high disag.	**-0.20**	**0.32**	**-0.16**	**-0.08**
	Low weigth for high disag.	**-0.16**	**0.41**	**-0.19**	-0.08
4-week	Equal weights (baseline)	-0.09	**0.44**	**-0.18**	-0.06
	High/low weigth for low/high disag.	**-0.20**	**0.38**	**-0.15**	-0.07
	Low weigth for high disag.	-0.12	**0.49**	**-0.18**	-0.07

Notes: the Table shows results from a robustness check on the baseline analysis regarding the treatment of forecaster uncertainty in the construction of the non-farm payroll surprise variable. The numbers report the estimated responses of allocations into investment funds to a one standard deviation surprise in the U.S. non-farm payroll data release. The funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as defined by MSCI). Sensitivities are measured in percentage of beginning of period assets. Estimates are obtained from Equation 2.2. The leftmost column reports the horizon considered (k). The second leftmost column indicates the NFP variable that is used. "Equal weights" indicates that each NFP release is given equal weight. "High/low weight for low/high disag." indicates that observations for which forecaster disagreement is below (above) the 25th (75th) percentile of its distribution are given weight equal to 1.5 (0.5), and observations in between these threshold are given weight equal to 1. "Low weight for high disag." indicates that observations for which forecaster disagreement is above the 66th percentile of its distribution are given weight equal to 0.5 (all the others have weight 1). Columns denoted by (A), (B), (C), and (D) report estimates for different Fed's guidance regimes: respectively the open-ended (March/18/2009 to August/8/2011), calendar-based (August/9/2011 to May/22/2013), normalization (May/23/2013 to December/12/2015) and post-liftoff (December/13/2015 to May/30/2018). Bold numbers indicate statistical significance at the 90 percent confidence level, based on clustered standard errors (at the fund-level).
Sources: Bloomberg, Datastream, EPFR and own calculations.

Table A.5: Cutoff date used to distinguish between the calendar-based and normalization guidance periods

		(A)	(B)	(C)	(D)
1-week	Taper tantrum (baseline)	0.03	**0.12**	**-0.09**	-0.03
	Start of tapering	0.03	0.04	**-0.06**	-0.03
	Threshold-based guidance	0.03	**0.16**	**-0.07**	-0.03
2-week	Taper tantrum (baseline)	0.00	**0.22**	**-0.13**	-0.03
	Start of tapering	0.00	**0.11**	**-0.12**	-0.03
	Threshold-based guidance	0.00	**0.26**	**-0.11**	-0.03
3-week	Taper tantrum (baseline)	**-0.10**	**0.35**	**-0.19**	**-0.07**
	Start of tapering	**-0.10**	**0.20**	**-0.18**	**-0.07**
	Threshold-based guidance	**-0.10**	**0.44**	**-0.16**	**-0.07**
4-week	Taper tantrum (baseline)	-0.09	**0.44**	**-0.18**	-0.06
	Start of tapering	-0.09	**0.30**	**-0.19**	-0.06
	Threshold-based guidance	-0.09	**0.57**	**-0.15**	-0.06

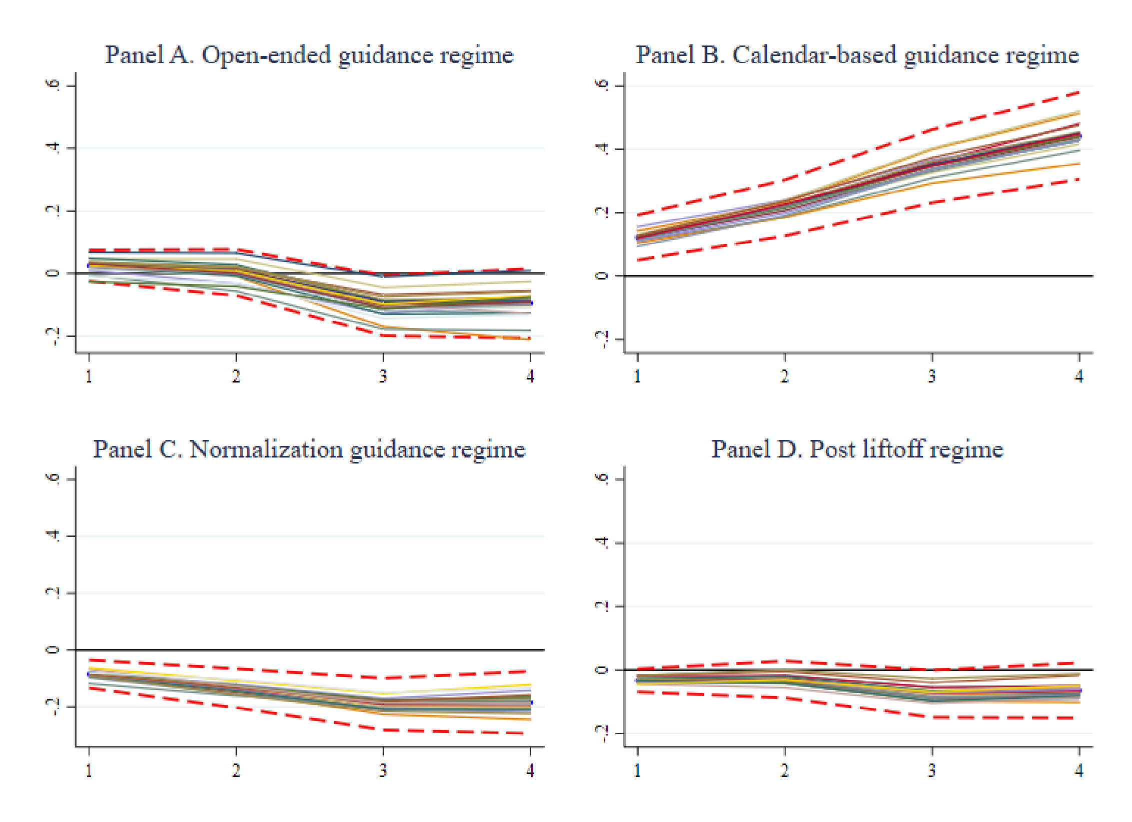
Notes: the Table shows results from a robustness check on the baseline analysis regarding the cutoff date chosen to distinguish between the Fed's calendar-based and normalization guidance regimes. The numbers report the estimated responses of allocations into investment funds to a one standard deviation surprise in the U.S. non-farm payroll data release. The funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as defined by MSCI). Sensitivities are measured in percentage of beginning of period assets. Estimates are obtained from Equation 2.2. The leftmost column reports the horizon considered (k). The second leftmost column denotes the chosen cutoff date to distinguish between the calendar-based and normalization guidance. The wording "Taper tantrum (baseline)" denotes to estimates obtained using the day of Bernanke's taper tantrum (May/22/2013). The wording "Official start of tapering" denotes to estimates obtained using the day in which the FOMC announced the reduction in the monthly amount of asset purchases under the third round of QE (December/18/2013). The wording "Threshold-based guidance" denotes to estimates obtained using the day in which the FOMC switched from calendar-based to threshold-based forward guidance (December/12/2012). Columns denoted by (A), (B), (C), and (D) report estimates for different Fed's guidance regimes: respectively the open-ended (March/18/2009 to August/8/2011), calendar-based (August/9/2011 to May/22/2013), normalization (May/23/2013 to December/12/2015) and post-liftoff (December/13/2015 to May/30/2018). Bold numbers indicate statistical significance at the 90 percent confidence level, based on clustered standard errors (at the fund-level).
Sources: Bloomberg, EPFR and own calculations.

Table A.6: Positive and negative surprises

		(A)	(B)	(C)	(D)
1-week	All surprises (baseline)	0.03	**0.12**	**-0.09**	-0.03
	Positive surprises	0.07	**0.24***	**-0.12**	0.03*
	Negative surprises	-0.01	0.01*	-0.06	**-0.09***
2-week	All surprises (baseline)	0.00	**0.21**	**-0.14**	-0.03
	Positive surprises	0.02	**0.25**	**-0.17**	0.02
	Negative surprises	-0.01	**0.18**	**-0.11**	-0.07
3-week	All surprises (baseline)	-0.09	**0.35**	**-0.19**	-0.07
	Positive surprises	**-0.2**	**0.38**	**-0.23**	0.00*
	Negative surprises	-0.02	**0.32**	**-0.16**	**-0.15***
4-week	All surprises (baseline)	-0.10	**0.44**	**-0.18**	-0.06
	Positive surprises	**-0.33***	**0.44**	**-0.20**	0.02*
	Negative surprises	0.09	**0.44**	**-0.18**	**-0.14***

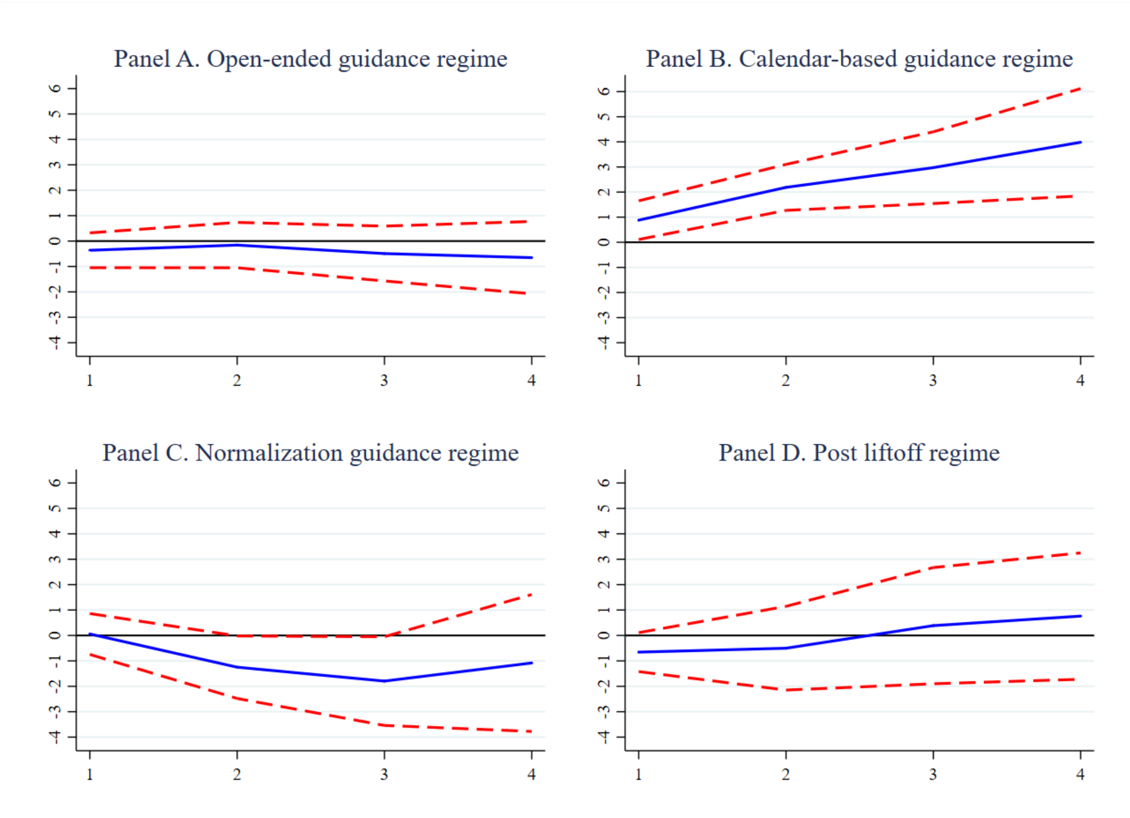
Notes: the Table shows results from an extension on the baseline analysis regarding the effects of positive and negative surprises. The numbers report the estimated responses of allocations into investment funds to U.S. non-farm payroll data release. The funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as defined by MSCI). Sensitivities are measured in percentage of beginning of period assets. The leftmost column reports the horizon considered (k). The second leftmost column denotes the chosen cutoff date to distinguish between the calendar-based and normalization guidance. The second leftmost column denotes the type of the surprise. The wording "All surprises (baseline)" refers to the $\hat{\beta}_k^r$ coefficients estimated from Equation 2.2. The wordings "Positive surprises" and "Negative surprises" refer to the $\hat{\beta}_k^{p,r}$ and $\hat{\beta}_k^{n,r}$ coefficients estimated from the following regression: $100 * \frac{\sum_{j=0}^{k} f_{i,t+j}}{a_{i,t}} = \sum_{r=1}^{4} \left(\beta_k^{p,r} d_t^r d_t^p news_t + \beta_k^{n,r} d_t^r d_t^n news_t\right) + A_k D_t + B_k Z_{i,t} + \gamma_i + \varepsilon_{i,t}$, where d_t^p (d_t^n) is a dummy variable taking value 1 for positive (negative) NFP surprises and 0 otherwise, and the rest of the notation is as in Equation 2.2. A * indicates that the $\hat{\beta}_k^{p,r}$ and $\hat{\beta}_k^{n,r}$ coefficients are statistically different from each other at the 90 percent confidence level (from a Wald test). Columns denoted by (A), (B), (C), and (D) report estimates for different Fed's guidance regimes: respectively the open-ended (March/18/2009 to August/8/2011), calendar-based (August/9/2011 to May/22/2013), normalization (May/23/2013 to December/12/2015) and post-liftoff (December/13/2015 to May/30/2018). Bold numbers indicate statistical significance at the 90 percent confidence level, based on clustered standard errors (at the fund-level).
Sources: Bloomberg, EPFR and own calculations.

Figure A.7: Surprise outliers



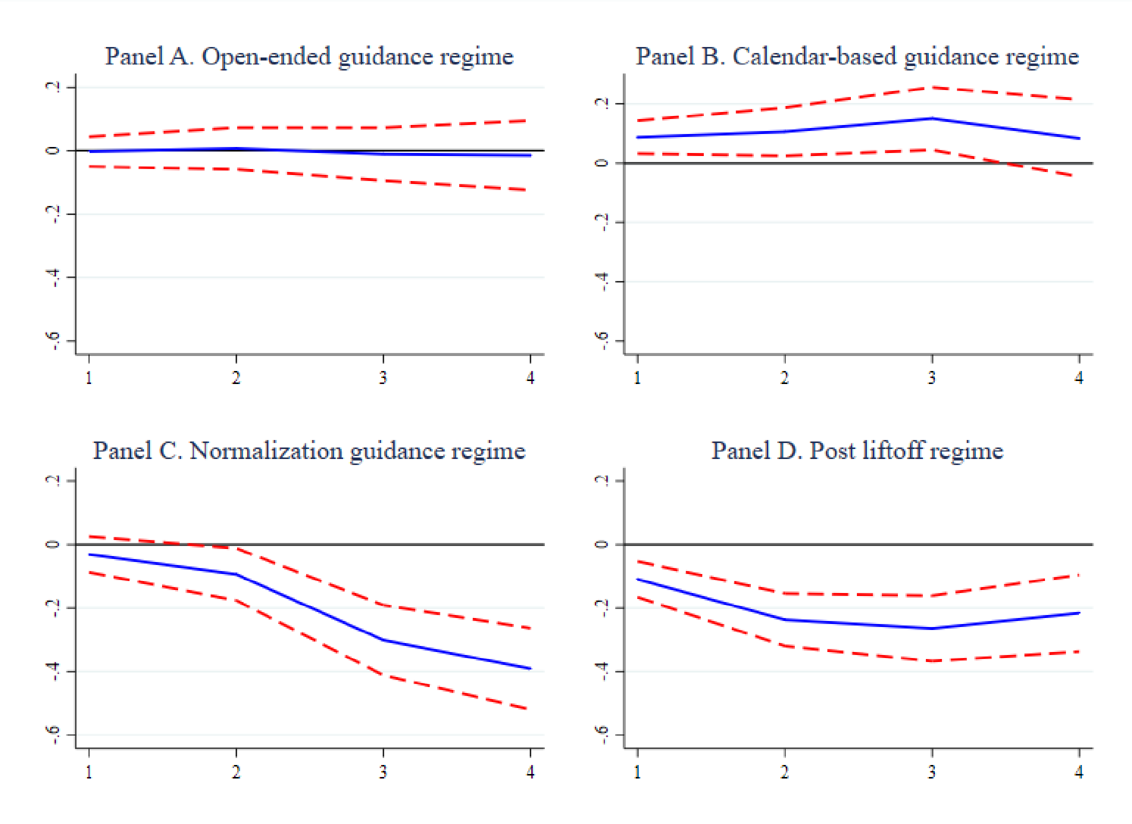
Notes: the Figure shows results from a robustness check on the baseline analysis regarding the presence of outliers in the non-farm payroll surprise series. Solid line represents the response to a one standard deviation surprise in the non-farm payroll release of allocations into investment funds. Each line is obtained estimating Equation 2.2 excluding from the sample one release at a time. The funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as defined by MSCI). Sensitivities are measured in percentage of beginning of period assets. The y-axis denotes the cumulated response. The x-axis denotes the horizon of the response (in weeks), with 1 being the week of the announcement. Red dotted lines are 90 percent confidence bands obtained using standard errors from the full sample estimation, clustered at the fund-level. The open-ended guidance, calendar-based guidance, normalization guidance and post-liftoff regimes range, respectively, from March/18/2009 to August/8/2011, from August/9/2011 to May/22/2013, from May/23/2013 to December/12/2015 and from December/13/2015 to May/30/2018.
Sources: Bloomberg, EPFR and own calculations.

Figure A.8: Variable used to measure capital flows



Notes: the Figure shows results from a robustness check on the baseline analysis regarding the variable used to measure portfolio capital flows. Solid line represents the response to a one standard deviation surprise in the non-farm payroll release of net portfolio capital flows to selected emerging market economies, in billion of U.S. dollar. Estimates are obtained from Equation A.1. The y-axis denotes the cumulated response. The x-axis denotes the horizon of the response (in weeks), with 1 being the week of the announcement. Red dotted lines are 90 percent confidence bands obtained using heteroskedasticity (white) robust standard errors. The open-ended guidance, calendar-based guidance, normalization guidance and post-liftoff regimes range, respectively, from March/18/2009 to August/8/2011, from August/9/2011 to May/22/2013, from May/23/2013 to December/12/2015 and from December/13/2015 to May/30/2018.
Sources: Bloomberg, Institute of International Finance and own calculations.

Figure A.9: Retail sales announcements



Notes: the Figure shows the estimated responses to U.S. retail sales announcements of allocations into investment funds. The funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as defined by MSCI). Sensitivities are measured in percentage of beginning of period assets. The y-axis denotes the cumulated effect of a one standard deviation surprise in the retail sale data release. The x-axis denotes the horizon of the response (in weeks), with 1 being the week of the announcement. The blue solid line shows the $\hat{\beta}_k^r$ coefficients obtained estimating Equation 2.2 replacing the non-farm payroll with the retail sales surprise series. Red dotted lines are 90 percent confidence bands obtained using respective standard errors, clustered at the fund-level. The open-ended guidance, calendar-based guidance, normalization guidance and post-liftoff regimes range, respectively, from March/18/2009 to August/8/2011, from August/9/2011 to May/22/2013, from May/23/2013 to December/12/2015 and from December/13/2015 to May/30/2018.
Sources: Bloomberg, EPFR and own calculations.

A.4 Robustness Checks and Other Extensions on the Role of ETFs

This appendix reports robustness checks on the differential response of flows to ETFs relative to mutual funds (Equation 2.3 in Section 2.5.3). The advantage of the diff-in-diff specification used to obtain the results is that it controls for all time varying unobserved factors as long as they impact flows into ETFs and mutual funds in the same way. Crucially, Converse, Levy-Yeyati, and Williams, 2018 show that flows into ETFs do not respond to local, country-specific, developments more than mutual funds. On the other hand, if ETFs responded more not only to employment releases but also to any kind of global shock, then it would still be possible that the results suffered from omitted variable bias. Although unlikely, since NFP surprises tend to behave as white noises (see Figure A.6, Appendix A.2), we check whether this is the case by estimating again Equation 2.3, this time including other macroeconomic surprises (also interacted with the ETF-dummy, $d^E_{i,t}$) to proxy for other shocks.

We consider other U.S. releases as well as the most important announcements stemming from the U.K., Germany, Japan and the Eurozone (all the news considered are listed in Table A.7, Appendix A.2). We estimate five additional specifications, each including all the releases from one of these countries or currency unions. The results from this sensitivity exercise, shown in Table A.7, are qualitatively similar and not statistically different from the baseline estimates.

