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Essays in Labour Economics: Employers and Employees behavior and labour market outcomes in a period of economic crisis

By

ANDREAS SINTOS

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Consulting Committee:

1) Theologos-Michail Chletsos, Professor at the Department of Economics, University of Piraeus (Supervisor)
2) Nikolaos Mylonidis, Professor at the Department of Economics, University of Ioannina
3) Antonis Adam, Associate Professor at the Department of Economics, University of Ioannina

Examination Committee:

1) Theologos-Michail Chletsos, Professor at the Department of Economics, University of Piraeus (Supervisor)
2) Nikolaos Mylonidis, Professor at the Department of Economics, University of Ioannina
3) Antonis Adam, Associate Professor at the Department of Economics, University of Ioannina
4) Nikos Benos, Associate Professor at the Department of Economics, University of Ioannina
5) Evangelia Papapetrou, Professor at the Department of Economics, National and Kapodistrian University of Athens
6) Christos Kollias, Professor at the Department of Economics, University of Thessaly
7) Athina Economou, Assistant Professor at the Department of Economics, University of Thessaly

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Abstract

The subject of the present dissertation focuses on four different topics related to labour economics. Therefore, the present doctoral thesis consists of four chapters and in each chapter a research question is investigated. In the first chapter, we examine the effect of International Monetary Fund (IMF) intervention on the labour market outcomes of recipient countries. The second chapter deals with whether participation in temporary work can lead to job stability in the post-crisis period. The main research question in the third chapter addresses whether participation in performance pay schemes can affect job satisfaction. Finally, the fourth chapter provides a meta-analysis of previous empirical literature that explores the relationship between financial development and income inequality in order to show if there is any publication selection bias in the current literature and the overall economic effect of financial development on income inequality.

The fundamental mission of the IMF is to ensure global financial stability and to assist countries in economic turmoil. Although there is a consensus that IMF-supported programs can have a direct effect on the labour market of recipient countries, it remains unclear how IMF participation decision and conditionalities attached to IMF loans can affect various labour market outcomes of borrowing countries. The first chapter of the thesis explores the effect of IMF lending programs on the labour market outcomes of borrowing countries. Specifically, this chapter is divided into three subchapters. The first subchapter[1] explicates the potential

[1] The first subchapter of this dissertation was presented at the 18th Conference on Research on Economic Theory and Econometrics (CRETE 2019) (http://www2.aueb.gr/conferences/Crete2019/#program) and at the Political Economy of International Organization (PEIO) 2021 seminar series (https://www.peio.me/peio-seminar/), and was published in the Structural Change and Economic Dynamics journal, Volume 59, pp 292-319 in December 2021 (https://doi.org/10.1016/j.strueco.2021.09.008).

mechanisms through which IMF intervention operates to investigate the effect of IMF intervention on the shadow economy using a panel of 141 countries from 1991 to 2014. The empirical analysis addresses sources of endogeneity related to, first, the IMF participation decision and, second, the conditions included within the program. The empirical findings suggest that both IMF program participation and conditionality increase the size of the shadow economy. Disaggregating IMF conditions into structural and quantitative shows that only structural conditions are significantly related to a larger shadow economy both in the short- and long-term. Financial development can reduce the size of the shadow economy, yet it cannot reverse the detrimental effect from IMF conditions.

The second subchapter investigates how lending conditional programs of the IMF affect the unemployment rate of recipient countries. The empirical analysis applies four different econometric approaches to account for the selection bias issue using a panel of 96 countries for the period of 1971-2015. The empirical findings suggest that IMF loan programs have detrimental effects on the unemployment rate. These detrimental effects remain robust across alternative specifications and using alternative measures of IMF loan arrangements. There is evidence that significant short-run effects hold robust in the long-run. Lastly, the empirical findings indicate that the conditions – policy reforms included within the program increase the unemployment rate.

The third subchapter examines the effect of IMF lending programs on income inequality of recipient countries for the period 1963-2015. The empirical approach that this subchapter utilizes to deal with selection bias follows a potential outcomes framework, which does not rely on the selection of matching variables and has the further advantage of uncovering the effect of the treatment on the outcome variable over time. The results obtain from this approach indicate an increase in income inequality. However, the effect of some targeted IMF programs, which allow the involvement of civil society to their design and grant governments larger scope in negotiating the policy conditions, shows no significant evidence on income inequality, supporting the view of a new policy strategy which does not worsen income inequality.

Temporary work is a significant and growing component of total employment in many developed countries. The second chapter[2] of the thesis investigates how temporary work affects the employment stability – employability of individuals in the case of the Netherlands for the period 2008 to 2018. Several models of estimation are employed, including proper econometric techniques to account for potential endogeneity. The empirical findings suggest that temporary contracts are negatively associated with employment stability. Nevertheless, this negative effect can be mitigated for temporary employees who have developed their professional skills through training programs. Therefore, this chapter indicates that the employment prospects of temporary employees in the Netherlands are poor unless they are highly skilled. Thus, the act of providing training opportunities to jobseekers, with the joint involvement of all actors (the Dutch government, employers and employees), can improve the development of skills of temporary workers and in the future provide employment prospects for better quality jobs.

The importance of differences in payment schemes and how these differences are affecting employees, employers and firms is not a new topic, but is one that has recently attracted increasing attention from academic scholars and compensation specialists. In the third chapter, we place the spotlight on the effect of performance pay schemes on individuals' job satisfaction. Using a sample of countries from the European Working Conditions Surveys

[2] The second chapter of this dissertation was presented at the 13th Workshop on Labour Economics organized by the Institute for Labour Law and Industrial Relations in the European Union (IAAEU) and the Chair of Personnel Economics at Trier University, 25-26 March, 2021 (https://www.iaaeg.de/images/workshop/WLE_2021-Programme.pdf).

2010–2015 (EWCS), the empirical results suggest that performance pay has a positive and statistically significant effect on job satisfaction. This finding remains significantly positive for all examined schemes of performance pay and is conditional upon worker's sleep quality and work-related exhaustion. The latter suggests that the positive effect of performance pay schemes on job satisfaction is stronger for performance pay workers who have better sleep quality and experience less work-related exhaustion. A series of robustness checks (including proper econometric techniques to account for selection bias) confirm these general patterns.

The development of financial markets and institutions has recently been shown to be an important factor of the distribution of income in the developed and developing world. The current evidence, however, regarding the effect of financial development on income inequality is quite mixed. To this end, the fourth chapter[3] conducts a meta-analysis of 1,329 estimates reported in 88 published studies that investigate the effect of financial development on income inequality. The empirical results suggest downward publication bias (i.e., the current literature favors the publication of studies which find that financial development decreases income inequality). To correct for publication bias we use linear and non-linear methods, and our findings report that the overall effect of financial development on income inequality is practically zero. Finally, the heterogeneity analysis shows that various study characteristics, such as controlling for endogeneity, the characteristics of data and estimation methods, the different measurement of financial development and the composition of countries matter significantly for the effect of financial development on inequality.

[3] The fourth chapter of this dissertation was presented at the 2021 Meta-Analysis in Economics Research (MAER) Network Colloquium at the University of Piraeus, Piraeus city, Greece (https://maer-net-athens-2021.org/).

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Introduction

The International Monetary Fund (IMF) is considered to be one of the most powerful international organizations (Stone, 2002), with its membership rising from 44 states in 1946 to 190 at present. The IMF provides financial assistance to countries in economic trouble in exchange for the implementation of IMF-mandated policy reforms. The IMF's lending programs are known for their strict and binding policy reforms (Kentikelenis et al., 2016). These policy reforms are able to affect the structure of the economy of the recipient countries. It is well-documented that IMF loan programs can have multi-level effects on borrowing countries, including their growth rates (Barro and Lee, 2005), poverty (Oberdabernig, 2013), health equity (Forster et al., 2020), labour rights (Lloyd and Weissman, 2002), public spending (Rickard and Caraway, 2019) and tax revenues (Crivelli and Gupta, 2015).

The first chapter of the present thesis investigates the impact of IMF intervention on the labour market of borrowing countries. Specifically, Chapter I is divided into three subchapters. The first subchapter of Chapter I examines the impact of IMF intervention (IMF participation and conditionality) on the informal economy using a panel of 141 developed and developing countries from 1991 to 2014. To address sources of endogeneity related to, first, the IMF participation decision and, second, the conditions included within the program, we follow the methodological approach proposed by Stubbs et al. (2020). The empirical findings suggest that both IMF program participation and conditionality increase the size of the shadow economy. Furthermore, we disaggregate IMF conditions into structural, which concern a wider range of reforms in the domestic economy (microeconomic reforms) and afford governments less flexibility in the construction of policy reforms, and quantitative conditions, which take the form of quantitative targets that countries have to meet and provide governments more flexibility, and we show that only structural conditions are significantly related to a larger shadow economy both in the short- and long-term. Financial development, a crucial factor of underground activities, can reduce the size of the shadow economy, yet it cannot reverse the detrimental effect from IMF conditions.

The second subchapter investigates the effect of IMF loan programs on the unemployment rate of borrowing countries using a sample of 97 countries across the world for the period 1971-2015. To account for the selection bias issue, we utilize proper econometric techniques which include two-stage panel sample selection methods (Vella, 1993; Vella and Verbeek, 1999), Heckman's (1979) two-stage procedure, two-stage least squares with instrumental variables and Stubbs' et al. (2020) technique. We first show that IMF loan-quota ratio and program participation both have a detrimental effect on the unemployment rate. These adverse effects persist in the long-term. In addition, we provide evidence that the conditions – policy reforms included within the program increase the unemployment rate.

In the third subchapter of Chapter I, we provide an evaluation on the effect of IMF lending programs on income inequality using a sample of 147 countries for the time period 1963 to 2015. To deal with selection bias we apply a potential outcomes framework proposed by Acemoglu et al. (2018), which does not rely on the selection of matching variables and has the further advantage of uncovering the effect of the treatment on the outcome variable over time. The findings suggest that IMF lending programs have a detrimental effect on income inequality. Furthermore, we conduct our analyses by considering only some specific IMF programs, which allow the involvement of civil society to their design and grant governments larger scope in negotiating the policy conditions. This part of our analyses indicates that some targeted IMF programs (specifically PRGF and ECF programs) have no significant effect on income inequality.

Temporary employment, a flexible form of employment, is a significant and growing component of total employment in many OECD countries (Vosko, 2008). It is well-documented that temporary work directly affects the job satisfaction (Waaijer et al., 2016), work safety (Amuedo-Dorantes, 2002) and health (Virtanen, 2005) of temporary workers, as well as job and income insecurity (Silla, 2005). In addition, the literature of labour economics points out that there are two opposing scenarios for the employment effects of temporary work. More specifically, some scholars indicate that temporary work functions as a stepping stone to regular jobs (e.g., Booth et al., 2002; Picchio, 2008; Steijn et al., 2006), while others connect temporary employment to a dead end (e.g., De Graaf-Zijl et al., 2009; D'Addio and Rosholm, 2005; Giesecke and Groß, 2003). The use of temporary work and its effects on the labour market often constitute a political debate among politicians of different political persuasions, scholars, employers and employees.

Chapter II provides an examination of the effect of temporary work on employment stability – employability for the case of the Netherlands, a developed country with relatively high shares of temporary employment among the EU countries (Eurostat, 2018a) and unique labour market institutions (De Lange, 2013; De Jong et al., 2007; Fagan and Ward, 2003). To examine the above benchmark relationship, we use pooled and fixed effects regressions, and we also account for potential endogeneity (e.g., selection bias) by employing Lewbel's (2012) two-stage least squares approach. Our main findings suggest that temporary work is negatively associated with employment stability – employability. The effect of temporary work is stronger for unmarried and foreign individuals. Nevertheless, Chapter II delves deeper in the nexus between temporary work and employment stability by considering temporary employee's participation in training programs. As such we examine the effect of temporary work on employment stability conditional on the number of training programs. The findings suggest that the adverse effect of temporary work can be mitigated for temporary employees who have developed their professional skills through training programs.

According to Murphy and Cleveland (1995), the use of performance-related pay is one of the most important human resource management practices. A performance pay system is often used by supervisors-employers to align the objectives of employees with those of the firm and to motivate and reward employees (Milkovich and Newman, 2004). In addition, a variety of decisions such as future promotions, pay raises, and career development moves are based on the results of performance pay schemes (Kampkötter, 2017). Previous studies show that performance-related pay can have multi-level effects on employees (e.g. wage inequality (Lemieux et al., 2009) and worker training (Gielen, 2007; Koffarnus et al., 2013)), the relationship between employers and employees (e.g. effects on the quality of relations of employees with the boss (Green and Heywood, 2010; Heywood et al., 2005)), and the firm as whole (e.g. effects on output and productivity (Dohmen and Falk, 2011; Lazear, 2000), profits (Bhargava, 1994), innovation (Harden et al., 2010) and employment growth (Gielen et al., 2009)). As such, the decision making regarding the differences in payment systems can affect both employers and employees, as well as the relationship between them.

Chapter III investigates the effect of performance pay schemes on job satisfaction using a sample of countries from the European Working Conditions Surveys 2010–2015 (EWCS). This data allows us to disaggregate performance pay schemes into department, company and individual performance-related pay. Because of the ordinal nature of the dependent variable, job satisfaction, ordered probit regressions are utilized and the findings suggest that all disaggregated performance pay schemes are associated with increased job satisfaction even after controlling for earnings. Moreover, we conduct sub-sample analyses to check if the job satisfaction effects of performance pay vary by gender, by sector and by firm size. To be specific, we show that women and private sector employees experience a greater satisfaction with their jobs under a performance pay scheme. The effect of individual performance pay on job satisfaction is higher in larger firms, while a greater effect is reported for group performance pay schemes in smaller firms. Furthermore, we explore two additional possible channels for which our data is well-suited. Specifically, we investigate how performance pay schemes affect job satisfaction conditional on self-reported sleep quality and work-related exhaustion. This analysis points out that the positive effect of performance-related pay on job satisfaction is stronger for performance pay workers who have better sleep quality and experience less work-related exhaustion. Our main findings remain robust even after controlling for selection bias using the Heckman's two-stage procedure and propensity score matching (PSM).

The literature of financial economics points out that financial development can have a direct effect on various aspects of the economy, such as economic growth (Christopoulos and Tsionas, 2004), trade openness (Beck, 2002), innovation (Hsu et al., 2014), productivity and investment (Benhabib and Spiegel, 2000). Another well-documented effect of financial development is on income inequality. Specifically, the literature indicates that the development of financial markets and institutions can shape the gap between the rich and the poor and the degree to which that gap persists across generations (Demirgüç-Kunt and Levine, 2009). However, the available theories in the current literature indicate that the impact of financial development on income inequality is controversial. The empirical literature regarding the distributional effects of financial development also provides conflicting results. Therefore, the current literature lacks a clear view on its impact. The magnitude of the estimated effect regarding the relationship between financial development and income inequality varies greatly.

Is there a stable relationship between financial development and income inequality? Does financial development protect the wealthy classes or provide more opportunities for low-income classes? Why do different studies reach such different conclusions? In order to answer the above questions and resolve the existing disputes, the meta-analysis method is utilized in the final chapter (Chapter IV) of the present thesis. The meta-analysis method incorporates rigorous quantitative survey techniques in order to disentangle the different factors driving the estimated effects (Stanley and Doucouliagos, 2012). This approach is used in the fourth chapter of the thesis to examine the results from the empirical literature and results in a total of 88 separate published studies containing 1,329 estimates of the effect of financial development on income inequality. The empirical findings from the meta-analysis suggest downward publication bias (i.e., the current literature favors the publication of studies which find that financial development decreases income inequality). After correcting for publication bias using linear and non-linear methods our findings indicate that the distributional effect of financial development is practically zero. Lastly, we explain the heterogeneity of the research using Bayesian model averaging (BMA) estimation in order to address model uncertainty. The findings from the heterogeneity analysis indicate that various study characteristics, such as controlling for endogeneity, the characteristics of data and estimation methods, the different measurement of financial development and the composition of countries matter significantly for the effect of financial development on inequality.

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Chapter I: The effect of IMF lending programs on the labour market outcomes

Subchapter I.I: Hide and seek: IMF intervention and the shadow economy – An empirical investigation

1.1.1 Introduction

The size of the shadow economy[4] worldwide is alarming (Buehn and Schneider, 2012). Medina and Schneider (2018) define the shadow economy as follows: "The shadow economy includes all economic activities which are hidden from official authorities for monetary, regulatory, and institutional reasons" and their new estimates highlight that over the last 25 years (from 1991 to 2015) the average size of the shadow economy of 158 countries is more than 25% of the average official GDP of these countries (see Figure 1.1.1). Although in the last years the size of the underground economy has decreased (based on the overall estimates of Medina and Schneider (2018), the average decline of the shadow economy from 1991 to 2015 is 5.3 percentage points), it remains a widespread and complex economic phenomenon in developed and developing world to varying degrees (Elgin and Oztunali, 2012; Medina and Schneider, 2018; Torgler and Schneider, 2009).

[4] The literature also uses other expressions such as informal, unofficial, hidden, black, parallel, second or underground economy (or sector) (e.g., Bagachwa and Naho, 1995; Capasso and Jappelli, 2013; Contini, 1981; Elgin and Oyvat, 2013; Giles, 1999; Ihrig and Moe, 2004; La Porta and Shleifer, 2009; Thomas, 1999). All these terms are used interchangeably.

Shadow economy activities lead to great losses of tax revenues and social security payments to the state (Buehn and Schneider, 2007), thus creating a vicious cycle of continuous inefficient increases of tax rates. When tax revenues decrease due to an increase in informal activities, governmental revenues decrease and in turn, the provision of public goods and services deteriorates. Therefore, the informal economy reduces public finance and investment, which in turn influence the development of the economy (Schneider et al., 2010). The agents participating in the informal economy absorb efficient labour and capital resources from the official economy, yet they are unproductive and add minimum value to the overall economy (Schneider et al., 2010). Most importantly, a shadow economy has a negative impact on a country's economic growth. Enste (2018) highlights that by worsening fiscal deficits and reducing infrastructure investment, the shadow economy reduces welfare and economic

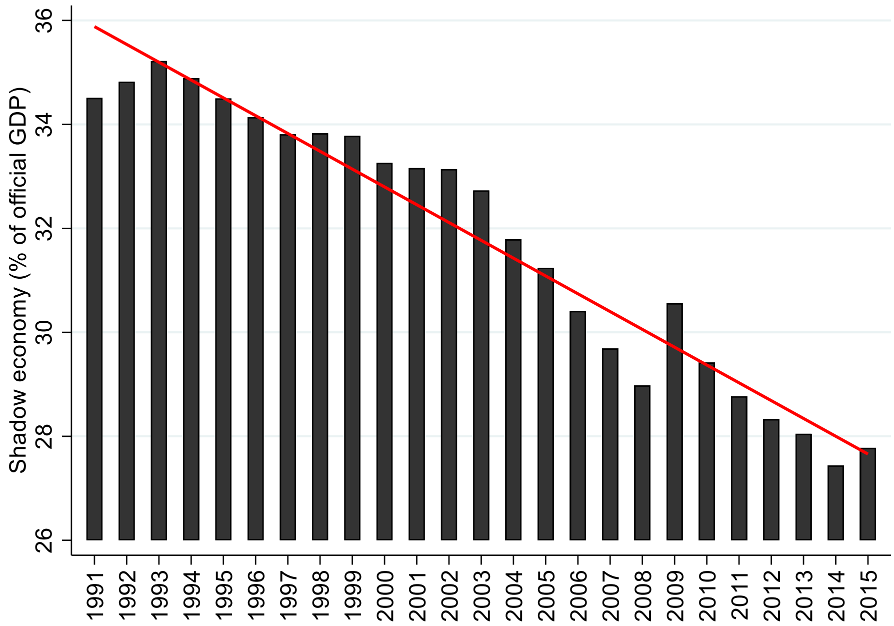


Figure 1.1.1 The average size of the shadow economy (% of official GDP) of 158 countries by year, from Medina and Schneider (2018)

growth. Loayza (1996) points out that the shadow economy is associated with a reduction in the official economic growth in Latin America due to a reduction in the provision of public goods and services and inefficient resource allocation. The informal economy can undermine state institutions, leading to more crime and less support for institutions, ultimately threatening economic and political development (Enste, 2018). Therefore, it is also characterized as an indicator of low institutional quality and illegitimacy (Scheinder and Enste, 2000). Another highlighted impact of informal activities is that they create distortions in the official statistics and thus provide policymakers with wrong indicators, leading to ineffective macroeconomic policies (Fleming et al., 2000; Scheinder and Enste, 2000; Tanzi, 1999).

The literature has identified several factors that affect the size and development of the underground economy. Among others, tax burden (Gërxhani, 2004; Johnson et al., 1997; Loayza et al., 2009; Schneider and Enste, 2000) and the quality of institutions (which consists of a variety of sub-factors, e.g., good governance, control of corruption, bureaucratic quality, rule of law, political instability, etc. (Dreher and Schneider, 2009; Dreher et al., 2009a; Elbahnasawy et al., 2016; Torgler and Schneider, 2009)) are some of the main drivers of the spread of the shadow economy. In addition, other determinants such as economic freedom (Berdiev and Saunoris, 2018; Berdiev et al., 2018), financial development (Berdiev and Saunoris, 2016; Capasso and Jappelli, 2013), and the cost of doing business (Goel and Saunoris, 2019; Loayza, 1996) have also been documented. Analysing and identifying factors of the underground economy is still ongoing (Friedman et al., 2000; Goel and Nelson, 2016; Goel et al., 2019), however based on the well-established studies in this field, we examine the effect of IMF intervention on the size of the unofficial economy.

As mentioned above, most of the documented determinants of the shadow economy are related to the structure of country's domestic economy (e.g., tax structure and institutional records), while others are associated with country's macroeconomic aspects, such as trade openness and financial development. The IMF along with the World Bank and regional development banks are singled out as the most powerful agents of economic reform (Kentikelenis and Seabrooke, 2017; Stone, 2011; Steinwand and Stone, 2008). The main role of the IMF is to uphold global financial stability, which endows the Fund with an unrivalled position as a global lender of last resort (Babb and Kentikelenis, 2018; Woods, 2006). Through its conditional lending programs, the IMF promoted pro-market policies (Chang, 2006; Kentikelenis et al., 2016; Vreeland, 2003; Woods, 2006). In exchange for low-cost financing, the IMF requires governments to implement a set of IMF-designed policy reform packages – or 'conditionality' – administered through a lending program.[5] IMF's structural adjustment involves a series of reforms in sensitive policy areas, such as privatization of state-owned enterprises, trade and financial liberalization, economic deregulation, social policy, labour market reforms, and 'good governance' (Babb, 2013; Chang, 2006; Rodrik, 2002; Serra and Stiglitz, 2008; Stiglitz, 2002; Stubbs et al., 2016; Summers and Pritchett, 1993; Toye, 1994; Williamson, 1990). These policy reforms – conditionalities require various adjustments on the structure of borrower's economy (Kentikelenis et al., 2016), which in turn may also affect the size of the shadow economy of recipient countries.

The current literature regarding the effect of IMF intervention on countries' shadow economy is rather inconclusive. Only few studies focus specifically on the link between IMF programs and the size of the informal economy, and their results are mixed (Blanton et al., 2018; Hunter and Biglaiser, 2020; Reinsberg et al., 2019c).

In this subchapter we provide new insights regarding the effect of IMF intervention on the shadow economy. Our study advances with the recent strand of the literature (e.g., Daoud and Reinsberg, 2019; Forster et al., 2019; Kern et al., 2019; Reinsberg et al., 2019a, 2019c; Stubbs et al., 2020) by examining both the effect of IMF program participation and conditionality. Previous studies treat IMF programs as homogenous (e.g., they include only a

[5] These signed programs can have a duration of six months to three years and the ability of countries to draw on the loan funds in pre-specified intervals depends upon the implementation of policy reforms. For more details about IMF lending programs see, e.g., IMF (2019, 2020) and Chletsos and Sintos (2020).

binary indicator to capture IMF program participation) and therefore are unable to distinguish specific mechanisms between IMF participation and conditionality on the outcome variables.[6] Furthermore, to account for the endogeneity bias related to both IMF participation and conditionality, we follow the methodological approach proposed by Stubbs et al. (2020), by estimating a system of three equations including instrumental variables via maximum likelihood and allowing for correlated errors across equations (Roodman, 2011). Stubbs et al. (2020) review and evaluate previous methods for studying the impact of IMF programs, namely matching methods, instrumental variable approaches, system GMM estimation, and variants of Heckman estimators, and by conducting Monte Carlo simulations they confirm that their novel approach to addressing endogeneity is unbiased and performs better than the alternatives, provided that instruments are not weak.

Using new data on IMF conditionality (Kentikelenis et al., 2016) to capture the impact of various types of policy reforms (conditions) that borrowing countries must implement to access IMF credit and cross-national shadow economy data (Medina and Schneider, 2018), we find that both IMF program participation and conditionality exert a significant positive impact on the informal economy in a sample of 141 countries. By disaggregating IMF conditions into structural conditions, which concern a wider range of reforms in the domestic economy (microeconomic reforms) and afford governments less flexibility in the construction of policy reforms, and quantitative conditions, which take the form of quantitative targets that countries have to meet and provide governments more flexibility, we find that only structural conditions have a significant and detrimental effect on the informal economy. We provide evidence that

[6] Conditions differ between loan programs (e.g., 122 conditions for Serbia in 2005, while 4 conditions for Morocco in 2013), and conditionality is a key mechanism through which IMF lending works. Therefore, IMF loan programs should have varying effect, which previous literature fails to account as it treats IMF programs as being identical and expect them to have a single, constant effect on the eligible countries-borrowers (Rickard and Caraway, 2019).

the detrimental effect of structural conditions maintains in the long-term. Additionally, we show that financial development can reduce the size of the shadow economy, yet this negative effect is not enough to reverse the detrimental effect from IMF conditions.

The rest of the subchapter is structured as follows: in the following section, we identify the relationship between IMF intervention and the size of the unofficial economy. Section 1.1.3 analyses our data and the empirical methodology used. Section 1.1.4 reports the results. Finally, Section 1.1.5 offers some concluding remarks.

1.1.2 The relationship between IMF intervention and the size of the shadow economy

1.1.2.1 Previous studies

The role of international financial institutions (IFIs) in the context of the shadow economy is still ongoing in the literature. With their novel research, Blanton et al. (2018) investigate the effect of IMF programs on the shadow economy. The study indicates that economic openness reduces the size of the shadow economy, shedding some light on an ongoing literature that connects countries' economic openness and the prevalence of the shadow economy (e.g., Berdiev et al., 2018; Berdiev and Saunoris, 2018), while IMF participation and structural conditionality are related to a larger shadow economy. While this novel study has improved our understanding of the links between IMF programs and the shadow economy, it suffers from two main drawbacks. First, it faces methodological challenges relating to endogeneity bias (Stubbs et al., 2020). Blanton et al. (2018) use a panel fixed effects model and their main method to account for endogeneity is the GMM (generalized method-of-moments) estimator. Stubbs et al. (2020) provide a review and evaluation of methods for studying the impact of IMF programs. Regarding the use of GMM estimation, the authors argue that this method has several limitations for addressing possible endogeneity biases "*because it carries stringent assumptions that are untenable in all but the most exceptional of circumstances; besides, the* *estimates are too sensitive to arbitrary changes in the model to inspire confidence*". Second, and more importantly, the study by Blanton et al. (2018) does not spend sufficient efforts to capture heterogeneity within IMF programs. More specifically, Blanton et al. (2018) develop two models, one that uses IMF program participation (binary variable) as the explanatory variable, and the other one uses only IMF structural conditions, as such they examine separately the effect of IMF participation and structural conditions. By examining the effect of IMF participation and structural conditions separately, Blanton et al. (2018) lack to differentiate the effects of structural conditionality from other pathways of program influence, outside of the conditionality channel. As Stubbs et al. (2020) mention, both IMF program participation (with a binary indicator) and a measure of conditionality should be included in the model to distinguish effects of conditionality from other aspects of IMF programs and capture heterogeneity within IMF programs. Moreover, Blanton et al. (2018) exclude from their analysis non-structural (quantitative) conditions. However, non-structural conditions with structural conditions may simultaneously affect the size of the shadow economy. Failure to account for all conditions incorporated in IMF programs causes omitted variable bias, and in the case of Blanton et al. (2018), structural conditions pick up the effects of other conditions due to collinearity (Forster et al., 2020).

Hunter and Biglaiser (2020) examine the connection between IMF loan arrangements and domestic terrorism (also including a proxy for the shadow economy). They incorporate only a binary indicator for IMF program participation (not a count for conditionality) and their techniques do not account for endogeneity issues. Regarding the effect on the informal economy, they show that IMF loans are negatively associated with the size of the shadow economy when the borrowers are democracies, arguing that a decline in the informal economy supports fewer domestic terrorist attacks.

The study by Reinsberg et al. (2019c) shows no significant effect of IMF labour conditions on the shadow economy, arguing that while IMF labour conditions can reduce labour rights for 'labour market insiders', they are unable to affect the labour rights of 'labour market outsiders' (e.g., to get jobs in the formal economy). To account for endogeneity, they estimate a system of equations including instrumental variables similar to Stubbs et al. (2020). A drawback in the empirical model of Reinsberg et al. (2019c) is that while they include both IMF program participation and labour conditions (a policy area of conditions related to labour), they exclude other conditions (non-labour conditions).

Moreover, the literature lacks a systematic empirical foundation to evaluate the role played by powerful international financial institutions (IFIs) – the IMF, the World Bank, and regional development banks – in establishing policy reforms related to the shadow economy. As the social, economic and political effects of IMF interventions have been well-documented (e.g., Baro and Lee, 2005; Crivelli and Gupta, 2015; Dreher, 2006; Forster et al., 2019; Gunaydin, 2018; Reinsberg et al., 2019a; Rickard and Caraway, 2019; Stubbs et al., 2020), we are able to explore the linkages between IMF lending programs and the shadow economy.

1.1.2.2 Pathways through which IMF intervention affects the shadow economy

We assume two basic pathways linking IMF intervention to the size of the shadow economy. IMF policy reforms – conditionalities that force countries to implement a series of reforms in order to access IMF credit. The other one refers to IMF operations outside of the conditionality channel.

What matters is how IMF mandate is put in practice. Not all the conditions follow the same rhetoric.[7] Thus, following previous studies and the IMF's own classificatory schema, we are able to distinguish between "structural conditions" and "quantitative conditions"[8] (e.g., Bird, 2009; Stubbs et al., 2020). Structural conditions concern a wider range of reforms in the domestic economy (microeconomic reforms) and afford governments less flexibility (Kentikelenis et al., 2016). In contrast, quantitative conditions take the form of quantitative targets that countries have to meet and provide governments more flexibility. Examples include specific targets on the stock of short-term external debt outstanding, the net international reserves of the central bank, public external arrears, or the net domestic assets of the banking system (Kentikelenis et al., 2016).

1.1.2.2.1 Pathways: Structural conditions

As discussed above, structural conditions refer to specific conditions that require the overhaul of the state administration and restructuring of the domestic economy. Structural conditions can affect the size of the shadow economy in different ways. Previous research shows that structural conditions lower the ability of the state to attract or retain qualified personnel through cut deeply into public sector entitlements, including working conditions, social security, average pay and additional benefits (Reinsberg et al., 2019a). Reduced state capacity may increase individual's willingness of doing business in the shadow economy. This can occur through two pathways. The "paralyzed" state administration will be an obstacle for citizens and businesses to interact with regulatory agencies increasing the transaction costs of

[7] For instance, to reduce public external arrears, governments are allowed to choose between different policy reforms (e.g., increase taxes, reduce expenditures, or a combination of both). While, other conditions afford government less flexibility (Kentikelenis et al., 2016).

[8] Quantitative conditions are also mentioned as "stabilization conditions" (e.g., Reinsberg et al., 2019a; Vreeland, 2007; Woo, 2013).

complying with government policies.[9] The new working conditions may also lead state regulators to be less willing to enforce regulations that are labour-intensive to implement (i.e., tax collection, financial audits) (Blanton et al., 2018).

Structural reforms that require privatization of state-owned enterprises may also drive individuals' decisions to participate in the informal economy. On the one hand, privatization may help governments accomplish more economic efficiency by eliminating public enterprises with poor performance and thus generating more revenue to finance their fiscal deficits (Detraz and Peksen, 2015).[10] On the other hand, workers' layoffs from privatization may lead individuals to go underground, as the formal labour market has been damaged and job opportunities are closed due to the economic downturn.

At the same time, restructuring the domestic economy of recipient countries through structural conditions might also decrease the potential benefits associated with the formal economy. For instance, another highlighted effect of structural conditions is their negative impact on the level of labour rights (e.g., Abouharb and Cingranelli, 2007; Blanton et al., 2015, 2016; Burgess, 2010; Gunaydin, 2018; Reinsberg et al., 2019c). Promoting labour laws that legalize temporary work contracts, extend probation periods, and reduce the cost of firing workers, all imposed by structural conditions, undermine workers' rights. On the one hand, for firms restrictive or burdensome labour market regulations encourage entry into the shadow, as they increase the cost of employers to operate in the formal economy (Schneider and Enste, 2000). On the other hand, for employees the protection of basic labour rights can be an important factor when looking for work. A functioning regime of labour rights is one of the

[9] Friedman et al. (2000) show that bureaucracy and the shadow economy are positively related.

[10] However, Crivelli (2013) shows that fiscal consolidation through privatization may not be beneficial for budget balances and tax revenue.

primary benefits associated with work in the formal sector compared to the flexible and sometimes transient nature of informal work. The reduction of these rights could thus minimize the advantages of formal work and potentially steer workers into the shadow economy (Blanton et al., 2018; Blanton and Peksen, 2019).

Furthermore, the implementation of structural conditions can imply concentrated losses for well-developed groups in society. One such group is businesses in a specific industry. For example, an IMF program to the Philippines required the government to 'abolish one half of the quantitative import restrictions' (IMF, 1994, p. 19), and Moldova's IMF loan required the government to '[r]emove quantitative restrictions on imports of meat and dairy products' (IMF, 2008, p. 65). Within this framework, to protect their assets and forego punishment by the state, well-defined, narrow social groups (e.g., businesses in a specific industry) could bribe a public official in exchange for favourable treatment (Coate and Morris, 2006; Martimort and Straub, 2009). As governments' political survival depends on the support of powerful economic interests, they can continue protecting these groups and allow them to operate underground by bending rules and devising arbitrary regulations for competing firms (Reinsberg et al., 2020). This theoretical argument helps predict the timing of effects. The (envisaged) removal of structural distortions during IMF lending programs immediately increases shadow activities, it will likely persist over the medium- and long-term. This is due to the fact that structural conditions prompt the state and the business sector to engage in patron–client relationships which take some time to emerge and solidify (Ganev, 2007; King and Szelenyi, 2005; Reinsberg et al., 2019b; Weber, 1978). Thus, our theoretical consideration generates the dynamic expectation that the adverse effect on the shadow economy maintain within the process of structural conditions (unless exogenous factors break the vicious circle of rent-seeking and weakening institutions).

There is also anecdotal evidence of a link between IMF structural conditions and increased shadow economy. Consider the case of Central African Republic. Between 1994 to 2000, the country faced 45 binding structural conditions, many of which imposed reduction in the overall size of the public sector wage bill and privatization of state-owned enterprises (Kentikelenis et al., 2016). As the implementation of structural reforms progressed, the size of the shadow economy increased according to the estimates of Medina and Schneider (2018). In other words, the shadow economy increased precisely when structural conditionality was at its peak (Figure 1.1.2a). In addition, a similar pattern in the size of the shadow economy is also observed for the case of Georgia. Between the period 1994-2005, Georgia faced a total of 90 binding structural conditions including public sector wage and employment limits, and privatization of state-owned enterprises (IMF, 2004). For the examined period in Figure 1.1.2b the size of the shadow economy and the number of binding structural conditions had a similar trend suggesting a positive association. Although the above correlations are merely suggestive, they show that our theoretical consideration is entirely plausible.

Figure 1.1.2a

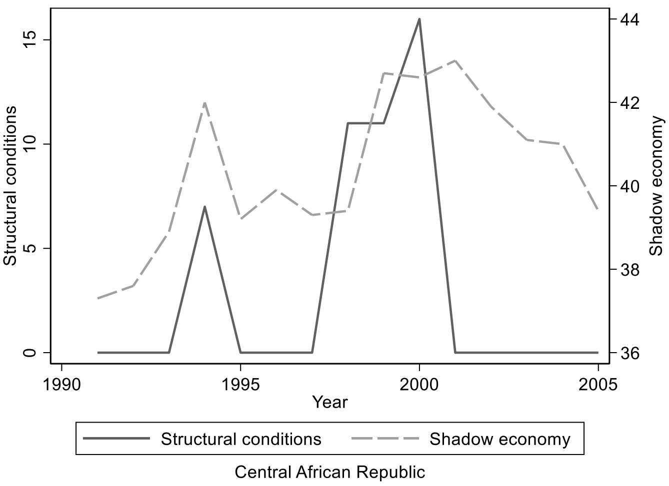
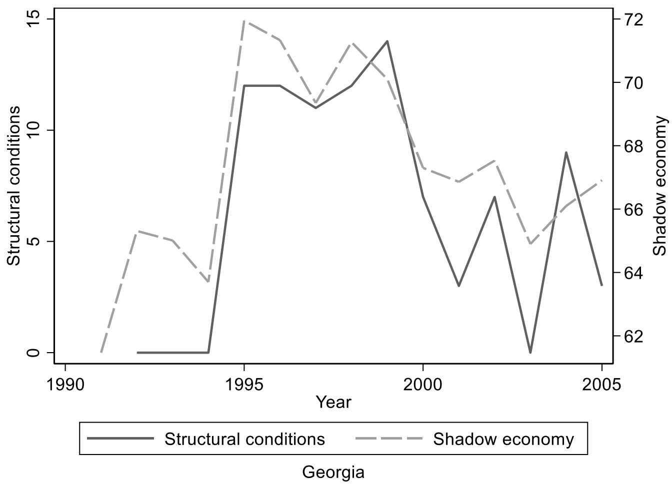


Figure 1.1.2b



The link between structural conditions and the shadow economy

1.1.2.2.2 Pathways: Quantitative conditions

Quantitative conditions are expressed as general macroeconomic targets and other objectives that governments have to meet and maintain throughout the program (Kentikelenis et al., 2016). Unlike structural conditions, quantitative conditions do not oblige governments to enact specific reforms, but leave them with some discretion in how to achieve economic policy objections through conditionality (Reinsberg et al., 2019a).

Under fiscal balance pressures, countries have taken different strategies depending on their relationship with the Fund. Although some studies suggest that IMF program participation improves fiscal outcomes (e.g., Dreher and Vaubel, 2004; Easterly, 2005), Brun et al. (2011) conclude that IMF programs had a negative impact on total revenues in Sub-Saharan Africa during the 1984-2007 period. Our argument is that adjusting tax policy to improve fiscal outcomes with increased taxation makes countries less competitive in the global economy because taxes increase the cost of doing business, which may induce some firms to move into the shadow sector (Gërxhani, 2004; Herwartz et al., 2011; Schneider and Enste, 2000). Nevertheless, Goel and Nelson (2016) show that not the burdensome taxation but tax complexity matters for the prevalence of the shadow economy. Thus, the design of tax policy is very crucial for the size of the shadow economy.

The literature provides several studies that analyse the socio-economic consequences of quantitative conditions (e.g., Dreher and Walter, 2010; Przeworski and Vreeland, 2000; Stubbs et al., 2020; Stubbs and Kentikelenis, 2018). However, the effect of these conditions may translate different for the shadow economy. On the one hand, the enforced austerity measures increase long-term unemployment resulting from declined economy activity (Ball et al., 2013), consequently individuals may seek for job outside of the official economy (Campbell, 2005). On the other hand, government size may influence the decision to engage in the formal versus the informal sector through, for example, high taxation. Larger governments may represent government overreach through burdensome taxes, thereby enticing economic agents to move to the informal sector (e.g., Johnson et al., 1998; Saunoris and Sajny, 2017; Schneider and Enste, 2000). Therefore, the decreased size of the government may reduce the incentive to engage in the shadow economy. Likewise, the policymaking of these conditions may drive agents' decision to participate or not in the informal economy.

The flexibility of such conditions has to use properly from governments to become a useful tool for economic development and not an economic "trap". Thus, for quantitative conditions, their effect on the size of the shadow economy depends on the degree of flexibility and the design of these conditions.[11]

1.1.2.2.3 Pathways: IMF program participation

The IMF may also affect the size of the shadow economy of recipient countries outside of the conditionality channel. The approval of an IMF program is associated with a certain amount of money (loan funds). However, the effect of this money is not evident (Dreher, 2006). Theoretically, it is expected that IMF credit is intended to alleviate the restructuring of the economy, however, in practice the result may be conflicting: Money disbursed increases borrowing governments' leeway, thereby reducing the incentives to reform (Boockmann and Dreher, 2003). Consequently, governments pursue inappropriate policies longer than they would otherwise (Bandow, 1994).

The IMF can influence the shadow economy of recipient countries by its policy advice (Boockmann and Dreher, 2003). Killick (1994) points out that IMF's advice is often discussed publicly and may influence politics in the long-run. In addition, one of the IMF's main

[11] The measurement of the flexibility and the design of quantitative conditions, although interesting and significant, are beyond the scope of this subchapter. For quantitative conditions, we assume that the degree of flexibility provided to governments could work negatively for the size of the shadow economy.

contributions to reforms is that it stands consistently for a particular approach to economic policy (Fischer, 2001). Therefore, the long-run impact of the IMF reaches beyond the immediate effects of conditions and finance. IMF advice to policymakers might thus reduce (or increase) the size of the shadow economy independent of policy reforms – conditionalities.

According to Vaubel (1983), IMF lending may be interpreted as a (subsidized) income insurance against adverse shocks (moral-hazard hypothesis). This insurance-credit cover makes the potential recipients-borrowers to be less cautious against such shocks (or even to intentionally generate a crisis) (Dreher, 2006). Previous studies point out that the balance of payments problems of IMF borrowers have been largely of their own making (Vaubel, 1991; Evrensel, 2002) and during inter-program years macroeconomic performance has been deteriorating as the past exposure to IMF programs increased (Evrensel, 2002). In addition, Dreher and Vaubel (2004) show that economic policy is more expansive in countries with higher IMF loans available (as measured by the country's undrawn quota with the Fund). If we assume that this hypothesis holds, then IMF money, through the moral hazard problem, deteriorates economic policy, which in turn may affect the shadow economy.

IMF programs can have highly pernicious effects on borrowing countries' domestic political environment. A variety of studies have shown that countries are more likely to experience protests (Auvinen, 1996; Sidell, 1988), civil war (Hartzell et al., 2010), government and currency crises (Dreher and Gassebner, 2012; Dreher and Walter, 2010), and the risk of a coup (Casper, 2015) when participating on an IMF program. Additionally, the likelihood of a re-election prospect (Dreher, 2004), if an IMF program is in active, and the interruption of an IMF arrangement (Dreher, 2003), if an election is on the horizon, may increase. All in all, this bad economic and political climate can work positively for the rise of the underground economy (Elbahnasawy et al., 2016).

1.1.3 Empirical strategy and data

1.1.3.1 Data

This study uses panel data for 141 countries across the world to investigate the effect of IMF intervention on the shadow economy over the period 1991 to 2014. Table A1 of the Appendix lists all countries included in the study.[12]

There is not a universal way to provide a complete picture of the size of the informal economy.[13] However, there are different approaches available in the literature which attempt to estimate informal economic activity (previous studies point out three basic categories of approaches, namely (a) the direct, (b) indirect and (c) model approaches).[14] In this study, data for the shadow economy are taken from Medina and Schneider (2018). They estimate the size of the shadow economy (% of official GDP) using a multiple indicators and multiple causes (MIMIC) approach which is essentially a structural model where the shadow economy is estimated from a system of equations composed of economic and institutional variables. The MIMIC method has been quite popular in this literature.[15] The main concern when using the MIMIC approach is that the variable of GDP (e.g., GDP per capita, growth rate of GDP or first differences in GDP) is used as a cause as well as indicator variable (e.g., Breusch, 2016; Schneider, 2016). Medina and Schneider (2018) apply for first time the light intensity approach by measuring the overall economic activity based on satellite data on night lights instead of

[12] The sample includes both program and nonprogram years, as well as countries with no programs.

[13] In general, the measurement of the shadow economy is inherently difficult due to its secretive nature (Schneider and Buehn, 2018).

[14] For a review of existing methods for estimating the size of the shadow economy see Schneider and Buehn (2018) and Dybka et al. (2019).

[15] See, e.g., Chaudhuri et al. (2006), Dell'Anno et al. (2007), Mai and Schneider (2016), Schneider (2005), Schneider and Buehn (2018), and Schneider et al. (2010).

GDP and calibrate their models using predictive mean matching, avoiding the problem that GDP is often used as a cause and indicator variable.

For our key explanatory variables, we use a new dataset of IMF conditionality based on original coding of loan agreements between the Fund and its borrowers (Kentikelenis et al., 2016).[16] This database provides detailed information on the conditions included in loans and their implementation sourced directly from internal IMF documents. First, IMF program participation is a binary variable, taking the value of one if an IMF program is in use for at least five months in a specific year, and zero otherwise (Dreher, 2006). Second, for IMF conditionality, we include the total number of binding IMF conditions applicable to a country in a given year.[17]

Control variables are a set of economic and political determinants of the shadow economy. Following standard practice, we lag all control variables by one period to allow for some delay in their associated effects on the size of the shadow economy. Economic conditions are controlled for by the growth rate of output, denoted GDP growth. We also control for trade openness (imports and exports in terms of GDP). The removal of barriers to trade and increasing levels of international trade are likely to reduce the shadow economy (Blanton et al., 2018; Goel et al., 2019). Moreover, we account for investments (capital formation, share of GDP), as the accumulation of investments could be related to a decline in the shadow sectors

[16] IMF conditionality dataset (Kentikelenis et al., 2016), available at: http://www.imfmonitor.org/conditionality.html

[17] Binding conditions known as 'prior actions' or 'performance criteria' (Stubbs et al., 2020). Loan disbursal is directly determined by the binding conditions and should be scheduled in order to continue the IMF program. Following Stubbs et al. (2020), we also use alternative measures of conditionality: an implementation-corrected count (which subtracts conditions waived by the IMF); an implementation-discounted count (which discounts conditions during program suspensions); and a combined binding and non-binding condition count.

(Blanton et al., 2018). Government balance as a share of GDP measures the difference of general government revenue and general government total expenditure. Government expenditures could reflect the size of government. Previous studies have shown a positive correlation with the size of the shadow economy as a result of a dissatisfaction of public preferences for the size of government spending (for example, in presence of unnecessary or irrational government spending), and additionally the existence of 'more State' in the market and subsequently an increase in regulation tend to increase the size of the unofficial sector (Dell'Anno and Schneider, 2003; Schneider et al., 2010). In addition, government revenues could be negatively associated with informal activity as a result of increased audits (Fleming et al., 2000; Johnson et al., 1997, 1998). We also include mineral rents as a percentage of GDP to capture country's richness in natural resources, and the age dependency ratio as a share of working-age population to account for the share of dependants up to 15 years of age. Our main political variable is the level of democracy (Teorell et al., 2016). The extent of informal economic activity might be higher in mixed regimes than consolidated democracies (Teobaldelli and Schneider, 2013) and authoritarian regimes (Elbahnasawy et al., 2016). These are the baseline control variables. Table 1.1.1 provides explicit definitions for all variables used in the empirical analysis, while Table 1.1.2 reports basic summary statistics.

Table 1.1.1 Definition of variables, sources and coverage

Variable name	Definition	Source	Year coverage
Shadow (Medina and Schneider, 2018)	Size of the shadow economy measured as a percentage of official GDP, based on the multiple indicators, multiple causes (MIMIC) method.	Medina and Schneider (2018)	1991-2014 (All)
Shadow (Elgin and Oztunali, 2012)	Size of the shadow economy (% of GDP) calculated by employing a two-sector dynamic general equilibrium model.	Elgin and Oztunali (2012)	Ends 2009
IMF participation	Dummy variable: equals to 1 if IMF program active for 5 or more months in a year, 0 otherwise.	Stubbs et al. (2020)	All
All conditions (binding)	Total count of binding conditions in IMF program.	Kentikelenis et al. (2016)	All
Implementation corrected conditions	An implementation-corrected count (which subtracts conditions waived by the IMF) of conditions in IMF program.	Kentikelenis et al. (2016)	Ends 2008
All conditions, non-binding included	Total count of binding and non-binding conditions in IMF program.	Kentikelenis et al. (2016)	All
Implementation discounted conditions	An implementation-discounted count (which discounts conditions during program suspensions) of conditions in IMF program.	Kentikelenis et al. (2016)	Ends 2008
Structural conditions	Total count of disaggregated (structural) binding conditions concerns a wider range of reforms in the domestic economy and afford governments less flexibility.	Kentikelenis et al. (2016)	All
Quantitative conditions	Total count of disaggregated (quantitative) binding conditions concerns quantitative targets that countries have to meet and often maintain throughout the program period.	Kentikelenis et al. (2016)	All
IMF loans (ln)	IMF purchases are total drawings on the General Resources Account of the IMF during the year specified, excluding drawings in the reserve tranche. Data are in current U.S. dollars. The variable is transformed as follows: $IMF\ loans\ (ln) = ln\ (1 + IMF\ loans)$	International Debt Statistics (2021)	All
IMF liquidity ratio (ln)	IMF liquid resources divided by liquid liabilities (ln).	Lang (2020)	Ends 2013
Countries under program	Number of countries participating in an IMF program (for at least five months in a given year).	Authors' calculation using Stubbs et al. (2020)	All
Financial development	Summarizes how developed financial markets and financial institutions are along three dimensions (depth, access, and efficiency) by country and year. It ranges between 0 and 1 (higher values more developed).	Svirydzenka (2016)	All
GDP growth	GDP growth (annual %).	World Bank (2018)	All
GDP per capita (ln)	ln GDP per capita (constant 2005 US$).	World Bank (2018)	All
Executive election	Binary indicator variable for whether an executive election was held in a given year.	Teorell et al. (2016)	All
Regime Durability	Regime durability (total years of existence of current regime).	Teorell et al. (2016)	All
Democracy	Average of Freedom House and Imputed Polity measures of democracy, transformed to a scale of 0 to 10.	Teorell et al. (2016)	All
Government balance	Difference of general government revenue and general government total expenditure as a share of GDP (%).	IMF (2016)	All
Government spending	General government total expenditure as a share of GDP (%).	IMF (2016)	All
Trade openness	The sum of exports and imports of goods and services measured as a share of GDP.	World Bank (2018)	All
Investments	Officially are named as gross capital formation (% of GDP) and it consists of outlays on additions to the fixed assets of the economy plus net changes in the level of inventories.	World Bank (2018)	All
Bureaucracy costs	An indicator which captures, in the normal business operations, the costs from bureaucracy – the regulatory	Gwartney et al. (2019)	From 1995-

	environment. This includes regulatory compliance and bureaucratic inefficiency and/or opacity. On a scale from 0 to 10; higher scores indicate lower cost.		
Hiring and firing costs	A sub-index of labour regulations which focuses on hiring and firing regulations-costs. On a scale from 0 to 10; higher scores indicate lower cost.	Gwartney et al. (2019)	From 1995-
Political Stability	Measures perceptions of the likelihood that the government will be destabilized/overthrown, on a scale of -2.5–2.5 (higher scores indicate greater stability).	Kaufmann et al. (2010)	From 1996-
Control of corruption	Measures perceptions by individuals of the extent to which public power is exercised for private gain. Higher index values indicate lower perceptions of corruption or higher control of corruption, whereas lower index values indicate higher perceptions of corruption or lower control of corruption, on a scale of -2.5–2.5.	Kaufmann et al. (2010)	From 1996-
Starting a business	An indicator which captures the amount of time and money it takes to start a new limited-liability business. Countries where it takes longer or is more costly to start a new business are given lower scores, on a scale from 0 to 10.	Gwartney et al. (2019)	From 1995-
Dependency ratio	Population aged under 15 as a share of working-age population (%).	World Bank (2018)	All
Mineral rents	Mineral rents (% of GDP).	World Bank (2018)	All
Top marginal tax rate	An indicator measuring the top marginal tax rate. The indicator is on a scale of 0 to 10 with higher values denoting more freedom from taxes.	Gwartney et al. (2019)	From 1995-
Rule of Law	A perception-based index measuring the strength and quality of the rule of law, on a scale of -2.5–2.5 (with higher values denoting stronger rule of law).	Kaufmann et al. (2010)	From 1996-
UNGA voting alignment	Voting similarity index with US on a scale ranging from 0 to 1, where 1 is perfect similarity and 0 is perfect difference.	Voeten et al. (2016)	All
UNSC temporary membership	Dummy variables: = 1 if country is a temporary member of UNSC, 0 otherwise.	Dreher et al. (2009b)	All

Table 1.1.2 Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Shadow (Medina and Schneider 2018)	2,557	30.8859	13.1336	6.16	71.34
Shadow (Elgin and Oztunali 2012)	1,954	32.0160	13.1543	8.07	79.06
IMF participation	2,557	0.3418	0.4744	0	1
All conditions (binding)	2,557	8.5999	15.1163	0	124
Implementation corrected conditions	2,035	8.8197	15.1564	0	114
All conditions, non-binding included	2,557	13.0223	21.6960	0	148
Implementation discounted conditions	2,035	7.7429	14.0496	0	93
Structural conditions	2,557	1.6738	4.6315	0	80
Quantitative conditions	2,557	6.9261	12.0251	0	63
IMF loans (ln)	2,499	7.2357	8.6975	0	23.5954
IMF liquidity ratio (ln)	2,557	5.6788	0.7562	4.5431	7.1092
Countries under program	2,557	55.0473	9.0439	35	66
Financial development	2,550	0.3109	0.2273	0	1
GDP growth	2,557	3.9151	4.6374	-50.2481	38.2007

GDP per capita (ln)	2,557	8.0241	1.6586	4.9175	11.1432
Executive election	2,557	0.6007	0.4898	0	1
Regime Durability	2,557	26.6625	32.0175	0	203
Democracy	2,557	6.4527	3.0386	0	10
Government balance	2,557	-1.8570	5.8923	-46.2340	43.3030
Government spending	2,557	30.1084	11.9051	2.1470	82.0810
Trade openness	2,557	80.6451	45.5209	15.2390	439.6567
Investments	2,557	23.6148	7.4981	1.0968	67.9105
Bureaucracy costs	1,237	5.3639	1.9389	0	10
Hiring and firing costs	1,390	4.6931	1.3971	1	8.8
Political Stability	1,795	-0.1249	0.9311	-2.8447	1.7601
Control of corruption	1,795	-0.0469	1.0160	-1.7229	2.4700
Starting a business	1,394	8.1905	1.6057	0	9.98
Dependency ratio	2,557	52.6616	24.3339	15.5184	106.4515
Mineral rents	2,557	1.0567	3.4252	0	44.6443
Top marginal tax rate	1,263	6.9287	2.4575	0	10
Rule of Law	1,795	-0.0637	0.9816	-2.1300	2.0137
UNGA voting alignment	2,557	0.3397	0.1494	0	0.9412
UNSC temporary membership	2,557	0.0760	0.2651	0	1

1.1.3.2 Empirical identification

Our analysis follows the methodological approach proposed by Stubbs et al. (2020). The main assumption of this process is that countries select into both IMF participation and conditionality. First, IMF program participation may be endogenous. Controlling for economic and political variables that determine IMF participation (e.g., Moser and Sturm, 2011; Sturm et al., 2005; Steinwand and Stone, 2008), as well as country and year fixed effects, mitigate the problem of endogeneity in the outcome equation to a certain extent. However, another germane issue in this context is that of reciprocal causation (Blanton et al., 2018). It is possible that there is a mutual interdependence (endogeneity) between IMF program participation and the size of the shadow economy. That is, countries that are relatively dependent on IMF loans might also be the ones that already have a larger informal sector.