Table A.7: Inclusion of other macroeconomic announcements

		(A)	(B)	(C)	(D)
1-week	Only NFP (baseline)	0.13	**0.26**	**-0.16**	-0.03
	Other U.S.	0.10	**0.24**	**-0.16**	-0.01
	U.K.	0.13	**0.25**	**-0.15**	-0.04
	Japan	0.12	**0.24**	**-0.15**	-0.03
	Germany	0.12	**0.24**	**-0.16**	-0.04
	Eurozone	**0.13**	**0.26**	**-0.18**	-0.02
2-week	Only NFP (baseline)	0.14	**0.35**	**-0.2**	0.09
	Other U.S.	0.13	**0.33**	**-0.21**	0.04
	U.K.	0.17	**0.35**	**-0.19**	0.08
	Japan	0.17	**0.34**	**-0.19**	0.08
	Germany	0.16	**0.31**	**-0.19**	0.07
	Eurozone	0.19	**0.38**	**-0.22**	0.1
3-week	Only NFP (baseline)	0.06	**0.35**	**-0.34**	0.17
	Other U.S.	0.04	0.3	**-0.37**	0.1
	U.K.	0.06	**0.33**	**-0.33**	0.17
	Japan	0.08	**0.33**	**-0.33**	0.15
	Germany	0.08	0.28	**-0.33**	0.15
	Eurozone	0.12	**0.41**	**-0.37**	0.17
4-week	Only NFP (baseline)	-0.04	**0.6**	**-0.28**	0.12
	Other U.S.	-0.04	**0.56**	**-0.3**	0.03
	U.K.	-0.07	**0.56**	**-0.26**	0.12
	Japan	0	**0.55**	**-0.26**	0.1
	Germany	-0.01	**0.5**	**-0.28**	0.1
	Eurozone	0.05	**0.6**	**-0.33**	0.11

Notes: the Table shows results from a robustness check on the difference-in-differences analysis regarding potential omitted variables biases. The numbers report the estimated differential responses to U.S. employment announcements of allocations into exchange-traded relative to mutual funds. The funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as defined by MSCI). Differential sensitivities are measured in percentage of beginning of period assets. Coefficients are estimated from Equation 2.3. Rows denoted by "Only NFP (baseline)" reports coefficients estimated leaving the Z_t vector empty. Rows denoted by "6-month fed fund future" report coefficients estimated including the 6-month ahead federal fund future rate in the Z_t vector. Rows denoted by "Other U.S." report coefficients estimated including other U.S. releases in the Z_t vector. Rows denoted by "U.K.", "Japan", "Germany" and "Eurozone" report coefficients estimated including respectively all releases from those countries in the Z_t vector. Refer to Table A.3 for a list of all releases considered. Columns (A), (B), (C), and (D) report estimates for the open-ended guidance, calendar-based guidance, normalization guidance and post-liftoff regimes respectively (see Sections 2.2.2 and 3.2.2 for a definition). Bold numbers indicate significance at the 90 percent confidence level, using clustered standard errors (at the investment fund-level).
Sources: Bloomberg, Investing.com, Datastream, EPFR and own calculations

Table A.8: Distinction by funds' size

		(A)	(B)	(C)	(D)
1-week	All funds (baseline)	0.03	**0.12**	**-0.09**	-0.03
	Small funds	-0.04	0.05	0.00	-0.01
	Medium-sized funds	0.04	**0.14**	-0.05	-0.06
	Large funds	0.02	**0.11**	**-0.19**	-0.01
2-week	All funds (baseline)	0	**0.22**	**-0.13**	-0.03
	Small funds	0.01	0.17	-0.03	-0.06
	Medium-sized funds	0.03	**0.20**	**-0.12**	-0.06
	Large funds	-0.04	**0.23**	**-0.2**	0.05
3-week	All funds (baseline)	**-0.10**	**0.35**	**-0.19**	**-0.07**
	Small funds	-0.27	**0.31**	-0.06	-0.13
	Medium-sized funds	-0.03	**0.34**	**-0.22**	**-0.14**
	Large funds	**-0.16**	**0.34**	**-0.2**	0.08
4-week	All funds (baseline)	-0.09	**0.44**	**-0.18**	-0.06
	Small funds	-0.20	**0.37**	-0.12	-0.07
	Medium-sized funds	-0.07	**0.47**	**-0.22**	**-0.17**
	Large funds	-0.12	**0.41**	-0.13	0.12

Notes: the Table shows results from an extension to the baseline analysis regarding non-linear effects depending on fund assets. The numbers report the estimated responses of allocations into investment funds to U.S. non-farm payroll data release. The funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as defined by MSCI). Sensitivities are measured in percentage of beginning of period assets. The leftmost column reports the horizon considered (k). The second leftmost column denotes the size of the funds considered. Rows denoted by "All funds (baseline)" report estimates for funds of all size, obtained from Equation 2.2. Rows denoted by "Small funds", "Medium-sized funds" and "Large funds" report respectively the $\hat{\beta}_k^{s,r}$, $\hat{\beta}_k^{m,r}$ and $\hat{\beta}_k^{l,r}$ coefficients obtained estimating the following regression: $\frac{\sum_{j=0}^{k} f_{i,t+j}}{a_{i,t}} = \sum_{r=1}^{4} \left(\alpha_k^r d_t^r + \left(\beta_k^{s,r} d_t^r d_i^s + \beta_k^{m,r} d_t^r d_i^m + \beta_k^{l,r} d_t^r d_i^l \right) x_t \right) + \sum_{l=1}^{12} \left(\varphi_k^l \frac{f_{i,t-l}}{a_{i,t-l}} + \vartheta_k^l \frac{n_{i,t-l}}{a_{i,t-l}} \right) + \gamma_i + \varepsilon_{i,t}$, where d_i^s and d_i^l are two dummy variables taking value one for funds in respectively the lower and upper quartile of the assets distribution and zero otherwise, d_i^m is a dummy variable taking value one when either d_i^s or d_i^l takes value zero, and the rest of the notation is as in Equation 2.2. A * (**) next to the $\hat{\beta}_k^{s,r}/\hat{\beta}_k^{m,r}/\hat{\beta}_k^{l,r}$ coefficient indicates that this is statistically different from one of the other two (both others) at the 90 percent confidence level, according to a Wald test for equal coefficients. Columns denoted by (A), (B), (C), and (D) report estimates for different Fed's guidance regimes: respectively the open-ended (March/18/2009 to August/8/2011), calendar-based (August/9/2011 to May/22/2013), normalization (May/23/2013 to December/12/2015) and post-liftoff (December/13/2015 to May/30/2018). Bold numbers indicate statistical significance at the 90 percent confidence level, based on clustered standard errors (at the fund-level).
Sources: Bloomberg, EPFR and own calculations.

A.5 Additional Results Using Country-level Data

Here we explore the presence of heterogeneities in the sensitivity to developments in the U.S. at the aggregate country level. We make use of the EPFR country flow dataset — covering bilateral flows from the U.S. to a panel of 77 EMs — to estimate country-specific flow responses to U.S. employment releases.

To carry out the analysis at the macro level we use the EPFR country flow dataset. This aggregates fund-level information to provide data on bilateral flows from the U.S. to 77 EMs. Tables A.11 and A.12 provide a list of the countries covered as well as relevant descriptive statistics, including the incidence of U.S. funds in terms of assets held as a share of the local GDP.

We start by constructing a variable measuring fund flows as a share of recipient country GDP and regressing it on the NFP surprise series interacted with the Fed's regime dummies. We do this operation separately for each country at a time to obtain country-specific estimates of flow sensitivities to U.S. employment releases. Figures A.10 and A.11 below plot these estimates for the calendar-based and normalization regimes respectively (results for the other regimes are available upon request). Each bar represents the 1-week fund flow response as a share of the local GDP (in basis points) to a one standard deviation NFP surprise. Stars denote statistical significance at the 90 percent confidence level.

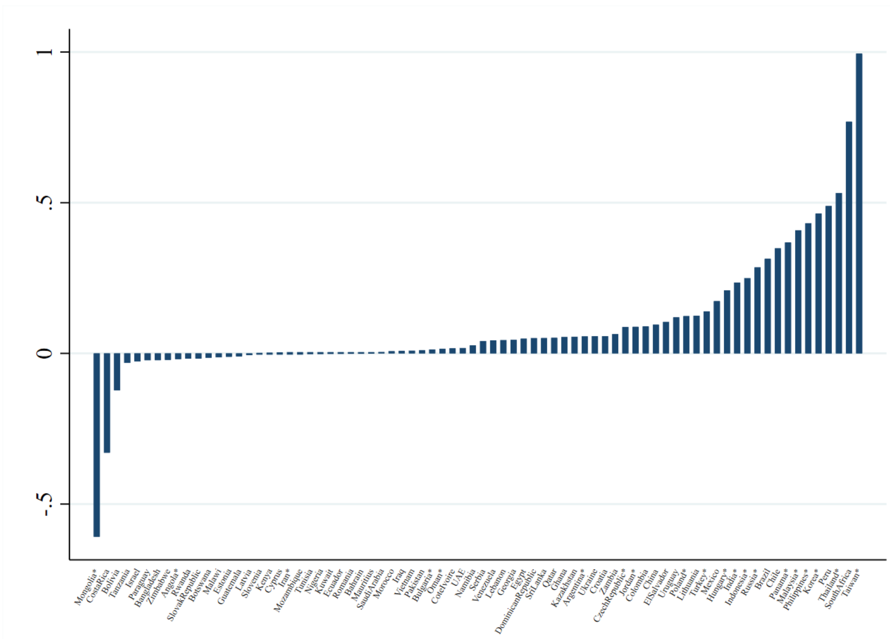
The coefficients display relatively large heterogeneity during the calendar-based regime, with a small group of countries even experiencing significant outflows. By contrast, the dispersion across country-specific responses is lower for the normalization regime. The higher heterogeneity observed for the calendar-based regime is confirmed even if the countries experiencing negative flows are dropped from the sample or when looking only at those displaying a statistically significant coefficient.

Not surprisingly, EMs where U.S. funds held on average a larger GDP share of assets seem to also have displayed a higher sensitivity to employment announcements. As an example, South Africa and Taiwan, the two countries with the largest asset share, were also the most sensitive. A standard deviation NFP surprise was associated with inflows (outflows) worth about 1 basis point of GDP during the calendar-based (normalization) regime. These are important figures, especially considering that they are calculated based on a fraction of all U.S. funds investing in EMs, possibly holding about half of the overall industry's assets.[4]

Next, we formally investigate whether country characteristics can account for the differences in sensitivity of flows across countries. We experiment with different sets of variables meant to proxy for a country's level of development, its macroeconomic management outcomes, its balance of payment and international investment positions, the bilateral trade and financial linkages with the US, as well as indicators of credit risk and institutional quality. A complete list, together with sources and country coverage is provided in Table A.13 in Appendix A.2.

[4] Funds reporting to EPFR at the weekly frequency accounted for just 42.5 percent of all industry assets at the end of 2017.

Figure A.10: Country flows sensitivities during the calendar-based guidance regime



Notes: the Figure shows the estimated 1-week response to a one standard deviation surprise in the U.S. non-farm payroll data release of bilateral gross portfolio capital flows from the U.S. to a set of emerging, frontier and other market economies (as defined by MSCI) during the August/9/2011 to May/22/2013 period. Sensitivities are measured in basis points of the recipient country GDP. Estimates are obtained from the following regression: $10000 * \frac{F_t}{Y_t} = \sum_{r=1}^{4} \beta^r d_t^r news_t + A_k D_t + B_k Z_t + \varepsilon_t$, where F_t is the country flow variable, Y_t is GDP, Z_t is a vector containing 12 lags of the dependent variable, and the rest of the notation is as in Equation 2.2. The y-axis denotes the magnitude of the estimated effect. Blue bars show the $\hat{\beta_k}$ coefficients. The x-axis reports the country concerned. A * indicates that the estimate is statistically different from zero (using heteroskedasticity robust standard errors).
Sources: Bloomberg, EPFR and own calculations.

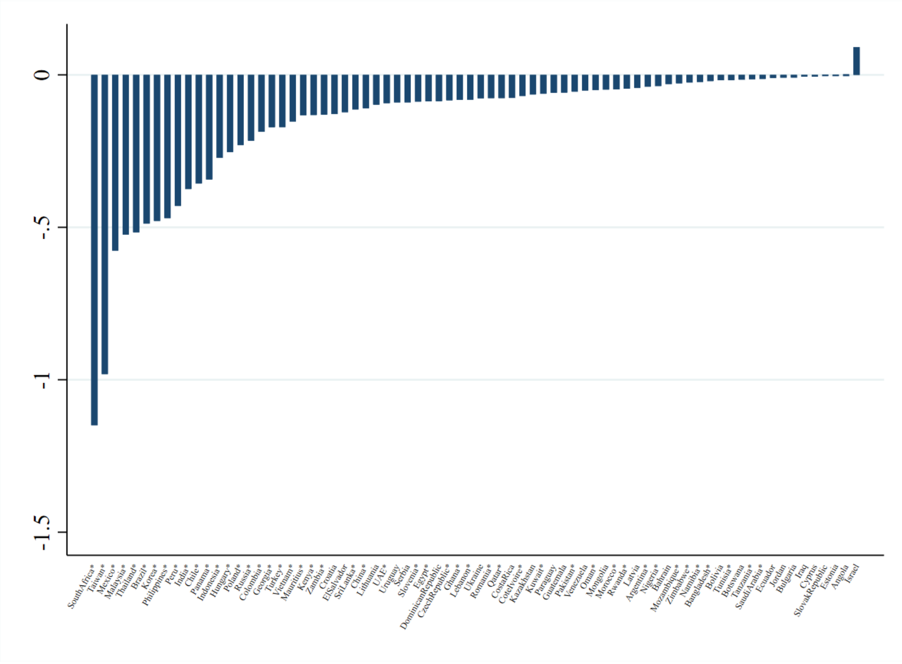
Since the variables we consider might be contemporaneously affected by capital flows, we instrument them with their lagged values to avoid reverse causality.[5] We also drop from the sample off-shore financial centers, which might have abnormal values in some of the variables we consider as pull factors, thus introducing noise in the analysis.[6]

We proceed estimating interaction models in which the NFP news variable is interacted with each country characteristic one at a time. A natural observation is that more financially developed countries are likely to display higher sensitivities. Therefore, we include the level of stock market capitalization as a share of GDP to

5 In practice, for each variable we take the value recorded the year preceding the beginning of a new forward guidance regime. Hence, for the open-ended regime, we use 2008 values. For the calendar-based, normalization and post-liftoff regimes, we use 2010, 2012 and 2015 values respectively.

6 Precisely, we exclude the countries classified by the International Monetary Fund as offshore financial centers. These are Bahrain, Botswana, Cyprus, Mauritius, Panama and Uruguay.

Figure A.11: Country flows sensitivities during the normalization guidance regime



Notes: the Figure shows the estimated 1-week response to a one standard deviation surprise in the U.S. non-farm payroll data release of bilateral gross portfolio capital flows from the U.S. to a set of emerging, frontier and other market economies (as defined by MSCI) during the May/23/2013 to December/13/2015 period. Sensitivities are measured in basis points of the recipient country GDP. Estimates are obtained from the following regression: $10000 * \frac{F_t}{Y_t} = \sum_{r=1}^{4} \beta^r d_t^r news_t + A_k D_t + B_k Z_t + \varepsilon_t$, where F_t is the country flow variable, Y_t is GDP, Z_t is a vector containing 12 lags of the dependent variable, and the rest of the notation is as in Equation 2.2. The y-axis denotes the magnitude of the estimated effect. Blue bars show the $\hat{\beta_k}$ coefficients. The x-axis reports the country concerned. A * indicates that the estimate is statistically different from zero (using heteroskedasticity robust standard errors).
Sources: Bloomberg, EPFR and own calculations.

proxy for financial development.[7] In practice, we estimate the following specification:

$$10000 * \frac{F_{j,t}}{Y_{j,t}} = \sum_{r=1}^{4} \left(\beta_x^r d_t^r news_t cap_j^r + \kappa_x^r d_t^r news_t x_j^r + \varphi_x d_t^r cap_j^r + \phi_x d_t^r x_j^r \right) + \\ + A_x D_t + B_x Z_t + \tau_t \varepsilon_{j,t} \tag{A.2}$$

where $F_{j,t}$ are total flows for country j at time t; $Y_{j,t}$ is country j's GDP; cap_j^r is market capitalization as a share of GDP in regime r; x_j^r is the country characteristic considered, also in regime r; τ_t are time fixed effects; and the rest of the notation is as in Equation 2.2.[8] The term $\frac{F_{j,t}}{Y_{j,t}}$ is multiplied by 10000 to express basis point responses. The estimation is carried out through OLS. Standard errors are clustered at the country level.

[7] The drawback is that due to limited data availability 12 countries are lost.

[8] The NFP surprise variable does not enter the regression on its own as it is absorbed by the time fixed effects.

In the interest of brevity, Table A.9 only reports the $\hat{\beta}_x^r$ and $\hat{\kappa}_x^r$ coefficients estimated for the calendar-based and normalization regimes. As expected, the stock market capitalization is always significant. It is estimated to be positive (negative) for the calendar-based (normalization) regime, meaning that inflows (outflows) following positive news were concentrated in countries with deeper financial markets.

Strikingly, only two other characteristics are significantly associated with country flows. These are the budget balance and the net portfolio position (NPP).[9] The former enters with a negative sign during the calendar-based regime, suggesting that capital inflows after positive news might have had a speculative nature (it is instead not significant during the normalization regime). This intuition is corroborated if considering equity flows only. Then also the current account balance is negative and significant (results available upon request). Turning to the NPP, this essentially measures how much, in net terms, a country's financial assets are owned by foreigners. The associated coefficient is negative (positive) for the calendar-based (normalization) regime. To better understand what drives this result, we decompose NPP into its equity and debt components and estimate Equation A.5 again using these two variables and allowing them to enter both separately and jointly.

Table A.10 below shows the results. These indicate that portfolio equity (debt) was more relevant during the calendar-based (normalization) guidance regime.[10] Countries with a higher share of foreign-owned equity might be better integrated in the global financial system. This would explain why they received more inflows during the calendar-based regime. Another possibility is that agents might have some degree of habit persistence in their investment decisions, thus tending to invest more in countries in which they already have an exposure. Finally, the net equity position might be capturing some other characteristics regarding the attractiveness of a country as investment destination that other variables fail to properly account for.

Turning to the normalization regime, EMs with more positive NPP debt positions experienced less outflows following better than expected U.S. employment news. The estimates suggest that a country with a NPP debt position equal to one standard deviation more than the average experienced less outflows for an amount equal to about one standard deviation of the distribution of country-specific flow sensitivities (Figure A.11). The importance of NPP debt is easily rationalized. Contrary to equity, debt liabilities need to be rolled over as they mature. Hence, countries with higher external debts are inherently more vulnerable to foreign capital withdrawals, which is indeed what the estimates in Table A.10 suggest. This is confirmed if considering debt flows only. Countries with higher external debts as a share of GDP and a higher stock of international bonds (that is, not issued on the domestic market)

9 The net portfolio position measures domestic holdings of foreign bond and equity assets minus foreign holdings of domestic equity and bond assets as a share of the domestic country GDP.

10 Neither the NPP equity nor the NPP debt are significant when they enter the regression jointly for the calendar-based regime. However, the NPP equity has a much larger coefficient (in absolute value) and a lower p-value (this is 0.18 for NPP equity and 0.48 for NPP debt).

Table A.9: Local pull factors

	Calendar-based		Normalization			
	$news * c$	$news * x$	$news * c$	$news * x$	R^2	**Obs.**
Trade openness	**0.33**	3.54	**-0.46**	-1.62	0.55	26051
KA openness	**0.28**	-0.01	**-0.41**	0.01	0.56	24647
GDP	**0.31**	0.01	**-0.43**	-0.02	0.55	26051
Public debt	**0.33**	-0.01	**-0.46**	0.01	0.57	19933
Budget	**0.35**	**-0.67**	**-0.46**	0.25	0.55	23115
Inflation	**0.34**	-0.46	**-0.49**	-0.13	0.54	22891
CA balance	**0.34**	-0.31	**-0.46**	0.14	0.55	25699
Reserves	**0.28**	-0.05	**-0.43**	0.12	0.56	21772
External debt	**0.28**	-0.01	**-0.42**	-0.09	0.56	25583
NIIP	**0.32**	0.02	**-0.46**	0.01	0.55	22240
NPP	**0.26**	**-0.3**	**-0.38**	**0.39**	0.56	21420
Credit risk	**0.35**	0.1	**-0.47**	-0.08	0.55	24493
Financial links	**0.33**	0.01	**-0.42**	-0.15	0.55	24351
Trade links	**0.33**	0.14	**-0.45**	-0.24	0.55	25205
Rule of law	**0.34**	-0.01	**-0.46**	0.02	0.55	26051

Notes: the Table shows the relationship between country-specific fund flow sensitivities to U.S. employment announcements and country characteristics. Estimates are obtained from Equation A.5. The leftmost column lists the country characteristic considered, of which the estimated coefficient is reported in the column denoted by "κ". "Trade openness" is the sum of imports and exports as share of GDP. "KA openness" is an index measuring the degree of capital account liberalization. "GDP" is the log of GDP. "Public debt" is the the debt of the general government as a share of GDP. "Budget balance" is the difference between government revenues and expenditures as a share of GDP. "Inflation" is the yearly change in the CPI index. "CA balance" is the current account balance as a share of GDP. "Foreign reserves" are foreign currency holdings as a share of GDP. "External debt" is debt held by the private-sector foreign agents as share of GDP. "NIIP" (net international investment position) is the difference between domestic holdings of foreign assets and foreign holdings of domestic assets, as a share of GDP. "NPP" (net portfolio position) the difference between domestic holdings of foreign portfolio assets and foreign holdings of domestic portfolio assets, as a share of GDP. "Credit risk" is the inverse of the sovereign credit rating. "Financial links" is the sum of domestic holdings of U.S. portfolio securities and U.S. holdings of the country portfolio securities. "Trade links" is the sum of imports from and exports to the U.S. as share of GDP. "Rule of law" is an index capturing perceptions of the extent to which agents have confidence in and abide by the rules of society. The column "β" reports estimates for the level of stock market capitalization as a share of GDP. The columns "R^2" and "Obs." report the explanatory power of the regression and the number of observations respectively. The multicolumns "Calendar-based regime" and "Normalization regime" report estimates for the period August/9/2011 to May/22/2013 and May/23/2013 to December/12/2015 respectively. Bold numbers indicate statistical significance at the 90 percent confidence level, based on clustered standard errors.
Sources: Bloomberg, EPFR, own calculations and others (listed in Table A.13).

Table A.10: Portfolio positions

	Calendar-based			Normalization		
$news * cap$	**0.29**	**0.27**	**0.29**	**-0.41**	**-0.40**	**-0.40**
$news * NPPdebt$	**-0.30**		-0.12	**0.50**		**0.44**
$news * NPPequity$		**-0.39**	-0.40		**0.36**	0.24
R^2	0.58	0.57	0.58	0.58	0.57	0.58
Obs.	18804	18885	17481	18804	18885	17481

Notes: the Table shows the relationship between country-specific fund flow sensitivities to U.S. employment announcements and country characteristics. Estimates are obtained from Equation A.5. The leftmost column lists the variable considered. "constant" is a constant term. "cap" is value of the domestic stock market. "NPP debt" is the difference between domestic holdings of foreign bonds and foreign holdings of domestic bonds. "NPP equity" is the difference between domestic holdings of foreign equity and foreign holdings of domestic equity. The variables are expressed as a share of the domestic country GDP. Bold numbers indicate statistical significance at the 90 percent confidence level, based on clustered standard errors. The columns "R^2" and "Obs." report the explanatory power of the regression and the number of observations respectively. The multicolumns "Calendar-based" and "Normalization" report estimates for the period August/9/2011 to May/22/2013 and May/23/2013 to December/12/2015 respectively.
Sources: Bloomberg, EPFR, IMF International Financial Statistics, Haver Analytics and own calculations.

experienced significantly more outflows following positive surprises (results available upon request).[11]

This appendix explored the presence of country-heterogeneities in the response of fund flows following U.S. employment surprises and sought to find potential determinants. The results uncovered a relatively large dispersion during the calendar-based regime. Inflows were generally concentrated in countries with a larger stock market capitalization. However, conditioning on the same level of capitalization, inflows were higher in countries running larger budget and current account deficits, which suggest that such flows had a speculative nature. During the normalization regime outflows were instead higher in countries with worse portfolio debt positions.