Likewise, IMF conditionality – i.e., the number of conditions – may also be endogenous and affect the validity of our analysis. The endogeneity issue of conditionality can arise from three sources (Forster et al., 2019). First, a country's selection in IMF conditionality in a given year is not randomly assigned.[18] As a result, endogeneity may arise from the systematic differences between countries that receive more IMF conditions and those that receive fewer conditions, thus uncorrected estimates would underestimate the true effect of conditionality on the outcome variable. The second issue of endogeneity rely on omitted variable bias (Woolridge, 2002, 2006). It is possible that IMF staff design lending arrangements based on unobserved factors, such as perceived economic outlook of a borrowing country. Additionally, preferences of government authorities and IMF staff for policy making may be different, as the former may have the willingness to reduce the size of the shadow economy (or even to neglect the size and growth of the informal economy in a view of upcoming elections (Skouras and Christodoulakis, 2013)). Eligible countries that select into conditionality may implement policy reforms that have an impact on the size of the shadow economy. In this case, the omitted variable – (unobserved) government preferences – is correlated with the selection into conditionality and the size of the shadow economy, as a result the validity of uncorrected estimates is violated. The third issue of endogeneity arises from measurement error of the explanatory variables (IMF program and conditionality). If measurement error exists in the explanatory variables, which are measured with noise and are correlated with the error term,

[18] The decision of IMF staff regarding the selection of conditionality depends on country's political environment. For example, the Fund may introduce less conditions in a lending program as it recognises that new elected governments face additional policymaking constraints, as well as in a view of upcoming elections, political stability is decreased (Rickard and Caraway, 2014; Stone, 2008). With regard to the shadow economy, the selection of conditionality may depend on the type of conditions. For example, specific conditions, such as conditions which force countries to adopt specific excise taxes based on volume for tobacco, alcohol and petroleum products which are associated with a larger informal sector (Gërxhani, 2004; Neck et al., 2012), may not be selected.

an estimation which does not account for the issue of measurement error yields to attenuation bias (Woolridge, 2009).

To mitigate these issues, we estimate a system of equations including instrumental variables and allowing for correlated errors across equations (Roodman, 2011). Instruments are hard to find, but we are able to draw on an instrumental technique which uses a compound instrument to account for endogeneity. This methodological approach has been popularized in political research, especially in aid effectiveness (e.g., Dreher and Langlotz, 2017; Dreher et al., 2019; Nunn and Qian, 2014), and has recently been used to evaluate the effects of IMF participation and conditionality (e.g., Daoud and Reinsberg, 2019; Forster et al., 2019; Lang, 2020; Reinsberg et al., 2019a, 2019c; Stubbs et al., 2020).

Following Lang (2020) and Stubbs et al. (2020), we use two separate compound instruments to account for endogeneity of IMF program participation and conditionality. The compound instruments are constructed as follows:

a) For selection into IMF programs, we interact the within-country average of IMF program participation across period of interest with the Fund's budget constraint, approximated by the natural log of the IMF liquidity ratio (Lang, 2020; Nelson and Wallace, 2017; Stubbs et al., 2020) – calculated as liquid resources divided by liquid liabilities.
b) Similarly, for conditionality, we interact the within-country average of the number of conditions across period of interest with the natural log of the IMF liquidity ratio (Stubbs et al., 2020).

First, we believe that the compound variable ($\overline{IMF_i} \times Budget_t$) as an instrument for IMF participation satisfies the relevance criterion because previous research shows that the number of countries participating in an IMF program is determined by the IMF's budget constraint. In years with resource abundance, i.e., a higher liquidity ratio, the Fund can provide more new lending programs, and vice versa (Lang, 2020). It could also denote that countries that have been in IMF program longer in the past are more likely to enter the program in the future, especially when the IMF's liquidity is abundant (Bird and Rowlands, 2002). Therefore, the country-specific mean exposure to IMF programs over the sample period approximates the general propensity of a country to participate in an IMF program in a given year, after controlling for observable factors that usually explain such variation. Thus, the interaction of the country-specific probability of participation and the Fund's budget constraint can predict selection into IMF programs.

Our reasoning for the use of $(\overline{Cond}_i \times Budget_t)$ as an instrument for selection into conditionality is similar. Once again, past studies show that if the demand for financial assistance increases, the Fund's budget constraint becomes binding and assigns a higher number of conditions to borrowing countries to balance the increased demand in a view of limited resources (Forster et al., 2019; Lang, 2020; Stubbs et al., 2020). Additionally, the mean number of conditions in IMF programs over the entire period represents the country-specific exposure to IMF conditionality. This exposure partly determines the bargaining position of government interlocutors in negotiations with the IMF and more broadly informs future IMF programs. As a result, the compound instrument predicts variation in IMF conditionality.

In this identifying assumption a time-varying exogenous variable (i.e., the Fund's budget constraint, approximated by the natural log of the IMF liquidity ratio) is interacted with an endogenous variable (i.e., the mean number of country-specific IMF program participation or the mean number of conditions) that varies only across countries to produce an instrument that then varies across countries and over time. Thus, with regards to $(\overline{IMF}_i \times Budget_t)$, the excludability assumption is that the shadow economy for countries with differing levels of past IMF exposure will not be affected differently by changes in the IMF liquidity ratio, other than its impact on IMF program participation. With regards to IMF conditionality, the excludability of the instrument ($\overline{Cond}_i \times Budget_t$) rests on the assumption that the shadow economy for countries with differing mean levels of number of conditions will not be affected differently by changes in the IMF liquidity ratio, other than through the impact of types of conditions.[19]

Our identification strategy is the following:

$$\widehat{IMF}_{it-1} = i_0 + i_1(\overline{IMF}_i \times Budget_t)_{t-1} + i_2 Z_{it-1} + i_3 X_{it-1} + \kappa_i + \delta_t \tag{1.1.1}$$

$$\widehat{Cond}_{it-1} = c_0 + c_1(\overline{Cond}_i \times Budget_t)_{t-1} + c_2 X_{it-1} + \mu_i + \delta_t \tag{1.1.2}$$

$$S_{it} = \beta_0 + \beta_1 \widehat{IMF}_{it-1} + \beta_2 \widehat{Cond}_{it-1} + \beta_3 X_{it-1} + \mu_i + \delta_t + \varepsilon_{it} \tag{1.1.3}$$

Equation (1.1.3) is the outcome equation, where S is the outcome of interest, the size of the shadow economy; $\widehat{IMF}$ is the fitted value for IMF program participation derived from Equation (1.1.1); $\widehat{Cond}$ is the fitted value for the total number of conditions derived from Equation (1.1.2); X denotes a vector of control variables described in subsection 1.1.3.1; μ and δ represent country and year fixed effects, respectively and ε is the error term. Subscript i indexes individual countries, whereas t indexes time.

Equation (1.1.1) is a probit model predicting IMF program participation as a function of the compound instrument, ($\overline{IMF}_i \times Budget_t$), the vector of controls from the outcome

[19] The interaction of an endogenous variable (i.e., the mean number of country-specific IMF program participation or the mean number of conditions) with an exogenous variable (i.e., the Fund's budget constraint, approximated by the natural log of the IMF liquidity ratio) can be interpreted as being exogenous. For econometric details on this point, see Bun and Harrison (2018) and Nizalova and Murtazashvili (2016). Even if there were endogeneity between the time-variant budget constraint and the size of the shadow economy, the exclusion restriction would only be violated if the unobserved variables driving this relation were correlated with the mean number of country-specific IMF participation/conditionality (see, e.g., Forster et al., 2019; Lang, 2020; Stubbs et al., 2020; Reinsberg et al., 2019a, and for analytical proofs see, e.g., Bun and Harrison, 2018; Nizalova and Murtazashvili, 2016).

equation, X, and the vector of explanatory variables specific to selection into IMF programs, Z. This vector includes: GDP per capita to capture for the macroeconomic conditions (Gündüz, 2016), the count variable of counties under programs, as program participation is affected by the extent to which the Fund has resources available, which depends on the current number of program countries (Vreeland, 2003), a variable for past IMF participation, as previous exposure is a reliable predictor of current and future participation (Bird et al., 2004), and two political variables, regime durability – the number of years that the current political order has survived since the last transformation – and executive elections since these influence IMF programs as well (Rickard and Caraway, 2014). We further include regional fixed effects, κ, and year fixed effects, δ.

Equation (1.1.2) instruments for the number of conditions using the compound instrument, $\overline{Cond}_i \times Budget_t$, and includes the vector of explanatory variables from Equation (3), X, country fixed effects, μ, and year fixed effects, δ.

To estimate the system of three equations we use maximum likelihood estimation (MLE),[20] combining an instrumental variable approach to address endogeneity of IMF participation with an instrumental variable approach to address endogeneity of conditionality (Stubbs et al., 2020).

[20] MLE can be implemented using the command *cmp* in STATA (see Roodman, 2011). For further assumptions and technical details on the estimation procedure, see Roodman (2009).

1.1.4 Empirical results

1.1.4.1 Baseline results

In Table 1.1.3, we present the results of our baseline quantitative analyses.[21] Specification in column 1 includes only the control variables and is estimated using simple OLS. Results on the coefficients of controls variables largely conform to established previous studies. GDP growth ($p<0.01$), trade openness ($p<0.01$), and investments ($p<0.05$) are all negatively correlated with the size of the shadow economy. The effect of government balance on the shadow sector is negative, but the coefficient is statistically insignificant. Likewise, the coefficient on democracy, although negative, is statistically insignificant and sensitive to the model specification. Finally, the coefficient on dependency ratio is positive, while the coefficient on mineral rents is negative, but both are statistically insignificant.

Specification 2 incorporates the IMF participation variable, but again is estimated using simple OLS without any endogeneity corrections. The control variables remain unchanged. The coefficient on the binary IMF variable is positive ($p<0.01$), indicating that IMF programs overall increase the size of the shadow economy. In specification 3, we correct for endogeneity of program participation using compound instrumentation: the interaction of the within-country average of IMF program participation across period of interest with the natural log of the IMF liquidity ratio. A similar result holds, the IMF participation remains positive, higher in magnitude and significant ($p<0.01$). Also, the coefficient on government balance ($p<0.10$) is negative as expected and now statistically significant.

Next, in specification 4, 5 and 6, we additionally control for the count of conditions, employing the preferred identification strategy. We begin with specification 4, using simple

[21] Before we proceed to the empirical analysis, we conducted augmented Dickey–Fuller tests for stationarity on the dependent variable (shadow economy), which indicated no problems ($p<0.01$).

OLS, the estimated coefficient on the total number of conditions is positive and significant ($p<0.05$), but close to zero, which is consistent with the sources of bias discussed above. Specification 5 only corrects for the endogeneity of program participation. We find similar results, the coefficient on conditionality is positive, significant ($p<0.01$), but close to zero. In specification 6, we use compound instrumentation for the total number of conditions and program participation. The number of total conditions is positive, higher in magnitude and statistically significant ($p<0.01$). For one additional binding condition, the size of the shadow economy increases by 0.1233, ceteris paribus. At the mean number of binding conditions, 8.5999, this corresponds to an average increase of the shadow economy by 1.06 (=0.1233*8.5999) percentage points, all other factors held constant. Figure 1.1.3 depicts the marginal plot with the predicted values of the shadow economy and the 95 percent confidence interval (specification 6). In the absence of an IMF program, our model predicts a value of 29.4723 (horizontal line) for the shadow economy (slightly below the mean). For countries with IMF programs, the estimated value is 30.4612, which subsequently increases to 45.7522 as the number of binding conditions increases. The difference of 15.2910 is greater than one standard deviation in the shadow economy measure (13.1336).

Outside of the conditionality channel, the sign of IMF program participation remains positive, significant and its magnitude depends on the model specification. An increase in IMF participation by one standard deviation (specification 6) results in an increase in the shadow economy by 0.4692 (=0.9890*0.4744) which corresponds to approximately 3.6% of its standard deviation. Results on the control variables maintain their direction effects, with slight changes in the significance level, and we refrain from discussing these from now on. Diagnostic statistics show that the compound instrument for program participation is strong across specification 3 and 5 (Kleibergen-Paap statistics of 151.82 and 149.76, respectively).[22] In specification 6, where we use compound instrumentation for program participation and conditionality, Kleibergen-Paap statistics confirm the validity of compound instruments (37.54 for conditionality instrument and 134.69 for participation instrument). Also, the instruments are jointly relevant, F-statistic of 177.88.

In the selection model (specification 3, 5 and 6), the compound instrument for IMF participation is highly significant (p<0.01) with a positive sign. This means that given the budget constraint of the Fund (approximated by the liquidity ratio), a higher mean exposure to IMF programs makes future participation more likely (Forster et al. 2020). Most of the variables are insignificant at standard thresholds, nevertheless, one determinant of IMF participation is past IMF programs (p<0.01). Higher GDP per capita is significantly linked to a lower probability of obtaining an IMF program. We also find evidence that democratic regimes are more likely to sign an IMF arrangement (p<0.05, specification 3 and 5). Turning to IMF conditionality equation (specification 6), we find that the compound instrument is strongly correlated with the number of conditions.

Table 1.1.3 Effect of IMF intervention on the shadow economy

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	*Controls only*	*Shadow economy*				
L. IMF participation		1.2712***	1.6397***	0.7890***	1.1724***	0.9890***
		(0.2576)	(0.3710)	(0.2988)	(0.3778)	(0.3766)
L. IMF conditions				0.0208**	0.0217***	0.1233***
				(0.0082)	(0.0080)	(0.0409)
L. Dependency ratio	0.0387	0.0313	0.0302	0.0302	0.0290	0.0126
	(0.0360)	(0.0333)	(0.0318)	(0.0331)	(0.0316)	(0.0311)
L. GDP growth	-0.0831***	-0.0814***	-0.0810***	-0.0833***	-0.0829***	-0.0895***
	(0.0206)	(0.0201)	(0.0193)	(0.0200)	(0.0193)	(0.0191)
L. Democracy	-0.0732	-0.1148	-0.1243	-0.1169	-0.1275	-0.1928
	(0.1514)	(0.1425)	(0.1357)	(0.1434)	(0.1365)	(0.1378)

[22] Staiger and Stock (1997) suggest that F-statistics of instrumental variables (IV) should be larger than ten to ensure that the maximum bias in IV estimators is less than 10% (Staiger-Stock rule of thumb).

L. Government balance	-0.0499	-0.0545*	-0.0554*	-0.0563*	-0.0573*	-0.0733**
	(0.0320)	(0.0318)	(0.0308)	(0.0318)	(0.0308)	(0.0335)
L. Trade openness	-0.0287***	-0.0293***	-0.0293***	-0.0299***	-0.0300***	-0.0343***
	(0.0108)	(0.0106)	(0.0102)	(0.0105)	(0.0102)	(0.0102)
L. Investments	-0.0617**	-0.0596**	-0.0594**	-0.0589**	-0.0587**	-0.0519**
	(0.0283)	(0.0268)	(0.0256)	(0.0267)	(0.0256)	(0.0253)
L. Mineral rents	-0.0837	-0.0840	-0.0839	-0.0785	-0.0781	-0.0532
	(0.0712)	(0.0693)	(0.0665)	(0.0696)	(0.0669)	(0.0723)
Constant	50.5484***	51.3874***	21.1051***	21.0820***	21.1735***	22.1474***
	(4.1074)	(3.7801)	(2.1053)	(2.1880)	(2.0876)	(1.9725)
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Dependent variable:			L. IMF participation		L. IMF participation	
L. Participation compound			0.3804***		0.3790***	0.4027***
			(0.0309)		(0.0310)	(0.0347)
L. Past programs			1.5842***		1.5882***	1.1682***
			(0.0857)		(0.0857)	(0.0632)
L. Countries under program			0.0495		0.0493	0.0736*
			(0.0428)		(0.0427)	(0.0376)
L. GDP per capita			-0.1885***		-0.1886***	-0.1004**
			(0.0642)		(0.0641)	(0.0469)
L. Executive election			0.1205		0.1202	0.1454
			(0.0891)		(0.0888)	(0.0889)
L. Regime Durability			-0.0031*		-0.0031*	-0.0007
			(0.0018)		(0.0018)	(0.0015)
L. Dependency ratio			0.0000		-0.0001	0.0021
			(0.0035)		(0.0035)	(0.0038)
L. GDP growth			-0.0072		-0.0072	-0.0031
			(0.0092)		(0.0092)	(0.0088)
L. Democracy			0.0497***		0.0497***	0.0277
			(0.0162)		(0.0162)	(0.0174)
L. Government balance			0.0169*		0.0168*	0.0019
			(0.0098)		(0.0098)	(0.0082)
L. Trade openness			0.0009		0.0009	0.0002
			(0.0010)		(0.0010)	(0.0009)
L. Investments			-0.0046		-0.0045	-0.0072
			(0.0057)		(0.0057)	(0.0057)
L. Mineral rents			-0.0055		-0.0055	-0.0037
			(0.0092)		(0.0092)	(0.0103)
Constant			-3.6495		-3.6354	-5.0685**
			(2.4361)		(2.4344)	(2.1561)
Region fixed effects			Yes		Yes	Yes
Year fixed effects			Yes		Yes	Yes
Dependent variable:						L. Conditionality
L. Conditionality compound						-0.3872***

						(0.0632)
L. Dependency ratio						0.1745*
						(0.0894)
L. GDP growth						0.0858
						(0.0739)
L. Democracy						0.8617**
						(0.3565)
L. Government balance						0.1589*
						(0.0913)
L. Trade openness						0.0284
						(0.0175)
L. Investments						-0.0338
						(0.0755)
L. Mineral rents						-0.0965
						(0.2214)
Constant						-11.5171**
						(5.4552)
Country fixed effects						Yes
Year fixed effects						Yes
F-statistic for participation instrument			151.82		149.76	134.69
F-statistic for conditionality instrument						37.54
Joint F-statistic						177.88
Number of observations	2,557	2,557	2,557	2,557	2,557	2,557
Number of countries	141	141	141	141	141	141

Notes: F-tests are Kleibergen-Paap statistics. Standard errors robust at the country-level in brackets. Significance level is denoted by *** (1%), ** (5%) and * (10%).

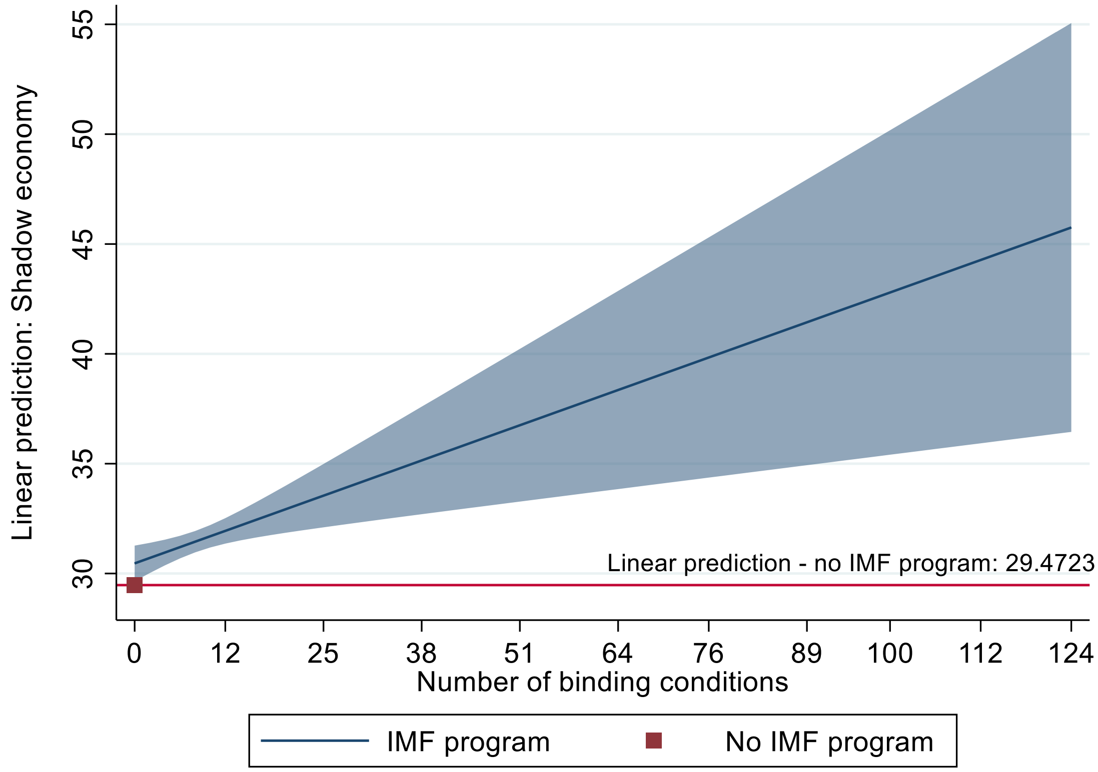


Figure 1.1.3 Marginal effect of total (binding) conditions on the shadow economy

1.1.4.2 Further analyses

In this part of our analyses, we examine the effect of IMF intervention on the shadow economy using alternative conditionality variables, presented in Table 1.1.4.[23] In some cases, the Fund's Executive Board can waive certain conditions in order to help countries pass the staff review without program terminations (Babb and Carruthers, 2008; Pop-Eleches, 2009; Stone, 2004). To account for this, we use an implementation-corrected count of conditions, which subtracts conditions waived by the IMF. As shown in specification 1, the estimated coefficients on IMF participation and conditions remain positive and significant. Next, we consider an implementation-discounted binding condition count, which discounts conditions during the

[23] All specifications of Table 1.1.4 use our preferred identification strategy (IV estimates for program participation and conditionality), addressing the endogeneity issues.

interruption period in case of delayed program review.[24] In specification 2, the results adopting an implementation-discounted measure of conditions remain substantively the same. In specification 3, we perform the same analysis using a combined (binding and non-binding) measure of conditions. The estimated coefficient on combined conditions remains positive and significant ($p<0.05$), but the coefficient declines in magnitude.[25] Diagnostic statistics across all specifications indicate that our compound instruments remain strong.

Furthermore, we conduct our analyses based on the quantitative-structural divide of conditionality, comparing the effect of two different conditionality types on the size of the unofficial economy. In Figure 1.1.4, we visualize the total count of structural and quantitative conditions per year in our sample. As we include two IMF conditionality variables in the model, the compound instrument for each conditionality profile is the interaction of the within-country average of the conditionality type with the year-on-year IMF's budget constraint (Stubbs et al., 2020), while for IMF participation we use the same compound instrumentation approach as above. In specification 4, the estimated coefficient on structural conditions is positive and statistically significant. Quantitative conditions do not have a significant impact. One structural condition increases the shadow economy by 0.3083 percentage points ($p<0.01$), all else equal. At the mean number of structural conditions, 1.6738, the predicted change in the size of the shadow economy is 0.5160 (=0.3083*1.6738). Figure 1.1.5 illustrates the marginal plot with the predicted values of the shadow economy and the 95 percent confidence interval (specification 4). In the absence of an IMF program, our model predicts a value of 29.9741 (horizontal line) for the shadow economy (slightly below the mean). For countries with IMF

[24] Using implementation corrected and discounted conditions our sample period is slightly reduced, since these counts of conditions are not available beyond 2009.

[25] Stubbs et al. (2017) point out that the inclusion of non-binding conditions may introduce noise to the analysis.

programs, the estimated value is 31.0768, which subsequently increases to 55.7407 as the number of structural conditions increases. The difference of 24.6639 is about 1.9 times greater than one standard deviation in the shadow economy measure (13.1336). Diagnostic statistics show that this instrumentation strategy is valid.

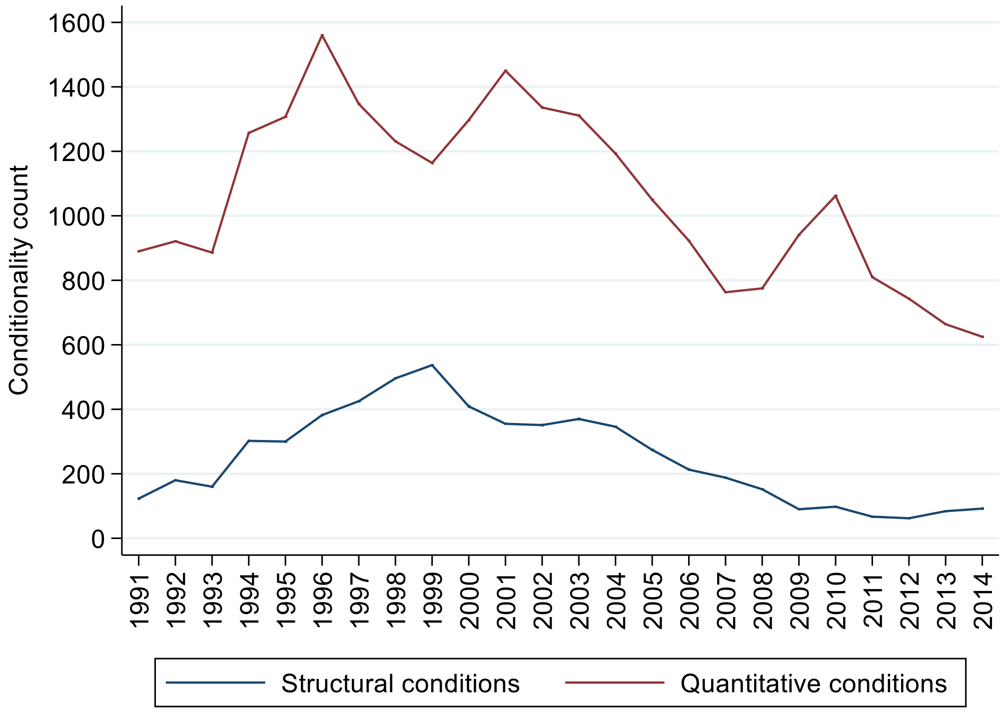


Figure 1.1.4 Total count of structural vs. quantitative conditions per year

Table 1.1.4 Effect of IMF intervention on the shadow economy, composite indicators of conditionality

Conditionality variable:	(1) Implementation-corrected	(2) Implementation-discounted binding	(3) Binding and non-binding	(4) Structural vs. quantitative
Dependent variable:		*Shadow economy*		
L. IMF participation	0.7645**	0.6836**	1.0939***	1.1027***
	(0.3229)	(0.2926)	(0.3971)	(0.3638)
L. IMF conditions	0.1153**	0.1377***	0.0772**	
	(0.0457)	(0.0454)	(0.0343)	
L. IMF structural conditions				0.3083***
				(0.0755)
L. IMF quantitative conditions				-0.0046
				(0.0558)
L. Dependency ratio	0.0176	0.0129	0.0173	0.0239
	(0.0338)	(0.0337)	(0.0315)	(0.0322)
L. GDP growth	-0.1027***	-0.1055***	-0.0842***	-0.0748***
	(0.0208)	(0.0211)	(0.0189)	(0.0191)
L. Democracy	-0.2098	-0.1764	-0.2108	-0.1225
	(0.1434)	(0.1477)	(0.1389)	(0.1360)

L. Government balance	-0.0622*	-0.0654*	-0.0697**	-0.0569*
	(0.0361)	(0.0348)	(0.0333)	(0.0331)
L. Trade openness	-0.0332**	-0.0326***	-0.0330***	-0.0333***
	(0.0130)	(0.0126)	(0.0102)	(0.0108)
L. Investments	-0.0702**	-0.0715**	-0.0501**	-0.0532**
	(0.0287)	(0.0293)	(0.0254)	(0.0262)
L. Mineral rents	-0.0007	-0.0231	-0.0645	-0.0373
	(0.0886)	(0.0840)	(0.0699)	(0.0682)
Constant	22.7814***	54.3428***	21.9727***	20.9384***
	(2.3595)	(3.9206)	(1.9777)	(2.1196)
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Dependent variable:		*L. IMF participation*		
L. Participation compound	0.4780***	0.4747***	0.4186***	0.4083***
	(0.0440)	(0.0439)	(0.0323)	(0.0356)
L. Past programs	1.2198***	1.2801***	1.1434***	1.1546***
	(0.0782)	(0.0807)	(0.0611)	(0.0639)
L. Countries under program	0.0618***	0.0569***	0.0562	0.0669*
	(0.0120)	(0.0116)	(0.0417)	(0.0372)
L. GDP per capita	-0.0781	-0.0769	-0.0587	-0.1093**
	(0.0667)	(0.0632)	(0.0521)	(0.0436)
L. Executive election	0.1192	0.0701	0.1393	0.1501*
	(0.1012)	(0.0996)	(0.0867)	(0.0883)
L. Regime Durability	-0.0053*	-0.0045*	-0.0009	-0.0004
	(0.0028)	(0.0024)	(0.0015)	(0.0015)
L. Dependency ratio	0.0058	0.0074	0.0012	0.0032
	(0.0052)	(0.0050)	(0.0038)	(0.0038)
L. GDP growth	0.0034	0.0015	-0.0076	-0.0021
	(0.0083)	(0.0086)	(0.0086)	(0.0085)
L. Democracy	0.0111	0.0201	0.0276	0.0344**
	(0.0217)	(0.0205)	(0.0178)	(0.0167)
L. Government balance	0.0188**	0.0231**	0.0027	0.0021
	(0.0093)	(0.0090)	(0.0089)	(0.0082)
L. Trade openness	-0.0011	-0.0010	-0.0008	0.0001
	(0.0011)	(0.0012)	(0.0009)	(0.0009)
L. Investments	0.0058	0.0068	-0.0028	-0.0064
	(0.0069)	(0.0065)	(0.0057)	(0.0058)
L. Mineral rents	-0.0124	-0.0170*	-0.0031	-0.0027
	(0.0096)	(0.0091)	(0.0094)	(0.0110)
Constant	-4.7921***	-4.7876***	-4.3864*	-4.7649**
	(0.7658)	(0.7205)	(2.3787)	(2.1712)
Region fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Dependent variable (conditions):	*L. Implementation-corrected*	*L. Implementation-discounted binding*	*L. Binding and non-binding*	*L. Structural conditions*
L. Conditionality compound	-0.3433***	-0.3396***	-0.3250***	-0.7281***
	(0.0600)	(0.0637)	(0.0584)	(0.0920)
L. Dependency ratio	0.1432	0.1617	0.2385*	0.0319
	(0.1075)	(0.1043)	(0.1323)	(0.0197)
L. GDP growth	0.1016	0.1040	0.0568	-0.0090
	(0.0709)	(0.0776)	(0.1106)	(0.0334)
L. Democracy	0.7890**	0.4770	1.6145***	0.1223
	(0.3692)	(0.3977)	(0.5528)	(0.1476)
L. Government balance	0.2110**	0.2054**	0.2094	0.0126

	(0.0956)	(0.0876)	(0.1304)	(0.0284)
L. Trade openness	0.0269	0.0198	0.0308	0.0048
	(0.0182)	(0.0183)	(0.0244)	(0.0050)
L. Investments	0.0146	0.0262	-0.0832	-0.0061
	(0.0881)	(0.0713)	(0.1077)	(0.0195)
L. Mineral rents	-0.1296	0.0545	-0.0366	-0.0769*
	(0.2769)	(0.2293)	(0.2921)	(0.0421)
Constant	-11.0392**	-22.7298**	-17.8428**	-1.4239
	(5.4025)	(11.0900)	(7.6767)	(1.6890)
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Dependent variable (conditions):				*L. Quantitative conditions*
L. Conditionality compound				-0.2918***
				(0.0652)
L. Dependency ratio				0.1403*
				(0.0744)
L. GDP growth				0.1005**
				(0.0504)
L. Democracy				0.7359***
				(0.2570)
L. Government balance				0.1401*
				(0.0727)
L. Trade openness				0.0241
				(0.0148)
L. Investments				-0.0264
				(0.0606)
L. Mineral rents				-0.0080
				(0.1893)
Constant				-10.7196**
				(4.3214)
Country fixed effects				Yes
Year fixed effects				Yes
F-statistic for participation instrument	118.19	117.16	167.56	131.87
F-statistic for conditionality instrument	32.69	28.45	30.99	
F-statistic for structural conditionality instrument				62.59
F-statistic for quantitative conditionality instrument				20.05
Joint F-statistic	166.01	155.97	196.26	196.35
Number of observations	2,035	2,035	2,557	2,557
Number of countries	140	140	141	141

Notes: F-tests are Kleibergen-Paap statistics. Standard errors robust at the country-level in brackets. Significance level is denoted by *** (1%), ** (5%) and * (10%).

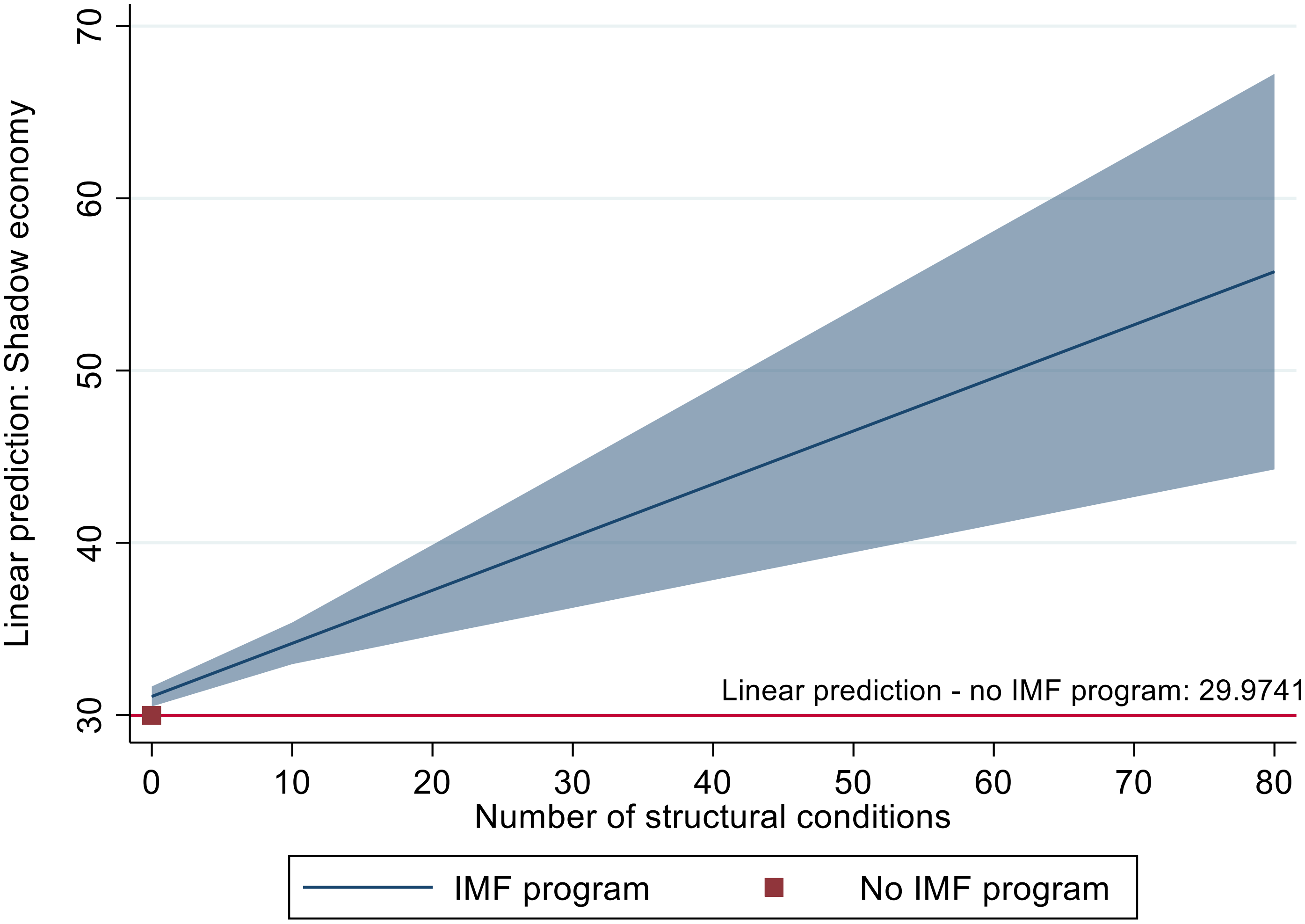


Figure 1.1.5 Marginal effect of structural conditions on the shadow economy

As mentioned in subsection 1.1.2.2.1, we expect that the detrimental effect of structural conditions will remain in the long-run. To test the long-run effect of structural conditions on the shadow economy, we re-estimate our model (by disaggregating IMF conditions into structural and quantitative) and include deeper lags in all predictors. We find that structural conditions maintain their detrimental effect in the long-term (across all lags structural conditions retain a positive and statistically significant sign). The results of this exercise are reported in Table 1.1.5.

Table 1.1.5 Long-run effects – structural vs. quantitative conditions

	(1)	(2)	(3)	(4)
	Dependent variable: Shadow economy			
	t-2	t-3	t-4	t-5
IMF participation	0.7464**	0.7767**	0.5273	0.3240
	(0.3216)	(0.3803)	(0.4077)	(0.3652)
IMF structural conditions	0.3163***	0.3164***	0.3516***	0.2909**
	(0.0721)	(0.0888)	(0.1230)	(0.1163)
IMF quantitative conditions	-0.0340	-0.0302	-0.0158	-0.0124
	(0.0450)	(0.0678)	(0.0933)	(0.0781)
Control variables	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
F-statistic for participation instrument	153.1	152.05	150.44	114.46
F-statistic for structural conditionality instrument	67.04	64.49	44.73	42.68
F-statistic for quantitative conditionality instrument	14.68	12.31	8.73	9.16
Joint F-statistic	222.16	214.25	194.50	162.16
Number of observations	2,455	2,313	2,173	2,034
Number of countries	141	141	139	139

Notes: Reported results for the outcome equation (Equation (3)) using deeper lags. All predictors are included at the lag specified in the column header. Each model includes an outcome equation and three selection equations (IMF participation, structural and quantitative conditions). The IMF participation variable is instrumented using the interaction of the within-country average of IMF program participation across period of interest with the natural log of the IMF liquidity ratio. Compound instrumentation for each conditionality profile (structural vs. quantitative conditions) is the interaction of the within-country average of the conditionality type with the year-on-year IMF budget constraint (approximated by the natural log of the IMF liquidity ratio). F-tests are Kleibergen-Paap statistics. Standard errors robust at the country-level in brackets. Significance level is denoted by *** (1%) and ** (5%).

Finally, we examine how financial development with IMF conditions co-determines the shadow economy (Table 1.1.6). The financial sector can have a direct effect on the informal economy (e.g., Antunes and Cavalcanti, 2007; Beck and Hoseini, 2014; Beck et al., 2014; Dabla-Norris et al., 2008; Ellul et al., 2015). Specifically, financial development is found to reduce the size of the shadow economy, as the development of financial sector decreases the barriers attaining capital, facilitate entrepreneurs access to needed credit, increases the opportunity cost of producing in the underground economy, which in turn, encourage economic agents to transition from the informal sector to the formal sector where they can make productive investments (e.g., Berdiev and Saunoris, 2016; Blackburn et al., 2012; Bose et al., 2012; Capasso and Jappelli, 2013; Straub, 2005).

Having this in mind, we use data from Svirydzenka (2016), who constructed an index of financial development,[26] and we provide evidence on the relationship between IMF intervention, financial development and the shadow economy by including in our analysis the index of financial development and the interaction term of financial development with IMF conditions. We do this not only to examine the impact of financial development, but also to test the effect of IMF conditions on the size of the shadow economy conditional upon financial development. We find that financial development has a negative effect on the size of the shadow economy ($p<0.10$). The coefficient on IMF conditions remains positive and statistically significant ($p<0.01$). The interaction term is negative, but statistically insignificant. We then examine the marginal effect of the interaction term (L. IMF conditions * L. Financial development) for different values of financial development index (results for the marginal effects are provided at the bottom of Table 1.1.6). Our results indicate that as the value of financial development is increasing the marginal effects of IMF conditions slightly decrease, however, the sign of the reported marginal effects remains positive for all different values of financial development, indicating that, while a higher level of financial development leads to a smaller shadow economy, financial development is unable to reverse the adverse effect from IMF conditions.

Table 1.1.6 Financial development, IMF intervention and the shadow economy

	(1)
Dependent variable:	*Shadow economy*
L. IMF participation	1.0582***
	(0.3767)
L. IMF conditions	0.1411***
	(0.0371)
L. Financial development	-4.1342*
	(2.4442)

[26] The index of financial development is a relative ranking of countries composed of eight sub-indexes that summarize how developed financial markets and financial institutions are along three dimensions (depth, access, and efficiency) using a large number of indicators. It ranges between 0 and 1 (higher values more developed).

L. Financial development * L. IMF conditions	-0.0260
	(0.0587)
L. Dependency ratio	0.0187
	(0.0333)
L. GDP growth	-0.0973***
	(0.0190)
L. Democracy	-0.2426*
	(0.1408)
L. Government balance	-0.0737**
	(0.0344)
L. Trade openness	-0.0341***
	(0.0101)
L. Investments	-0.0513**
	(0.0247)
L. Mineral rents	-0.0445
	(0.0728)
Constant	23.8205***
	(2.1112)
Country fixed effects	Yes
Year fixed effects	Yes
Dependent variable:	*L. IMF participation*
L. Participation compound	0.4008***
	(0.0344)
L. Past programs	1.1778***
	(0.0618)
L. Countries under program	0.0721*
	(0.0372)
L. GDP per capita	-0.1158**
	(0.0507)
L. Executive election	0.1552*
	(0.0860)
L. Regime Durability	-0.0012
	(0.0016)
L. Dependency ratio	0.0023
	(0.0038)
L. GDP growth	-0.0031
	(0.0088)
L. Democracy	0.0250
	(0.0176)
L. Government balance	0.0037
	(0.0082)
L. Trade openness	0.0002
	(0.0009)
L. Investments	-0.0073
	(0.0058)
L. Mineral rents	-0.0037
	(0.0104)
L. Financial development	0.2239
	(0.2719)
Constant	-4.9200**

	(2.1381)
Region fixed effects	Yes
Year fixed effects	Yes
Dependent variable:	*L. Conditionality*
L. Conditionality compound	-0.4197***
	(0.0601)
L. Dependency ratio	0.2046**
	(0.0902)
L. GDP growth	0.0770
	(0.0745)
L. Democracy	0.7866**
	(0.3612)
L. Government balance	0.1749**
	(0.0871)
L. Trade openness	0.0322*
	(0.0169)
L. Investments	-0.0182
	(0.0746)
L. Mineral rents	-0.0883
	(0.2208)
L. Financial development	-19.3818***
	(6.6678)
Constant	-5.6651
	(5.6679)
Country fixed effects	Yes
Year fixed effects	Yes
F-statistic for participation instrument	135.89
F-statistic for conditionality instrument	48.77
Joint F-statistic	194.71
Marginal effects of IMF conditions on the shadow economy	
at Financial development=0	0.1411***
	(0.0371)
at Financial development=0.25	0.1346***
	(0.0332)
at Financial development=0.50	0.1281***
	(0.0354)
at Financial development=0.75	0.1216***
	(0.0429)
at Financial development=1	0.1151**
	(0.0535)
Changing from 1 to 0	-0.0260
Number of observations	2,550
Number of countries	140

Notes: F-tests are Kleibergen-Paap statistics. Standard errors robust at the country-level in brackets. Significance level is denoted by *** (1%), ** (5%) and * (10%).

1.1.4.3 Robustness tests

In the Appendix, we conduct robustness tests and briefly report the findings here. First, we examine our baseline results (Table 1.1.3) using alternative instruments for IMF participation. According to Dreher et al. (2018), variables approximating geopolitical importance impact upon the decision to participate in IMF programs without necessarily affecting most domestic economic outcomes of interest. Thus, the first alternative instrumental variable that we use is United Nations General Assembly (UNGA) voting similarity with the US. Previous studies document that, all else equal, countries that vote similarly to the US are more likely to participate in IMF programs (Dreher and Gassebner, 2012; Steinwand and Stone, 2008; Woo, 2013). An alternative candidate proxy for geopolitical importance is temporary membership in the United Nations Security Council (UNSC). Major shareholders of the IMF may care about how countries vote and some countries are willing to trade their votes for IMF loans (Dreher et al., 2009c). In addition, UNSC membership can certainly affect IMF's decision to extend a program to a country (Caraway et al., 2012; Chwieroth, 2015; Dreher and Jensen, 2007; Dreher et al., 2015; Nelson, 2014; Woo, 2013). Furthermore, we make use of a compound instrument that is the interaction of the within-country average of IMF program participation across period of interest with the Fund's budget constraint, approximated by the number of countries with an IMF program in a given year (Forster et al., 2019; Vreeland, 2003). Previous studies highlight that if the IMF need to assist more countries, its liquid resources become more constrained and so it tends to sign fewer new lending programs (Dreher and Vaubel, 2004; Lang, 2020; Vreeland, 2003). We believe that the above alternative instruments can explain variation in IMF program participation and none of them directly affect country's shadow economy. Using these alternative instruments to account for the endogeneity of IMF participation do not substantively alter results, however only the compound instrument (specification 3) appears to be strong (with a Kleibergen-Paap F-statistic above ten). The results are reported in Appendix Table A1.1.2.

Second, we replicate our findings using an alternative proxy for the shadow economy from Elgin and Oztunali (2012) who estimate the size of the shadow economy (% of GDP) by employing a two-sector dynamic general equilibrium model. Using this alternative proxy for the shadow economy, we show that, while IMF participation (binary variable) is found to be statistically insignificant throughout, IMF binding conditions (specification 1), implementation-corrected conditions (specification 2), implementation-discounted conditions (specification 3), binding and non-binding conditions (specification 4), and structural conditions (specification 5) all have a positive and statistically significant coefficient. The results of this exercise are reported in Appendix Table A1.1.3. In addition, using this alternative measure for the shadow economy, we show that the effect of IMF structural conditions remains detrimental and statistically significant in the long-term (Appendix Table A1.1.4).

In Appendix Table A1.1.5, we probe robustness to dynamic estimation using error correction models (ECMs). In ECMs, the dependent variable is the change of the shadow economy, regressed on its lagged level, and levels and changes of all predictors. The benefit of these models is to provide a flexible model structure to uncover dynamic relationships, but this comes at the cost of additional complexity,[27] and the results from this exercise need to be interpreted with caution. As reflected in the respective coefficients on the differenced variables, none of the conditions exerts an instantaneous effect on the shadow economy. The same holds

[27] There is a debate in the empirical political economy literature on the conditions under which ECMs are appropriate. More specifically, some methodologists argue that ECMs provide a flexible estimation structure even in the absence of cointegration relationships (De Boef and Keele, 2008; Beck and Katz, 2011), while others question the use of ECMs as a single-step estimation method (Grant and Lebo, 2016).

for the differenced and lagged variables of IMF program participation. In contrast, we find that IMF binding conditions (specification 1), implementation-corrected conditions (specification 2), implementation-discounted conditions (specification 3), binding and non-binding conditions (specification 4), and structural conditions (specification 5) all have detrimental long-term implications for the shadow economy. This result is consistent with our previous findings obtained from a simpler statistical model.

As has been argued in subsection 1.1.2.2.3, the IMF can also influence the shadow economy via its money (loan size). To account for the amount of money agreed, we incorporate in our empirical model the measure of IMF loans disbursed. The variable of IMF loans disbursed may be endogenous (e.g., it is possible that there is a mutual interdependence between the amount of money agreed and the size of the shadow economy) and invalidate our analysis. To account for endogeneity of IMF loans disbursed we use the compound variable ($\overline{IMF\ loans}_i \times Budget_t$, where $\overline{IMF\ loans}_i$ is the within-country average of the natural log of disbursed loans across period of interest and $Budget_t$ is the Fund's budget constraint, approximated by the natural log of the IMF liquidity ratio) as an instrument for IMF loans disbursed. Our argument for the validity of this compound instrument is similar to the compound instrument for IMF participation (see subsection 1.1.3.2). The IMF loans equation includes in the right-hand side the compound instrument, $\overline{IMF\ loans}_i \times Budget_t$, GDP per capita, regime durability, executive elections and the vector of explanatory variables from Equation (1.1.3), X, country fixed effects, μ, and year fixed effects, δ. Incorporating IMF loans disbursed to our model, we first show that IMF participation (binary variable) is found to be positive and statistically significant throughout. IMF binding conditions (specification 1), implementation-corrected conditions (specification 2), implementation-discounted conditions (specification 3), binding and non-binding conditions (specification 4), and structural conditions (specification 5) all have a positive and statistically significant coefficient. In addition, the variable of IMF loans disbursed exert a significant positive impact on the shadow economy throughout. The results of this exercise are reported in Appendix Table A1.1.6.

In Appendix Table A1.1.7, we augment our models by including additional explanatory variables in separate specifications.[28] We control for political stability, omitted in the baseline models due to concerns of multicollinearity with democracy. As mentioned, it is expected to be negatively correlated with the shadow economy (e.g., Elbahnasawy et al., 2016; Torgler and Schneider, 2009). Further, we account for the cost of bureaucracy, higher bureaucracy costs may lead individuals to go underground (Friedman et al., 2000). In addition, we add to the vector of controls a variable which is related to the enforcement of the law, namely the rule of law. As previous studies have shown, a weaker legal environment is associated with a larger unofficial economy (e.g., Berdiev et al., 2018; Friedman et al., 2000; Torgler and Schneider 2009). We also include the cost of starting a business. According to Goel et al. (2016), greater startup costs increase entry of shadow entrepreneurs. Finally, we account for the top marginal tax rate. High taxes increase the cost of doing business, which may induce some firms to move into the shadow sector (Gërxhani, 2004; Herwartz et al., 2011; Schneider and Enste, 2000).[29] Recall that these variables are excluded from the baseline analyses since they block potential pathways we aim to measure (post-treatment bias). For instance, by controlling for bureaucracy costs, we do not allow for IMF programs to affect the size of the shadow economy through the hollowing out of state capacity (Reinsberg et al., 2019a).

The inclusion of political stability, bureaucracy costs, rule of law and top income tax rate do not affect any of our analyses. When we include the cost of starting a business

[28] A description of these variables is also provided in Table 1.1.1 and summary statistics are reported in Table 1.1.2.

[29] Inclusion of the additional variables in separate specifications reduces the number of observations.

(specification 4), the variable of IMF program participation turns insignificant, however, the coefficient on IMF conditions remains positive and statistically significant ($p<0.01$). Political stability (specification 1) and the rule of law (specification 3) are important (and statistically significant ($p<0.05$)) predictors of the shadow economy, and as we expected they both have a negative sign. Overall, the results remain substantively the same compared to our baseline analyses (Table 1.1.3), with the exception of specification 4 where we include the cost of starting a business and the variable of IMF participation turns insignificant.

Finally, we include in our models a set of variables that may be driving the relationship between IMF intervention and the informal economy (see subsection 1.1.2.2). To reflect fiscal policy, we include government spending as a share of GDP (we replace the variable of government balance with government spending). We control for the cost of bureaucracy, reflecting the state capacity. In addition, we control for hiring and firing costs, reflecting the level of labour rights. To capture the potential effect of burdensome taxes, we include top marginal tax rate. Further, to capture the potential effect of bribes, we incorporate control of corruption. Our sample is reduced by more than 50% due to missing data on most of the additional variables. Including these variables in specification 1 to 4 does not substantively alter our results. The estimated coefficients on IMF program participation and conditions maintain their positive and statistically significant sign. In specification 5, where we disaggregate IMF conditions into structural and quantitative, the coefficient on IMF participation is positive but now statistically insignificant. In addition, the coefficient on structural conditions is positive, less significant ($p<0.10$) and the magnitude of this effect is smaller compared to our baseline analysis (Table 1.1.4). Quantitative conditions do not have a consistent effect on the shadow economy. The results of this exercise are reported in Appendix Table A1.1.8.

1.1.5 Concluding remarks

In this subchapter we provide new insights regarding the impact of IMF program participation and conditionality on the size of the informal economy using a world sample over the 1991-2014 period. Our baseline results suggest that both IMF participation and IMF-mandated conditions increase the size of the shadow economy after controlling for politico-economic factors and endogeneity. Once we differentiate IMF conditions, we show that structural conditions – microeconomic reforms – are significantly related to a larger shadow economy. In contrast, quantitative (or stabilization) conditions – quantitative targets for monetary and fiscal indicators – do not have a consistent effect on the shadow economy. We also find that structural conditions exert a long-term adverse effect on the shadow economy of recipient countries. Finally, financial development, a crucial factor of underground activities, is negatively linked to the informal economy, however it cannot reverse the detrimental effect from IMF conditionality.