[11] Other factors positively associated with debt outflows following positive news during the normalization regime were a country's current account deficit and sovereign credit risk. Outflows were also concentrated in smaller countries and countries with less financial linkages relative to the US. However, when all the variables enter the specification jointly, only the net portfolio position and the external debt ratio remain statistically significant.

Table A.11: Mean assets and other characteristics by recipient country (1)

	assets	% equity	% ETF	% of GDP
Angola	0.1	/	27.2	0.1
Argentina	4.0	68.5	29.1	0.7
Bahrain	0.1	6.5	27.7	0.2
Bangladesh	0.7	98.3	39.5	0.3
Bolivia	0.0	/	30.1	0.1
Botswana	0.0	100.0	51.8	0.2
Brazil	50.9	88.9	44.6	2.3
Bulgaria	0.0	14.3	25.0	0.1
Chile	4.9	81.9	40.6	2.0
China	84.5	98.2	42.4	0.9
Colombia	3.1	42.8	31.9	1.0
Costa Rica	0.4	/	26.1	0.6
Cote D'Ivoire	0.2	0.2	22.7	0.5
Croatia	0.5	3.7	21.5	0.9
Cyprus	0.0	59.7	23.3	0.1
Czech Republic	1.4	87.6	34.1	0.7
Dominican Republic	0.4	/	21.1	0.6
Ecuador	0.2	0.7	22.7	0.2
Egypt	1.6	87.4	39.9	0.5
El Salvador	0.2	/	22.4	0.9
Estonia	0.0	95.6	26.6	0.2
Georgia	0.2	67.3	34.8	1.1
Ghana	0.3	39.4	22.6	0.7
Guatemala	0.1	/	25.3	0.2
Hungary	3.1	40.5	26.8	2.3
India	36.2	97.7	42.2	1.8
Indonesia	13.0	73.1	35.5	1.5
Iran	0.0	37.2	42.8	0.0
Iraq	0.1	/	21.2	0.1
Israel	6.6	93.6	27.8	2.3
Jordan	0.2	106.4	35.3	0.4
Kazakhstan	1.2	32.7	26.5	0.7
Kenya	0.5	89.1	46.1	0.8
Korea	49.4	95.3	39.4	3.8
Kuwait	0.4	94.8	44.2	0.4
Latvia	0.2	/	23.6	0.6
Lebanon	0.2	27.4	26.9	0.5
Lithuania	0.5	2.1	18.4	1.1
Malawi	0.0	100.0	89.8	0.0

Notes: the Table provides information on the geographical distribution of investment funds assets. Funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as classified by MSCI). The period considered goes from March 2009 to May 2018. The leftmost column reports the country of investment. The column denoted by "assets" reports the mean U.S. $ amount invested in the particular country (in billion). The column denoted by "% equity" reports the share of equity assets. The column denoted by "% ETF" reports the share of assets held by ETFs. The column denoted by "% of GDP" reports the value of assets in terms of the country's GDP. Notice that Table A.12 report statistics for the remaining countries.
Sources: EPFR, IMF World Economic Outlook and own calculations.

Table A.12: Mean assets and other characteristics by recipient country (2)

	assets	% equity	% ETF	% of GDP
Malaysia	8.7	78.5	38.7	2.9
Mauritius	0.0	86.8	47.0	0.3
Mexico	22.3	70.5	36.6	1.9
Mongolia	0.1	/	30.5	0.6
Morocco	0.3	58.5	35.2	0.3
Mozambique	0.0	/	31.1	0.1
Myanmar	0.0	100.0	39.8	0.0
Namibia	0.0	2.6	27.7	0.3
Nigeria	1.1	78.9	37.9	0.2
Oman	0.2	72.9	39.2	0.3
Pakistan	1.1	90.1	36.3	0.4
Panama	1.0	52.4	28.7	2.2
Paraguay	0.2	/	28.4	0.5
Peru	3.6	59.5	36.3	1.9
Philippines	4.8	80.6	35.9	1.8
Poland	6.3	49.2	28.4	1.3
Qatar	1.0	43.1	29.9	0.6
Romania	0.8	37.6	30.5	0.4
Russia	24.6	83.0	40.2	1.4
Rwanda	0.0	9.3	41.1	0.2
Saudi Arabia	0.5	80.4	38.9	0.1
Serbia	0.3	6.1	19.1	0.7
Slovak Republic	0.0	/	28.7	0.0
Slovenia	0.3	37.4	23.4	0.6
South Africa	22.3	88.0	37.2	6.4
Sri Lanka	0.8	43.0	26.0	1.1
Taiwan	34.1	99.9	41.8	6.7
Tanzania	0.1	91.7	33.7	0.1
Thailand	10.8	93.0	38.6	2.7
Tunisia	0.0	23.8	36.4	0.1
Turkey	9.8	74.4	37.3	1.1
Ukraine	1.0	16.4	22.7	0.8
UAE	1.8	63.7	29.6	0.5
Uruguay	0.6	/	20.0	1.1
Venezuela	1.1	0.6	20.8	0.4
Vietnam	1.2	91.0	58.6	0.6
Zambia	0.2	42.5	36.8	0.6
Zimbabwe	0.0	100.0	54.1	0.1
Total	425.6	87.0	40.1	1.5

Notes: the Table provides information on the geographical distribution of investment funds assets. Funds considered are legally domiciled in the U.S. and invest in emerging, frontier and other market economies (as classified by MSCI). The period considered goes from March 2009 to May 2018. The leftmost column reports the country of investment. The column denoted by "assets" reports the mean $ amount invested in the particular country (in billion). The column denoted by "% equity" reports the share of equity assets. The column denoted by "% ETF" reports the share of assets held by ETFs. The column denoted by "% of GDP" reports the value of assets in terms of the country's GDP.
Sources: EPFR, IMF World Economic Outlook and own calculations.

Table A.13: Country characteristics

Variable	**complied by**	**sourced from**	**#**
Stock market (% of GDP)	WFE/NS/WEO	Haver/IMF	63
Trade openess (% of GDP)	NS	PWT	77
Financial openness index	Chinn and Ito, 2008	Haver	72
GDP (log)	NS/WEO	Haver/IMF	77
Government debt (% of GDP)	NS/WEO	Haver/IMF	56
Government balance (% of GDP)	NS/WEO	Haver/IMF	65
Inflation (%)	NS/WEO	Haver/IMF	67
Current account (% of GDP)	NS/WEO	Haver/IMF	72
Foreign reserves (% of GDP)	IFS	IMF	65
External debt (% of GDP)	IDS	BIS	76
NIIP (% of GDP)	IFS	IMF	66
NPP (% of GDP)	IFS	IMF	62
NPP - equity (% of GDP)	IFS	IMF	57
NPP - debt (% of GDP)	IFS	IMF	49
Credit risk (index)	OE	Datastream	68
Financial links U.S. (% of GDP)	IFS	IMF	74
Trade links U.S. (% of GDP)	NS	Haver	70
Rule of law (index)	WB/NRGI/Brookings	Haver	77

Notes: the Table describes the country characteristics considered in Appendix A.5. Column "#" indicates the number of countries covered. In the leftmost column, "NIP" and "NPP" stand respectively for net international investment position and net portfolio position. In the second leftmost column, "WFE", "NS", "WEO, "IFS", "IDS" "WB" and "NRGI" stand respectively for World Federation of Exchange, national sources, World Economic Outlook, International Finance Statistics, International Debt Statistics, World Bank and National Resource Governance Institute. In the third leftmost column "IMF", "PWT" and "BIS" stand respectively for International Monetary Fund, Penn World Tables and Bank of International Settlements.

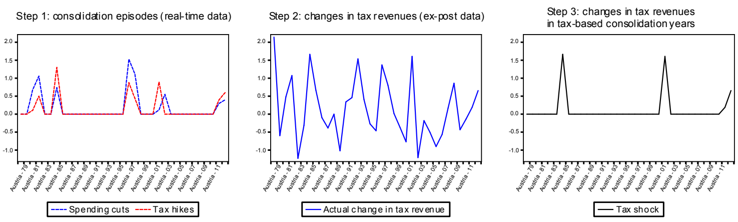
Appendix B

Appendix to Chapter 3

B.1 Identification Strategy

Figure B.1 illustrates how we construct our tax shocks, taking the case of Austria as an example. We start from the action-based consolidation datasets compiled by Devries et al., 2011 and Alesina et al., 2015 (step 1). Next, we consider changes in total tax revenues as recorded in the OECD Revenue Statistics Database (step 2). Finally, we select years in which tax hikes, as identified through the narrative approach, were larger than spending cuts (i.e. tax-based consolidation years). *Ex-post* realized changes of tax revenues during those years constitute our shocks (step 3).

Figure B.1: Construction of the tax shocks



Source: Devries et al., 2011, Alesina et al., 2015, OECD Revenue Statistics database and authors' own calculations.

Rescaling the narratively identified consolidation episodes by the *ex-post* realized value of changes in tax revenues could introduce endogeneity since tax revenues respond to contemporaneous change in economic activity. If this feedback from GDP to tax revenues were to be large, our identifying assumption that the tax shock variable is not contemporaneously affected by GDP might be wrong, thus leading to biased estimates. Following Guajardo, Leigh, and Pescatori, 2014 we test whether our fiscal consolidation shock is uncorrelated with unexpected movements of output. We construct two measures of economic "news". The first measure is based on the difference between the *ex-post* real GDP growth for year t and the IMF forecasts of real GDP growth for year t made in year *t-1*. The second measure is the one used by Guajardo, Leigh, and Pescatori, 2014 and defined as the difference between the IMF forecast of real GDP growth for year t made in year t and the IMF forecasts of real GDP growth for year t made in year *t-1*. Data on GDP forecasts comes from the Fall editions of the IMF WEO over the post-1990 period and are taken from Guajardo, Leigh, and Pescatori, 2014.[1] We then estimate the following equation:

$$X_{i,t} = \alpha_i + \tau_t + \beta News_{i,t} + \epsilon_{i,t}$$

where $X_{i,t}$ is either our *ex-post* tax-based consolidation variable or the original real-time narrative measure of Devries et al., 2011 and Alesina, Favero, and Giavazzi, 2015, α_i and τ_t are respectively country and time fixed effects, $News_{i,t}$ is either the the *ex-post* or the real-time economic "news" measures, and $\epsilon_{i,t}$ is an error term.

Table B.1 shows the results. In all cases the coefficient on the news variable is not statistically significantly different from 0. Overall these tests are reassuring and indicate that our *ex-post* measures are not subject to more severe endogeneity problems than the Devries et al., 2011 real-time narrative measures.

Table B.1: Orthogonality check of tax-based consolidations to GDP news

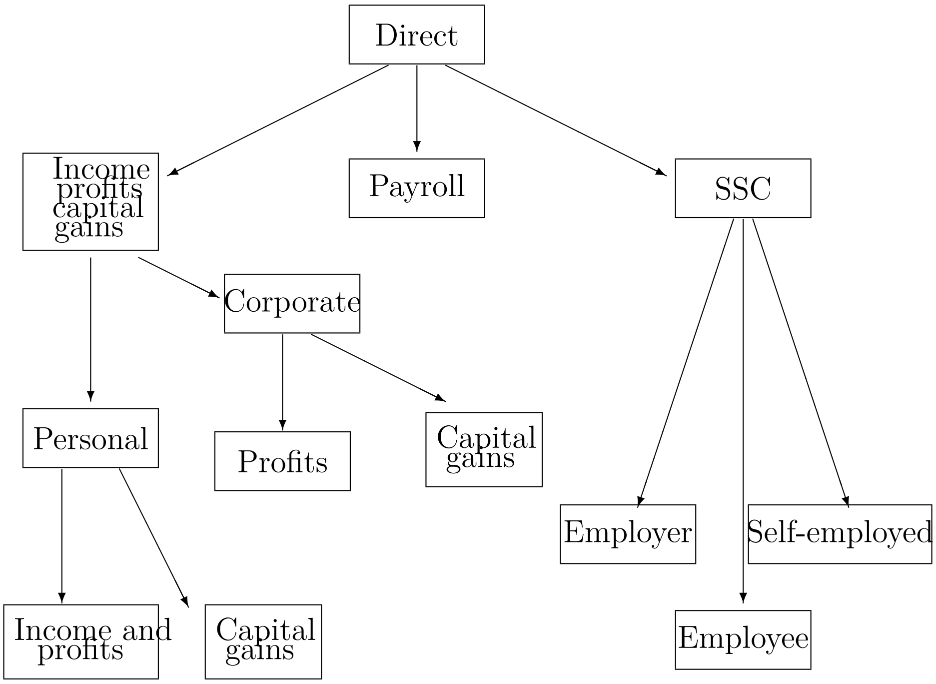
Measure of tax changes	Estimate	s.e.	R-squared	Obs
a) Ex-post economic "news"				
Ex-post measure	0.19	(1.34)	0.06	304
Real-time measure (Devries et al., 2011)	-0.05	(1.24)	0.07	304
b) Real-time economic "news"				
Ex-post measure	0.21	(1.05)	0.05	304
Real-time measure (Devries et al., 2011)	0.23	(1.60)	0.08	304

Notes: Estimates obtained from estimating the following equation: $X_{i,t} = \alpha_i + \tau_t + \beta News_{i,t} + \epsilon_{i,t}$, where $X_{i,t}$ is the either the *ex-post* or the real-time tax-based consolidation variable and $News_{i,t}$ is either the *ex-post* (Panel a) or the real-time (Panel b) economic news variable. Data on forecasted GDP is taken from Guajardo, Leigh, and Pescatori, 2014. See the text for details.

[1] We thank an anonymous referee for suggesting this orthogonality check.

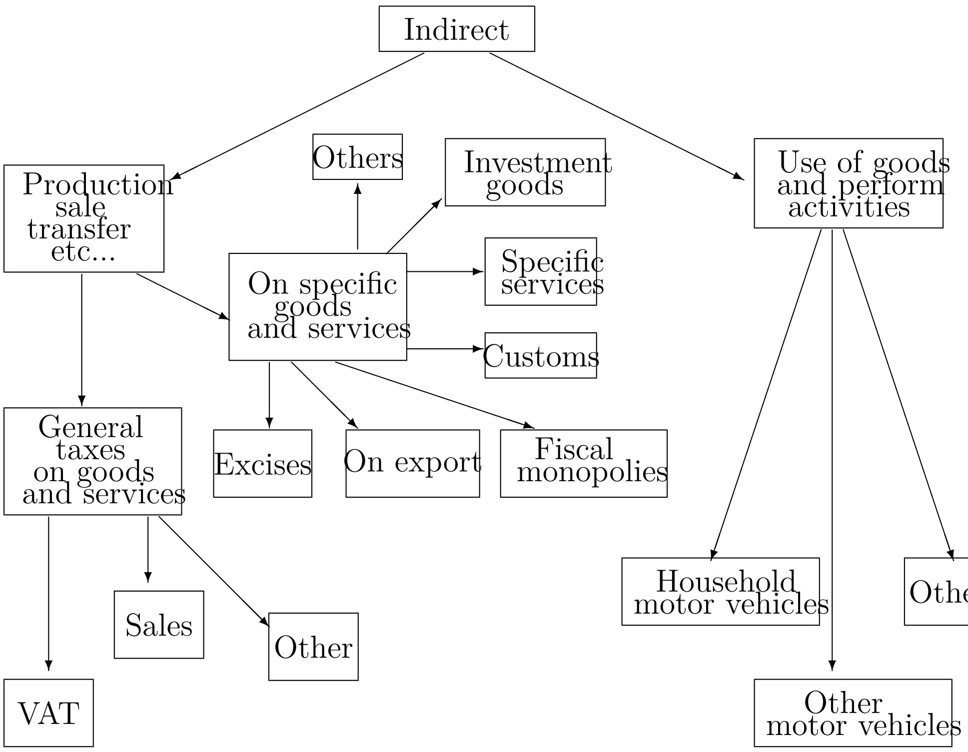
B.2 Breakdown of Tax Instruments

Figure B.2: Direct taxes



Notes: Direct taxes are generally defined as to include (i) taxes on income, profits and capital gains, (ii) social security contributions and (iii) taxes on payroll and workforce. The breakdown of such tax categories presented above follows the OECD classification method. For more information refer to the OECD Interpretative guide and methodology.

Figure B.3: Indirect taxes



Notes: Indirect taxes are generally defined as taxes on goods and services. The breakdown presented above follows the OECD classification method. For more information refer to the OECD Interpretative guide and methodology.

B.3 Robustness Checks on Overall Effects of Tax-Based Consolidations

To verify the validity of our baseline results, presented in Section 3 we carry out a number of robustness checks. We present the tables with the results below. To ease comparison, in Panel (a) of each table we report estimates from our baseline specification.

We start by assessing the response of the economy to different types of consolidation episodes. Panels (b) and (c) of Table B.2 report the estimated responses to a 1% of GDP increase in tax revenues during, respectively, any consolidation year and spending-based consolidation episodes. The estimated coefficients suggest that the contemporaneous presence of spending cuts and tax hikes might confound the results concerning the effects of tax shocks on the economy. This is why we exclusively consider tax-based consolidation years in our baseline. Second, we address the potential concern that anticipation effects may bias our results. To this end, we use information contained in Alesina et al., 2015 and Alesina, Favero, and Giavazzi, 2015 to identify unanticipated tax-based consolidations (i.e. decided at year t for implementation in the same year) and we estimate relevant IRFs. The results reported in Panel (d) are qualitatively similar to those obtained in our baseline estimation.[2]

Next, we check whether our results are robust to the use of alternative tax shock variables (Table B.3). To control for potential endogeneity in the response of tax revenues to the business cycle, we estimate the model using cyclically-adjusted revenues.[3] We find results very similar to our baseline (Panel (b)). As a further check, and in order to facilitate comparison between our results and those of Woo et al., 2017 and Agnello and Sousa, 2014, we also estimate the model employing the original real-time data collected by Devries et al., 2011 and Alesina et al., 2015.

Next, we observe that some tax-based consolidations spanned over several consecutive years. This could potentially introduce a bias in the estimation. To understand why, consider a consolidation cycle lasting from period t to $t+1$. If the consolidation of period $t+1$ was decided by the government in the same period after observing the outcome of the consolidation at t, our results would be biased due to reverse causality.

2 Although not statistically different from our baseline, the response of the disposable Gini index becomes insignificant. This might be due the fact that the sample of consolidation episodes is greatly reduced, from 73 to 43.

3 Cyclically-adjusted tax revenues are computed according to the following formula (OECD Economic Outlook):

$$t_t^{ad,i} = t_t^i (y_t^n / y_t)^{\varepsilon^i} \tag{B.1}$$

where $t_t^{ad,i}$ and t_t^i respectively stand for cyclically and not cyclically-adjusted tax revenues stemming from tax instrument i; y_t^n is potential per capita output (derived from the IMF output gap measure); y_t is real per capita output and ε_i refer to the elasticity of tax instrument i. Elasticities are taken from the OECD Economic Outlook database inventories (OECD Economic Outlook).

To circumvent this problem, Ball et al., 2013 employ a dummy shock taking value 1 in the first year of the tax-based consolidation cycle and 0 otherwise. However, their approach has two main drawbacks. First, it treats all consolidation cycles as if they were equal in size and length. Second, it unnecessarily sacrifices a large number of observations. An alternative approach is to exclude all consolidation years that might suffer from reverse causality issues. To do so, we use information contained in Alesina, Favero, and Giavazzi, 2015 and Alesina et al., 2015 in order to construct a shock variable which is the same as in our baseline with the exception that it takes value 0 in all years of tax-based consolidations that were (i) unanticipated (i.e. decided at year t for implementation in the same year), (ii) part of a multi-year consolidation cycle, and (iii) not the first year of such cycle.[4] Next, we estimate the model using both a dummy variable à la Ball et al., 2013 and our alternative shock variable. We show results in Table B.4. In both cases the IRFs are qualitatively similar to our baseline.[5] We conclude that our baseline results do not suffer from a reverse causality bias.

In Tables B.5 and B.6 we show that our baseline results are robust to the inclusion of different deterministic components, the use of different lag specifications, and the use of local projections as an alternative estimation method.[6] When estimating IRFs from local projections (Table B.6), we employ both our standard shock variable

4 The shock variable is constructed according to the following formula: $X^j_{i,t} = d^1_t(1 - d^u_t(1 - d^f_t))\Delta t^j_{i,t}$ where d^u_t and d^f_t are two dummy variables: d^u_t takes value 1 in every year of unanticipated tax-based consolidations and 0 otherwise, while d^f_t takes value 1 in each first year of a tax-based consolidation cycle and 0 otherwise.

5 As expected, since our baseline shock variable and the dummy à la Ball et al., 2013 measure different things, some quantitative differences emerge when using the latter. However, results remain qualitatively similar.

6 To estimate IRFs directly from local projections, we employ the original specification proposed by Jordà, 2005 and augment it with the correction proposed by Teulings and Zubanov, 2014. Omitting such correction would leave the model misspecified and thus introduce a bias. To understand this point, consider a country i featuring only one fiscal policy shock at $t = 2$. When estimating an IRF(k) using the specification proposed by Jordà, 2005, the estimator for $k = 1$ will be biased, since for $t = 1$ $y^j_{i,t+2}$ is already affected by the shock, although this does not appear among the regressors. Hence, after including the Teulings and Zubanov, 2014 correction, we estimate the following equation:

$$y_{i,t+k} = c + \sum_{l=1}^{2} \beta^k_l y_{i,t-l} + \gamma^{j,k} X^j_{i,t} + \sum_{l=1}^{k} \theta^k_l X^j_{i,t+l} + \alpha_i + \delta_t + \sum_{l=1}^{2} \varphi^k_l Z^j_{i,t-l} + \tau_{it} + \epsilon_{i,t} \quad \text{(B.2)}$$

where $y_{i,t}$ denotes either the log of real GDP per capita, the Gini coefficient, or the unemployment and the labor force participation rate; $X_{i,t+l}$ denotes the shock variable; the term $\sum_{l=1}^{k} \theta^{j,k}_l X_{i,t+l}$ represents the Teulings and Zubanov, 2014 correction; $Z_{i,t-l}$ is a vector of the other endogenous variables used as control variables; as in the PVAR specification. Finally, α_i, δ_t, τ_{it} denote, respectively, country-fixed effects, time-fixed effects and country-specific trends, and $k = 0, .., 10$ is the time horizon. To obtain the IRFs and construct confidence bands, we use respectively the estimated $\gamma^{j,k}$ coefficients and $\pm$ 1.645 cross-section heteroskedasticity robust standard errors.