In contrast to previous studies which examine the effect of IMF programs on the shadow economy, this study, first, by accounting for both IMF program participation and IMF conditions and, second, by disaggregating IMF conditions into structural and quantitative, highlights the heterogeneous effect of IMF program participation and conditionality on the size of the shadow economy. Our results confirm the theoretical arguments on the pathways through which IMF intervention affects the informal economy. With respect to IMF participation, the possible effect of moral hazard, the persistence of inappropriate policies and pernicious effects on borrowing countries' domestic political environment, all have a detrimental effect on the shadow economy. Regarding IMF conditionality, and especially structural conditions: The findings support our theoretical considerations which point out that the implementation of structural conditions is linked to the deterioration of labour rights, and the reduction of government capacity and employment opportunities in the formal economy due to the privatization of state-owned enterprises, all of which have detrimental effects on the informal economy. In addition, these results support a theoretical argument which points out that structural conditions induce concentrated losses on well-defined social groups, and the latter will offer bribes to public officials to secure protection through alternative means. These officials are susceptible to this pressure because their own political survival depends on the support of key groups. In turn, governments tolerate the size of the shadow economy in order to continue rent distribution to powerful groups.

Lastly, as mentioned above, due to its secret nature, it is inherently difficult to provide a complete picture of the shadow economy. All available methods which estimate the size of the shadow economy on a cross country-level have their advantages and disadvantages. Therefore, further data (e.g., informal labour, illicit finance, or smuggling) can enable more detailed analyses. We also welcome case studies that complement this quantitative evidence and shed further light on the causes of the shadow economy, explicating individual pathways within the policy reforms discussed.

Appendix 1.1

Table A1.1.1 List of countries

Albania	Comoros	Honduras	Mexico	Slovak Republic
Algeria	Congo, Dem. Rep.	Hungary	Moldova	Slovenia
Angola	Congo, Rep.	India	Mongolia	Solomon Islands
Argentina	Costa Rica	Indonesia	Morocco	South Africa
Armenia	Cote d'Ivoire	Iran, Islamic Rep.	Mozambique	Spain
Australia	Croatia	Ireland	Namibia	Sri Lanka
Austria	Cyprus	Israel	Nepal	Sweden
Azerbaijan	Czech Republic	Italy	Netherlands	Switzerland
Bahrain	Denmark	Jamaica	New Zealand	Syrian Arab Republic
Bangladesh	Dominican Republic	Japan	Nicaragua	Tajikistan
Belarus	Ecuador	Jordan	Niger	Tanzania
Belgium	Egypt, Arab Rep.	Kazakhstan	Nigeria	Thailand
Benin	El Salvador	Kenya	Norway	Togo
Bhutan	Equatorial Guinea	Korea, Rep.	Oman	Tunisia
Bolivia	Eritrea	Kuwait	Pakistan	Turkey
Bosnia and Herzegovina	Estonia	Kyrgyz Republic	Papua New Guinea	Uganda
Botswana	Fiji	Lao PDR	Paraguay	Ukraine
Brazil	Finland	Latvia	Peru	United Arab Emirates
Bulgaria	France	Lebanon	Philippines	United Kingdom
Burkina Faso	Gabon	Lesotho	Poland	United States
Burundi	Gambia, The	Liberia	Portugal	Uruguay
Cambodia	Georgia	Libya	Qatar	Venezuela, RB
Cameroon	Ghana	Lithuania	Romania	Vietnam
Canada	Greece	Madagascar	Russian Federation	Zambia
Central African Republic	Guatemala	Malawi	Rwanda	Zimbabwe
Chad	Guinea	Malaysia	Saudi Arabia	
Chile	Guinea-Bissau	Mali	Senegal	
China	Guyana	Mauritania	Sierra Leone	
Colombia	Haiti	Mauritius	Singapore	

Table A1.1.2 Alternative instrumentation strategy

Instrumentation strategy for IMF participation:	(1) UNGA	(2) UNSC	(3) Compound
Dependent variable:		*Shadow economy*	
L. IMF participation	1.0033***	0.9893***	0.9659**
	(0.3684)	(0.3623)	(0.3759)
L. IMF conditions	0.1156***	0.1167***	0.1187***
	(0.0428)	(0.0428)	(0.0385)
L. Dependency ratio	0.0117	0.0112	0.0149
	(0.0315)	(0.0316)	(0.0309)
L. GDP growth	-0.0885***	-0.0878***	-0.0896***
	(0.0193)	(0.0193)	(0.0190)
L. Democracy	-0.1984	-0.2086	-0.1818
	(0.1405)	(0.1414)	(0.1374)
L. Government balance	-0.0726**	-0.0722**	-0.0724**
	(0.0336)	(0.0336)	(0.0333)
L. Trade openness	-0.0342***	-0.0339***	-0.0343***
	(0.0102)	(0.0104)	(0.0101)
L. Investments	-0.0545**	-0.0550**	-0.0519**
	(0.0251)	(0.0251)	(0.0253)
L. Mineral rents	-0.0673	-0.0652	-0.0518
	(0.0718)	(0.0721)	(0.0722)
Constant	22.2656***	22.3645***	22.0100***
	(2.0113)	(2.0299)	(1.9646)
Country fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Dependent variable:		*L. IMF participation*	
L. UNGA	1.4343**		
	(0.5754)		
L. UNSC		0.1665*	
		(0.0951)	
L. Participation compound			0.0433***
			(0.0036)
L. Past programs	1.4056***	1.3921***	1.1313***
	(0.0826)	(0.0857)	(0.0640)
L. Countries under program	0.0179	0.0381	0.0218
	(0.0370)	(0.0349)	(0.0379)
L. GDP per capita	-0.2686***	-0.2487***	-0.0808*
	(0.0883)	(0.0857)	(0.0455)
L. Executive election	0.3060***	0.2700**	0.1083
	(0.1145)	(0.1162)	(0.0929)
L. Regime Durability	-0.0032	-0.0028	0.0000
	(0.0022)	(0.0023)	(0.0014)
L. Dependency ratio	0.0037	0.0057	0.0023
	(0.0046)	(0.0048)	(0.0037)
L. GDP growth	-0.0045	-0.0057	-0.0022
	(0.0094)	(0.0089)	(0.0088)
L. Democracy	0.0604**	0.0731***	0.0289
	(0.0263)	(0.0261)	(0.0181)
L. Government balance	0.0044	0.0006	0.0034
	(0.0085)	(0.0087)	(0.0085)
L. Trade openness	0.0004	0.0004	-0.0002
	(0.0013)	(0.0012)	(0.0009)
L. Investments	-0.0033	-0.0035	-0.0056

	(0.0062)	(0.0060)	(0.0060)
L. Mineral rents	0.0097	0.0091	-0.0022
	(0.0109)	(0.0115)	(0.0109)
Constant	-1.0328	-2.0451	-2.3236
	(2.1517)	(2.0405)	(2.1386)
Region fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Dependent variable:		*L. Conditionality*	
L. Conditionality compound	-0.3958***	-0.3926***	-0.4109***
	(0.0614)	(0.0619)	(0.0624)
L. Dependency ratio	0.1995**	0.2016**	0.1626*
	(0.0913)	(0.0902)	(0.0882)
L. GDP growth	0.0792	0.0716	0.0911
	(0.0752)	(0.0749)	(0.0739)
L. Democracy	0.9976**	1.0828***	0.8137**
	(0.3877)	(0.3819)	(0.3564)
L. Government balance	0.1536*	0.1503	0.1560*
	(0.0910)	(0.0916)	(0.0914)
L. Trade openness	0.0286	0.0257	0.0292*
	(0.0189)	(0.0177)	(0.0177)
L. Investments	-0.0166	-0.0122	-0.0354
	(0.0737)	(0.0730)	(0.0754)
L. Mineral rents	0.0289	0.0120	-0.1070
	(0.2346)	(0.2371)	(0.2249)
Constant	-13.6536**	-14.4350***	-10.9068**
	(5.7091)	(5.5250)	(5.4728)
Country fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
F-statistic for participation instrument	6.21	3.07	141.52
F-statistic for conditionality instrument	41.50	40.24	43.34
Joint F-statistic	46.40	42.20	177.05
Number of observations	2,557	2,557	2,557
Number of countries	141	141	141

Notes: F-tests are Kleibergen-Paap statistics. Standard errors robust at the country-level in brackets. Significance level is denoted by *** (1%), ** (5%) and * (10%).

Table A1.1.3 Alternative proxy for the shadow economy (Elgin and Oztunali, 2012)

Conditionality variable:	(1) Binding	(2) Implementation-corrected	(3) Implementation-discounted binding	(4) Binding and non-binding	(5) Structural vs. quantitative
Dependent variable:	*Shadow economy (Elgin and Oztunali, 2012)*				
L. IMF participation	-0.0105	-0.0265	-0.1906	-0.0446	-0.0494
	(0.2999)	(0.3047)	(0.2833)	(0.3105)	(0.2855)
L. IMF conditions	0.0758**	0.0892**	0.1135***	0.0581**	
	(0.0374)	(0.0365)	(0.0374)	(0.0269)	
L. IMF structural conditions					0.2059*
					(0.1140)
L. IMF quantitative conditions					-0.0205
					(0.0464)
L. Dependency ratio	-0.0071	-0.0078	-0.0119	-0.0044	0.0011
	(0.0367)	(0.0368)	(0.0375)	(0.0370)	(0.0338)
L. GDP growth	0.0073	0.0057	0.0040	0.0113	0.0177
	(0.0228)	(0.0230)	(0.0237)	(0.0225)	(0.0234)
L. Democracy	0.0155	0.0103	0.0397	-0.0281	0.0792
	(0.0879)	(0.0882)	(0.0900)	(0.0948)	(0.0796)
L. Government balance	0.0230	0.0212	0.0158	0.0239	0.0402**
	(0.0183)	(0.0186)	(0.0182)	(0.0182)	(0.0200)
L. Trade openness	-0.0129	-0.0131	-0.0127	-0.0122	-0.0128
	(0.0098)	(0.0099)	(0.0098)	(0.0097)	(0.0096)
L. Investments	-0.0734***	-0.0725***	-0.0738***	-0.0719***	-0.0730***
	(0.0170)	(0.0171)	(0.0178)	(0.0167)	(0.0162)
L. Mineral rents	0.0664	0.0684	0.0467	0.0707	0.0842
	(0.0545)	(0.0559)	(0.0578)	(0.0557)	(0.0529)
Constant	21.5486***	21.6165***	21.4373***	21.7010***	20.5814***
	(1.6457)	(1.6403)	(1.6459)	(1.6439)	(1.5869)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Dependent variable:	*L. IMF participation*				
L. Participation compound	0.5127***	0.5129***	0.4999***	0.5228***	0.4406***
	(0.0484)	(0.0478)	(0.0478)	(0.0430)	(0.0323)
L. Past programs	1.2345***	1.2202***	1.2844***	1.2057***	0.0638*
	(0.0787)	(0.0797)	(0.0855)	(0.0782)	(0.0329)
L. Countries under program	0.0284**	0.0321***	0.0305***	0.0208*	1.0802***
	(0.0110)	(0.0104)	(0.0116)	(0.0108)	(0.0734)
L. GDP per capita	-0.0683	-0.0636	-0.0529	-0.0132	-0.1325***
	(0.0675)	(0.0670)	(0.0633)	(0.0743)	(0.0438)
L. Executive election	0.0758	0.0872	0.0504	0.0636	0.2001**

	(0.1109)	(0.1105)	(0.1117)	(0.1049)	(0.0897)
L. Regime Durability	-0.0051*	-0.0051*	-0.0046**	-0.0039	-0.0041*
	(0.0026)	(0.0027)	(0.0023)	(0.0027)	(0.0021)
L. Dependency ratio	0.0055	0.0055	0.0079	0.0049	0.0010
	(0.0051)	(0.0049)	(0.0048)	(0.0051)	(0.0037)
L. GDP growth	0.0025	0.0039	0.0026	-0.0026	0.0013
	(0.0087)	(0.0085)	(0.0088)	(0.0087)	(0.0087)
L. Democracy	0.0290	0.0244	0.0331	0.0287	0.0288
	(0.0244)	(0.0243)	(0.0228)	(0.0236)	(0.0187)
L. Government balance	0.0211**	0.0206**	0.0226**	0.0187	0.0109
	(0.0106)	(0.0104)	(0.0100)	(0.0127)	(0.0098)
L. Trade openness	-0.0017	-0.0017	-0.0015	-0.0026**	-0.0009
	(0.0012)	(0.0012)	(0.0013)	(0.0012)	(0.0010)
L. Investments	0.0051	0.0048	0.0061	0.0088	-0.0001
	(0.0068)	(0.0068)	(0.0066)	(0.0065)	(0.0057)
L. Mineral rents	-0.0230**	-0.0220**	-0.0232**	-0.0212**	-0.0166
	(0.0105)	(0.0106)	(0.0107)	(0.0100)	(0.0104)
Constant	-2.8008***	-3.0102***	-3.3781***	-2.8488***	-4.2210**
	(0.6624)	(0.6491)	(0.6966)	(0.6749)	(1.9099)
Region fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Dependent variable (conditions):	*L. Binding*	*L. Implementation-corrected*	*L. Implementation-discounted binding*	*L. Binding and non-binding*	*L. Structural conditions*
L. Conditionality compound	-0.3451***	-0.3235***	-0.3234***	-0.2806***	-0.6062***
	(0.0825)	(0.0722)	(0.0805)	(0.0753)	(0.1035)
L. Dependency ratio	0.1820	0.1574	0.1721*	0.2109	0.0367
	(0.1118)	(0.1031)	(0.1024)	(0.1524)	(0.0245)
L. GDP growth	0.0754	0.0882	0.0846	0.0166	0.0028
	(0.0808)	(0.0731)	(0.0778)	(0.1159)	(0.0356)
L. Democracy	0.9226**	0.8444**	0.4965	2.0398***	0.1601
	(0.4151)	(0.3936)	(0.4310)	(0.6133)	(0.1740)
L. Government balance	0.2627**	0.2444**	0.2465***	0.3380**	0.0151
	(0.1041)	(0.0992)	(0.0915)	(0.1426)	(0.0325)
L. Trade openness	0.0257	0.0244	0.0159	0.0216	0.0047
	(0.0210)	(0.0205)	(0.0186)	(0.0292)	(0.0063)
L. Investments	0.0307	0.0239	0.0396	0.0101	0.0050
	(0.0905)	(0.0856)	(0.0705)	(0.1236)	(0.0229)
L. Mineral rents	-0.2424	-0.1949	0.0259	-0.3935	-0.0790
	(0.2627)	(0.2541)	(0.2523)	(0.3427)	(0.0565)
Constant	-14.0073**	-12.0987**	-8.9277*	-22.8717***	-2.2452
	(5.8512)	(5.3943)	(5.3828)	(7.7549)	(1.9713)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Dependent					*L.*

variable (conditions):					*Quantitative conditions*
L. Conditionality compound					-0.3038***
					(0.0742)
L. Dependency ratio					0.1349
					(0.0825)
L. GDP growth					0.1119**
					(0.0484)
L. Democracy					0.7533***
					(0.2727)
L. Government balance					0.1647**
					(0.0742)
L. Trade openness					0.0257*
					(0.0152)
L. Investments					0.0025
					(0.0668)
L. Mineral rents					0.0126
					(0.2332)
Constant					-11.3014***
					(4.3368)
Country fixed effects					Yes
Year fixed effects					Yes
F-statistic for participation instrument	112.11	115.07	109.17	148.13	186.02
F-statistic for conditionality instrument	17.52	20.09	16.16	13.87	
F-statistic for structural conditionality instrument					34.30
F-statistic for quantitative conditionality instrument					16.76
Joint F-statistic	138.46	152.26	137.55	161.21	244.71
Number of observations	1,954	1,954	1,954	1,954	1,954
Number of countries	141	141	141	141	141

Notes: F-tests are Kleibergen-Paap statistics. Standard errors robust at the country-level in brackets. Significance level is denoted by *** (1%), ** (5%) and * (10%).

Table A1.1.4 Long-run effects – structural vs. quantitative conditions, alternative proxy for the shadow economy (Elgin and Oztunali, 2012)

	(1)	(2)	(3)	(4)
	Dependent variable: Shadow economy (Elgin and Oztunali, 2012)			
	t-2	t-3	t-4	t-5
IMF participation	-0.1267	-0.0958	-0.0302	-0.0286
	(0.2704)	(0.2733)	(0.2589)	(0.2455)
IMF structural conditions	0.2271**	0.2310***	0.2527***	0.2395***
	(0.0915)	(0.0754)	(0.0656)	(0.0604)
IMF quantitative conditions	-0.0366	-0.0529**	-0.0522**	-0.0448**
	(0.0362)	(0.0265)	(0.0209)	(0.0182)
Control variables	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
F-statistic for participation instrument	132.91	113.06	106.78	89.40
F-statistic for structural conditionality instrument	26.02	17.66	11.82	11.06
F-statistic for quantitative conditionality instrument	8.44	7.91	15.95	13.63
Joint F-statistic	167.72	137.06	121.55	101.42
Number of observations	1,851	1,709	1,569	1,431
Number of countries	141	141	137	136

Notes: Reported results for the outcome equation (Equation (3)) using deeper lags and an alternative proxy for the shadow economy (Elgin and Oztunali, 2012). All predictors are included at the lag specified in the column header. Each model includes an outcome equation and three selection equations (IMF participation, structural and quantitative conditions). The IMF participation variable is instrumented using the interaction of the within-country average of IMF program participation across period of interest with the natural log of the IMF liquidity ratio. Compound instrumentation for each conditionality profile (structural vs. quantitative conditions) is the interaction of the within-country average of the conditionality type with the year-on-year IMF budget constraint (approximated by the natural log of the IMF liquidity ratio). F-tests are Kleibergen-Paap statistics. Standard errors robust at the country-level in brackets. Significance level is denoted by *** (1%) and ** (5%).

Table A1.1.5 Dynamic effects of IMF participation and conditions on the shadow economy

	(1)	(2)	(3)	(4)	(5)
Conditionality variable:	Binding	Implementation-corrected	Implementation-discounted binding	Binding and non-binding	Structural vs. quantitative
D.(IMF participation)	-0.0162	-0.1254	-0.1603	-0.0428	-0.027
	-0.109	-0.1246	-0.129	-0.133	-0.1713
Lagged IMF participation	0.0848	0.1227	0.0517	0.0375	0.0353
	-0.1746	-0.2014	-0.1598	-0.2014	-0.196
D.(IMF conditions)	-0.0014	0.0009	-0.0005	0.0006	
	-0.0038	-0.0042	-0.0047	-0.0029	
Lagged IMF conditions	0.1027***	0.1131**	0.0977**	0.0755***	
	-0.0337	-0.0485	-0.0352	-0.0234	
D.(IMF structural conditions)					0.0016
					-0.0046
Lagged IMF structural conditions					0.2809**
					-0.1095
D.(IMF quantitative conditions)					-0.0112
					-0.0107
Lagged IMF quantitative conditions					-0.0031
					-0.0395
Lagged dependent variable	-0.1850***	-0.1407***	-0.1405***	-0.1857***	-0.1835***
	-0.0382	-0.0278	-0.0274	-0.0379	-0.0382
Control variables	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
F-statistic for participation instrument	111.9	145.17	118.58	177.83	136.09
F-statistic for conditionality instrument	19.79	20.18	14.89	25.29	
F-statistic for structural conditionality instrument					62.19
F-statistic for quantitative conditionality instrument					20.07
Joint F-statistic	143.08	175.35	141.06	200.57	198.75
Number of observations	2,499	1,977	1,977	2,499	2,499
Number of countries	141	140	140	141	141

Notes: Error Correction Model implemented via multiple-equation instrumental-variable maximum-likelihood regression. The dependent variable in the outcome equation is the difference in the shadow economy (Medina and Schneider, 2018), regressed on lags and differences of IMF participation, conditions (as indicated in the column head), and control variables (not shown), respectively. The level of the IMF dummy is instrumented using the interaction of the within-country average of IMF program participation across period of interest with the natural log of the IMF liquidity ratio. The level of the specified conditions is instrumented using the interaction of the within-country average of the conditionality type with the natural log of the IMF liquidity ratio. F-tests are Kleibergen-Paap statistics. Standard errors robust at the country level in brackets. Significance level is denoted by *** (1%) and ** (5%).

Table A1.1.6 Effect of IMF participation, loans and conditions on the shadow economy

Conditionality variable:	(1) Binding	(2) Implementation-corrected	(3) Implementation-discounted binding	(4) Binding and non-binding	(5) Structural vs. quantitative
Dependent variable:			*Shadow economy*		
L. IMF participation	0.9779**	0.9750***	0.8180**	1.1401**	0.9808***
	(0.3992)	(0.3741)	(0.3482)	(0.4444)	(0.3742)
L. IMF loans	2.6070***	2.2141***	2.2417***	2.8839***	2.6404***
	(0.5639)	(0.6184)	(0.5868)	(0.5411)	(0.5902)
L. IMF conditions	0.6325***	0.7190***	0.9339***	0.5486***	
	(0.1810)	(0.2244)	(0.3365)	(0.1567)	
L. IMF structural conditions					0.8664***
					(0.3343)
L. IMF quantitative conditions					0.1803
					(0.1239)
L. Dependency ratio	-0.0909	-0.1014	-0.1412	-0.0918	-0.1016
	(0.0907)	(0.1066)	(0.1183)	(0.1012)	(0.1014)
L. GDP growth	-0.1277	-0.1789	-0.1955	-0.1139	-0.1578
	(0.1488)	(0.1330)	(0.1552)	(0.1726)	(0.1556)
L. Democracy	-2.0165***	-2.1223***	-1.9489**	-2.4125***	-2.1523***
	(0.6528)	(0.7737)	(0.7974)	(0.7084)	(0.7140)
L. Government balance	-0.3530*	-0.3570*	-0.3938*	-0.3823*	-0.3833*
	(0.2081)	(0.1937)	(0.2115)	(0.2283)	(0.2217)
L. Trade openness	-0.0305	-0.0119	-0.0083	-0.0275	-0.0309
	(0.0271)	(0.0336)	(0.0337)	(0.0286)	(0.0279)
L. Investments	-0.1414	-0.2509*	-0.2756*	-0.1244	-0.1474
	(0.1107)	(0.1398)	(0.1426)	(0.1238)	(0.1167)
L. Mineral rents	0.0865	0.3642	0.2391	0.0563	0.0437
	(0.2599)	(0.3180)	(0.3024)	(0.2773)	(0.2849)
Constant	54.8962***	58.7157***	61.6989***	51.9498***	55.9031***
	(9.0602)	(9.7712)	(10.6632)	(9.9080)	(9.6902)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Dependent variable:			*L. IMF participation*		
L. Participation compound	0.3493***	0.4004***	0.4039***	0.3671***	0.3524***
	(0.0375)	(0.0500)	(0.0466)	(0.0357)	(0.0379)
L. Past programs	0.0609	0.0545***	0.0493***	0.0516	0.0528
	(0.0379)	(0.0116)	(0.0116)	(0.0411)	(0.0377)
L. Countries under program	1.1167***	1.1637***	1.2229***	1.0932***	1.1072***
	(0.0611)	(0.0746)	(0.0783)	(0.0610)	(0.0614)
L. GDP per capita	-0.2455***	-0.2502***	-0.2431***	-0.1998***	-0.2551***
	(0.0464)	(0.0626)	(0.0591)	(0.0501)	(0.0444)
L. Executive	0.1335	0.1454	0.1040	0.1153	0.1332

election					
	(0.0873)	(0.0966)	(0.0893)	(0.0849)	(0.0852)
L. Regime Durability	0.0008	-0.0039	-0.0030	0.0004	0.0010
	(0.0014)	(0.0027)	(0.0023)	(0.0014)	(0.0014)
L. Dependency ratio	-0.0017	0.0007	0.0020	-0.0019	-0.0008
	(0.0041)	(0.0054)	(0.0052)	(0.0040)	(0.0040)
L. GDP growth	-0.0030	0.0037	0.0018	-0.0070	-0.0023
	(0.0090)	(0.0082)	(0.0085)	(0.0086)	(0.0088)
L. Democracy	0.0415**	0.0303	0.0384*	0.0405**	0.0481***
	(0.0186)	(0.0228)	(0.0218)	(0.0187)	(0.0179)
L. Government balance	0.0022	0.0181**	0.0240***	0.0044	0.0031
	(0.0085)	(0.0089)	(0.0086)	(0.0091)	(0.0086)
L. Trade openness	0.0001	-0.0011	-0.0009	-0.0009	0.0001
	(0.0009)	(0.0012)	(0.0011)	(0.0009)	(0.0010)
L. Investments	-0.0060	0.0063	0.0068	-0.0012	-0.0052
	(0.0060)	(0.0067)	(0.0064)	(0.0059)	(0.0060)
L. Mineral rents	-0.0048	-0.0136	-0.0176*	-0.0038	-0.0038
	(0.0113)	(0.0106)	(0.0098)	(0.0098)	(0.0123)
Constant	-3.1477	-2.9207***	-2.9369***	-2.9656	-2.7535
	(2.1955)	(0.7467)	(0.7176)	(2.3591)	(2.2282)
Region fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Dependent variable:			*L. IMF loans*		
L.IMF loans compound	0.1102***	0.1257***	0.1296***	0.1135***	0.1091***
	(0.0200)	(0.0275)	(0.0238)	(0.0201)	(0.0202)
L. GDP per capita	-2.7211***	-2.6167***	-2.5453***	-2.6178***	-2.7330***
	(0.3431)	(0.4838)	(0.4745)	(0.3179)	(0.3429)
L. Executive election	-0.1273	-0.2348	-0.2238	-0.1421	-0.1310
	(0.2621)	(0.2655)	(0.2560)	(0.2339)	(0.2604)
L. Regime Durability	-0.0066	-0.0036	-0.0046	-0.0066*	-0.0062
	(0.0043)	(0.0076)	(0.0074)	(0.0039)	(0.0043)
L. Dependency ratio	-0.0207	-0.0021	-0.0013	-0.0194	-0.0203
	(0.0227)	(0.0292)	(0.0287)	(0.0222)	(0.0228)
L. GDP growth	0.0132	0.0251	0.0208	0.0146	0.0130
	(0.0448)	(0.0456)	(0.0458)	(0.0449)	(0.0452)
L. Democracy	0.5389***	0.5974***	0.5847***	0.5246***	0.5430***
	(0.1169)	(0.1463)	(0.1394)	(0.1139)	(0.1178)
L. Government balance	0.0798	0.0826	0.0814	0.0788	0.0791
	(0.0544)	(0.0540)	(0.0533)	(0.0533)	(0.0548)
L. Trade openness	-0.0046	-0.0124	-0.0119	-0.0040	-0.0046
	(0.0066)	(0.0090)	(0.0088)	(0.0064)	(0.0066)
L. Investments	0.0393	0.0737**	0.0761**	0.0353	0.0396
	(0.0267)	(0.0325)	(0.0322)	(0.0261)	(0.0268)
L. Mineral rents	0.0049	-0.0847	-0.0885	0.0029	0.0057

	(0.0535)	(0.0624)	(0.0627)	(0.0532)	(0.0535)
Constant	20.6147***	18.6480***	18.1171***	19.6523***	20.7501***
	(3.1929)	(4.5081)	(4.4682)	(3.0583)	(3.1960)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Dependent variable (conditions):	*L. Binding*	*L. Implementation-corrected*	*L. Implementation-discounted binding*	*L. Binding and non-binding*	*L. Structural conditions*
L. Conditionality compound	-0.2794***	-0.2227***	-0.1972***	-0.2344***	-0.6935***
	(0.0630)	(0.0612)	(0.0637)	(0.0548)	(0.0934)
L. Dependency ratio	0.1359*	0.1241	0.1416*	0.1768*	0.0249
	(0.0722)	(0.0825)	(0.0785)	(0.1019)	(0.0178)
L. GDP growth	0.0740	0.0864	0.0922	0.0462	-0.0106
	(0.0717)	(0.0676)	(0.0752)	(0.1088)	(0.0329)
L. Democracy	0.7824**	0.8336**	0.4972	1.4571***	0.1215
	(0.3263)	(0.3640)	(0.3994)	(0.4762)	(0.1501)
L. Government balance	0.1641*	0.2075**	0.1996**	0.2158	0.0120
	(0.0933)	(0.0965)	(0.0868)	(0.1348)	(0.0294)
L. Trade openness	0.0227	0.0174	0.0100	0.0199	0.0029
	(0.0145)	(0.0169)	(0.0157)	(0.0192)	(0.0051)
L. Investments	0.0107	0.0600	0.0659	-0.0121	0.0043
	(0.0707)	(0.0825)	(0.0648)	(0.0984)	(0.0186)
L. Mineral rents	-0.2012	-0.1819	0.0036	-0.1663	-0.0938**
	(0.1859)	(0.2120)	(0.1622)	(0.2236)	(0.0366)
Constant	-10.3702**	-11.6992**	-8.8505	-15.2938**	-1.2976
	(5.1338)	(5.4504)	(5.3930)	(6.9872)	(1.7357)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Dependent variable (conditions):					*L. Quantitative conditions*
L. Conditionality compound					-0.2240***
					(0.0553)
L. Dependency ratio					0.1064*
					(0.0603)
L. GDP growth					0.0915*
					(0.0498)
L. Democracy					0.6746***
					(0.2285)
L. Government balance					0.1508**
					(0.0726)
L. Trade openness					0.0168
					(0.0123)
L. Investments					0.0112
					(0.0568)
L. Mineral rents					-0.0814

					(0.1667)
Constant					-9.5866**
					(3.9125)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
F-statistic for participation instrument	86.95	64.09	74.99	105.67	86.56
F-statistic for loans instrument	30.34	20.91	29.73	31.82	29.20
F-statistic for conditionality instrument	29.68	23.26	29.58	28.28	
F-statistic for structural conditionality instrument					55.16
F-statistic for quantitative conditionality instrument					16.41
Joint F-statistic	110.68	87.59	98.14	121.62	146.86
Number of observations	2,499	1,977	1,977	2,499	2,499
Number of countries	141	140	140	141	141

Notes: F-tests are Kleibergen-Paap statistics. Standard errors robust at the country-level in brackets. Significance level is denoted by *** (1%), ** (5%) and * (10%).