(Panels (b) and (c) for results with and without control variables) and the dummy à la Ball et al., 2013 (Panel (d)), so as to directly compare their results with ours. In all cases, the IRFs obtained using local projections are qualitatively similar to those generated by the PVAR methodology.

Furthermore, in Table B.7 we show that our results are not driven by particular groups of countries, time periods, or type of shocks. More specifically, we repeat the estimation excluding from the sample, in turn, (i) the period following the global financial crisis (2008-2012), (ii) non-EU countries, (iii) shocks occurring during, or 1 or 2 years after, systemic banking crises, and (iv) shock outliers, i.e. those above the 97.5^{th} percentile or below the 2.5^{th} percentile. We also run the baseline regression by dropping one country at a time (Figure B.4).

Next, we show that our results are robust to the selection of alternative endogenous variables and to the inclusion of several control variables. First, we estimate the model employing GDP per hour worked, average hours worked by employed individuals and the employment rate, instead of the GDP, unemployment and participation rates (Table B.8). This exercise confirms the validity of our baseline results and suggests that the observed decline in real economic activity following tax-based consolidations is due to a drop in productivity. Second, we verify that our results are not biased by the omission of variables commonly used in the literature as a proxy for: (i) the degree of a country openness (import plus exports as a percentage of GDP), (ii) the progressivity of the tax system (the ratio of direct-to-indirect tax revenues), and (iii) other macroeconomic conditions (Tables B.9 and B.10).

Finally, since an issue when using the Gini index in cross-country studies is data comparability, we check whether our baseline results are robust to different measures of inequality. As alternative inequality measures, we employ the shares of income belonging to the richest 0.01%, 0.01-1%, and 1-10% individuals, which have been shown by Leigh, 2007 to be good proxies of inequality across the income distribution. Additionally, we also use the income ratios of individuals in the 90^{th}, 50^{th} and 10^{th} percentiles of the income distribution. However, these alternative measures of inequality are not without caveats. First, they are based on market rather than disposable incomes. Second, due to data availability, the sample size is reduced by 46.3% and and 35.5% when using top income shares and the income ratios respectively. Bearing in mind these limitations, we present the main results in Tables B.11-B.12 for top income shares and Table B.13 for the income ratios.[7] Although in some cases they are not significant, the new estimates have the expected sign and thus broadly confirm our baseline result that tax-based consolidations reduce income inequality.[8]

[7] In Table B.12 we also show results using top income shares and excluding Spain from the sample, since this country partially drives some of the results.

[8] We also notice that when we use top income shares, inequality seems to decrease faster than when we use the Gini index. This might be due to the fact that top income shares are estimated based on yearly data, whereas the Gini index provided by the SWIID is constructed through imputation, with the original data being available only at 3 to 5-year intervals.

Table B.2: Type of consolidation

	Impact	**1y**	**3y**	**5y**	**10y**
a) Tax-based consolidation (baseline)					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) Any consolidation					
GDP	**-0.30**	**-0.55**	**-0.59**	-0.30	-0.01
Disposable Gini	**0.15**	**0.24**	0.14	0.05	-0.01
Unemployment	-0.03	0.14	0.19	0.07	-0.07
Participation	0.00	-0.07	-0.06	-0.03	0.02
c) Spending-based consolidation					
GDP	-0.11	-0.34	-0.17	-0.01	0.00
Disposable Gini	**0.24**	**0.41**	**0.40**	**0.20**	-0.01
Unemployment	-0.07	0.07	0.14	-0.03	-0.08
Participation	-0.03	-0.12	0.00	0.04	0.06
d) Unanticipated tax-based consolidation					
GDP	**-0.73**	**-1.27**	**-1.61**	**-1.04**	-0.03
Disposable Gini	0.02	-0.06	-0.31	-0.21	-0.01
Unemployment	0.10	0.41	0.48	0.35	-0.09
Participation	0.04	0.05	-0.21	-0.19	-0.05

Notes: The table reports the response to a 1% of GDP overall tax-based consolidation shock. Bold numbers indicate significance at the 10% confidence level. The spending-based sample comprises episodes in which spending cuts, as identified through the narrative approach, were larger than tax hikes, and vice versa for the tax-based sample. The unanticipated tax-based sample comprises tax-based episodes in which unanticipated tax hikes, announced during the same year of implementation, were larger than anticipated tax hikes (that is announced in years preceding the implementation year), according to the accounts of Alesina, Favero, and Giavazzi, 2015 and Alesina et al., 2015.

Table B.3: Alternative shock variables

	Impact	**1y**	**3y**	**5y**	**10y**
a) Ex-post actual tax revenues (baseline)					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) Ex-post cyclically adjusted tax revenues					
GDP	**-0.52**	**-0.78**	**-0.96**	**-0.83**	-0.44
Disposable Gini	-0.02	-0.10	**-0.34**	**-0.24**	-0.05
Unemployment	0.11	0.33	0.21	0.19	0.03
Participation	0.06	0.04	-0.13	-0.15	**-0.11**
c) Real-time estimates - all consolidations					
GDP	**-0.60**	**-1.29**	**-1.62**	**-1.14**	-0.15
Disposable Gini	-0.09	-0.02	-0.09	-0.11	-0.04
Unemployment	0.13	**0.49**	**0.52**	**0.37**	-0.09
Participation	0.11	0.08	-0.10	-0.13	-0.03

Notes: Panels (a), (b) and (c) report the response to a 1% of GDP overall tax-based consolidation shock, using alternative tax revenue data (respectively ex-post actual revenues, cyclically adjusted ex-post revenues and real-time estimates). Bold numbers indicate significance at the 10% confidence level. .

Table B.4: Reverse causality issues

	Impact	**1y**	**3y**	**5y**	**10y**
a) All tax-based consolidation years (baseline)					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) Excluding years of potential reverse causality issues					
GDP	**-0.49**	**-0.89**	-0.85	-0.54	-0.01
Disposable Gini	-0.01	-0.10	**-0.43**	**-0.29**	0.01
Unemployment	0.08	0.32	0.08	0.06	-0.02
Participation	0.09	0.02	-0.11	-0.11	-0.05
c) Dummy for first year of tax-based consolidation cycle					
GDP	**-0.58**	**-1.38**	**-1.92**	**-1.39**	-0.22
Disposable Gini	-0.12	-0.07	-0.19	-0.17	-0.05
Unemployment	**0.23**	**0.54**	**0.75**	**0.56**	-0.04
Participation	**0.18**	0.08	-0.12	**-0.17**	-0.07

Notes: Panels (a) reports the baseline results, that is the response to a 1% of GDP overall tax shock during all tax-based consolidation years. Panel (b) reports the response to a 1% of GDP overall tax shock during all tax-based consolidation years except those when the consolidation was (i) unanticipated (i.e. decided at year t for implementation in the same year), (ii) part of a multi-year consolidation cycle, and (iii) not the first year of such cycle. Panel (c) reports the response to a tax-based consolidation cycle. This is estimated using a dummy variable taking value 1 for the first year of a tax-based consolidation cycle and 0 otherwise. Bold numbers indicate significance at the 10% confidence level.

The shock variable used to estimate the IRFs reported in Panel (b) is constructed according to the following equation: $X_{i,t}^{j,3} = d_t^1(1 - d_t^u(1 - d_t^f))\Delta t_{i,t}^j$, where d_t^u takes value 1 in every year of unanticipated tax-based consolidations and 0 otherwise, while d_t^f takes value 1 in each first year of a tax-based consolidation cycle and 0 otherwise.

Table B.5: Alternative deterministic components and lag specifications

	Impact	**1y**	**3y**	**5y**	**10y**
a) Country and time f.e., country-specific linear trends (baseline)					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) Country and time f.e., common linear trend					
GDP	**-0.63**	**-1.08**	**-1.61**	**-1.41**	**-0.78**
Disposable Gini	-0.02	-0.09	**-0.36**	**-0.24**	0.06
Unemployment	0.10	**0.41**	0.48	0.40	0.02
Participation	0.02	-0.06	**-0.34**	**-0.37**	-0.30
c) First differences, country and time f.e., no trends					
GDP	**-0.47**	-0.72	-1.04	-1.01	-0.99
Disposable Gini	0.07	0.06	-0.24	-0.27	-0.27
Unemployment	0.11	0.24	0.17	0.15	0.13
Participation	-0.01	-0.06	-0.29	-0.32	-0.32
d) 3 lags of the endogenous variables					
GDP	**-0.40**	-0.60	-0.93	-0.61	0.11
Disposable Gini	0.04	-0.03	**-0.50**	**-0.43**	0.04
Unemployment	0.13	0.24	0.14	0.08	0.03
Participation	0.04	0.04	-0.23	-0.21	-0.09
e) 4 lags of the endogenous variables					
GDP	-0.32	**-0.74**	-0.83	-0.46	-0.04
Disposable Gini	0.02	-0.03	**-0.48**	-0.25	0.05
Unemployment	0.16	0.36	0.20	0.07	0.11
Participation	0.07	0.10	-0.18	-0.10	-0.04

Notes: The table reports the response to a 1% of GDP tax-based consolidation shock. Bold numbers indicate significance at the 10% confidence level. Estimates in Panel (c) report accumulated responses.

Table B.6: Estimation from local projections

	Impact	**1y**	**3y**	**5y**	**10y**
a) PVAR (baseline)					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) Local projections method - with no control variables					
GDP	**-0.40**	-0.53	**-1.17**	**-1.30**	-0.78
Disposable Gini	-0.06	-0.17	**-0.57**	-0.28	0.03
Unemployment	0.10	0.21	0.31	**0.86**	0.14
Participation	0.08	-0.01	**-0.34**	-0.23	-0.18
c) Local projections method - with control variables					
GDP	**-0.49**	-0.66	-0.76	-1.00	-0.06
Disposable Gini	0.00	-0.06	**-0.49**	-0.15	-0.06
Unemployment	0.09	0.16	0.13	**0.59**	-0.27
Participation	0.05	0.00	-0.25	-0.21	0.95
d) Local projections method - first year dummy					
GDP	-0.37	-0.62	**-1.70**	**-1.56**	-0.22
Disposable Gini	-0.14	-0.13	-0.32	-0.21	0.64
Unemployment	0.16	0.33	**0.69**	**1.04**	-0.27
Participation	**0.20**	0.20	-0.05	-0.19	0.57

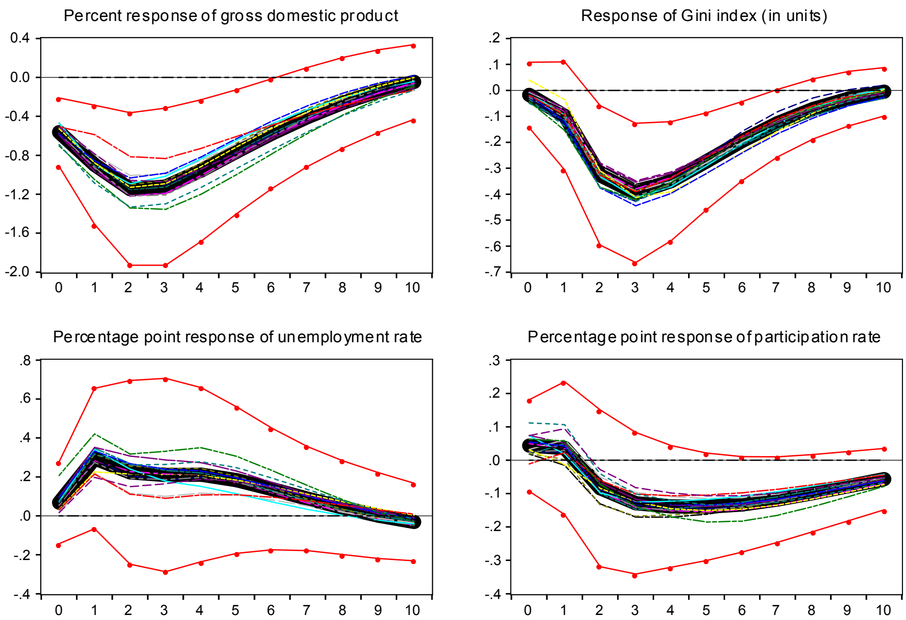
Notes: Panels (a), (b) and (c) report the response to a 1% of GDP overall tax-based consolidation shock. Estimates from panel (d) are obtained replacing the total tax shock variable with a dummy taking value 1 in the first year of a tax-based consolidation episodes and 0 otherwise. Coefficients from Panel (a) are estimated using the PVAR methodology, according to Equation 3.1. Coefficients from Panels (b), (c) and (d) are estimated using local projections method, according to Equation B.2, with $X^j_{i,t-l}$ being an empty vector for estimates of Panels (b) and (d). Bold numbers indicate significance at the 10% confidence level.

Table B.7: Sample selection

	Impact	**1y**	**3y**	**5y**	**10y**
a) All sample (baseline)					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) Excluding great financial crisis period (sample 1978-2007)					
GDP	**-0.55**	**-0.85**	**-0.81**	-0.47	0.03
Disposable Gini	-0.02	-0.11	**-0.42**	**-0.23**	0.04
Unemployment	0.00	0.18	0.10	0.01	-0.07
Participation	-0.01	-0.01	-0.09	-0.11	-0.03
c) Excluding non-EU countries					
GDP	**-0.50**	**-0.68**	-0.86	-0.51	0.09
Disposable Gini	0.00	-0.09	**-0.35**	**-0.24**	-0.01
Unemployment	0.03	0.20	0.17	0.16	-0.03
Participation	0.02	0.00	-0.16	-0.13	-0.03
d) Excluding consolidations in years of banking crisis					
GDP	**-0.60**	**-0.87**	**-1.07**	**-0.71**	-0.03
Disposable Gini	0.00	-0.07	**-0.37**	**-0.26**	0.00
Unemployment	0.06	0.26	0.16	0.14	-0.04
Participation	0.03	0.02	-0.13	-0.13	-0.05
e) Excluding shock outliers					
GDP	**-0.55**	**-1.07**	**-1.26**	**-0.89**	-0.11
Disposable Gini	0.00	-0.07	**-0.39**	**-0.27**	0.00
Unemployment	0.14	**0.45**	0.28	0.23	-0.03
Participation	0.08	0.02	-0.16	-0.16	-0.06

Notes: The table reports the response to a 1% of GDP overall tax-based consolidation shock. Bold numbers indicate significance at the 10% confidence level.

Figure B.4: Sample stability



Notes: The figure shows 16 different IRFs to a 1% of GDP tax-based consolidation shock. The solid black line represents the baseline estimation. Each other colored line represents an IRF estimated over a sample of 15 different countries, rather than all the 16 countries of the baseline specification. The red lines with circles represent the confidence bands of the baseline specification (Figure 1 in Section 3).

Table B.8: Alternative specifications with productivity, hours worked and employment

	Impact	1y	3y	5y	10y
GDP per hour worked	**-0.23**	**-0.40**	**-0.47**	**-0.40**	-0.27
Disposable Gini	-0.06	-0.15	**-0.41**	**-0.26**	0.01
Hours worked	0.10	-0.04	-0.03	0.01	0.05
Employment rate	-0.01	-0.08	-0.17	-0.17	-0.09

Notes: The table reports the response to a 1% of GDP overall tax-based consolidation shock. Bold numbers indicate significance at the 10% confidence level. Hours worked refer to employed individuals. Employment is measured as employed individuals as share of the active population.

Table B.9: Omitted variables (1)

	Impact	**1y**	**3y**	**5y**	**10y**
a) 5-variable PVAR (baseline)					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) Government consumption					
GDP	**-0.55**	**-0.88**	**-1.11**	**-0.72**	0.02
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.29**	-0.05
Unemployment	0.07	0.29	0.21	0.17	-0.05
Participation	0.05	0.04	-0.12	-0.12	-0.03
Government consumption	0.07	0.00	-0.04	-0.09	-0.08
c) Inflation					
GDP	**-0.62**	**-0.95**	**-1.08**	**-0.75**	-0.14
Disposable Gini	-0.02	-0.10	**-0.38**	**-0.26**	-0.01
Unemployment	0.04	0.26	0.16	0.14	0.00
Participation	0.07	0.09	-0.07	-0.10	-0.06
Inflation	**0.33**	0.18	**-0.29**	-0.04	-0.03
d) Savings					
GDP	**-0.55**	**-0.86**	**-1.08**	**-0.78**	0.03
Disposable Gini	-0.02	-0.11	**-0.41**	**-0.28**	-0.03
Unemployment	0.07	0.29	0.19	0.18	-0.06
Participation	0.06	0.06	-0.11	-0.14	-0.02
Saving rate	-0.22	-0.23	-0.30	-0.08	0.13

Notes: The table reports the response to a 1% of GDP overall tax-based consolidation shock. Bold numbers indicate significance at the 10% confidence level.

Table B.10: Omitted variables (2)

	Impact	**1y**	**3y**	**5y**	**10y**
e) Trade balance					
GDP	**-0.55**	**-0.49**	**-0.88**	-0.56	0.09
Disposable Gini	-0.02	-0.02	**-0.34**	**-0.37**	-0.03
Unemployment	0.07	0.06	0.12	0.02	-0.08
Participation	0.05	0.05	-0.05	-0.08	-0.03
Trade balance	0.07	-0.07	0.19	0.16	0.07
b) Trade openness					
GDP	**-0.55**	**-0.88**	**-1.12**	**-0.79**	-0.02
Disposable Gini	-0.02	-0.10	**-0.38**	**-0.25**	0.00
Unemployment	0.06	0.28	0.20	0.18	-0.07
Participation	0.04	0.04	-0.11	-0.12	-0.04
Import + Exports	0.00	0.58	-0.17	0.32	0.43
c) Employment					
GDP	**-0.58**	**-0.98**	**-1.30**	**-0.99**	-0.16
Disposable Gini	-0.01	-0.08	**-0.38**	**-0.26**	-0.01
Unemployment	0.07	0.32	0.28	0.28	0.00
Participation	0.07	0.06	-0.13	-0.16	-0.08
Employment rate	0.01	-0.17	-0.31	-0.35	-0.07
d) Direct-to-indirect tax ratio					
GDP	**-0.52**	**-0.78**	**-0.91**	-0.59	0.02
Disposable Gini	-0.04	-0.14	**-0.46**	**-0.32**	-0.02
Unemployment	0.09	0.33	0.22	0.19	0.01
Participation	0.04	0.04	-0.12	-0.13	-0.07
Direct-to-indirect tax ratio	**0.03**	0.01	0.00	0.00	0.00

Notes: The table reports the response to a 1% of GDP overall tax-based consolidation shock. Bold numbers indicate significance at the 10% confidence level.

Table B.11: Alternative inequality measures (1)

	Impact	**1y**	**3y**	**5y**	**10y**
a) Disposable Gini index (baseline)					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) Top 0.01% income share					
GDP	-0.28	**-0.85**	**-1.22**	**-0.90**	-0.27
Top 0.01% share	-0.03	-0.05	0.01	0.00	0.00
Unemployment	**-0.41**	-0.28	-0.13	0.03	-0.06
Participation	**0.37**	**0.39**	-0.03	-0.01	0.01
c) Top 0.01-1% income share					
GDP	-0.32	**-0.85**	**-1.13**	**-0.79**	-0.25
Top 0.01-1% share	-0.09	-0.11	0.05	**0.06**	0.02
Unemployment	**-0.46**	-0.22	-0.05	0.03	-0.06
Participation	**0.44**	0.30	-0.13	-0.03	0.02
d) Top 1-10% income share					
GDP	-0.32	**-0.89**	**-1.25**	**-0.90**	-0.18
Top 1-10% share	-0.03	-0.03	0.05	**0.10**	0.03
Unemployment	**-0.52**	-0.25	0.00	0.09	-0.05
Participation	**0.50**	0.36	-0.09	-0.05	-0.02

Notes: The table reports the response to a 1% of GDP overall tax-based consolidation shock. Bold numbers indicate significance at the 10% confidence level.

Table B.12: Alternative inequality measures (2)

	Impact	**1y**	**3y**	**5y**	**10y**
a) Disposable Gini index (baseline)					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) Top 0.01% income share (excluding Spain)					
GDP	-0.49	**-1.02**	-0.96	-0.53	-0.10
Top 0.01% share	-0.04	**-0.09**	0.01	0.01	0.00
Unemployment	0.18	**0.58**	0.17	0.03	-0.05
Participation	0.04	0.03	-0.23	-0.13	0.01
c) Top 0.01-1% income share (excluding Spain)					
GDP	-0.50	**-1.04**	-1.00	-0.54	-0.09
Top 0.01-1% share	-0.08	**-0.23**	0.01	0.04	0.02
Unemployment	0.18	**0.59**	0.23	0.07	-0.06
Participation	0.03	0.02	-0.22	-0.14	0.00
d) Top 1-10% income share (excluding Spain)					
GDP	-0.45	**-0.97**	-1.05	-0.66	-0.10
Top 1-10% share	-0.09	-0.10	0.06	0.07	0.01
Unemployment	0.12	**0.51**	0.24	0.17	-0.06
Participation	0.03	0.05	-0.18	-0.14	-0.01

Notes: The table reports the response to a 1% of GDP overall tax-based consolidation shock. Bold numbers indicate significance at the 10% confidence level.

Table B.13: Alternative inequality measures (3)

	Impact	**1y**	**3y**	**5y**	**10y**
a) Disposable Gini index (baseline)					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) 90/10 Income ratio					
GDP	-0.27	-0.53	-0.46	-0.47	-0.27
P90/P10 income ratio	0.12	-1.48	-0.84	0.03	-0.11
Unemployment	0.10	0.33	-0.30	-0.04	0.06
Participation	0.00	0.12	0.07	0.04	-0.02
c) 90/50 Income ratio					
GDP	-0.33	-0.66	-0.58	-0.49	-0.28
P90/P50 income ratio	0.02	-0.63	-1.51	-0.37	-0.08
Unemployment	0.14	0.44	-0.21	-0.03	0.05
Participation	-0.01	0.11	0.05	0.04	-0.01
d) 50/10 Income ratio					
GDP	-0.29	-0.56	-0.53	-0.44	-0.25
P50/P10 income ratio	0.67	0.34	0.75	0.09	0.00
Unemployment	0.15	0.40	-0.31	-0.14	0.05
Participation	0.01	0.14	0.07	0.04	-0.02

Notes: The table reports the response to a 1% of GDP overall tax-based consolidation shock. Bold numbers indicate significance at the 10% confidence level.

B.4 Robustness Checks on Composition Effects of Tax-based Consolidations

Direct and indirect tax-based consolidations

In this section we present the robustness checks on our results for direct and indirect tax-based consolidations.

First, we address a potential criticism regarding our methodology. Introducing only one shock at a time (i.e. either direct or indirect taxes) might lead to neglect potential interactions among the different tax instruments. Although the null correlation between direct and indirect tax shocks during tax-based consolidation years makes this line of argument implausible, we check whether our results remain valid once including both shocks simultaneously. The new estimates, reported in Table B.14, highlight the robustness of our results to this new specification.

Further, we check whether our results are driven by a particular country. To this purpose, we estimate the model excluding one country at a time. We conclude that our results are robust (Figures B.5 and B.6).

Finally, we estimate the model using our alternative measures of income inequality: the the top income shares and the income ratios. IRFs are presented in Tables B.15-B.20. The results are broadly in line with what found earlier. Direct taxes significantly reduce the share of income of the very rich agents (the top 0.01%), by 0.1 percentage points on impact and after one year.[9] Conversely, indirect taxes do not significantly reduce the share of the top 0.01% income earners, but do have some short-term significant negative effects on the income share of the richest 0.01-1% and 1-10% individuals. Moreover, the specifications with the income ratios confirm to a large extent our result that indirect tax-based consolidations reduce income inequality, with the P90/P10 and the P50/P10 ratios shrinking, respectively, by 14.16 and 3.82 percentage points on impact.

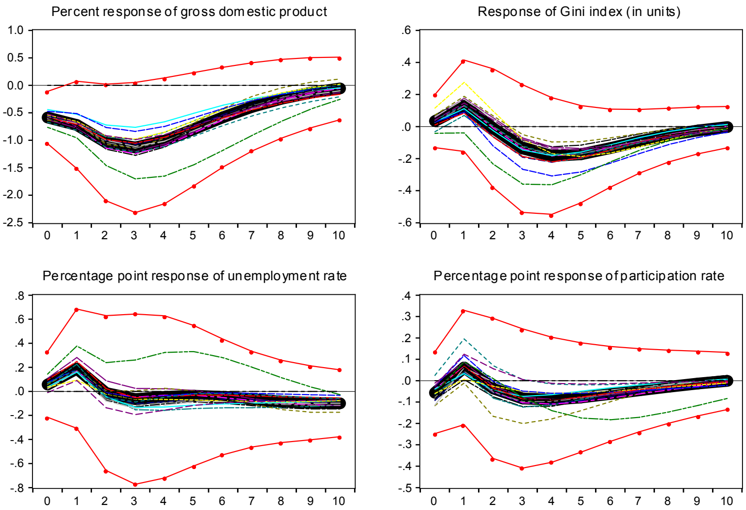
9 Tax avoidance practices are likely to partially reduce the egalitarian effect of direct taxes, as high-income earners may shift income over time and country more easily than middle and low-income earners (see also Atkinson, Piketty, and Saez, 2011).

Table B.14: Ordering of shocks

	Impact	**1y**	**3y**	**5y**	**10y**
a) Direct tax-based					
GDP	**-0.59**	-0.72	-1.13	-0.80	-0.06
Disposable Gini	0.03	0.13	-0.14	-0.17	0.00
Unemployment	0.06	0.19	-0.06	-0.04	-0.10
Participation	-0.06	0.06	-0.08	-0.08	0.00
b) Indirect tax-based					
GDP	**-1.79**	**-4.10**	**-4.90**	**-3.86**	-0.74
Disposable Gini	-0.32	**-0.79**	**-1.25**	**-0.88**	-0.17
Unemployment	**0.75**	**1.90**	**2.43**	**1.82**	0.10
Participation	**0.59**	0.22	-0.26	**-0.56**	-0.32
c) Direct taxes (ordered 1st) and indirect taxes (ordered 2nd) - shock to direct taxes					
Direct taxes	**1.00**	**0.19**	0.00	0.01	0.00
Indirect taxes	-0.02	-0.01	0.00	0.00	0.00
GDP	**-0.62**	-0.77	-1.15	-0.77	-0.02
Disposable Gini	0.03	0.12	-0.14	-0.15	0.01
Unemployment	0.07	0.22	-0.04	-0.04	-0.12
Participation	-0.06	0.05	-0.09	-0.09	0.01
d) Direct taxes (ordered 1st) and indirect taxes (ordered 2nd) - shock to indirect taxes					
Direct taxes	0.00	-0.08	0.05	0.03	0.01
Indirect taxes	**1.00**	**-0.12**	**0.04**	0.00	0.00
GDP	**-1.91**	**-4.28**	**-5.05**	**-3.90**	-0.76
Disposable Gini	-0.33	**-0.81**	**-1.27**	**-0.87**	-0.18
Unemployment	**0.74**	**1.90**	**2.45**	**1.85**	0.07
Participation	**0.59**	0.22	-0.28	**-0.57**	-0.31
e) Indirect taxes (ordered 1st) and direct taxes (ordered 2nd) - shock to direct taxes					
Indirect taxes	0.00	-0.01	0.00	0.00	0.00
Direct taxes	**1.00**	**0.19**	0.00	0.01	0.00
GDP	**-0.66**	**-0.87**	**-1.26**	-0.86	-0.03
Disposable Gini	0.02	0.10	-0.17	-0.17	0.00
Unemployment	0.09	0.26	0.01	0.00	-0.12
Participation	-0.05	0.06	-0.10	-0.10	0.00
f) Indirect taxes (ordered 1st) and direct taxes (ordered 2nd) - shock to indirect taxes					
Indirect taxes	**1.00**	**-0.12**	**0.04**	0.00	0.00
Direct taxes	-0.11	-0.10	0.05	0.03	0.01
GDP	**-1.83**	**-4.18**	**-4.90**	**-3.81**	-0.76
Disposable Gini	-0.33	**-0.82**	**-1.25**	**-0.85**	-0.18
Unemployment	**0.73**	**1.87**	**2.45**	**1.85**	0.09
Participation	**0.59**	0.21	-0.27	**-0.55**	-0.31

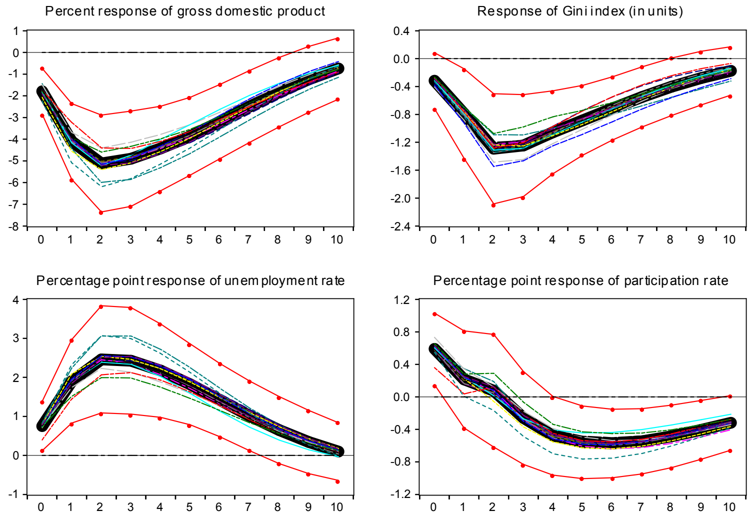
Notes: The table reports the response to a 1% of GDP overall tax-based consolidation shock under alternative ordering of shocks. Bold numbers indicate significance at the 10% confidence level.

Figure B.5: Sample stability - direct tax-based consolidation



Notes: The figure shows 16 different IRFs to a 1% of GDP direct tax-based consolidation shock. The solid black line represents the baseline estimation. Each other colored line represents an IRF estimated over a sample of 15 different countries, rather than all the 16 countries of the baseline specification. The red lines with circles represent the confidence bands of the baseline specification.

Figure B.6: Sample stability - indirect tax-based consolidation



Notes: The figure shows 16 different IRFs to a 1% of GDP indirect tax-based consolidation shock. The solid black line represents the baseline estimation. Each other colored line represents an IRF estimated over a sample of 15 different countries, rather than all the 16 countries of the baseline specification. The red lines with circles represent the confidence bands of the baseline specification.

Table B.15: Alternative inequality measures - direct tax-based consolidation (1)

	Impact	**1y**	**3y**	**5y**	**10y**
a) Disposable Gini index					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) Top 0.01% income share					
GDP	-0.37	-0.57	-0.93	-0.80	-0.22
Top 0.01% share	**-0.07**	**-0.09**	0.01	0.01	0.00
Unemployment	-0.24	-0.33	-0.55	-0.07	-0.05
Participation	0.16	0.35	-0.14	-0.03	0.01
c) Top 0.01-1% income share					
GDP	-0.40	-0.58	-0.84	-0.64	-0.19
Top 0.01-1% share	-0.14	-0.18	0.01	**0.07**	0.02
Unemployment	-0.29	-0.28	-0.46	-0.05	-0.05
Participation	0.23	0.27	-0.27	-0.06	0.02
d) Top 1-10% income share					
GDP	-0.45	-0.69	-1.20	**-1.06**	-0.21
Top 1-10% share	0.01	0.02	-0.06	**0.11**	0.04
Unemployment	-0.37	-0.30	-0.45	0.06	-0.06
Participation	0.34	0.34	-0.07	-0.04	-0.02

Notes: The table reports the response to a 1% of GDP direct tax-based consolidation shock using alternative inequality measures. Bold numbers indicate significance at the 10% confidence level.

Table B.16: Alternative inequality measures - direct tax-based consolidations (2)

	Impact	**1y**	**3y**	**5y**	**10y**
a) Disposable Gini index					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) Top 0.01% income share (excluding Spain)					
GDP	**-0.79**	**-1.20**	-1.41	-0.88	-0.22
Top 0.01% share	**-0.08**	**-0.14**	-0.01	0.01	0.00
Unemployment	0.24	**0.66**	0.15	0.01	-0.07
Participation	0.11	0.19	-0.27	-0.18	0.01
c) Top 0.01-1% income share (excluding Spain)					
GDP	**-0.75**	**-1.15**	-1.43	-0.93	-0.22
Top 0.01-1% share	-0.11	**-0.28**	-0.02	0.05	0.03
Unemployment	0.19	**0.60**	0.19	0.10	-0.09
Participation	0.10	0.19	-0.23	-0.16	0.01
d) Top 1-10% income share (excluding Spain)					
GDP	**-0.74**	**-1.12**	-1.41	-1.00	-0.21
Top 1-10% share	-0.04	-0.03	-0.04	0.08	0.01
Unemployment	0.18	0.57	0.17	0.19	-0.08
Participation	0.08	0.18	-0.21	-0.14	-0.01

Notes: The table reports the response to a 1% of GDP direct tax-based consolidation shock using alternative inequality measures. Bold numbers indicate significance at the 10% confidence level.

Table B.17: Alternative inequality measures - direct tax-based consolidation (3)

	Impact	**1y**	**3y**	**5y**	**10y**
a) Disposable Gini index					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) 90/10 Income ratio					
GDP	-0.55	-0.41	-0.40	-0.47	-0.21
P90/P10 income ratio	1.73	-0.36	-3.13	0.25	-0.09
Unemployment	-0.02	0.16	-0.46	0.06	0.06
Participation	-0.11	0.01	-0.03	-0.02	-0.03
c) 90/50 Income ratio					
GDP	-0.62	-0.55	-0.52	-0.45	-0.19
P90/P50 income ratio	-0.01	-0.52	-1.69	-0.26	-0.05
Unemployment	0.04	0.29	-0.37	0.02	0.04
Participation	-0.11	0.01	-0.07	-0.02	-0.02
d) 50/10 Income ratio					
GDP	-0.58	-0.44	-0.49	-0.50	-0.19
P50/P10 income ratio	1.36	0.72	-0.14	0.15	-0.02
Unemployment	0.04	0.23	-0.45	0.02	0.05
Participation	-0.09	0.04	-0.03	-0.04	-0.03

Notes: The table reports the response to a 1% of GDP direct tax-based consolidation shock using alternative inequality measures. Bold numbers indicate significance at the 10% confidence level.

Table B.18: Alternative inequality measures - indirect tax-based consolidation (1)

	Impact	**1y**	**3y**	**5y**	**10y**
a) Disposable Gini index					
GDP	**-1.79**	**-4.10**	**-4.90**	**-3.86**	-0.74
Disposable Gini	-0.32	**-0.79**	**-1.25**	**-0.88**	-0.17
Unemployment	**0.75**	**1.90**	**2.43**	**1.82**	0.10
Participation	**0.59**	0.22	-0.26	**-0.56**	-0.32
b) Top 0.01% income share					
GDP	**-1.48**	**-4.81**	**-4.81**	**-3.10**	-0.86
Top 0.01% share	-0.03	0.06	-0.04	0.02	0.01
Unemployment	-0.34	1.30	**1.63**	0.38	-0.18
Participation	**1.37**	**1.05**	-0.02	-0.11	0.01
c) Top 0.01-1% income share					
GDP	**-1.59**	**-4.90**	**-4.77**	**-2.89**	-0.83
Top 0.01-1% share	**-0.44**	-0.16	0.15	**0.18**	**0.09**
Unemployment	-0.55	1.41	**1.91**	0.42	-0.21
Participation	**1.69**	0.92	-0.40	-0.16	0.07
d) Top 1-10% income share					
GDP	**-1.44**	**-4.70**	**-4.12**	**-2.58**	-0.47
Top 1-10% share	-0.29	-0.37	**0.38**	**0.24**	0.08
Unemployment	-0.60	1.26	**1.75**	0.49	-0.14
Participation	**1.44**	0.87	-0.36	-0.20	-0.05

Notes: The table reports the response to a 1% of GDP indirect tax-based consolidation shock using alternative inequality measures. Bold numbers indicate significance at the 10% confidence level.

Table B.19: Alternative inequality measures - indirect tax-based consolidation (2)

	Impact	**1y**	**3y**	**5y**	**10y**
a) Disposable Gini index					
GDP	**-1.79**	**-4.10**	**-4.90**	**-3.86**	-0.74
Disposable Gini	-0.32	**-0.79**	**-1.25**	**-0.88**	-0.17
Unemployment	**0.75**	**1.90**	**2.43**	**1.82**	0.10
Participation	**0.59**	0.22	-0.26	**-0.56**	-0.32
b) Top 0.01% income share (excluding Spain)					
GDP	-1.42	**-4.52**	-2.17	-0.49	0.34
Top 0.01% share	-0.09	0.04	-0.04	0.03	0.00
Unemployment	0.25	**1.65**	1.35	0.29	-0.10
Participation	-0.09	-0.77	-0.79	-0.50	0.00
c) Top 0.01-1% income share (excluding Spain)					
GDP	-1.53	**-4.69**	-2.22	-0.45	0.43
Top 0.01-1% share	**-0.51**	-0.39	-0.12	0.08	-0.02
Unemployment	0.37	**1.87**	1.41	0.27	-0.13
Participation	-0.06	-0.75	-0.90	-0.57	-0.01
d) Top 1-10% income share (excluding Spain)					
GDP	-1.39	**-4.50**	-2.39	-0.85	0.42
Top 1-10% share	-0.20	-0.48	**0.60**	0.23	-0.04
Unemployment	0.12	1.48	1.33	0.45	-0.21
Participation	-0.02	-0.64	-0.80	-0.58	0.01

Notes: The table reports the response to a 1% of GDP indirect tax-based consolidation shock using alternative inequality measures. Bold numbers indicate significance at the 10% confidence level.

Table B.20: Alternative inequality measures - indirect tax-based consolidation (3)

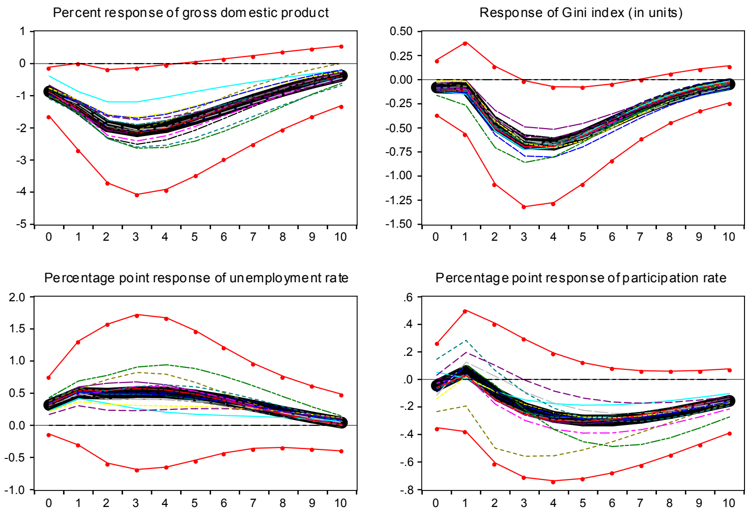
	Impact	**1y**	**3y**	**5y**	**10y**
a) Disposable Gini index					
GDP	**-0.59**	-0.72	-1.13	-0.80	-0.06
Disposable Gini	0.03	0.13	-0.14	-0.17	0.00
Unemployment	0.06	0.19	-0.06	-0.04	-0.10
Participation	-0.06	0.06	-0.08	-0.08	0.00
b) 90/10 Income ratio					
GDP	-1.23	**-3.59**	**-4.69**	**-3.32**	-1.35
P90/P10 income ratio	**-14.16**	-9.74	3.00	-1.60	-0.51
Unemployment	**0.94**	**2.00**	**2.33**	1.04	0.19
Participation	0.20	0.10	-0.11	-0.29	-0.07
c) 90/50 Income ratio					
GDP	-1.30	**-3.70**	**-4.78**	**-3.39**	-1.38
P90/P50 income ratio	-3.47	-2.35	-2.85	-1.04	-0.30
Unemployment	**0.95**	**2.08**	**2.56**	1.15	0.14
Participation	0.19	0.08	-0.09	-0.25	-0.05
d) 50/10 Income ratio					
GDP	-1.19	**-3.51**	**-4.57**	**-3.08**	-1.29
P50/P10 income ratio	**-3.82**	-2.13	2.04	-0.40	0.03
Unemployment	**0.98**	**2.01**	**2.10**	0.73	0.17
Participation	0.23	0.15	-0.09	-0.20	-0.07

Notes: The table reports the response to a 1% of GDP indirect tax-based consolidation shock using alternative inequality measures. Bold numbers indicate significance at the 10% confidence level.

Composition effects of direct-tax based consolidations

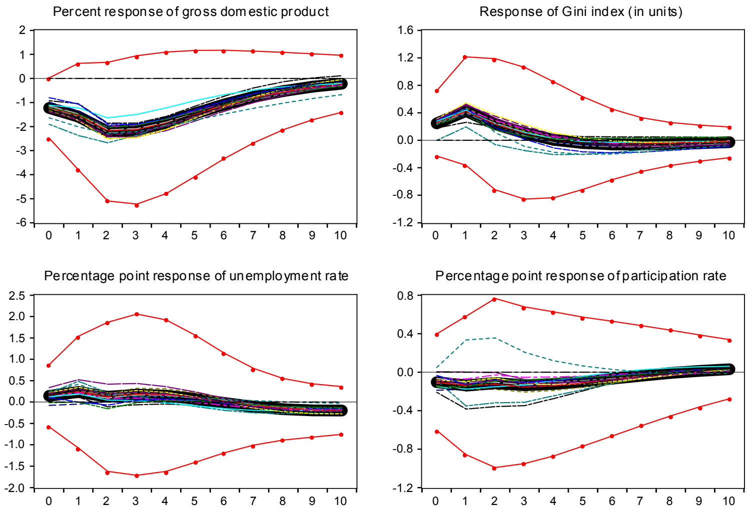
Below we show the country stability robustness checks for personal, corporate and SSC tax-based consolidations. This exercise entails repeating the estimation 16 number of times, each time excluding one different country. While for personal and corporate our results are robust, the estimated responses of the labor force participation to a SSC tax-based consolidation is driven by a single country. When this country is excluded, the response of the labor force participation is not statistically different from 0.

Figure B.7: Sample stability - personal tax-based consolidation



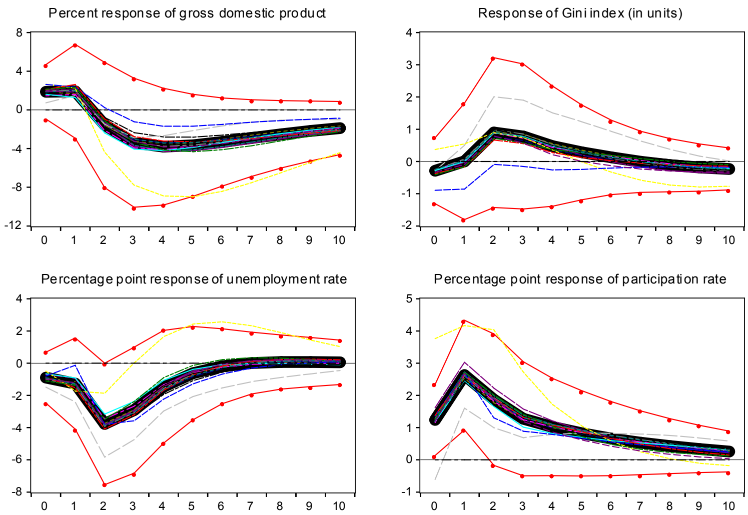
Notes: The figure shows 16 different IRFs to a a 1% of GDP personal tax-based consolidation shock. The solid black line represents the baseline estimation. Each other colored line represents an IRF estimated over a sample of 15 different countries, rather than all the 16 countries of the baseline specification. The red lines with circles represent the confidence bands of the baseline specification.

Figure B.8: Sample stability - corporate tax-based consolidation



Notes: The figure shows 16 different IRFs to a 1% of GDP corporate tax-based consolidation shock. The solid black line represents the baseline estimation. Each other colored line represents an IRF estimated over a sample of 15 different countries, rather than all the 16 countries of the baseline specification. The red lines with circles represent the confidence bands of the baseline specification.

Figure B.9: Sample stability - SSC-based consolidation

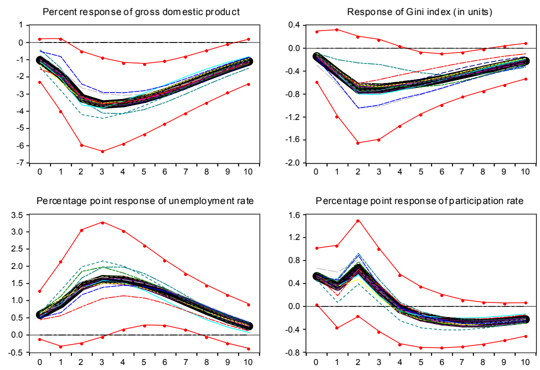


Notes: The figure shows 16 different IRFs to a 1% of GDP SSC-based consolidation shock. The solid black line represents the baseline estimation. Each other colored line represents an IRF estimated over a sample of 15 different countries, rather than all the 16 countries of the baseline specification. The red lines with circles represent the confidence bands of the baseline specification.