Table A1.1.7 Effect of IMF intervention on the shadow economy, additional control variables

	(1)	(2)	(3)	(4)	(5)
Dependent variable:			*Shadow economy*		
L. IMF participation	0.9920**	1.0824**	0.9859**	0.7107	0.8605*
	(0.4067)	(0.5183)	(0.4014)	(0.4884)	(0.5117)
L. IMF conditions	0.1385***	0.1220***	0.1366***	0.1295***	0.1288***
	(0.0363)	(0.0423)	(0.0385)	(0.0446)	(0.0397)
L. Dependency ratio	0.0225	0.0454	0.0275	0.0101	0.0096
	(0.0380)	(0.0618)	(0.0396)	(0.0475)	(0.0582)
L. GDP growth	-0.0684***	-0.0604**	-0.0762***	-0.0871***	-0.0801***
	(0.0228)	(0.0279)	(0.0235)	(0.0278)	(0.0304)
L. Democracy	-0.0459	-0.0198	-0.0275	-0.0691	0.0752
	(0.1653)	(0.2735)	(0.1663)	(0.2310)	(0.1672)
L. Government balance	-0.0572*	-0.0474	-0.0636**	-0.0498	-0.0980**
	(0.0301)	(0.0304)	(0.0309)	(0.0335)	(0.0418)
L. Trade openness	-0.0273***	-0.0287***	-0.0251**	-0.0168**	-0.0263***
	(0.0104)	(0.0088)	(0.0101)	(0.0081)	(0.0091)
L. Investments	-0.0467**	-0.0249	-0.0475**	-0.0366**	-0.0292
	(0.0235)	(0.0174)	(0.0232)	(0.0168)	(0.0222)
L. Mineral rents	-0.0675	-0.1971***	-0.0744	-0.1111	-0.1077
	(0.0665)	(0.0684)	(0.0630)	(0.0698)	(0.0976)
L. Political Stability	-0.9446**				
	(0.4589)				
L. Bureaucracy costs		-0.0016			
		(0.0512)			
L. Rule of Law			-1.6758**		
			(0.8353)		
L. Starting a business				-0.1271	
				(0.1716)	
L. Top marginal tax rate					0.0730
					(0.1093)
Constant	17.5153***	15.5050***	17.6564***	16.6935***	14.2251***
	(2.4572)	(3.3853)	(2.3993)	(2.9800)	(2.4128)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Dependent variable:			*L. IMF participation*		
L. Participation compound	0.3501***	0.2777***	0.3434***	0.3178***	0.3326***
	(0.0378)	(0.0536)	(0.0393)	(0.0428)	(0.0499)
L. Past programs	1.2741***	1.3294***	1.2736***	1.2557***	1.3940***
	(0.0819)	(0.1237)	(0.0812)	(0.0890)	(0.1110)
L. Countries under program	-0.1568**	-0.1865**	-0.1532*	-0.1415*	-0.1515*
	(0.0766)	(0.0760)	(0.0783)	(0.0724)	(0.0777)
L. GDP per capita	-0.0367	-0.2394***	-0.0574	-0.0751	-0.2071**
	(0.0474)	(0.0869)	(0.0519)	(0.0683)	(0.0811)
L. Executive election	0.2234*	0.2225	0.2141*	0.1898	0.2988*
	(0.1161)	(0.1471)	(0.1151)	(0.1272)	(0.1531)
L. Regime Durability	0.0005	0.0001	0.0002	0.0006	0.0012
	(0.0019)	(0.0025)	(0.0024)	(0.0018)	(0.0021)
L. Dependency ratio	0.0029	0.0073	0.0034	0.0065	-0.0019
	(0.0039)	(0.0060)	(0.0041)	(0.0048)	(0.0058)
L. GDP growth	-0.0102	-0.0270**	-0.0115	-0.0152	-0.0171
	(0.0107)	(0.0126)	(0.0107)	(0.0129)	(0.0144)
L. Democracy	0.0377**	0.0483	0.0284	0.0257	0.0227
	(0.0184)	(0.0299)	(0.0231)	(0.0251)	(0.0266)
L. Government balance	-0.0080	-0.0115	-0.0091	0.0050	-0.0028
	(0.0091)	(0.0143)	(0.0089)	(0.0144)	(0.0203)
L. Trade openness	0.0007	0.0012	0.0003	-0.0006	-0.0009
	(0.0011)	(0.0016)	(0.0012)	(0.0015)	(0.0018)
L. Investments	-0.0129*	-0.0203**	-0.0144**	-0.0143*	-0.0174*

	(0.0070)	(0.0091)	(0.0069)	(0.0085)	(0.0094)
L. Mineral rents	-0.0021	0.0096	-0.0028	0.0010	0.0079
	(0.0114)	(0.0124)	(0.0115)	(0.0124)	(0.0137)
L. Political Stability	-0.1683**				
	(0.0728)				
L. Bureaucracy costs		0.0392			
		(0.0344)			
L. Rule of Law			-0.0705		
			(0.1143)		
L. Starting a business				0.0308	
				(0.0480)	
L. Top marginal tax rate					-0.0378
					(0.0245)
Constant	6.7395	9.7450**	6.8459	6.1662	8.3790*
	(4.3684)	(4.3497)	(4.5052)	(4.2132)	(4.6213)
Region fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Dependent variable:			*L. Conditionality*		
L. Conditionality compound	-0.3707***	-0.4120***	-0.3684***	-0.3716***	-0.4383***
	(0.0664)	(0.1149)	(0.0658)	(0.0997)	(0.0975)
L. Dependency ratio	0.0962	0.2069	0.1063	0.0969	0.1538
	(0.1178)	(0.2274)	(0.1178)	(0.1703)	(0.1610)
L. GDP growth	0.0105	-0.1402	0.0072	0.0230	-0.0548
	(0.1093)	(0.1048)	(0.1118)	(0.1139)	(0.1170)
L. Democracy	1.0566**	0.4458	1.2063**	0.7097	0.2612
	(0.5149)	(0.6949)	(0.5437)	(0.7667)	(0.6010)
L. Government balance	0.0573	-0.0415	0.0487	0.0630	0.1367
	(0.1073)	(0.1231)	(0.1090)	(0.1128)	(0.1449)
L. Trade openness	0.0215	0.0110	0.0228	-0.0138	0.0166
	(0.0214)	(0.0357)	(0.0221)	(0.0334)	(0.0297)
L. Investments	-0.0447	-0.0651	-0.0515	-0.0659	-0.1331
	(0.0901)	(0.1125)	(0.0911)	(0.1035)	(0.1060)
L. Mineral rents	0.0821	0.1228	0.0586	-0.0169	-0.1149
	(0.2395)	(0.3029)	(0.2379)	(0.2992)	(0.3359)
L. Political Stability	-0.7430				
	(1.3288)				
L. Bureaucracy costs		0.3275			
		(0.3043)			
L. Rule of Law			-3.6904		
			(2.3965)		
L. Starting a business				0.1341	
				(0.7338)	
L. Top marginal tax rate					-0.8686*
					(0.5185)
Constant	-13.5814**	-10.5952	-12.5191*	-7.2977	3.8901
	(6.8689)	(9.9401)	(7.1464)	(10.2033)	(8.8053)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
F-statistic for participation instrument	85.81	26.83	76.42	55.02	44.40
F-statistic for conditionality instrument	31.21	12.86	31.32	13.90	20.21
Joint F-statistic	112.66	31.53	102.49	59.43	52.90
Number of observations	1,795	1,237	1,795	1,394	1,263
Number of countries	141	131	141	130	129

Notes: F-tests are Kleibergen-Paap statistics. Standard errors robust at the country-level in brackets. Significance level is denoted by *** (1%), ** (5%) and * (10%).

Table A1.1.8 Effect of IMF intervention on the shadow economy, controlling for observable pathways

	(1)	(2)	(3)	(4)	(5)
Dependent variable:			*Shadow economy*		
Conditionality variable:	Binding	Implementation-corrected	Implementation-discounted binding	Binding and non-binding	Structural vs. quantitative
L. IMF participation	1.1021*	1.4366**	1.2050**	1.3403**	0.9602
	(0.6490)	(0.7270)	(0.5490)	(0.6259)	(0.7027)
L. IMF conditions	0.1127***	0.0894**	0.1106***	0.0613***	
	(0.0353)	(0.0378)	(0.0412)	(0.0212)	
L. IMF structural conditions					0.1800*
					(0.1086)
L. IMF quantitative conditions					0.1090
					(0.1879)
L. Dependency ratio	0.0762	0.1243	0.1232	0.0922	0.0630
	(0.0596)	(0.0902)	(0.0822)	(0.0597)	(0.0620)
L. GDP growth	-0.0537*	-0.1123***	-0.1262***	-0.0539*	-0.0491
	(0.0308)	(0.0433)	(0.0428)	(0.0295)	(0.0335)
L. Democracy	0.1266	0.1808	0.1986	0.1508	0.1295
	(0.1751)	(0.1765)	(0.2031)	(0.1564)	(0.2106)
L. Trade openness	-0.0307***	-0.0371***	-0.0372***	-0.0307***	-0.0317***
	(0.0094)	(0.0118)	(0.0114)	(0.0094)	(0.0098)
L. Investments	-0.0359	0.0043	0.0099	-0.0313	-0.0285
	(0.0231)	(0.0255)	(0.0306)	(0.0246)	(0.0253)
L. Mineral rents	-0.2082**	-0.1786**	-0.1783**	-0.2130***	-0.2154***
	(0.0809)	(0.0716)	(0.0717)	(0.0824)	(0.0788)
L. Government spending	0.0430	-0.0025	-0.0090	0.0452	0.0341
	(0.0315)	(0.0401)	(0.0431)	(0.0307)	(0.0364)
L. Bureaucracy costs	0.0092	0.0353	0.0209	0.0038	0.0080
	(0.0480)	(0.0601)	(0.0609)	(0.0484)	(0.0627)
L. Hiring and firing costs	0.2576**	0.2493**	0.3106**	0.2542**	0.2532*
	(0.1298)	(0.1234)	(0.1250)	(0.1251)	(0.1447)
L. Top marginal tax rate	0.1655	-0.0645	-0.0601	0.1594	0.1974
	(0.1388)	(0.1489)	(0.1424)	(0.1388)	(0.1487)
L. Control of corruption	-1.6848**	-0.7564	-0.9155	-1.8326**	-1.3839*
	(0.7281)	(0.6513)	(0.7061)	(0.7135)	(0.8387)
Constant	10.2117***	14.5854***	20.5339***	9.5113***	10.5313***
	(3.1001)	(3.5379)	(4.6620)	(2.9879)	(3.4333)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Dependent variable:			*L. IMF participation*		
L. Participation compound	0.3724***	0.4825***	0.4054***	0.4029***	0.3782***
	(0.0571)	(0.1070)	(0.1132)	(0.0591)	(0.0593)
L. Past programs	-0.1773**	0.1718	0.0148	-0.1996***	-0.1570*
	(0.0825)	(0.4008)	(0.4269)	(0.0727)	(0.0815)
L. Countries under program	1.4613***	1.7274***	1.7789***	1.4743***	1.4232***

	(0.1553)	(0.2056)	(0.2057)	(0.1535)	(0.1621)
L. GDP per capita	-0.1855	0.0371	-0.1772	-0.1757	-0.1871
	(0.1246)	(0.3392)	(0.2752)	(0.1281)	(0.1287)
L. Executive election	0.2094	-0.0081	-0.1251	0.2439	0.2097
	(0.1786)	(0.2244)	(0.2286)	(0.1651)	(0.1868)
L. Regime Durability	0.0039^{*}	-0.0093	-0.0099	0.0045^{**}	0.0036
	(0.0024)	(0.0095)	(0.0099)	(0.0023)	(0.0024)
L. Dependency ratio	0.0039	0.0195	0.0080	0.0030	0.0037
	(0.0077)	(0.0207)	(0.0159)	(0.0081)	(0.0077)
L. GDP growth	-0.0242^{*}	-0.0118	-0.0152	-0.0310^{**}	-0.0280^{**}
	(0.0135)	(0.0188)	(0.0194)	(0.0139)	(0.0136)
L. Democracy	0.0733^{*}	0.0308	0.0311	0.0859^{**}	0.0681^{*}
	(0.0407)	(0.0637)	(0.0531)	(0.0409)	(0.0392)
L. Trade openness	0.0003	-0.0038	-0.0038	-0.0007	-0.0003
	(0.0023)	(0.0043)	(0.0042)	(0.0021)	(0.0023)
L. Investments	-0.0171	0.0140	0.0170	-0.0085	-0.0149
	(0.0109)	(0.0164)	(0.0151)	(0.0100)	(0.0114)
L. Mineral rents	0.0090	0.0088	0.0032	0.0068	0.0088
	(0.0131)	(0.0192)	(0.0163)	(0.0137)	(0.0138)
L. Government spending	0.0008	-0.0027	-0.0157	-0.0014	0.0010
	(0.0093)	(0.0170)	(0.0175)	(0.0099)	(0.0096)
L. Bureaucracy costs	0.0966^{**}	0.0643	0.0807	0.0855^{**}	0.0935^{**}
	(0.0377)	(0.0765)	(0.0652)	(0.0391)	(0.0374)
L. Hiring and firing costs	-0.0191	-0.1332	-0.1695^{**}	-0.0303	-0.0149
	(0.0537)	(0.0949)	(0.0864)	(0.0588)	(0.0530)
L. Top marginal tax rate	-0.0302	0.0218	-0.0280	-0.0490^{*}	-0.0289
	(0.0317)	(0.0524)	(0.0512)	(0.0287)	(0.0311)
L. Control of corruption	-0.3029^{**}	-0.5548^{**}	-0.4280^{**}	-0.3527^{**}	-0.2495^{*}
	(0.1420)	(0.2660)	(0.2115)	(0.1494)	(0.1358)
Constant	8.2058^{*}	-11.0793	-2.5200	9.3199^{**}	7.2404
	(4.9483)	(14.8309)	(15.1592)	(4.4297)	(4.8977)
Region fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Dependent variable (conditions):	*L. Binding*	*L. Implementation-corrected*	*L. Implementation-discounted binding*	*L. Binding and non-binding*	*L. Structural conditions*
L. Conditionality compound	-0.4746^{***}	-0.4399^{***}	-0.4333^{***}	-0.5070^{***}	-0.4478^{***}
	(0.1045)	(0.0987)	(0.0932)	(0.1147)	(0.1661)
L. Dependency ratio	0.2069	0.1490	0.1804	0.2531	0.1513^{***}
	(0.2126)	(0.4207)	(0.3645)	(0.3219)	(0.0585)
L. GDP growth	-0.1093	-0.0471	0.0795	-0.1618	-0.0271
	(0.1104)	(0.1590)	(0.1174)	(0.1651)	(0.0340)
L. Democracy	0.1907	0.3874	0.0830	-0.0891	-0.0569
	(0.7213)	(0.7939)	(0.8996)	(0.8318)	(0.2338)
L. Trade openness	-0.0024	0.0403	0.0499	-0.0048	0.0027
	(0.0355)	(0.0479)	(0.0500)	(0.0484)	(0.0080)
L. Investments	-0.1146	-0.1037	-0.1474	-0.2612	-0.0057

	(0.1115)	(0.1586)	(0.1064)	(0.1711)	(0.0238)
L. Mineral rents	0.2748	0.5979***	0.5109***	0.5355	0.0599
	(0.2787)	(0.1648)	(0.1613)	(0.4196)	(0.0800)
L. Government spending	0.2796**	0.3219*	0.3336*	0.4556**	0.0399
	(0.1337)	(0.1799)	(0.1705)	(0.1979)	(0.0443)
L. Bureaucracy costs	0.5402*	0.5630	0.5836	0.9856**	0.2759***
	(0.2759)	(0.4625)	(0.3853)	(0.4634)	(0.1006)
L. Hiring and firing costs	0.0532	-0.2565	-0.7739	0.1631	-0.0625
	(0.6325)	(0.7930)	(0.8085)	(0.8781)	(0.3482)
L. Top marginal tax rate	-0.4764	-0.2398	-0.3566	-0.8197	-0.2022
	(0.6168)	(0.8757)	(0.7397)	(0.9360)	(0.2715)
L. Control of corruption	-5.5509**	-6.2273**	-4.0333	-7.4718**	-2.8351***
	(2.6700)	(3.1701)	(3.3603)	(3.7296)	(0.8804)
Constant	-12.6466	-15.3814	79.3142***	-14.8186	-3.7802
	(12.2104)	(17.7493)	(24.9775)	(16.6897)	(4.2620)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Dependent variable (conditions):					*L. Quantitative conditions*
L. Conditionality compound					-0.3070***
					(0.1009)
L. Dependency ratio					0.1071
					(0.1765)
L. GDP growth					-0.0833
					(0.0927)
L. Democracy					0.1315
					(0.6098)
L. Trade openness					0.0043
					(0.0323)
L. Investments					-0.1157
					(0.1039)
L. Mineral rents					0.1775
					(0.2698)
L. Government spending					0.2012*
					(0.1061)
L. Bureaucracy costs					0.1779
					(0.2088)
L. Hiring and firing costs					0.1126
					(0.4415)
L. Top marginal tax rate					-0.3678
					(0.4255)
L. Control of corruption					-3.5028
					(2.1765)
Constant					-7.1983

					(9.8597)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
F-statistic for participation instrument	42.51	20.33	12.82	46.40	40.72
F-statistic for conditionality instrument	20.63	19.85	21.62	19.54	
F-statistic for structural conditionality instrument					19.27
F-statistic for quantitative conditionality instrument					17.27
Joint F-statistic	49.03	41.23	36.91	50.62	45.36
Number of observations	1,138	701	701	1,138	1,138
Number of countries	120	100	100	120	120

Notes: F-tests are Kleibergen-Paap statistics. Standard errors robust at the country-level in brackets. Significance level is denoted by *** (1%), ** (5%) and * (10%).

Subchapter I.II: The effects of IMF conditional programs on the unemployment rate

1.2.1 Introduction

The International Monetary Fund (IMF) has become an almost universal financial institution, with its membership rising from 44 states in 1946 to 190 at present. The basic conception of the IMF's role is to uphold global financial stability, and it is often considered as the "crisis manager" for developing and developed countries. With its loan programs the IMF promoted policy reforms, such as the privatization of state-owned enterprises, trade and financial liberalization, economic deregulation, social policy, labour market reforms, and 'good governance', able to restructure the domestic economy of borrowing countries (Kentikelenis et al., 2016). Previous studies highlight that IMF loan programs can have multi-level effects on borrowing countries, including their growth rates (Barro and Lee, 2005; Bas and Stone, 2014), poverty and income inequality (Oberdabernig, 2013), labour rights (Lloyd and Weissman, 2002), public spending (Rickard and Caraway, 2018) and health outcomes (Forster et al., 2020).

Arguably, the IMF through its policy reforms, can alter the underlying structure of an economy, including the labour market (Kentikelenis et al., 2016). Therefore, we focus our attention on labour market issues, and specifically on the level of unemployment.

Should we care about unemployment? During the last century, unemployment has been considered one of the most important factors behind the rise in poverty (e.g., Corcoran and Hill, 1980; Osinubi, 2005; Saunders, 1990). Furthermore, the level of unemployment is linked to various health outcomes. An increased level of unemployment is associated with several mental health issues, such as mixed symptoms of distress, depression, anxiety, psychosomatic symptoms, subjective well-being, and self-esteem (e.g., Backhans and Hemmingsson, 2012; Björklund and Eriksson, 1998; McKee-Ryan, et al., 2005; Paul and Moser, 2009). There is also evidence that unemployment increases mortality rates and suicidal behavior (e.g., Córdoba-Doña et al., 2014; Gerdtham and Johannesson, 2003; Lundin et al., 2010; Platt, 1984). Moreover, previous research points out that unemployment increases alcohol consumption and drug use (Henkel, 2011). In addition, several studies show that there is a causal positive relationship between unemployment and both property and violent crime (Lee and Holoviak, 2006; Lin, 2008; Papps and Winkelmann, 2000; Raphael and Winter-Ebmer, 2001). All of these suggest that unemployment is damaging for those who experience it.

In this subchapter we explore the influence of IMF programs on the unemployment rate. In exchange for low-cost financing, the IMF requires governments to implement a set of reforms, known as *conditionalities*. Allegedly, these conditionalities foster macroeconomic stability through "*correct[ing] maladjustments in [government] balance of payments without resorting to measures destructive of national or international prosperity*" (IMF, 2000). Nevertheless, these obligatory policy reforms (conditionality) may lead to inactive business investment, poorer government service, severe social instability - all of which may damage the economic development of bailed-out countries in the long term (Li et al., 2015). Furthermore, previous research documents that IMF conditionality has adverse effects on a range of labour related indicators, including employment levels, wages and employment protection policies (Lloyd and Weissman, 2002; Blanton et al., 2015). Pastor (1987) and Vreeland (2002) empirically investigate the impact of IMF programs on labour's share income and they show that IMF programs are linked to a decline in labour's share income.

Using a sample of 96 countries across the world between 1971 and 2015, first, using quota share at the IMF (sum of all current IMF loans a country is eligible to as a share of its quota at the IMF) to capture countries' IMF loan agreements and a two-stage estimator to account for selection bias, second, by examining IMF program participation using a binary variable and applying a Heckman two-stage estimator for selection bias, and third, using a two stage instrumental variables approach (2SLS IV) to causally interpret the effect of IMF program participation on unemployment, we compile evidence that IMF programs increase country's unemployment rate. Our results remain robust across alternative specifications. Additionally, we provide some evidence that the short-run effects remain significant for a long-run period. Lastly, we account for the number of total conditions – policy reforms included within the program using a system of three simultaneous equations – estimated through maximum likelihood estimation (MLE) (Stubbs et al., 2020) and find that IMF conditions have an adverse effect on the unemployment rate of recipient countries.

The rest of the subchapter proceeds as follows. Section 1.2.2 describes the potential pathways through which IMF programs affect the unemployment rate. Section 1.2.3 provides an overview of IMF programs used in this subchapter and their goals. Section 1.2.4 describes the sample and the selection of the covariates. In the next four sections we discuss in more detail our strategies for addressing the methodological challenges, followed by the presentation of the results from four sets of statistical tests. Finally, Section 1.2.9 concludes. Details concerning the data set we collected for this subchapter and additional robust checks of the empirical results are described in appendices at the end of the current subchapter.

1.2.2 Pathways through which IMF programs affect the unemployment rate

A strand of literature highlights the impact of international financial institutions (IFIs) on labour (Blanton et al., 2015; Martin and Brady, 2007; Nooruddin and Vreeland, 2010; Vreeland, 2002). Previous work points out that these organizations advocate the increased flexibility of labour markets which involves increasing employers' ease in hiring, firing, or setting working hours (Burgess, 2010) - with the promise of future economic growth in borrowing countries. In addition, previous studies examine the link between IMF programs and labour rights. While Abouharb and Cingranelli (2007) and Blanton et al. (2015) conclude that

IMF programs has a negative impact on labour rights in individual and collective level, Reinsberg et al. (2019) demonstrate that collective labour rights increase in the wake of IMF programs. In this context, studies also examine the effects of IMF program on public sector. Research results on the effects of IMF loans on public spending are mixed and depend on the sample used and estimated procedure (e.g., Bulíř and Moon, 2006; Clements et. al., 2013; Kentikelenis et al., 2015; Nooruddin and Simmons, 2006; Nooruddin and Vreeland, 2010). More specific results are reported in Rickard and Caraway (2018), who find that the implementation of reforms related to public sector to a country's IMF program reduces government spending on the public sector wage bill. Finally, Ohanyan and Androniceanu (2017) using a combination of propensity score matching with the differences-in-differences estimator for the EU-28 between 1993 and 2013 conclude that IMF participation decreases the level of employment.