Composition effects of indirect-tax based consolidations

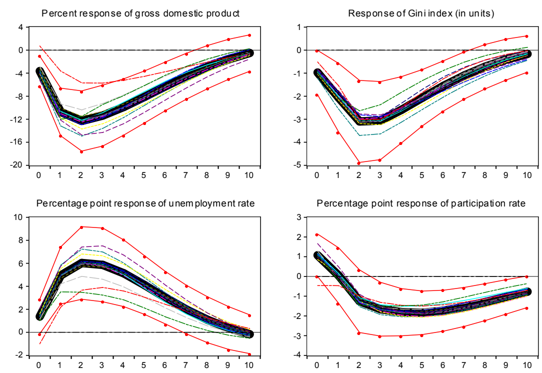
Below we show the country stability robustness checks for GT and SGS tax-based consolidations. This exercise entails repeating the estimation 16 number of times, each time excluding one different country. While for GT our results are robust, those for SGS tax-based consolidations are not.

Figure B.10: Sample stability - GT-based consolidation



Notes: The figure shows 16 different IRFs to a 1% of GDP GT-based consolidation shock. The solid black line represents the baseline estimation. Each other colored line represents an IRF estimated over a sample of 15 different countries, rather than all the 16 countries of the baseline specification. The red lines with circles represent the confidence bands of the baseline specification.

Figure B.11: Sample stability - SGS-based consolidation



Notes: The figure shows 16 different IRFs to a 1% of GDP SGS-based consolidation shock. The solid black line represents the baseline estimation. Each other colored line represents an IRF estimated over a sample of 15 different countries, rather than all the 16 countries of the baseline specification. The red lines with circles represent the confidence bands of the baseline specification.

Appendix C

Appendix to Chapter 4

C.1 Dataset — Reforms

Table C.1: Reform events

Country	Year	Area	Score
Australia	2006	procedural inconvenience	1
Australia	2010	notice for individual dismissal procedural inconvenience	-1
Austria	2003	severance pay	1
Belgium	1970	notice for individual dismissal	-1
Belgium	1971	notice for individual dismissal	1
Belgium	1985	severance pay	1
Czech Republic	2007	procedural inconvenience	1
Czech Republic	2012	notice period, severance pay	1
Finland	1989	notice for individual dismissal	-1
Finland	1997	notice for individual dismissal	1
France	1987	procedural inconvenience	1
France	2003	collective dismissal	-1

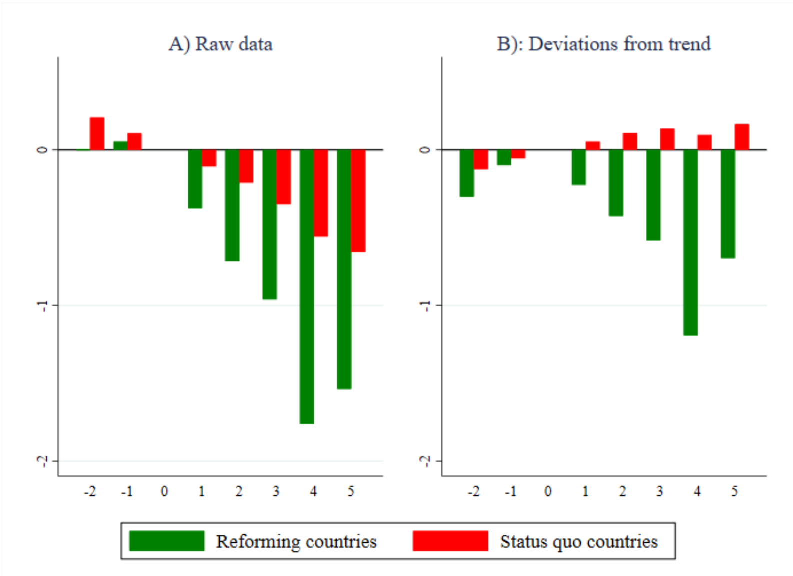
France	2009	procedural inconvenience	1
Germany	1994	notice for individual dismissal	-1
Germany	1997	procedural inconvenience	1
Germany	2004	procedural inconvenience	-1
Greece	2011	notice for individual dismissal severance pay collective dismissal	1
Greece	2012	severance pay	1
Ireland	1973	notice for individual dismissal	-1
Ireland	1977	procedural inconvenience notice for individual dismissal	-1
Ireland	2006	notice for individual dismissal	1
Ireland	2012	severance pay	-1
Italy	1970	procedural inconvenience	-1
Italy	1991	procedural inconvenience	1
Italy	2013	procedural inconvenience	1
Korea	1998	procedural inconvenience notice for individual dismissal collective dismissal	1
Japan	2007	procedural inconvenience	1
Netherlands	1976	collective dismissal	-1
Netherlands	1996	procedural inconvenience	1
New Zealand	2001	procedural inconvenience	-1
New Zealand	2012	trial period	1

Norway	1977	procedural inconvenience	-1
Portugal	1975	collective dismissal	-1
Portugal	1976	procedural inconvenience	-1
Portugal	1978	procedural inconvenience	1
Portugal	1990	procedural inconvenience	1
Portugal	1992	procedural inconvenience	1
Portugal	2004	procedural inconvenience	1
Portugal	2010	notice for individual dismissal severance pay procedural inconvenience	1
Slovak Republic	2004	notice period, severance pay	1
Slovak Republic	2012	notice period, severance pay	1
Spain	1978	procedural inconvenience collective dismissal	1
Spain	1981	procedural inconvenience collective dismissal	1
Spain	1995	procedural inconvenience collective dismissal	1
Spain	1998	severance pay	1
Spain	2001	procedural inconvenience severance pay	1
Spain	2011	severance pay	1
Spain	2013	procedural inconvenience collective dismissal severance pay	1
Sweden	1975	notice for individual dismissal	-1

Sweden	1997	notice for individual dismissal	1
United Kingdom	2000	severance pay	-1

Source: Duval et al., 2018

Figure C.1: Major employment protection reforms over time



Notes: the figure reports the total number (y-axis) of reforms to employment protection legislation implemented across all countries in the sample by year (x-axis).
Sources: Duval et al., 2018 and own calculations

C.2 Dataset — Layoff Rates

We use data contained in the 2014 Displaced Workers Survey (DWS), conducted in the context of the IPUMS-CPS (Flood et al. (2017)). The survey covers around ninety thousand individuals and provides information about (i) whether the individual held at least one job in the last three years, (ii) the industry of the current or last job, (iii) whether the individual was displaced in the last three years, (iv) the reason for the displacement, and (v) the industry in which the worker was employed when she/he was displaced. Using this information, for each industry we compute the total number of workers that were displaced for either of the following three reasons: (a) the plant or company closed or moved, (b) work was insufficient, (c) the position or shift was abolished. We do so for each of the three years covered by the survey (2011, 2012 and 2013). Since individuals were only asked in which industry they were employed in January 2014, we use data from the Current Population Survey (BLS) to compute industry-level employment changes in 2014, relative to 2011, 2012 and 2013, and multiply them by the employment level in 2014 to obtain employment levels for each year covered by the survey. Table C.2 below reports the layoff rates we computed in this way.

Table C.2: Layoff rates

	2011	**2012**	**2013**	**Average**	**Industry code**
Food, Beverages & Tobacco	3.04	1.99	4.07	3.03	10t12
Textiles	2.18	3.1	5.92	3.73	13t15
Wood, Paper & Reproduction	4.31	3.21	3.9	3.81	16t18
Coke & Refined Petroleum	0	0	3.21	1.07	19
Chemicals	3.43	2.11	2.18	2.58	20t21
Rubber & Plastics	2.82	2.22	2.17	2.41	22t23
Basic Metals	2.44	3.35	3.92	3.24	24t25
Electrical & Optical	4.67	5.96	6.25	5.62	26t27
Machinery & Equipment	3.04	2.39	3.15	2.86	28
Transport Equipment	2.94	2	3.37	2.77	29t30
Others Manufacturing	8.54	7.48	5.92	7.31	31t33
Wholesale & Retail, Motor vehicles	2.3	2.18	2.48	2.32	45
Wholesale ex. Motor Vehicles	1.85	2.84	3.39	2.69	46
Retail ex. Motor Vehicles	2.04	2.46	3.22	2.57	47
Transport & Storage	2.48	2.92	3.41	2.94	49t52
Postal & Courier	1.58	1.4	1.34	1.44	53
Publishing & Audiovisual	2.7	2.56	4.36	3.21	58t60
Telecommunications	2.08	1.81	2.05	1.98	61
IT & Others	2.47	3.14	3.69	3.1	62t63
Agriculture	0	5.2	5.62	3.61	A
Mining & Quarrying	/	/	/	/	B
Utilities	1.14	2.21	1.43	1.59	DtE
Construction	4.51	5.63	8.98	6.37	F
Accommodation & Food Services	1.96	2.86	4.43	3.08	I
Financial & Insurance	2.51	1.93	2.59	2.34	K
Real Estate	1.28	1.53	2.91	1.9	L
Professional & Support Activities	2.62	3.59	4.84	3.68	MtN
PA, Defense & SS	/	/	/	/	O
Education	0.9	1.43	1.72	1.35	P
Health & Social	1.31	1.5	2.54	1.78	Q
Arts & Recreation	2.06	2.81	5.33	3.4	R
Other Service Activities	1.85	2.61	3.72	2.73	S
Average	2.5	2.81	3.74	3.02	/

Notes: Layoff rates for the years 2011, 2012 and 2013 are calculated as the ratio of displaced workers for (i) company/plant closing, (ii) insufficient work, (iii) position or shift abolished over wage and salary employment, computed using individual-level data contained in the 2014 Displaced Workers Survey of the IPUMS-CPS (Flood et al. (2017)).

C.3 Dataset — Elasticities of Substitution

Let there be an aggregate production function under which gross output is produced using, among others, labor and capital as inputs. That is, $Y = H(K, L, X_i, A)$, where K and L stand respectively for the flow of capital and labor services, X_i other inputs and A is an index denoting technical efficiency. Assuming further that the change in technical efficiency is Hicks-neutral and capital and labor are weakly separable from other inputs, we can rewrite the production function as $Y = AJ(X_i, F(K, L))$, where $F(K, L)$ denotes the aggregate input. Next, we characterize F as a constant elasticity of substitution production function:

$$Y = F(K, L) = (\alpha(K)^{\varepsilon} + (1-\alpha)(L)^{\varepsilon})^{1/\varepsilon}$$

Profit maximization implies the familiar condition equating the price of capital to its marginal product expressed in similar units:

$$P^K = PF_K(K, L)$$

where P^K is the price of capital services and P is the price of the aggregate input $F(K, L)$ Using $F_K(K, L) = \alpha K^{\varepsilon-1}F(K, L)^{1-\varepsilon}$ this equation can be rewritten as:

$$\frac{P^K}{P} = \alpha\left(\frac{F(K, L)}{K}\right)^{1-\varepsilon}$$

Notice that $1-\varepsilon$ is the inverse of the EOS (that is, $1-\varepsilon = 1/\sigma$). Taking logs and adding a disturbance term that captures potential errors in the firm optimization process, the EOS can be estimated from the following equation:

$$Ln\left(\frac{P^K_{j,t}}{P_{j,t}}\right) = ln(\alpha_j) + \frac{1}{\sigma}ln\left(\frac{F_{j,t}(K_{j,t}, L_{j,t})}{K_{j,t}}\right) + \epsilon_{j,t} \tag{C.1}$$

where we added the subscripts j and t to indicate country and time. We estimate Equation C.1 separately for each industry using OLS. For the data, we rely on the EU KLEMS 2017 database (Jäger, 2017). This readily provides estimates of the flow of capital services, which is computed applying geometric depreciation rates (different by capital asset and industry) to the capital stock data (taken from Eurostat). For a sensitivity analysis, we also estimate a set of EOS using data on the real capital stock to proxy for the flow of capital services. Ideally, for the aggregate input we would need some measure of value added deflated by technical change. Lacking a measure of technical change, we use data on value added (volumes and prices) as proxies. These are contained in Jäger, 2017 and are consistent with Eurostat. To obtain an estimate of the rental price of capital services we follow two alternative approaches. One assumes that the rental rate is proportional to the price of capital services; this is the approach followed, among others, by Baccianti, 2013. In this case, we simply divide the nominal capital stock by the volume of capital services and use that as the rental rate. Our second, and preferred concept, is the 'Jorgensonian' rental rate, which also accounts for the depreciation of capital and the opportunity cost of the investment. Following Jorgenson, 1963, we assume that in equilibrium an investor is indifferent between (i) buying a unit of capital at price q^k_{t-1}, earning a rental fee P^k_t, and selling the depreciated end of period capital to get $q^k_t(1-\delta^k)$, or (ii) earning a nominal interest rate i_{t-1} on a different investment opportunity.[1] Hence, we calculate the rental price of capital, for each industry-country-year observation, as: [2]

$$P^k_{i,j,t} = q^k_{i,j,t-1} i_{j,t-1} + \delta^k_i q^k_{i,j,t} - (q^k_{i,j,t} - q^k_{i,j,t-1})$$

For the nominal interest rate, $i_{j,t}$, we follow O'Mahony and Timmer (2009) to derive an internal rate of return of capital as a residual of capital compensation, depreciation and capital gains. In practice, we calculate $i_{i,j,t}$ as:

$$i_{i,j,t} = \frac{P^K_{i,t} K_{i,j,t} + \sum_k (q^k_{i,j,t} - q^k_{i,j,t-1}) A^k_{i,j,t} - \sum_k \delta^k_i q^k_{i,j,t} A^k_{i,j,t}}{\sum_k q^k_{i,j,t-1} A^k_{i,j,t}}$$

where $A^k_{i,j,t}$ is the capital stock for asset k, in country j, industry i, at time t, and $P^K_{i,t} K_{i,j,t}$ is total capital compensation (calculated as value added minus labor compensation).[3] Due to the limited availability of capital stock and services data in the EU KLEMS database (Jäger, 2017), the estimation is restricted to a sample of 13 countries. [4] One potential concern in estimating Equation C.1 through OLS is that the variables might be non-stationary. If this were to be the case, the estimation would yield biased and inconsistent estimates. To deal with potential non-stationarity, we estimate Equation C.1 in first differences.[5] As observed by Antras, 2004, EOS

1 We do not account for the impact of taxation.

2 $P^k_{i,j,t}$ is calculated for each different type of capital asset k covered in Jäger, 2017: (i) residential structures, (ii) total non-residential investment, (iii) transport equipment, (iv) computing equipment, (v) communications equipment, (vi) other machinery and equipment, (vii) cultivated assets, (viii) other intellectual property products, (ix) research and development, and (x) computer and software database. To obtain a measure of the industry-wide price of capital, $P^K_{i,j,t}$ the price of each asset is multiplied its relative share in capital services and the sum is used as the aggregate price of capital.

3 An alternative (*ex-ante*) approach would be to use an exogenous measure for the rate of return, such as government bond interest rates plus a default risk premium. The (*ex-post*) approach we use does not require us to estimate risk premia and has the further advantage of ensuring consistency between income and production accounts and allows us to obtain country-industry-specific measures of the interest rate. See O'Mahony and Timmer (2009) for more details.

4 These countries are Austria, Czech Republic, Denmark, Finland, France, Germany, Italy, Luxembourg, the Netherlands, Slovak Republic, Spain, Sweden, and the United Kingdom.

5 Another concern is that the regressor in Equation C.1 might be endogenous, as it represents the firms' demand for capital. To deal with potential endogeneity Antras, 2004, who estimates

estimates derived from Equation C.1 might be biased if the assumption of Hicks-neutral technical change does not hold in practice. Therefore, we also relax this assumption and estimate a set of EOS from a production function allowing for labor- (A^l) and capital- (A^k) augmenting technical change. Following the literature, we assume those to grow at constant rates (τ^l) and (τ^k) respectively. The aggregate input is then produced according to:

$$Y = F(K, L, A^k, A^l) = \left(\alpha(A_0^k e^{\tau^k t} K)^\varepsilon + (1-\alpha)(A_0^l e^{\tau^l t} L)^\varepsilon\right)^{1/\varepsilon}$$

Taking the first-order condition equating the price of capital to its marginal product and after simple manipulations, we can estimate the EOS, σ, from the following equation:

$$ln\left(\frac{P_{j,t}^K}{P_{j,t}}\right) = ln(\alpha_j) + \left(\frac{1-\sigma}{\sigma}\right) ln(A_0^k) + \frac{1}{\sigma} ln\left(\frac{F_{j,t}(K_{j,t}, L_{j,t})}{K_{j,t}}\right) + \left(\frac{1-\sigma}{\sigma}\right)\tau^k t + \epsilon_{j,t} \tag{C.2}$$

Practically, this amounts to adding a linear trend to Equation C.1. These various approaches, which rely on different assumptions regarding the capital stock, the user cost of capital and the form of technical change, yield four alternative sets of EOS that are reported in Table C.3 below. The correlation among them ranges from 0.4 to 0.9. On average, our estimated EOS are below one in all four cases, going from 0.68 (when we allow for labor- and capital-augmenting technical change) to 0.85 (when we divide the nominal capital stock by capital services to obtain the rental rate of capital). The EOS is estimated to exceed 1 for just 4 to 6 industries. Our baseline set of EOS (EOS1) are those that assume Hicks-neutral technical change, use capital services data and calculate the rental rate following Jorgenson (1963). The other sets of EOS are used for sensitivity analyses.

an EOS for the aggregate U.S. economy, employs an IV strategy, using the stock of capital owned by the government as an instrument for capital services. Formulating an IV strategy is more problematic in our context since we estimate industry-specific EOS in a panel of countries. We proceed using OLS.

Table C.3: Elasticities of substitution

	EOS1	EOS2	EOS3	EOS4	Industry code
Food, Beverages & Tobacco	0.69	0.74	0.67	0.69	10t12
Textiles	0.38	0.45	0.4	0.36	13t15
Wood, Paper & Reproduction	0.49	0.54	0.49	0.46	16t18
Coke & Refined Petroleum	0.88	0.88	0.89	0.87	19
Chemicals	0.9	0.81	0.88	0.91	20t21
Rubber & Plastics	0.53	0.58	0.52	0.55	22t23
Basic Metals	0.45	0.47	0.94	0.45	24t25
Electrical & Optical	0.58	0.65	0.62	0.57	26t27
Machinery & Equipment	0.49	0.53	0.51	0.49	28
Transport Equipment	0.47	0.52	0.48	0.47	29t30
Others Manufacturing	0.44	0.51	0.4	0.42	31t33
Wholesale & Retail, Motor vehicles	0.47	0.46	0.57	0.47	45
Wholesale ex. Motor Vehicles	0.52	0.65	0.52	0.51	46
Retail ex. Motor Vehicles	0.39	0.36	0.36	0.35	47
Transport & Storage	1.36	0.54	1.55	1.8	49t52
Postal & Courier	0.73	0.6	1.1	0.66	53
Publishing & Audiovisual	0.54	0.47	0.64	0.51	58t60
Telecommunications	1.48	1.05	2.32	1.52	61
IT & Others	0.37	0.47	0.36	0.34	62t63
Agriculture	0.9	0.57	0.7	0.71	A
Mining & Quarrying	0.79	0.69	0.8	0.81	B
Utilities	0.95	0.94	0.87	0.94	DtE
Construction	0.3	0.31	0.35	0.31	F
Accommodation & Food Services	0.47	0.8	0.4	0.43	I
Financial & Insurance	0.88	1.09	1.16	0.83	K
Real Estate	1.2	1.29	1.04	1.2	L
Professional & Support Activities	0.56	0.68	0.67	0.49	MtN
PA, Defense & SS	0.86	1.53	1.39	0.8	O
Education	0.58	1.22	3.08	0.6	P
Health & Social	0.63	0.98	0.98	0.54	Q
Arts & Recreation	1.11	0.82	0.8	1.09	R
Other Service Activities	1.31	1.48	0.74	0.62	S
Average	0.71	0.74	0.85	0.68	/

Notes: Column "EOS1" reports elasticities of substitution (EOS) estimated according to Equation C.1 and using data on capital services and capital rental rates calculated as in Jorgenson (1963), and assuming Hicks-neutral technical change. Column "EOS2" report estimates obtained using capital stock rather than services data. Column "EOS3" report estimates obtained using data on nominal capital stock divided by capital services to proxy for the rental rate. Column "EOS4" report estimates obtained based on Equation C.2.

C.4 Dataset — Summary Statistics of Industry Data

Table C.4: Summary statistics of industry data

	VA share	Labor share	EOS	Layoff rate	Industry code
Food, Beverages & Tobacco	2.66	57.25	0.69	3.03	10t12
Textiles	1.25	77.51	0.38	3.73	13t15
Wood, Paper & Reproduction	1.86	69.14	0.49	3.81	16t18
Coke & Refined Petroleum	0.44	41.42	0.88	1.07	19
Chemicals	2.19	47.82	0.9	2.58	20t21
Rubber & Plastics	1.75	65.16	0.53	2.41	22t23
Basic Metals	2.84	68.34	0.45	3.24	24t25
Electrical & Optical	2.33	62.48	0.58	5.62	26t27
Machinery & Equipment	1.67	71.09	0.49	2.86	28
Transport Equipment	1.85	68.92	0.47	2.77	29t30
Others Manufacturing	1.34	77.38	0.44	7.31	31t33
Wholesale & Retail, Motor vehicles	1.43	69.2	0.47	2.32	45
Wholesale ex. Motor Vehicles	5.69	62.13	0.52	2.69	46
Retail ex. Motor Vehicles	5	76.44	0.39	2.57	47
Transport & Storage	4.75	67.43	1.36	2.94	49t52
Postal & Courier	0.89	82.18	0.73	1.44	53
Publishing & Audiovisual	1.36	65.86	0.54	3.21	58t60
Telecommunications	1.73	39.35	1.48	1.98	61
IT & Others	1.3	78.76	0.37	3.1	62t63
Agriculture	3.35	79.65	0.9	3.61	A
Mining & Quarrying	1.53	41.27	0.79	/	B
Utilities	2.84	34.35	0.95	1.59	DtE
Construction	6.71	78.85	0.3	6.37	F
Accommodation & Food Services	2.69	76.86	0.47	3.08	I
Financial & Insurance	5.95	57.42	0.88	2.34	K
Real Estate	8.95	6.83	1.2	1.9	L
Professional & Support Activities	7.2	72.87	0.56	3.68	MtN
PA, Defense & SS	7.21	76.15	0.86	/	O
Education	4.9	90.2	0.58	1.35	P
Health & Social	6.01	84.21	0.63	1.78	Q
Arts & Recreation	1.12	72.34	1.11	3.4	R
Other Service Activities	1.55	84.49	1.31	2.73	S
Average	3.2	65.73	0.71	3.02	/

Notes: "VA" stands for value added; "EOS" for elasticities of substitution. Shares in value added and labor shares are averages across countries and years, computed in Jäger, 2017. Elasticities of substitution are estimated according to Equation C.1. The natural layoff rate is calculated as the average ratio of displaced over wage and salary employment across the years 2011-2013, computed using individual-level data contained in the 2014 Displaced Workers Survey of the IPUMS-CPS.

C.5 Sylized Facts

In this appendix, we discuss in more detail the stylized facts summarized in Section 4.3.5. Figure C.2 plots the coefficients of year fixed effects from a regression of country-industry labor shares on country-industry fixed effects, year fixed effects and a constant. We observe that the labor share has been on a declining trend since the mid-1970s, with the magnitude of such decline somewhat accelerating in the 1990s. Two peculiar periods are the global recessions of the early 1990s and 2009, during which the labor share increased due to a minimal decline in labor compensation relative to value added. This is in line with the finding of Kehrig and Vincent, 2017 that the labor share tends to modestly increase in recessions, as well as with the presence of sluggish wages as in the model of Rios-Rull and Santaeulalia-Llopis, 2010. By including country-time fixed effects, we ensure that this feature is controlled for in our econometric analysis. We now explore cross-country and cross-industry heterogeneity in the decline of the labor share. In Figure C.3, we plot estimated linear trends in country labor shares for the 22 countries in our sample; the trend is negative and significant in 15 countries.[6] Next, we perform a similar exercise for industry labor shares (Figure C.4).[7] Of the 32 industries considered, 23 display a negative and statistically significant coefficient, whereas only 4 have a significant positive coefficient. We find some differences in the magnitude of the estimated time trends, but no industry emerges as an outlier. Overall, this exercise confirms that the trend decline in the labor share was rather broad-based, taking place both within countries and within industries, while at the same time displaying significant heterogeneity to be explained.[8] Changes in industrial composition could be important drivers of aggregate country labor share trends. Since our analysis focuses mostly on explaining within-industry changes in the labor share, it is important to quantify how much of the overall time-series variation at the country level is explained by within as opposed to between (shifts in industrial composition) changes. To do so, we decompose overall changes according to the following formula (see e.g. Karabarbounis and Neiman,

[6] In Figure C.6 we show linear trends of within-industry labor shares by country. For only two countries (Spain and the United Kingdom) does the sign of the estimated linear trend flip (and is significant) when moving from aggregate country to within-industry labor shares. Importantly, in 13 out of 22 countries we estimate a negative and significant trend, regardless of whether we consider within-industry or aggregate country shares. In Figure C.7 we plot the median, 25th and 75th percentile of industry labor shares for each country in our sample.

[7] Figure C.9 reports estimated linear trends in (global) labor shares for each of the 31 industries. Figure A5.8 shows the median, 25th and 75th percentiles of country-specific labor shares for each industry.

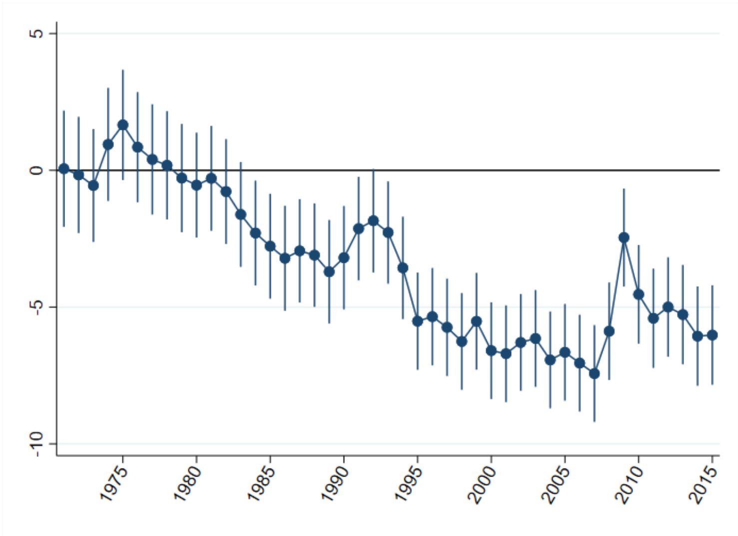
[8] Linear trends are more precisely estimated (lower standard errors) across different countries for specific sectors and industries, rather than across industries for specific countries. This provides further rationale for an econometric specification that, like ours, also considers industry-specific deterministic components.

2013):

$$\Delta LS^j = \sum_i \overline{\omega}_i^j \Delta LS_i^j + \sum_i \overline{LS}_i^j \Delta_i^j \tag{C.3}$$

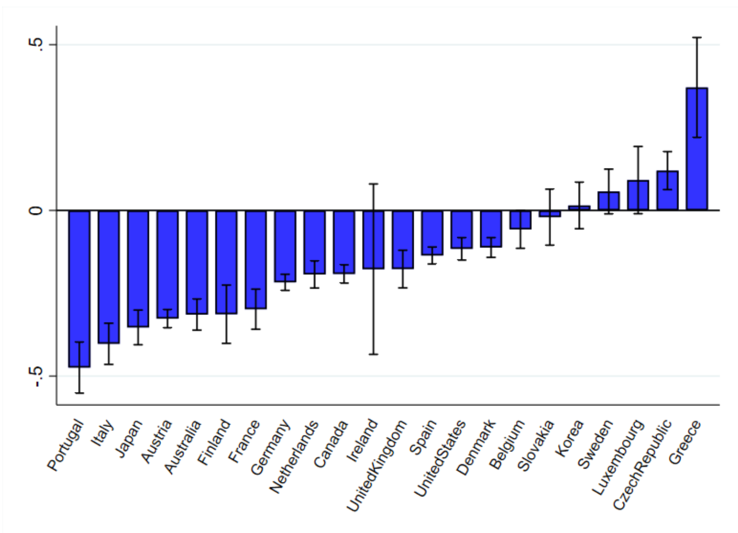
where Δx denotes the estimated linear trend and $\overline{x}$ the mean of variable x. LS refers to the labor share, ω is the share of added value, while superscript j and subscript i denote respectively country and industry. The first and second terms of the right-hand side of Equation C.3 represent the within- and between-industry components of changes in the aggregate country labor share, respectively. Figure C.5 plots the estimated aggregate country trends in the labor share (y-axis) against the within-industry component (x-axis). The linear regression explains about 70 percent of the country variation. This indicates that within-industry changes are more important than changes in industrial composition in explaining movements at the country level, which supports our country-industry-level analysis.

Figure C.2: The global decline in the labor share of income - 1970-2015



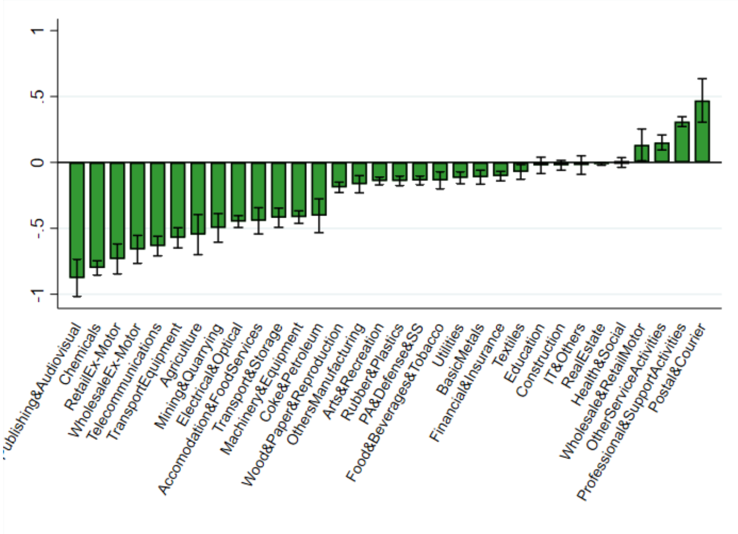
Notes: the figure shows the coefficients of the year fixed effect from the following regression: $LS_{i,j,t} = \alpha + \tau_t + \gamma_{i,j} + \epsilon_{i,j,t}$, where the subscripts i, j and t denote, respectively, industry, country and year. LS is the labor share, α is a constant term, τ are year fixed effects, γ are country-industry fixed effects, and ϵ is an error term. The (blue) red line show estimates from a regression in which industries are (un-)weighted by their relative share. Vertical lines show 1.645 standard errors. Estimates can be interpreted as the average labor share change in percentage points relative to 1970, the base year.
Sources: Jäger, 2017 and own calculations

Figure C.3: Time trends in country labor shares



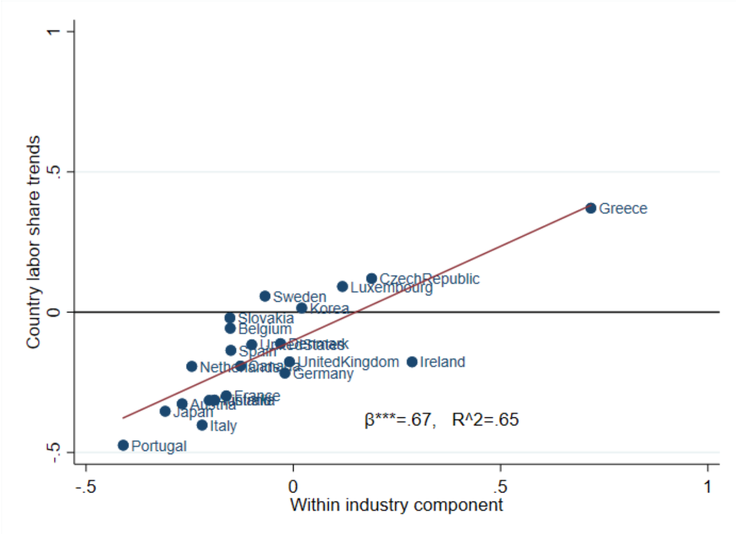
Notes: the figure shows estimated linear trends in industry labor shares (y-axis) for each country. Trends are estimated from the following regressions: $LS^j_t = \alpha^j + \tau^j_t + \epsilon^j_t$ where the subscript t and superscript j denote respectively year and country. LS is the labor share, α is a constant term, τ is the linear trend, and ϵ is an error term. Capped spikes denote 90 percent confidence intervals. Estimates should be interpreted as the average yearly change in country labor shares over the period considered. The period considered is country-specific and depends on the availability of labor income data in the EU KLEMS database, but is 1970-2015 in most cases.
Sources: Jäger, 2017 and own calculations

Figure C.4: Time trends in industry labor shares



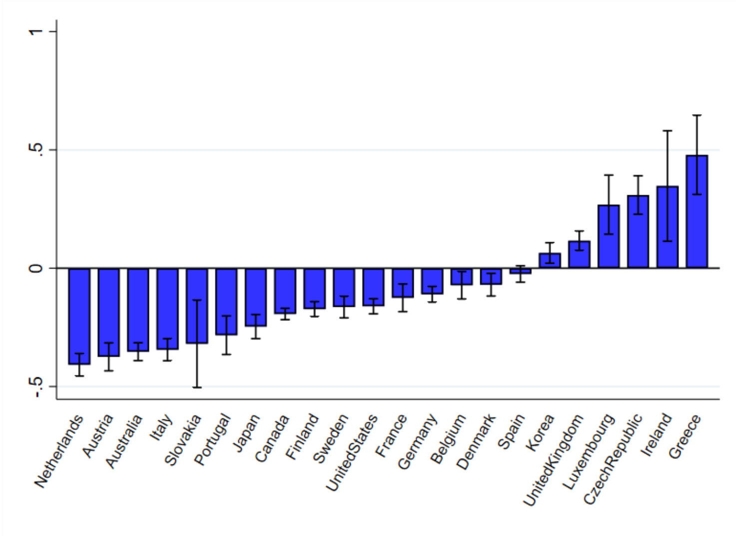
Notes: the figure shows estimated linear trends in aggregate labor shares (y-axis) for each industry. Trends are estimated from the following regressions: $LS_t^i = \alpha^i + \tau_t^i + \epsilon_t^i$, where the subscript t and superscript i denote, respectively, year and industry. LS is the labor share, α is a constant term, τ is the linear trend, and ϵ is an error term. Capped spikes denote 90 percent confidence intervals. Estimates should be interpreted as the average yearly change in industry labor shares over the period considered. The period considered is country- and industry-specific and depends on the availability of labor income data in the EU KLEMS database, but is 1970-2015 in most cases. *Sources:* Jäger, 2017 and own calculations

Figure C.5: Within vs. between industry decomposition of changes in labor shares



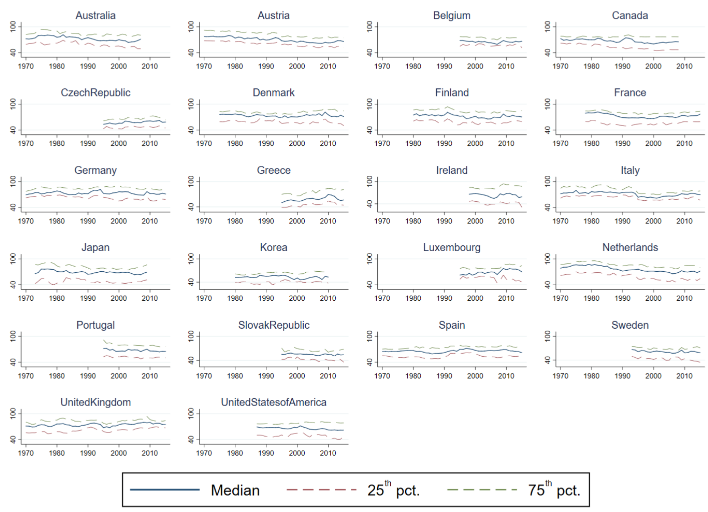
Notes: the figure plots country aggregate labor share trends (y-axis) over the within industry component in labor share trends (x-axis). Country trends are estimated from the following regression: $LS_t^j = \alpha^j + \tau_t^j + \epsilon_t^j$ where the subscript t and superscript j denote respectively year and country. LS is the labor share, α is a constant term, τ is the linear trend, and ϵ is an error term. Within industry components are estimated according to the following expression: $y^j = \sum_i \overline{\omega}_i^j \Delta LS_i^j$, where the superscript j and subscript i denote respectively country and industry, ΔLS denotes the estimated linear trend in the labor share, $\overline{\omega}$ refer to the mean of the share of added value. *Sources:* Jäger, 2017 and own calculations

Figure C.6: Time trends in country-industry labor shares by country



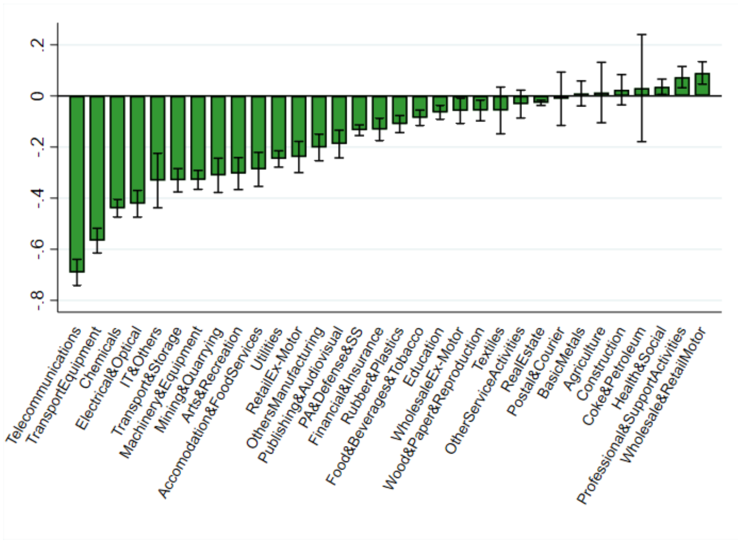
Notes: the figure shows estimated linear trends in industry labor shares (y-axis) for each country. Trends are estimated from the following regressions: $LS^j_{i,t} = \alpha^j + \gamma^j_i + \tau^j_t + \epsilon^j_{i,t}$, where the subscripts i and t denote, respectively, industry and year, while the superscript j denotes country. LS is the labor share, α is a constant term, γ are industry fixed effects, τ is the linear trend, and ϵ is an error term. Capped spikes denote 90 percent confidence intervals. Estimates should be interpreted as the average yearly change in country-industry labor shares over the period considered. The period considered is country-specific and depends on the availability of labor income data in the EU KLEMS database, but is 1970-2015 in most cases. *Sources:* Jäger, 2017 and own calculations

Figure C.7: Country-industry labor shares



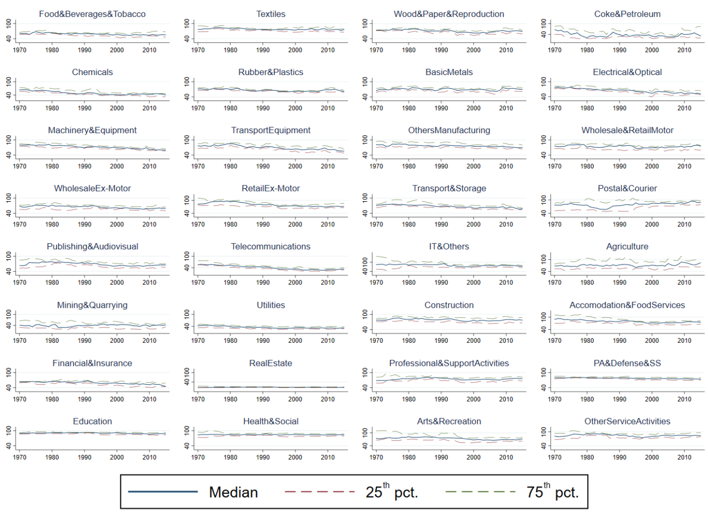
Notes: the figure shows the median (solid blue line), 25th percentile (dashed red line) and 75th percentile (dashed green line) of industry labor shares (x-axis) over time (y-axis), for each country in the sample, from 1970 to 2010. *Sources:* Jäger, 2017

Figure C.8: Time trends in country-industry labor shares by industry



Notes: the figure shows estimated linear trends in country-specific labor shares (y-axis) for each sector. Trends are estimated from the following regressions: $LS^{j}_{j,t} = \alpha^{i} + \gamma^{i}_{j} + \tau^{i}_{t} + \epsilon^{i}_{j,t}$, where the subscripts i and t denote, respectively, industry and year, while the superscript j denotes country. LS is the labor share, α is a constant term, γ are industry fixed effects, τ is the linear trend, and ϵ is an error term. Capped spikes denote 90 percent confidence intervals. Estimates should be interpreted as the average yearly change in country labor shares over the period considered. The period considered is country- and industry-specific and depends on the availability of labor income data in the EU KLEMS database, but is 1970-2015 in most cases. *Sources:* Jäger, 2017 and own calculations

Figure C.9: Industry-country labor shares



Notes: the figure shows the median (solid blue line), 25th percentile (dashed red line) and 75th percentile (dashed green line) of industry labor shares (x-axis) over time (y-axis), for each industry in the sample, from 1970 to 2010. *Sources:* Jäger, 2017

C.6 Robustness Checks on the Industry-country Analysis

Table C.5: Sample composition

	Impact	**1y**	**2y**	**3y**	**4y**	**5y**
Panel A) Identification through layoff rates						
Baseline	0.01	**-0.5**	-0.42	-0.76	**-0.83**	**-0.93**
All manufacturing	-0.06	**-0.33**	-0.22	-0.31	-0.3	**-0.49**
Control group	**-0.25**	**-0.56**	**-0.54**	**-1.04**	**-0.97**	**-0.62**
KLEMS 2017 database	0.06	-0.44	-0.48	**-0.94**	**-0.89**	**-0.96**
Panel B) Identification through elasticities of substitution						
Baseline	**-0.44**	**-0.86**	**-1.28**	-0.93	**-1.24**	**-1.51**
All manufacturing	-0.46	**-0.89**	**-1.25**	-0.86	**-1.15**	**-1.5**
Control group	**-0.73**	**-1.15**	**-1.53**	**-1.64**	**-1.74**	**-1.52**
KLEMS 2017 database	-0.35	-0.66	**-1.2**	**-1.08**	**-1.29**	**-1.51**
Panel C) Identification through layoff rates and elasticities of substitution						
Baseline	-0.47	**-1.22**	**-1.56**	**-1.35**	**-1.42**	**-1.7**
All manufacturing	-0.39	**-0.96**	**-1.05**	-0.8	-0.77	**-1.21**
Control group	**-0.77**	**-1.5**	**-1.75**	**-2.02**	**-1.95**	**-1.66**
KLEMS 2017 database	-0.4	**-1.02**	**-1.57**	**-1.59**	**-1.52**	**-1.77**

Notes: estimates based on Equation 4.10. The row "Baseline" reports estimates obtained from the baseline specification, which includes 22 countries and excludes the (i) Coke, Refined Petroleum and Nuclear Fuel, (ii) Other Manufacturing, (iii) Public Administration, Defense and Social Security, (iv) Education, (v) Health and Social Work, (vi) Agriculture, and (vii) Construction industries. The row "All manufacturing" reports estimates obtained also including industries (i) and (ii). The row "Control group" reports estimates obtained using industries (iii)-to-(vii) as control group. The row "KLEMS 2017 database" report estimates obtained using only the 18 countries covered in Jäger, 2017. Coefficients are in percentage points. Bold numbers indicate significance at the 90 percent confidence interval, based on clustered standard errors at the country-industry level. For Panels B and C standard errors are obtained through bootstrapping (500 replications). For a definition of Panels A, B, C, see notes to Figure 4.4.
Sources: Jäger, 2017, Duval et al., 2018, Flood et al. (2017) and own calculations