On the one hand, country's unemployment rate participating on an IMF arrangement could be indirectly affected through other channels which IMF participation affects, such as economic growth (e.g., Bird and Rowlands, 2017; Dreher, 2006; Przeworski and Vreeland, 2000), government spending (e.g., Nooruddin and Simmons, 2006; Rickard and Caraway, 2018), labour rights (e.g., Abouharb and Cingranelli, 2007; Blanton et al., 2015), income inequality (e.g., Forster et al., 2019; Garuda, 2000; Oberdabernig, 2013) and political stability (e.g., Dreher, 2004; Dreher and Gassebner, 2012; Dreher and Walter, 2010). On the other hand, policy reforms - conditions (specifically close related to labour market) signed through IMF programs could have a direct impact on employment/unemployment. For example, the text for a condition in Albania's 1994 program stipulated the "reduction in the number of civil service employees to not more than 179,000 employees" (Kentikelenis et al., 2016). Based on Kentikelenis' et al. (2016) classification, conditions related to labour issues include wages and employment limits, pensions, social security institutions and any other measures affecting labour. In Table 1.2.1, we provide the full text of some specific conditions (Kentikelenis et al., 2016) for recipient countries in our sample which are directly connected to workers' layoffs – affecting the unemployment rate.

Table 1.2.1 Examples of policy conditions which are connected to workers' layoff

Recipient country	Text of the condition	Year
Albania	Establish ceiling on employment in budgetary institutions at 135,000, and reduce workforce to this level.	1998
Armenia	Reduce budgetary sector employment to 340,000.	1996
Croatia	Reduction of government employment to no more than 168,804 people by March 31, 2002.	2002
Honduras	Reduction of employment in the public sector by 2,000	1995
Macedonia, North	Implement labor shedding plans for firms included in the Special Restructuring Program. Cumulative layoffs of: 10,650 by June	1995
Romania	Implementation of 4,200 layoffs (not externalization) in companies under the Ministry of Industry, and of the 750 layoffs in companies under APAPS as described in 117 and 127.	2002
Russian Federation	Reduce employment in the federal executive authorities by 41,000 compared with end- 1998.	1999
Tajikistan	Reduce the number of employees in the education sector by 5 percent	2004

Source: Kentikelenis et al. (2016).

In addition, fiscal consolidation measures implemented through IMF loan agreements entail governments to increase their fiscal revenues or/and decrease their government expenditure. These measures, independent of IMF programs, have already been documented to decrease spending on safety, education, health and social protection (Castro, 2017), increase unemployment (Agnello and Sousa, 2014; Agnello et al., 2014) and are linked to higher inequality (Schaltegger and Weder, 2014; Woo et al., 2013). Thus, fiscal consolidation may increase the unemployment rate via the decrease in government consumption or the cut in government investment.

1.2.3 Types of programs and their goals

In this subchapter, we consider 10 types of loan arrangement[30] involving policy conditions that the IMF has been offering. These are: the Stand-By Arrangement (SBA), the Extended Fund Facility (EFF), the Structural Adjustment Facility (SAF), the Enhanced Structural Adjustment Facility (ESAF), subsequently replaced by the Poverty Reduction and Growth Facility (PRGF), later was relabeled as the Extended Credit Facility (ECF), the Standby Credit Facility (SCF), the Flexible Credit Line (FCL), the Precautionary Credit Line (PCL), which replaced by the Precautionary and Liquidity Line (PLL). The IMF's Stand-By Arrangement (SBA) has been used since 1952. The SBA is signed between the IMF and the corresponding country's government if the country is experiencing a short-term balance-of-payments problem. SBAs typically cover a period of one to two years, but they might last up to three years. The Extended Fund Facility (EFF) was set up in 1974 to help countries encountering long-term balance of payments problems, with focus on structural reforms. Extended arrangements would normally be approved for a period up to four years.[31]

After the 2008 crisis, the IMF expanded their programs, as 'crisis resolution'. The Flexible Credit Line (FCL) was introduced in 2009 and it is designed to provide a credit line with large and upfront financing (present, prospective, or potential balance of payment needs) to members with very strong fundamentals and institutional policy frameworks that have sustained track records of implementing very strong policies and remain committed to maintaining such policies in the future. An FCL arrangement may be approved for one or two years. The Precautionary Credit Line (PCL) introduced in 2010. The PCL was created with the aim of providing an effective crisis prevention window for member countries whose

[30] Five of them are only for low-income countries, namely concessional loan programs.

[31] Detailed information on the SBA and EFF is available under IMF (2016d) and IMF (2016a), respectively.

fundamentals were in an intermediate range and hence did not qualify for the FCL. PCL arrangements could be approved for a period of one to two years. The Precautionary and Liquidity Line (PLL) program was introduced in November 2011, replacing and broadening the scope of the previously established Precautionary Credit Line (PCL). The PLL is designed to provide liquidity to countries with sound policies under broad circumstances, including countries affected by regional or global economic and financial stress. PLL arrangements can have a duration of either six months or one or two years with the six-month duration available for countries with actual or potential short-term balance of payments needs.[32]

In 1986, the Fund's facilities were extended to provide flexible and tailored support to low-income countries (LICs) via concessional loan facilities[33] with strong and durable poverty reduction and growth. Concessional financing was first provided through the Structural Adjustment Facility (SAF) for LICs for a period of three to five years. The second concessional loan program, similar to SAF, was the Enhanced Structural Adjustment Facility (ESAF) beginning in 1987 under a three-year arrangement. The ESAF involves stricter conditionality criteria and higher loan amounts. The ESAF was replaced by the Poverty Reduction and Growth Facility (PRGF) in November 1999, after the East-Asian crisis. The goal of the PRGF was broadened to include poverty reduction and to grant governments larger scope in negotiating the policy conditions. Typically, PRGF programs are pursued for up to four years.[34]

[32] Detailed information on the FCL, PCL and PLL is available under IMF (2016b), IMF (2011) and IMF (2016c), respectively.

[33] More details for the first use of concessional assistance are available under IMF (1986).

[34] Detailed information on the IMF concessional financing through the SAF, ESAF, and PRGF is available under IMF (2004b).

Following the needs of LICs, after the 2008 crisis, the Fund upgraded the agenda of concessional financing.[35] The PRGF was relabeled as the Extended Credit Facility (ECF) in 2009 and provides financial assistance to countries with protracted balance of payments problems. The ECF is the Fund's main tool for providing medium-term support to LICs with strong and durable poverty reduction and growth. Assistance under an ECF arrangement is provided for an initial duration from three to up to four years, with an overall maximum duration of five years. The Standby Credit Facility (SCF) also designed in 2009 and provides financial assistance to LICs that may experience episodic, short-term financing and adjustment needs (short-term balance of payments needs), including those caused by shocks, with strong and durable poverty reduction and growth. An SCF arrangement can range from one to two years. As the SCF is intended to address episodic short-term needs, its use is normally limited to two and a half out of any five years.

For all the above loan arrangements, the IMF evaluates whether the eligible member consents with the requirements-conditions imposed; if so, the country can draw on the loan funds in pre-specified intervals.

1.2.4 Data sample and covariates

Our sample consists of 96 countries across the world observed between 1971 and 2015. Table A1.2.1 of the Appendix lists all countries included in the study.[36] Our main outcome of interest is the unemployment rate for each country in each given year of the sample. The dependent

[35] Detailed information on the overhaul of the Fund's concessional facilities for LICs in 2009 is available under IMF (2012).

[36] The sample includes both program and nonprogram years, as well as countries with no programs. Not all countries in the sample are observed for the full 45-year window and some observations are excluded due to data availability in different specification.

variable measures the number of unemployed persons as a percentage of the total labour force and is available from the International Financial Statistics (IFS) of the IMF.

Our key explanatory variable is IMF loan-quota ratio in country i in year t. IMF loan-quota ratio is defined as the sum of all IMF loan programs a country/member i is eligible to as a share of its quota at the IMF. We drew data for IMF loan-quota ratio from the IFS. As an alternative, to capture the presence of a conditional IMF lending arrangement we use IMF program participation, a binary (dummy) variable that equals to 1 if country i was under an IMF program (see the previous section for the IMF programs we consider in this subchapter) for at least five months in year t (definition based on Dreher, 2006). Data for IMF program participation is from Dreher (2006) and IMF Lending Arrangements data (available at: https://www.imf.org/external/np/fin/tad/extarr1.aspx, which provides lending arrangements with the IMF data by country/member).

Following previous research (e.g., Dreher, 2006; Bal Gunduz, 2016; Przeworski and Vreeland, 2000; Steinwand and Stone, 2008; Sturm et al., 2005), we include several variables to determine country's participation in IMF loan programs. The variables we use are investment share, trade openness, government share, current account (all expressed as a share of GDP) and inflation, all from World Development Indicators. We also include public debt (% of GDP) available from the Abbas et al. (2010), democracy index rating from Polity IV project and country's cumulative number of years in an IMF program.

Moreover, we control for economic, political and demographic determinants of the unemployment rate. Specifically, we include trade openness, the natural logarithm of nominal GDP, annual population growth, secondary schooling, measured as the gross secondary schooling enrolment ratio (all from World Development Indicators) and democracy index rating.[37] An extensive description and summary statistics of the variables is provided in Table A1.2.2 of the Appendix. The following sections describe the different methodological approaches we employ and provide a discussion of the results we find.

1.2.5 Estimating the association between IMF loan-quota ratio and the unemployment rate

In our framework, when using non-randomly selected samples for model estimation, the performance outcomes of participants' countries may systematically differ from non-participants, therefore a country's participation in IMF loan programs may causes the issue of selection bias.[38]

Next, following Vella and Verbeek (1999), we describe our panel empirical specification to deal with sample selection bias. Consider the following fixed effects panel selection model:

$$Y^*{}_{it} = \xi + \kappa_i + \lambda_t + \delta\, IMFq_{it} + X'_{it}\beta + \varepsilon_{it} \qquad \text{(participation effects equation)} \qquad (1.2.1)$$

$$IMFq^*{}_{it} = \pi + \mu_i + \nu_t + W'_{it}\gamma + u_{it} \qquad \text{(participation selection equation)} \qquad (1.2.2)$$

where:

$$IMFq_{it} = \begin{cases} IMFq^*{}_{it} \text{ if } IMFq^*{}_{it} > 0, \\ \quad 0 \text{ otherwise,} \end{cases} \qquad (1.2.3)$$

$$Y_{it} = \begin{cases} Y^*{}_{it} & \text{if } IMFq^*{}_{it} > 0, \\ \breve{\xi} + \breve{\kappa}_i + \breve{\lambda}_t + \breve{X}'_{it}\breve{\beta} + \breve{\varepsilon}_{it} & \text{if } IMFq^*{}_{it} = 0, \end{cases} \qquad (1.2.4)$$

$i = 1, 2, \dots, N$ (indexes individual countries) and $t = 1, 2, \dots, T$ (indexes time periods), where $Y^*{}_{it}$ and $IMFq^*{}_{it}$ are latent endogenous variables for cross-sectional unit i at time t with

[37] In the Appendix we extent the set of control variables and provide additional robustness checks.

[38] See e.g., Heckman's (1979) framework for sample selection bias.

observed counterparts Y_{it} (the unemployment rate – participation effects equation) and $IMFq_{it}$ (IMF loan-quota ratio – participation selection equation), π, ξ and $\breve{\xi}$ are intercepts, κ_i, μ_i, $\breve{\kappa}_i$, λ_t, ν_t and $\breve{\lambda}_t$ represent country and year fixed effects, respectively, X_{it}, W_{it} and $\breve{X}_{it}$ are vectors of control variables,ε_{it}, u_{it} and $\breve{\varepsilon}_{it}$ are *i.i.d.* error terms.

To account for the issue of selection bias, we follow previous studies (Baser et al., 2003; Terza et al., 2007; Binder and Bluhm, 2017) and we first regress the participation selection equation (Equation (1.2.2)). From Equation (1.2.2) using Tobit regression we extract the generalized residuals (Gourieroux et al., 1987)[39], which comprise the additional information of the participation selection equation. Then, we enter this term as explanatory variable into the participation effects equation (Equation (1.2.1)) taking into account the selection bias part (Vella, 1993).

Thus, consistent estimation of Equation (1.2.1) can be based on OLS estimation of the coefficients δ, β and $\breve{\eta}$ in:

$$Y_{it} = \xi + \kappa_i + \lambda_t + \delta\ IMFq_{it} + X'_{it}\beta + \breve{\eta}\hat{u}_{it} + \varepsilon_{it}, \qquad (1.2.5)$$

where $\hat{u}_{it}$ is an estimate of u_{it} based on maximum likelihood estimation of the participation selection equation (Equation (1.2.2)).[40]

In Table 1.2.2, we report results of Tobit regression. The estimated coefficients of investment share and inflation[41] are significant negative, and of debt and number of years under

[39] Vella (1993) argues that this methodology can address endogeneity and unconventional forms of selection bias, and provides analytic proofs.

[40] The participation selection equation, Equation (1.2.2), is a fixed effects Tobit model, as the dependent variable, IMF loan-quota ratio, is left-censored at zero.

[41] The policies implemented under IMF agreements are planned to reduce inflation by setting more realistic inflation targets.

IMF program participation are significant positive. If investment share or inflation increase by one percentage point, then the loan-quota ratio decreases by 17.677 percentage points, or 0.127 percentage points, respectively. This finding is similar to that in Przeworski and Vreeland (2000) and Binder and Bluhm (2017), who also find an inverse relationship between IMF lending and investment share. If the debt increases by one percentage point, then the loan-quota ratio increases by 0.679 percentage point.[42] Finally, if the number of years under IMF increases by one year, the quota of IMF lending increases by 47.3 percentage points. This effect is similar to Przeworski and Vreeland (2000) and Binder and Bluhm (2017), who also document that, the number of years under an IMF program increases the probability to enter into a program.[43]

Table 1.2.2 IMF loan-quota ratio

Independent Variables:	Marginal effects
Investment share	-17.677***
	(2.692)
Trade	-0.273
	(0.792)
Government share	-5.144
	(5.125)
Current account	0.235
	(2.966)
Debt	0.679***
	(0.202)
Democracy index	-0.010
	(0.123)
Inflation	-0.127***
	(0.044)
Number of years under IMF	0.473***
	(0.100)
Observations	1,834

Notes: Estimation results are obtained by estimating Equation (1.2.2). A constant (not displayed) has been included in the regression. The chi-square Wald test for joint significance of all variables is significant at the one percent significance level. All reported effects are average marginal effects evaluated at the independent variables' sample averages. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses. Significance level is denoted by *** (1%).

[42] Previous literature documents that a higher debt service increases demand for IMF programs (Dreher, 2006; Eichengreen et al., 2006; Przeworski and Vreeland, 2000; Stone, 2008; Sturm et al., 2005).

[43] To compute the generalized residuals after Tobit, we use STATA software and follow the instructions of Cameron and Trivedi (2009) for the procedure.

Our next step is to investigate the impact of loan-quota ratio to the unemployment rate, taking into account sample selection bias. First, we estimate the participation effects model (Equation (1.2.1)), without controlling for sample selection bias. Next, we estimate our full model, fixed effects participation effects model (Equation (1.2.5), as described above), correcting for sample selection. The results are reported in Table 1.2.3.[44] As can been seen, the results with and without sample selection correction are significant similar.[45] IMF quota lending tends to increase the unemployment rate.[46] In Model 2, an increase by one standard deviation in the loan-quota ratio variable (1.932) increases the unemployment rate by 0.008 (=1.932*0.004), which corresponds to approximately 14% of its standard deviation.

Table 1.2.3 Effects on the unemployment rate - IMF loan-quota ratio

	(1)	(2)
Loan-quota ratio	0.004^{***}	0.004^{***}
	(0.001)	(0.001)
Trade	-0.004	0.001
	(0.011)	(0.011)
Democracy index	-0.001	-0.001

[44] In Table A1.2.3 of the Appendix we examine our main results using the GMM estimator. In Table A1.2.4 of the Appendix we extend the set of control variables with variables related to labour market regulations. Specifically, we add part-time workers rights, unionization and waiting period; all from Adams et al. (2017) dataset (description of these variables is also provided in Table A1.2.2 at the Appendix). The inclusion of further explanatory variables corresponds to a more stringent test for the effect of IMF programs on the unemployment rate and addresses concerns of omitted variable bias. Moreover, in Table A1.2.5 of the Appendix we perform some robustness checks. We show that all our findings are robust to these alternative specifications.

[45] $\breve{\eta}$ (the coefficient of generalized residuals (GR), $\hat{u}_{it}$) is significant at the 1 percent level, providing evidence that a selection mechanism is present.

[46] Regarding the rest of the control variables, only Nominal GDP (logs) and population growth have a significant negative sign.

	(0.001)	(0.001)
Nominal GDP (logs)	-0.022***	-0.015**
	(0.006)	(0.006)
Population growth	-0.980**	-0.994**
	(0.399)	(0.399)
School enrollment	0.000	0.007
	(0.016)	(0.016)
Observations	1,834	1,834
Adjusted R^2	0.206	0.234
Sample selection correction	No	Yes
GR (generalized residuals)		0.049***
		(0.011)

Notes: Estimation results for Model 1 are obtained by estimating Equation (1.2.1).
Estimation results for Model 2 are obtained by estimating Equation (1.2.5) to control for sample selection. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses. Significance level is denoted by *** (1%) and ** (5%)

We next turn our focus to medium and long-run effects of IMF loan programs on the unemployment rate. These results are reported in Table 1.2.4. The first column of the table uses the first lag of all independent variables. The results are very similar to our baseline findings.

Table 1.2.4 Medium and long-run effects, IMF loan-quota ratio

Variables	(1)	(2)	(3)	(4)	(5)	(6)
Loan-quota ratio(*t*-1)	0.004***	0.003***				
	(0.001)	(0.001)				
Loan-quota ratio(*t*-2)			0.003***			
			(0.001)			
Loan-quota ratio(*t*-3)				0.003**		
				(0.001)		
Loan-quota ratio(*t*-4)					0.003**	
					(0.001)	
Loan-quota ratio(*t*-5)						0.002**
						(0.001)
Trade		-0.001	-0.002	-0.002	-0.002	-0.003
		(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Trade(*t*-1)	-0.011					
	(0.011)					
Democracy index		-0.000	-0.000	-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Democracy index(*t*-1)	-0.000					
	(0.000)					
Nominal GDP (logs)		-0.015**	-0.017***	-0.018***	-0.019***	-0.021***
		(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Nominal GDP (logs)(*t*-1)	-0.008					
	(0.005)					

	(1)	(2)	(3)	(4)	(5)	(6)
Population growth		-1.005**	-1.049**	-1.104***	-1.171***	-1.250***
		(0.400)	(0.405)	(0.415)	(0.440)	(0.453)
Population growth(*t*-1)	-0.731**					
	(0.328)					
School enrollment		0.009	0.011	0.012	0.013	0.013
		(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
School enrollment(*t*-1)	0.003					
	(0.015)					
GR	0.053***	0.049***	0.050***	0.050***	0.052***	0.053***
	(0.011)	(0.011)	(0.012)	(0.012)	(0.013)	(0.013)
Observations	1828	1831	1824	1813	1799	1778
Adjusted R^2	0.203	0.226	0.214	0.205	0.197	0.200

Notes: Estimation results are obtained by estimating Equation (1.2.5), controlling for selection bias. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses. Significance level is denoted by *** (1%) and ** (5%).
Model 1 features all independent variables entering with a lag.
Model 2 - 6 uses 1 to 5 lags of the explanatory variable (loan-quota ratio).

Next, columns (2)-(6) of Table 1.2.4 report estimates of Equation (1.2.5) using 1 to 5 numbers of lags of the loan-quota ratio. The coefficients of loan-quota ratio using different level of lags are slightly smaller than our baseline estimates (Figure 1.2.1), but still plausible. Figure 1.2.1 illustrates the estimates of the coefficient of interest with different levels of lags (Table 1.2.4, column (2)-(6)). It indicates that the effect is statistically significant during all of the following five years. For each year lag, the coefficient slightly decreases, but in all cases remains positive. Thus, the positive effect of IMF quota lending on unemployment remains significant in the long-run.

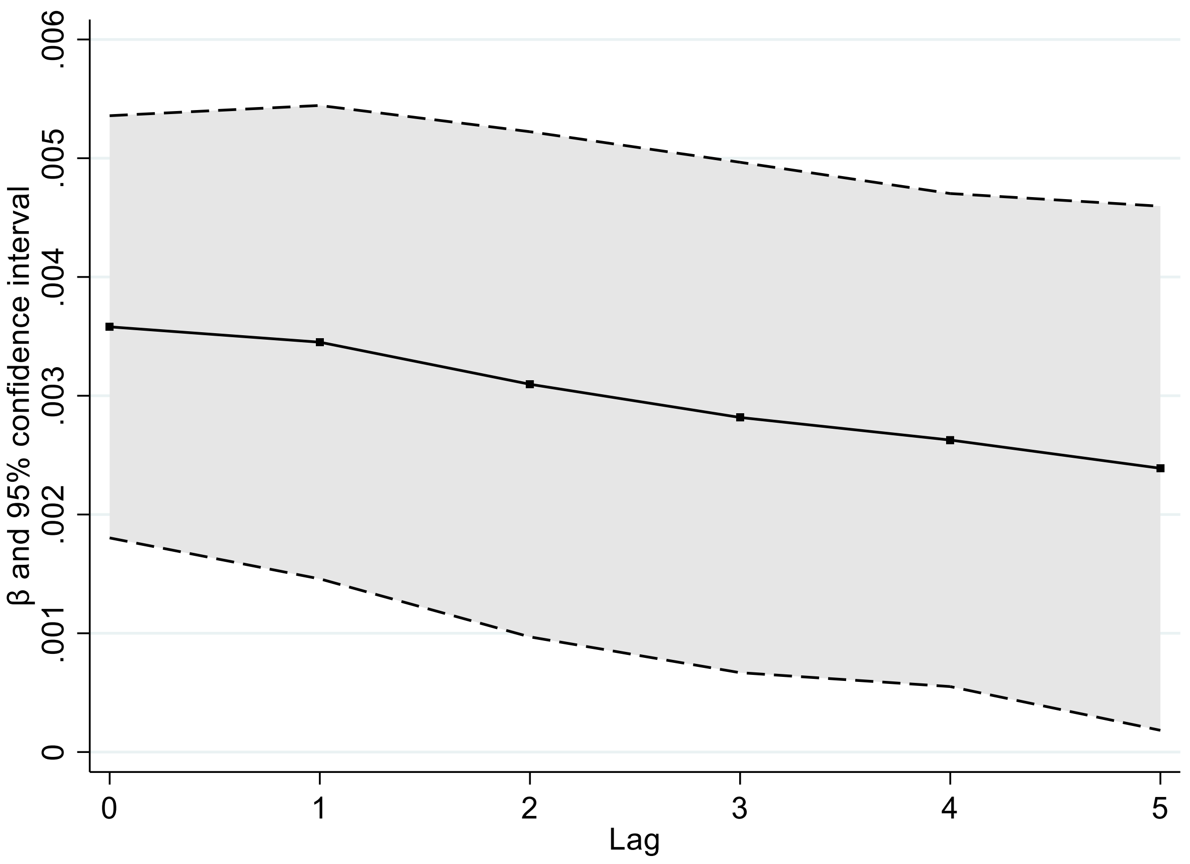


Figure 1.2.1 Long run effects of IMF loan-quota ratio

1.2.6 Estimating the association between IMF program participation and the unemployment rate

In this section, we substitute our main explanatory variable, loan quota ratio, with IMF program participation (a binary variable) to support our initial results. Likewise, countries are not randomly assigned into a 'treatment group' of IMF arrangements program in a given year, therefore we also need to control for unobservable factors—such as the political will to implement reforms—that affect both IMF participation and our outcome variable (Vreeland, 2003). If we fail to account for these unobserved factors, then their effect will be incorrectly attributed to IMF participation. Following previous studies (e.g., Clements et al., 2013; Dreher and Walter, 2010; Nooruddin and Simmons, 2006; Stubbs et al., 2017; Wei and Zhang, 2010), we control for bias due to non-random country selection into IMF programs by including the inverse-Mills ratio in our model (Heckman, 1979). Results on the Probit model, which generates the values of the inverse-Mills ratio are reported in Table A1.2.6 of the Appendix. A significant negative coefficient on the inverse-Mills ratio indicates that unobserved variables that make IMF program participation more likely are associated with lower levels of the unemployment rate; a significant positive coefficient indicates that unobserved factors that make IMF participation more likely are associated with higher levels of the unemployment rate (Stubbs et al., 2017; Kentikelenis et al., 2015).

Our model specification, estimated using OLS, is the following:

$$Y_{it} = \xi + \kappa_i + \lambda_t + \delta\, IMFp_{it} + X_{it}'\beta + \rho\, IMR_{it} + e_{it}, \tag{1.2.6}$$

Y_{it} is the dependent variable, the unemployment rate, as in Equation (1.2.2).$IMFp_{it}$ is a dummy variable for IMF participation, it takes the value of 1 if an IMF program is in active for at least 5 months in a specific year t and 0 otherwise (Dreher, 2006). IMR_{it} is the inverse-Mills ratio that controls for non-random country selection into IMF programs. X_{it} denotes a vector of control variables as discussed above. Finally, κ is a set of country dummies (i.e., country fixed effects), λ is a set of period dummies (i.e., year fixed effects), and e_{it} is the error term.

Having replaced our explanatory variable with IMF program participation we evaluate the relationship between IMF participation (now expressed with a binary variable) and the unemployment rate. Table 1.2.5 presents the results of the association of IMF participation with the unemployment rate. In Model 1 we examine the contemporaneous effect of IMF participation, which yields to a positive and statistically significant association with the unemployment rate. A one standard deviation increase in the IMF program participation variable (0.45) results in an increase in the unemployment rate by 0.005 (=0.45*0.012), which corresponds to approximately 10% of its standard deviation. This effect remains the same in

Model 2, where we lag all independent variables by one year for a delay effect.[47] Medium and long run effects of IMF participation are reported in Table 1.2.6 and an illustration of the coefficient results are provided in Figure 1.2.2. The figure illustrates the coefficient estimates of Equation (1.2.6) with different levels of lags (Table 1.2.6 reports the estimates of the coefficient of interest, to save space we only report results on IMF program participation). The effect of IMF program participation is statistically significant for the next following three years, while after four years the effect is no longer significantly different from zero. For each year lag, the coefficient slightly decreases, but in all cases remains positive.