Table C.6: Layoff rates

	Impact	1y	2y	3y	4y	5y
Panel A) Identification through layoff rates						
Baseline	0.01	**-0.5**	-0.42	-0.76	**-0.83**	**-0.93**
Qualitative measure	0.01	-0.34	-0.61	**-1.48**	-1.05	-1.46
2013 layoff rate	0.11	**-0.74**	-0.46	-0.68	-0.69	-0.77
Panel B) Identification through layoff rates and elasticities of substitution						
Baseline	-0.47	**-1.22**	**-1.56**	**-1.35**	**-1.42**	**-1.7**
Qualitative measure	0.02	-0.07	-0.1	**-0.2**	**-0.22**	**-0.26**
2013 layoff rate	-0.4	**-1.17**	**-1.37**	**-1.09**	**-1.13**	**-1.27**

Notes: estimates based on Equation 4.10. The row "Baseline" reports estimates obtained from the baseline specification, relying on the average layoff rate calculated using the 2014 Displaced Workers Survey (covering the 2011-2013 period). The row "Qualitative measure" relies on a binary variable that takes value 1 (0) in industries whose layoff rate was above (below) the median for all the three years covered by the 2014 Displaced Workers Survey. The row "2013 layoff rate" report estimates obtained using the layoff rate calculated for the year 2013. Coefficients are in percentage points. Bold numbers indicate significance at the 90 percent confidence interval, based on clustered standard errors at the country-industry level. For Panel B standard errors are obtained through bootstrapping (500 replications).
Sources: Jäger, 2017, Duval et al., 2018, Flood et al. (2017) and own calculations

Table C.7: Elasticities of substitution

	Impact	1y	2y	3y	4y	5y
Panel A) Identification through elasticities of substitution						
Baseline	**-0.44**	**-0.86**	**-1.28**	-0.93	**-1.24**	**-1.51**
Stock	-0.53	-0.52	**-1.49**	-0.78	-0.93	-1.45
Rental rate	-0.3	**-0.83**	-0.82	-0.7	-0.99	**-1.28**
Technical change	-0.38	**-0.72**	**-0.86**	-0.65	**-1.1**	**-1.19**
Panel B) Identification through layoff rates and elasticities of substitution						
Baseline	-0.47	**-1.22**	**-1.56**	**-1.35**	**-1.42**	**-1.7**
Stock	-0.5	-1.05	**-1.92**	**-1.43**	-1.41	**-1.86**
Rental rate	-0.38	**-1.19**	**-1.11**	-1.08	-1.17	**-1.53**
Technical change	-0.41	**-1.09**	**-1.17**	**-1.1**	**-1.26**	**-1.4**

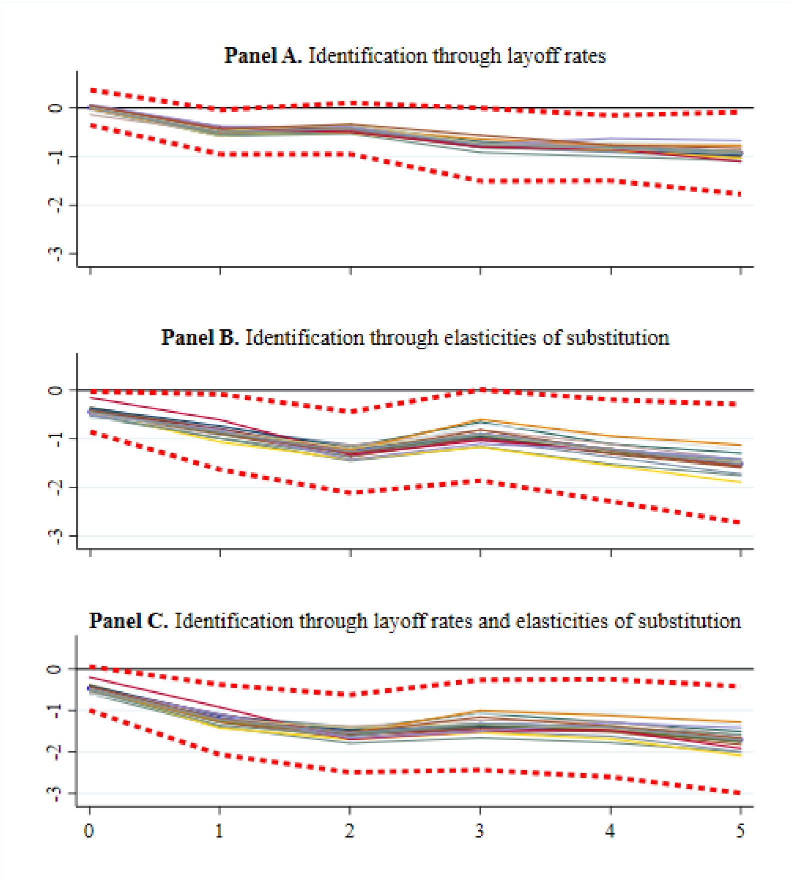
Notes: estimates based on Equation 4.10. The row "Baseline" reports estimates obtained from the baseline specification, relying on elasticities of substitution (EOS) estimated using data on capital services and capital rental rates calculated as in Jorgenson, 1963, and (iii) assuming Hicks-neutral technical change. The row "Stock" report estimates obtained using real capital stock rather than capital services data. The row "Rental rate" report estimates obtained using data on nominal capital stock divided by capital services to proxy for the rental rate of capital. The row "Technical change" report estimates obtained when relaxing the assumption of Hicks-neutral technical change. Bold numbers indicate significance at the 90 percent confidence interval, based on bootstrapped standard errors (500 replications), clustered at the country-industry level.
Sources: Jäger, 2017, Duval et al., 2018, Flood et al. (2017) and own calculations

Table C.8: Omitted variables

	Impact	1y	2y	3y	4y	5y
Panel A) Identification through layoff rates						
Baseline	0.01	**-0.5**	-0.42	-0.76	**-0.83**	**-0.93**
Relative investment price	0.03	-0.42	-0.48	**-0.79**	**-0.84**	**-1.1**
Trade openness	0.01	-0.45	-0.5	**-0.83**	**-0.88**	**-1.13**
Trade union density	0.02	-0.43	-0.48	**-0.81**	**-0.78**	**-1**
Panel B) Identification through elasticities of substitution						
Baseline	**-0.44**	**-0.86**	**-1.28**	-0.93	**-1.24**	**-1.51**
Relative investment price	-0.15	-0.6	**-1.34**	**-1.04**	**-1.24**	**-1.57**
Trade openness	-0.18	-0.63	**-1.36**	**-1.08**	**-1.31**	**-1.67**
Trade union density	-0.16	-0.6	**-1.39**	**-1.11**	**-1.19**	**-1.47**
Panel C) Identification through layoff rates and elasticities of substitution						
Baseline	-0.47	**-1.22**	**-1.56**	**-1.35**	**-1.42**	**-1.7**
Relative investment price	-0.19	**-0.9**	**-1.7**	**-1.51**	**-1.48**	**-1.93**
Trade openness	-0.23	**-0.94**	**-1.72**	**-1.56**	**-1.56**	**-2.02**
Trade union density	-0.2	**-0.91**	**-1.73**	**-1.59**	**-1.41**	**-1.75**

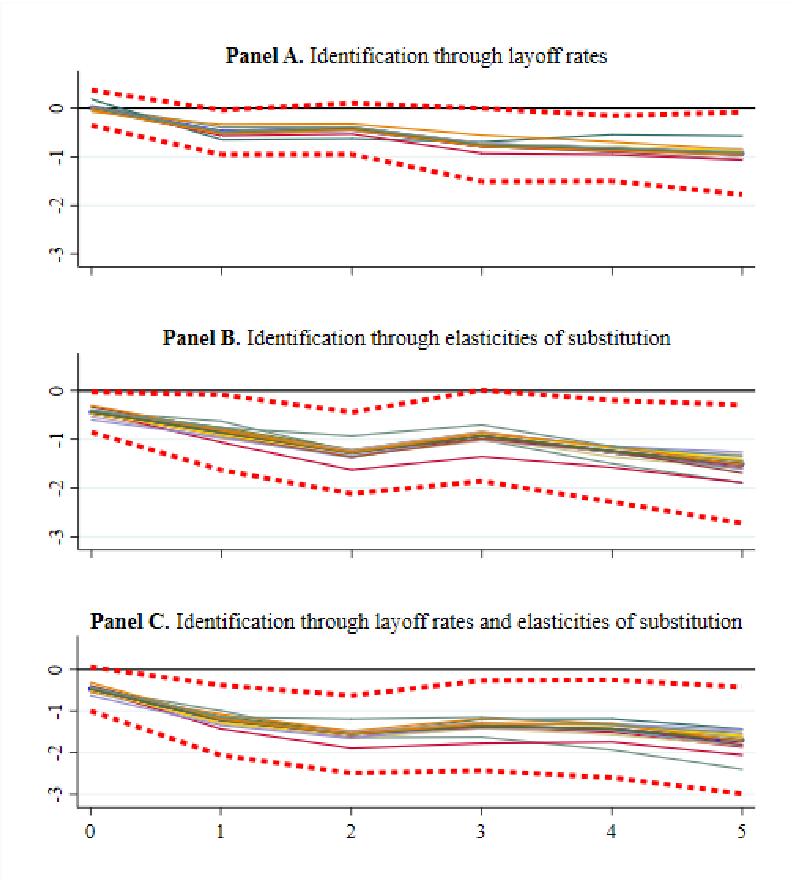
Notes: estimates based on Equation 4.10. The row "Baseline" reports estimates obtained from the baseline specification, relying on elasticities of substitution (EOS) estimated using data on capital services and capital rental rates calculated as in Jorgenson, 1963, and (iii) assuming Hicks-neutral technical change. The row “Stock” report estimates obtained using real capital stock rather than capital services data. The row “Rental rate” report estimates obtained using data on nominal capital stock divided by capital services to proxy for the rental rate of capital. The row “Technical change” report estimates obtained when relaxing the assumption of Hicks-neutral technical change. Bold numbers indicate significance at the 90 percent confidence interval, based on bootstrapped standard errors (500 replications), clustered at the country-industry level.
Sources: Jäger, 2017, Duval et al., 2018, Flood et al. (2017), Visser, 2016, Feenstra, Inklaar, and Timmer, 2015 and own calculations

Figure C.10: Country sample stability



Notes: estimates based on Equation 4.10. Each solid line represents estimates obtained excluding one country at a time. Red dotted lines indicate the 90 percent confidence interval based on standard errors clustered at the country-industry level obtained from the baseline specification, including all countries. For Panels B and C standard errors are obtained through bootstrapping (500 replications). The Y-axis reports the magnitude of the estimated coefficients (in percentage points), while the X-axis reports the response horizon (in years). For a definition of Panels A, B, C, see notes to 4.4.
Sources: Jäger, 2017, Duval et al., 2018, Flood et al. (2017) and own calculations

Figure C.11: Industry sample stability



Notes: estimates based on Equation 4.10. Each solid line represents estimates obtained excluding one industry at a time. Red dotted lines indicate the 90 percent confidence interval based on standard errors clustered at the country-industry level obtained from the baseline specification, including all countries. For Panels B and C standard errors are obtained through bootstrapping (500 replications). The Y-axis reports the magnitude of the estimated coefficients (in percentage points), while the X-axis reports the response horizon (in years). For a definition of Panels A, B, C, see notes to 4.4.
Sources: Jäger, 2017, Duval et al., 2018, Flood et al. (2017) and own calculations

C.7 Back-of-the-envelope Calculations

Let us start by writing the labor share in country j at time t as the weighted sum of industry labor shares, with the weight on each industry i given by its value-added share. That is:

$$LS_t^i \equiv \frac{w_t^j L_t^j}{p_t^j Y_t^j} = \sum_i LS_{i,t}^j \theta_{i,t}^j$$

where $\theta_{i,t}^j$ denotes the added value share of industry i. Assuming for simplicity that EPL does not affect the value-added shares of different industries in the economy, the marginal impact of an EPL reform can be written as:

$$\frac{\partial LS_t^i}{\partial EPL_t} = \sum_i \frac{\partial LS_{i,t}^j}{\partial EPL_t} \theta_{i,t}^j$$

Dividing all industries into two groups, denoted by superscripts 1 and 2, and further considering the impact of EPL reform to be the same within each of these groups, one obtains:

$$\frac{\partial LS_t}{\partial EPL_t} = \frac{\partial (LS_{i,t}^1 - LS_{i,t}^2)}{\partial EPL_t} \theta_{i,t}^1 + \frac{\partial LS_{i,t}^2}{\partial EPL_t}$$

using the fact that $\theta_{i,t}^2 = 1 - \theta_{i,t}^1$ by construction. If we can further assume that EPL reform has a negligible effect on the labor share in group 2 industries, then the impact of EPL reform on the aggregate labor share becomes:

$$\frac{\partial LS_t}{\partial EPL_t} = \frac{\partial (LS_{i,t}^1 - LS_{i,t}^2)}{\partial EPL_t} \theta_{i,t}^1$$

We apply this simple formula in the following way. We split industries into two groups, with the first group having a natural layoff rate above the median and an EOS below 1, and the other one consisting of all remaining industries. We then estimate Equation 4.10 using, as the industry identification variable θ_i a dummy taking value 1/0 for industries belonging to the first/second group. Results are reported in Table C.9. We then further assume that EPL reforms do not have effects in the second group, in line with the statistically insignificant coefficient at the five-year horizon reported in Table 4.3. Applying the five-year-ahead coefficient estimate to the number of net liberalizing reforms in each country over the period considered, we find that changes in EPL may explain about 15 percent of the overall labor share decline. This is roughly similar to the figure obtained using our country-level estimates. [9]

[9] As noted above, this simple back-of-the-envelope calculation assumes that EPL reforms have no reallocation effects across the two groups of industries. This assumption appears to hold in our data. When re-running Equation 4.10 with (log) value added as a dependent variable, we do not find any significant effects of EPL reform. Since this specification controls for aggregate

Table C.9: Extensions on the back-of-the-envelope calculation of aggregate effects

	Impact	1y	2y	3y	4y	5y
Identification through qualitative 0/1 dummy variable						
Labor share	-0.22	**-1.39**	**-1.54**	**-1.58**	**-1.33**	**-1.48**
Value added	0.1	0.64	0.79	1.55	1.38	1.26

Notes: estimates based on Equation 4.10 and using for identification a dummy taking value 1 for industries with an elasticity of substitution below 1 and layoff rate above the median, and 0 otherwise. The rows "Labor share" and "Value added" report estimates obtained using, respectively, the labor share and value added as dependent variables. Bold numbers indicate significance at the 90 percent confidence interval, based on clustered standard errors at the country-industry level.
Sources: Jäger, 2017, Duval et al., 2018 and own calculations

effects by including country-time dummies, the estimated coefficient can be interpreted as the effect of deregulation on the change in value-added shares.

Appendix D

Appendix to Chapter 5

Countries included in the analysis:

- **Advanced Economies (AEs):** Australia, Austria, Belgium, Canada, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hong Kong SAR, Iceland, Ireland, Israel, Italy, Japan, Korea, Latvia, Lithuania, Luxembourg, Macao SAR, Malta, Netherlands, New Zealand, Norway, Portugal, Puerto Rico, Singapore, Slovak Republic, Slovenia, Spain, Sweden, Switzerland, Taiwan Province of China, United Kingdom, United States

- **Emerging Markets and Developing Economies (EMDEs):** Albania, Argentina, Armenia, Azerbaijan, Bangladesh, Barbados, Belize, Bhutan, Bolivia, Bosnia and Herzegovina, Botswana, Brazil, Bulgaria, Chile, Colombia, Costa Rica, Croatia, Dominican Republic, Ecuador, Egypt, El Salvador, Ethiopia, FYR Macedonia, Georgia, Guatemala, Honduras, Hungary, Indonesia, Iran, Jamaica, Kyrgyz Republic, Mauritius, Mexico, Moldova, Mongolia, Montenegro, Morocco, Pakistan, Panama, Paraguay, Peru, Philippines, Poland, Qatar, Romania, Russian Federation, Saudi Arabia, Serbia, South Africa, Sri Lanka, Suriname, Thailand, Trinidad and Tobago, Tunisia, Turkey, Uruguay, Venezuela, Zimbabwe.

Table D.1: Robustness checks on baseline specification – time fixed effects

	AEs			EMDEs		
	β	**s.e.**	R^2	β	**s.e.**	R^2
All working age	-0.29**	-0.06	0.53	-0.16**	-0.03	0.17
Adult women	-0.22**	-0.04	0.39	-0.12**	-0.04	0.11
Adult men	-0.27**	-0.06	0.48	-0.13**	-0.03	0.16
Youth women	-0.5**	-0.1	0.43	-0.21**	-0.07	0.1
Youth men	-0.62**	-0.13	0.5	-0.29**	-0.06	0.16

Notes: the table presents estimates obtained estimating an alternative specification including time fixed effects. Standard errors, clustered at the country level, are in parenthesis. *, and ** denote significance at the 90 percent, and 1 percent confidence level, respectively. AEs and EMDEs stand respectively for advanced economies and emerging markets and developing economies. The sample of AEs comprises 38 countries and 908 observations. The sample of EMDEs comprises 57 countries and 751 observations.
Sources: Authors' estimation based on ILO Key Indicators of the Labour Market and IMF World Economic Outlook.

Table D.2: Robustness checks on baseline specification – per capita output gap

	AEs			EMDEs		
	β	**s.e.**	R^2	β	**s.e.**	R^2
All working age	-0.31**	-0.05	0.47	-0.16**	-0.03	0.13
Adult women	-0.21**	-0.04	0.34	-0.14**	-0.03	0.07
Adult men	-0.3**	-0.06	0.43	-0.13**	-0.03	0.12
Youth women	-0.53**	-0.1	0.37	-0.22**	-0.07	0.05
Youth men	-0.67**	-0.12	0.45	-0.29**	-0.06	0.11

Notes: the table presents estimates obtained estimating Equation (1) and using real GDP per capita to compute the output gap. For other notes and sources refer to Table D.1.

Table D.3: Robustness checks on baseline specification – WEO output gap

	AEs			EMDEs		
	β	**s.e.**	R^2	β	**s.e.**	R^2
All working age	-0.31**	-0.04	0.39	-0.25**	-0.05	0.21
Adult women	-0.22**	-0.03	0.29	-0.2**	-0.04	0.11
Adult men	-0.31**	-0.04	0.37	-0.2**	-0.04	0.17
Youth women	-0.53**	-0.07	0.28	-0.33**	-0.11	0.08
Youth men	-0.68**	-0.09	0.36	-0.47**	-0.1	0.17

Notes: the table presents estimates obtained estimating Equation 5.1 and using the output gap as estimated in the IMF World Economic Outlook. AEs and EMDEs stand respectively for advanced economies and emerging markets and developing economies. The sample of AEs comprises 38 countries and 908 observations. The sample of EMDEs comprises 36 countries and 493 observations. For other notes and sources refer to Table D.1.

Table D.4: Robustness checks on baseline specification – sample composition

	AEs			EMDEs		
	β	**s.e.**	R^2	β	**s.e.**	R^2
All working age	-0.27**	-0.07	0.39	-0.13**	-0.03	0.1
Adult women	-0.2**	-0.05	0.27	-0.12**	-0.03	0.06
Adult men	-0.25**	-0.06	0.36	-0.11**	-0.03	0.09
Youth women	-0.45**	-0.12	0.28	-0.22**	-0.06	0.05
Youth men	-0.59**	-0.14	0.37	-0.25**	-0.05	0.1

Notes: the table presents estimates obtained estimating Equation 5.1 excluding from the sample new EU member states, Taiwan and Korea. AEs and EMDEs stand respectively for advanced economies and emerging markets and developing economies. The sample of AEs comprises 30 countries and 728 observations. The sample of EMDEs comprises 52 countries and 650 observations. For other notes and sources refer to Table D.1.

Table D.5: Robustness checks on baseline specification – first difference specification

	AEs			EMDEs		
	β	**s.e.**	R^2	β	**s.e.**	R^2
All working age	-0.24**	-0.05	0.35	-0.18**	-0.03	0.14
Adult women	-0.16**	-0.03	0.21	-0.14**	-0.03	0.14
Adult men	-0.23**	-0.05	0.33	-0.16**	-0.03	0.17
Youth women	-0.41**	-0.09	0.23	-0.29**	-0.06	0.17
Youth men	-0.55**	-0.11	0.32	-0.35**	-0.05	0.22

Notes: the table presents estimates obtained estimating an alternative, first difference, specification where the potential levels of GDP growth rate and the unemployment rate are assumed to be constant and for the dependent and explanatory variables the first difference in the unemployment rate and in log output are used. For other notes and sources refer to Table D.1.

Appendix E

Summary in Dutch

Samenvatting in het Nederlands

De mondiale financiële crisis (GFC) van 2007-2009 betrof de grootste economische neergang sinds de Grote Depressie. Hoewel de beleidsreactie van overheden van land tot land verschilde, waren patronen te onderscheiden in het macro-economische beleid in ontwikkelde economieën. Dit proefschrift identificeert drie van deze beleidsterreinen - (i) kwantitatieve verruiming en forward guidance door centrale banken, (ii) bezuinigingen van de overheidsfinanciën en (iii) deregulering van de arbeidsmarkt - en bestudeert de effecten van dit beleid op een aantal economische en financiële variabelen. Hoofdstuk 2 richt zich op het onconventionele monetaire beleid van de Amerikaanse Federal Reserve (Fed) in reactie op de GFC en laat zien dat dit beleid van invloed is geweest op de reactie van kapitaalstromen op economische schokken. Hoofdstuk 3 onderzoekt de middellangetermijneffecten van bezuinigingen in de vorm van belastingverhogingen op inkomensongelijkheid. De analyse suggereert dat stijgingen van de belastinginkomsten in de nasleep van de GFC hoogstwaarschijnlijk niet hebben bijgedragen aan een toename van ongelijkheid. Integendeel, dit beleid heeft de ongelijkheid mogelijk verlaagd. Hoofdstuk 4 onderzoekt of periodes van deregulering van werkgelegenheidsbescherming schadelijke gevolgen hebben voor de inkomensverdeling tussen arbeid en kapitaal. Uit de analyse volgt dat dit inderdaad het geval is. Hoofdstuk 5 bestudeert een van de effecten van de GFC - namelijk de grote stijging van de jeugdwerkloosheid - in plaats van een van de beleidsreacties. Uit het hoofdstuk blijkt dat het effect van de conjunctuur op de werkloosheidskloof (het verschil tussen de werkloosheidsgraad en het evenwichtspercentage) gemiddeld twee keer zo groot is voor jongeren als voor volwassenen, omdat arbeidsvoorwaarden voor jongeren meer kwetsbaar zijn.

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