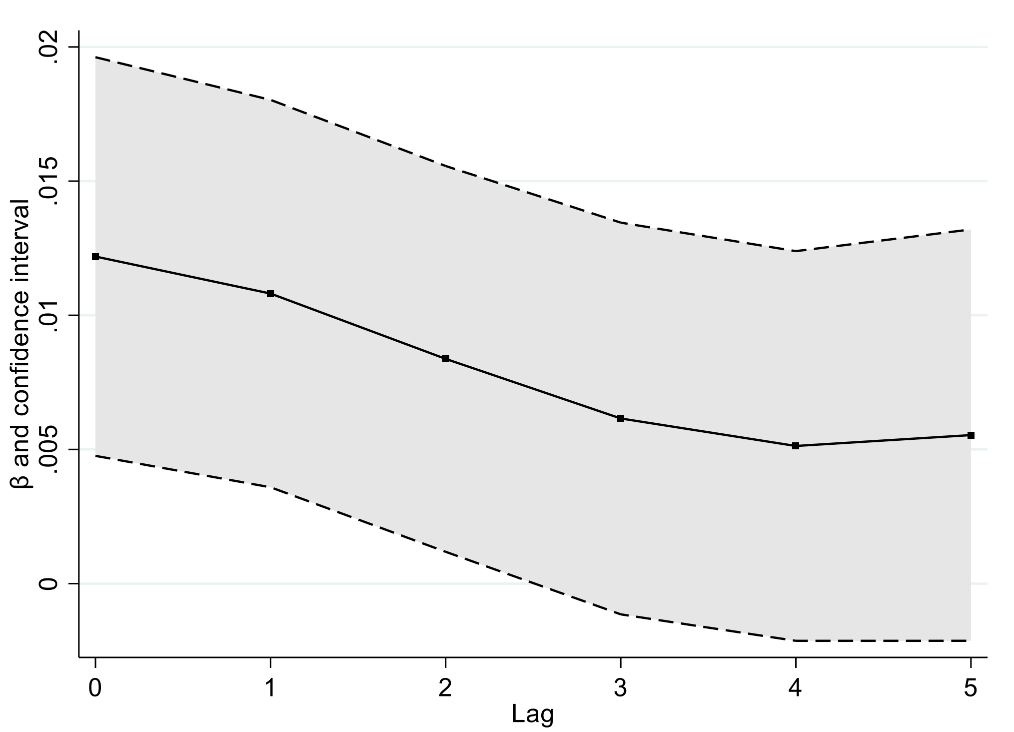
Table 1.2.5 Effects on the unemployment rate - IMF program participation

	(1)	(2)
IMF participation	0.012***	
	(0.004)	
IMF participation (*t*-1)		0.011***
		(0.004)
Trade	0.009	
	(0.011)	
Trade(*t*-1)		-0.002
		(0.012)
Democracy index	0.000	
	(0.000)	
Democracy index(*t*-1)		0.000
		(0.000)
Nominal GDP (logs)	-0.017***	
	(0.006)	
Nominal GDP (logs)(*t*-1)		-0.013**
		(0.006)
Population growth	-1.150***	
	(0.425)	
Population growth(*t*-1)		-0.747**
		(0.359)
School enrollment	-0.001	
	(0.017)	
School enrollment(*t*-1)		-0.002
		(0.016)
IMR (Inverse-Mills Ratio)	-0.072***	-0.076***

[47] In Table A1.2.7 and A1.2.8 of the Appendix we provide results with extended set of control variables and additional robustness checks, respectively. Using these alternative specifications does not substantively alter our initial results.

	(0.019)	(0.021)
Observations	1,802	1,797
Adjusted R^2	0.222	0.178

Notes: Estimation results are obtained by estimating Equation (1.2.6), controlling for selection bias. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses. Significance level is denoted by *** (1%), ** (5%) and * (10%). Model 2 features all independent variables entering with a lag.



	t	t-1	t-2	t-3	t-4	t-5
IMF program participation	0.012^{***}	0.011^{***}	0.008^{**}	0.006^{*}	0.005	0.006
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	1,802	1,802	1,799	1,796	1,794	1,791

Notes: Reported beta-coefficients and standard errors for different lags of the variable IMF program participation ($IMFp$) (Equation (1.2.6)).
Significance level is denoted by *** (1%), ** (5%) and * (10%).

Figure 1.2.2 and **Table 1.2.6** Long run effects of IMF program participation

1.2.7 Instrumenting for IMF program participation

To support our previous strong evidence, we focus on additional tests that can address our initial problem of selection bias and other forms of endogeneity. For this purpose, in this section, we use an instrumental variable approach to identify the direction of the association between IMF program participation and the unemployment rate.

In general, instruments are hard to find; as a variable is likely to be related to the outcome through channels outside of IMF participation and thus violate the exclusion criterion. However, we are able to draw on an ongoing literature that focuses on the effects of foreign aid (e.g., Dreher and Langlotz, 2017; Nunn and Qian, 2014) using a compound instrumental approach and recently implemented to evaluate the effects of IMF programs (Lang, 2020). Lang (2020) uses the interaction of the Fund's liquidity ratio, defined as the amount of liquid resources available to the IMF in a given year divided by its liquid liabilities, with the likelihood that a country in year t is under an IMF arrangement as a plausibly exogenous instrument for IMF program participation, providing strong defense for the instrument's exclusion restriction. In times with high liquidity ratios, the Fund is more generous providing new loan arrangements, and the liquidity ratio is not driven by factors that have to do with borrowing countries characteristics (Nelson and Wallace, 2017). The probability of IMF program participation is calculated as the share of years between 1971 and year t[48] in which a country was under a loan arrangement (Dreher, 2006; Lang, 2020).[49]

[48] If a country enters an IMF arrangement at the starting year of the examined period (i.e., in 1971) the probability equals to one (from country to country the first year of the period can vary (e.g., after 1971) due to missing data).

[49] Lang (2020) argues that even if the time-variant level variable (probability of IMF program) was endogenous, the exclusion restriction would only be violated if the unobserved variables driving this relationship were correlated with the country-specific probability (for analytic proofs see Bun and Harrison, 2018; Nizalova and Murtazashvili, 2016).

Following Lang's strategy, we constructed a compound instrument by interacting the IMF liquidity ratio variable with a time-varying measure of countries' propensities to enter into IMF programs.

Our model specification, rely on a 2SLS panel regression, looks as follows:

$$Y_{it} = \xi + \kappa_i + \lambda_t + \delta\, \widehat{IMF}_{it-1} + \theta\, prob_{it} + X'_{it-1}\beta + \varepsilon_{it} \qquad \text{Second-stage} \qquad (1.2.7)$$

$$IMFp_{it} = \pi + \varphi_i + \nu_t + \tau(prob_{it} \times liq_t) + \theta\, prob_{it} + W'_{it}\gamma + u_{it} \qquad \text{First-stage} \qquad (1.2.8)$$

In the second stage $\widehat{IMFp}_{it}$ is the fitted value of IMF participation obtained from the first stage. In the first stage, as discussed above, we use a compound instrument for IMF participation $(prob_{it} \times liq_t)$, liq is the Fund's liquid ratio in a given year t and $prob_{it}$ is a time varying measure of countries' propensities to enter into an IMF program – estimated using a Probit model as a function of the compound instrument, the vector of control variables (W), year (ν) and region (φ) fixed effects. To estimate our model, we use a 2SLS IV approach, implemented through STATA using *cmp* command (Roodman, 2011).

The results using the IV approach (Table 1.2.7)[50] support our previous evidence. We find a significant positive association between IMF participation and the unemployment rate. A one standard deviation (0.45) increase in the IMF program participation variable results in an increase in the unemployment rate by 0.007 (=0.45*0.015) which corresponds to approximately 13% of its standard deviation. Diagnostic statistic (Kleibergen-Paap Wald statistic = 22.07) indicates that the instrument used is strong (above ten, see Staiger and Stock, 1997).

[50] First-stage results are reported in Table A1.2.9 at the Appendix.

Table 1.2.7 IV estimation results

	(1)
IMF participation (*t*-1)	0.015***
	(0.005)
Probability(*t*-1)	0.015
	(0.028)
Trade(*t*-1)	-0.013
	(0.012)
Democracy index(*t*-1)	0.000
	(0.000)
Nominal GDP (logs)(*t*-1)	-0.018***
	(0.007)
Population growth(*t*-1)	-0.773**
	(0.364)
School enrollment(*t*-1)	0.007
	(0.017)
F-statistic	22.07
Observations	1,785

Notes: Estimation results are obtained by estimating Equation (1.2.7). F-test is Kleibergen-Paap Wald statistic. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses. Significance level is denoted by *** (1%), ** (5%) and * (10%). All time-variant explanatory variables are lagged at t-1.

1.2.8 IMF program participation, conditionality and the unemployment rate

In this section along with IMF program participation, we also account for the number of conditions – policy reforms included within the program. To capture IMF conditionality, we use data from Kentikelenis et al. (2016) for a period between 1980 to 2014, therefore, in this part of our analyses, the year period is restricted to 1980-2014. This database provides detailed information on the conditions included in loans and their implementation sourced directly from internal IMF documents. In our analysis, countries select into both IMF program participation and conditionality, as such we face a well-known inferential challenge of nonrandom selection into IMF programs and policy conditions. First, as discussed in the previous section, countries are not randomly assigned into a treatment group of IMF programs in a given year. Second, a country's selection in IMF conditionality in a given year is also not randomly assigned, consequently, endogeneity may arise from the systematic differences between countries that receive more IMF conditions and those that receive fewer conditions, therefore, in order to estimate the true effect of conditionality on the unemployment rate, we have to mitigate this issue.

In order to mitigate this issue, we follow Stubbs et al. (2020) and estimate a system of equations including instrumental variables and allowing for correlated errors across equations (Roodman, 2011). The instrument for IMF program participation is the same we used in Section 1.2.7 (Lang, 2020). Following Stubbs et al. (2020), the instrument for IMF conditionality is the interaction of the within-country average of the number of conditions across period of interest with the Fund's liquid ratio. We argue that this instrument is valid. First, with respect to the instrument's relevant criterion, in times of scarce resources the Fund assigns a higher number of conditions to borrowing countries (Lang, 2020; Stubbs et al., 2020). Second, regarding the exclusion criterion, we argue that the compound instrument likely fulfils the exclusion restriction because the Fund's liquid ratio – determined independent of a given country – affects the unemployment rate only through the IMF measure of interest, conditional on a country's mean exposure to IMF programs, the controls, year and country fixed effects (Forster et al., 2020).

Our identification strategy is the following:

$$Y_{it} = \xi + \kappa_i + \lambda_t + \delta_1 \widehat{IMFp}_{it-1} + \theta\, prob_{it} + \delta_2 \widehat{Cond}_{it-1} + X'_{it-1}\beta + \varepsilon_{it} \qquad \text{Second-stage} \qquad (1.2.9)$$

$$\widehat{IMFp}_{it} = \pi + \varphi_i + \nu_t + \tau(prob_{it} \times liq_t) + \theta\, prob_{it} + W'_{it}\gamma \qquad \text{First-stage} \qquad (1.2.10)$$

$$\widehat{Cond}_{it} = c_0 + \kappa_i + \lambda_t + c_1(\overline{Cond}_{it} \times liq_t) + X'_{it}c_2 \qquad \text{First-stage} \qquad (1.2.11)$$

In the second stage $\widehat{Cond}_{it}$ is the fitted value of IMF conditionality obtained from the first stage. In the first stage, as discussed above, we use a compound instrument for IMF conditionality $(\overline{Cond}_{it} \times liq_t)$, liq is Fund's liquid liabilities (logs) in a given year t and $\overline{Cond}_{it}$ is the within-country average of the number of conditions across period of interest –

estimated as a function of the compound instrument, the vector of control variables (X), year (ν) and year (λ) fixed effects.

The system of three simultaneous equations is estimated through maximum likelihood estimation (MLE), combining an instrumental variable approach to address endogeneity of IMF participation (Lang, 2020) with an instrumental variable approach to address endogeneity of conditionality (Stubbs et al., 2020).

The results of the above exercise are reported in Table 1.2.8.[51] IMF program participation and conditionality exert a positive and statistically significant sign. An increase in total conditions by one standard deviation (14.232) increases the unemployment rate by 0.014 (=14.232*0.001) which corresponds to approximately 26% of its standard deviation. Diagnostic statistics, reported at the bottom of the table, indicate that our compound instruments are strong, as suggested by the respective Kleibergen-Paap Wald statistics (Staiger and Stock, 1997). Figure 1.2.3 illustrates the marginal plot with the predicted values of the unemployment rate and the 95 percent confidence interval. In the absence of an IMF program, our models predict a value of 0.076 for the unemployment rate (slightly below the mean). For countries with IMF programs, the estimated value is 0.098 (IMF program with 0 total conditions), which subsequently increases as the number of total conditions increases. The difference of 0.022 (=0.098-0.076) is equal to approximately 41% of the standard deviation of the outcome variable.

Table 1.2.8 IMF participation, total conditions and the unemployment rate

	(1)
IMF participation (*t*-1)	0.022**
	(0.010)
Probability (*t*-1)	0.029
	(0.032)
Total conditions (*t*-1)	0.001**

[51] First-stage results are reported in Table A1.2.10 at the Appendix.

	(0.000)
Trade(*t*-1)	-0.012
	(0.012)
Democracy index(*t*-1)	0.000
	(0.000)
Nominal GDP (logs)(*t*-1)	-0.012*
	(0.007)
Population growth(*t*-1)	-0.779**
	(0.363)
School enrollment(*t*-1)	0.011
	(0.017)
F-statistic IMF program	24.32
F-statistic total conditions	33.25
Observations	1,717

Notes: Estimation results are obtained by estimating Equation (1.2.9). F-tests is Kleibergen-Paap Wald statistics. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses. Significance level is denoted by ** (5%) and * (10%). All time-variant explanatory variables are lagged at t-1.

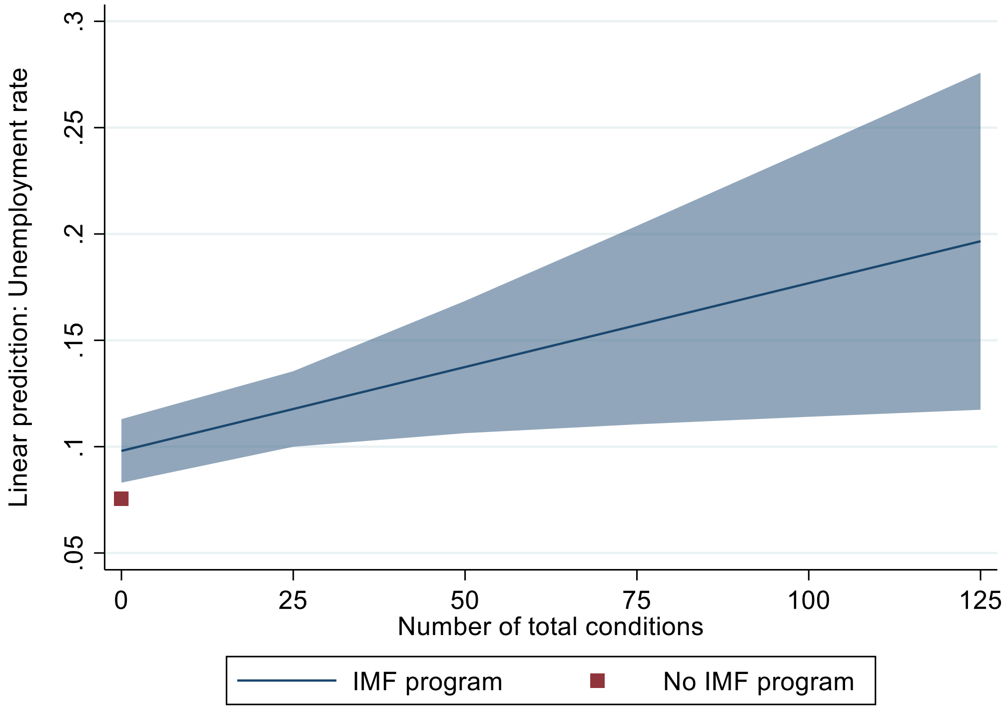


Figure 1.2.3 Marginal effect of total conditions on the unemployment rate

1.2.9 Conclusion

In this subchapter, we explore the extent to which participation in loan conditional programs administered by the IMF affects the unemployment rate in recipient countries. With an (unbalanced) panel dataset covering 96 countries across the world during the period 1971-

2015, we confront the issue of selection bias and potential endogeneity. Our results suggest that countries' participation in IMF conditional programs tends to increase the unemployment rate. The existence of an IMF agreement in the long run holds the effect robust. Lastly, our results indicate that the effect of IMF program participation is conditional on the number of conditions: the adverse effect of IMF participation becomes larger as the number of total conditions increases.

Like much empirical work in economics, the conclusions of this study raise more questions than they answer. In fact, virtually all conditional loans made by the IMF come with conditions-reforms that include: enhancing labour flexibility, weakening of labour rights, reduction in government spending (public sector layoffs and wage cuts) and privatizing public sector industries (e.g., in 1995 a public sector condition was added to Honduras' loan program and the government was subsequently obliged to reduce expenditures on public sector employment (IMF, 1998); Moldova's IMF-designed labour-related reforms included measures to optimize the number of employees in the budgetary sector (IMF, 2010)).

At the micro level, greater labour flexibility enhances the ability of firms to hire and fire workers, and alter the conditions and terms of employment, with minimal regulatory restrictions. But the lower wages would generate more investment, more demand for labour. Ideally, the beneficial results of these policies would be: reduce unemployment, increase the competitiveness of industries, and facilitate the adjustment of the private sector during an economic decline. Nevertheless, the evidence in Latin America is not supportive of those conclusions, wage flexibility has not been associated with lower unemployment, nor has there been more job creation in general (Freeman 2005; Lloyd and Weissman 2002).

Yet there are some loans that do not include conditions that specify cuts to spending on public sector employment. Independent Evaluation Office concludes that reductions in public employment or public sector wages induced by IMF programs are short-lived because they are easily reversed (Independent Evaluation Office, 2003). Furthermore, empirical studies find no evidence that IMF programs reduce wages or salaries (Nooruddin and Vreeland, 2010). Similarly, Nooruddin and Rudra (2009) find no evidence that IMF programs provoke layoffs in the public sector.

Despite the mixed evidence the literature provides and our finding which focus on the unemployment rate, the IMF still advocates reforms that aim at labour market liberalization, public sector employment reduction, or reductions in government wage spending (Kentikelenis et al., 2016).

Appendix 1.2

Table A1.2.1 List of countries

Albania	Georgia	Nicaragua
Algeria	Germany	Norway
Argentina	Greece	Pakistan
Armenia	Guatemala	Panama
Australia	Honduras	Paraguay
Austria	Hungary	Peru
Bahrain	India	Philippines
Bangladesh	Indonesia	Poland
Belarus	Ireland	Portugal
Belgium	Israel	Qatar
Bhutan	Italy	Romania
Bolivia	Jamaica	Russian Federation
Botswana	Japan	Saudi Arabia
Brazil	Jordan	Slovak Republic
Bulgaria	Kazakhstan	Slovenia
Canada	Korea, Rep.	Solomon Islands
Chile	Kuwait	South Africa
China	Kyrgyz Republic	Spain
Colombia	Latvia	Sri Lanka
Costa Rica	Lithuania	Suriname
Croatia	Luxembourg	Sweden
Cyprus	Macedonia, North	Switzerland
Czech Republic	Malaysia	Syrian Arab Republic
Denmark	Mauritius	Tajikistan
Dominican Republic	Mexico	Thailand
Ecuador	Moldova	Tunisia
Egypt, Arab Rep.	Mongolia	Turkey
El Salvador	Montenegro	Ukraine
Estonia	Morocco	United Kingdom
Fiji	Namibia	United States
Finland	Netherlands	Uruguay
France	New Zealand	Venezuela, RB

Countries without program participation:
Australia, Austria, Belgium, Botswana, Canada, Denmark, France, Germany, Japan, Malaysia, Namibia, Netherlands, New Zealand, Norway, Qatar, Sweden, Suriname, Syrian Arab Republic, United States.

Table A1.2.2 Description of Variables

Variables	Mean	SD	Min	Max
Unemployment rate: The unemployment rate, (%) percent (the number of unemployed persons as a percentage of the total labour force). Source: International Financial Statistics.	0.085	0.054	0	0.373
Loan-quota ratio: Sum of all current IMF loans a country is eligible to as a share of its quota at the IMF. Source: International Financial Statistics and own calculations.	0.646	1.932	0	22.28
IMF program participation: Dummy variable: equals to 1 if IMF program active for 5 or more months in a year, 0 otherwise. Source: IMF Lending Arrangements data and Dreher (2006).	0.282	0.45	0	1
Total conditions: Number of binding conditions in a given year. Source: Kentikelenis et al. (2016).	6.687	14.232	0	124
Investment share (% of GDP): Gross domestic investment (formally gross capital formation) measured as a share of GDP. A low ratio of investment to GDP may indicate limited access to international capital markets, thereby making it more likely that it requests Fund assistance (Przeworski and Vreeland, 2000). In addition, higher investment share may decrease unemployment (Arestis et al., 2007). Source: World Development Indicators.	0.241	0.066	0.003	0.707
Trade (% of GDP): The sum of exports and imports of goods and services measured as a share of GDP. Trade openness reduces aggregate unemployment (Felbermayr, 2011). Source: World Development Indicators.	0.783	0.414	0.107	4.102
Government share (% of GDP): Government (final consumption expenditure) share of GDP. Higher government expenditure may increase the probability of signing an IMF arrangement (Dreher, 2004). Source: World Development Indicators.	0.167	0.048	0.031	0.435
Current account balance (% of GDP): Current account balance is the sum of net exports of goods and services, net primary income, and net secondary income. A country with current account deficit will be more likely to demand IMF credit (Sturm et al., 2005). Source: World Development Indicators.	-0.017	0.065	-0.497	0.389
Debt (% of GDP): Public debt, namely gross government debt to GDP ratio. A high debt ratio may lead to more demand for IMF credit (Thacker, 1999). Source: Historical Public Debt Database (HPDD) (Abbas et al., 2010).	0.559	0.615	0.016	20.929
Number of years under IMF: Cumulative years under IMF arrangement participation. The number of years under an IMF arrangement increases the probability to enter into a program (Przeworski and Vreeland, 2000; Binder and Bluhm, 2017). Source: IMF Lending Arrangements data and own calculations.	10.906	10.211	0	33
Democracy index: Proxy for democracy, captures this regime authority spectrum on a 21-pont scale ranging from -1 (hereditary monarchy) to +1 (consolidated democracy).	0.724	0.738	-1	1

Democratic regime is associated with lowering the probability of signing an IMF arrangement (Sturm et al., 2005). Source: Polity IV Project and own calculations.				
Inflation: Inflation as measured by the annual growth rate of the GDP implicit deflator shows the rate of price change in the economy as a whole. The GDP implicit deflator is the ratio of GDP in current local currency to GDP in constant local currency. From the one hand, countries experiencing high inflation are more likely in need of IMF assistance (Dreher and Vaubel, 2004). From the other hand, the willingness of the IMF to provide fund may be lower in the case of high inflation (Bird, 1995). Source: World Development Indicators.	0.211	2.069	-0.263	50.182
School enrolment: Referring to school enrollment, secondary (% gross). Gross enrollment ratio is the ratio of total enrollment, regardless of age, to the population of the age group that officially corresponds to the level of secondary education. The relationship between education (years of schooling) and the duration of unemployment is documented to be negative (Nickell, 1979). Source: World Development Indicators.	0.885	0.219	0.196	1.639
Population growth: Annual percentage growth rate of total population by country and year. David and Freeman (1986) argue that population growth and employment may have a positive relationship via productivity, e.g., countries are able to "absorb" labour supply at increased productivity and with a shift towards more productive employment. Source: World Development Indicators.	0.008	0.01	-0.038	0.068
Nominal GDP (logs): GDP (current US dollars). Source: World Development Indicators and own calculations.	25.295	1.99	19.994	30.528
GDP per capita (logs): Data are in constant 2010 U.S. dollars. Source: World Development Indicators and own calculations.	9.206	1.229	5.96	11.626
IMF liquidity ratio: IMF liquid resources divided by liquid liabilities (logged) Source: Lang (2020)	5.565	0.771	3.489	7.109
Part time workers rights: The index measures how legal system treats part time workers rights – compared to full-time workers (values close to 1 reflect that part-time workers rights are equally with full-time workers rights). Source: Adams et al. (2017)	0.896	0.264	0	1
Unionization: The index measures the protection of the right to form trade unions in the country's constitutions (higher values entail more rights). Source: Adams et al. (2017)	0.84	0.295	0	1
Waiting period: The index measures the required waiting period prior to industrial action (lower values entail longer waiting period). Source: Adams et al. (2017)	0.19	0.345	0	1

Table A1.2.3 Effects of IMF loan quota ratio on the unemployment rate - Arellano and Bond estimates

	(1)	(2)	(3)	(4)	(5)
Dependent variable(t-1)	0.538***	0.658***	0.660***	0.657***	0.721***
	(0.061)	(0.098)	(0.098)	(0.103)	(0.084)
Dependent variable(t-2)		-0.141**	-0.085	-0.085	-0.101
		(0.065)	(0.103)	(0.108)	(0.114)
Dependent variable(t-3)			-0.077	-0.040	-0.029
			(0.061)	(0.060)	(0.064)
Dependent variable(t-4)				-0.063**	-0.037
				(0.025)	(0.031)
Dependent variable(t-5)					-0.039
					(0.027)
Loan-quota ratio	0.002**	0.002**	0.002**	0.002**	0.001**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Trade	-0.036***	-0.032***	-0.030***	-0.027***	-0.024***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Democracy index	-0.002*	-0.002**	-0.002***	-0.002*	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Nominal GDP (logs)	-0.027***	-0.026***	-0.027***	-0.028***	-0.026***
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)
Population growth	-0.198	-0.261	-0.327	-0.249	-0.195
	(0.289)	(0.290)	(0.297)	(0.300)	(0.290)
School enrollment	0.004	0.002	-0.000	0.000	0.001
	(0.017)	(0.017)	(0.016)	(0.017)	(0.016)
Observations	1,743	1,672	1,597	1,520	1,445
AR2 test (p value)	0.826	0.303	0.902	0.777	0.637

Notes: This table presents results using Arellano and Bond's (1991) GMM estimator. Estimated equation: $\Delta Y_{it} = \Delta\lambda_t + \sum_{j=1}^{p} \gamma_j \Delta Y_{it-j} + \delta\ \Delta IMFq_{it} + \beta\ \Delta X'_{it} + \Delta\varepsilon_{it}$. The specification includes$p$lags (where$p = 1, \ldots, 5$) of the dependent variable on the right hand side. AR2 test is the Arellano–Bond test of second order autocorrelation in the residuals. Year dummies are included in all regressions. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses.

Significance level is denoted by *** (1%), ** (5%) and * (10%).

Comparing with the baseline results (Table 1.2.3), GMM estimates imply a slightly smaller coefficient of IMF quota lending (the IMF loan-quota ratio coefficient is estimated at 0.002 including 1 to 4 lags of the dependent variable, Model 5 adds up to five lags of the dependent variable, in this case the loan-quota ratio coefficient becomes smaller and equal to 0.001). These results are qualitatively similar with the long-run effects (Table 1.2.4) we found.

Table A1.2.4 Extended control variables IMF loan-quota ratio

	(1)	(2)	(3)
Loan-quota ratio(*t*-1)	0.003***	0.003***	0.003***
	(0.001)	(0.001)	(0.001)
Trade(*t*-1)	-0.011	-0.011	-0.013
	(0.011)	(0.011)	(0.011)
Democracy index(*t*-1)	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Nominal GDP (logs) (*t*-1)	-0.008	-0.009	-0.008
	(0.006)	(0.006)	(0.006)
Population growth(*t*-1)	-0.884**	-0.969**	-0.872**
	(0.365)	(0.371)	(0.356)
School enrollment(*t*-1)	0.001	0.004	0.005
	(0.015)	(0.015)	(0.015)
Part-time workers rights(*t*-1)	-0.032***		
	(0.010)		
Unionization (*t*-1)		0.027**	
		(0.010)	
Waiting period(*t*-1)			0.035***
			(0.012)
GR	0.061***	0.059***	0.060***
	(0.012)	(0.012)	(0.012)
Observations	1,627	1,627	1,627
Adjusted R^2	0.232	0.225	0.230

Notes: Estimation results are obtained by estimating Equation (1.2.5), controlling for selection bias. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses. Significance level is denoted by *** (1%) and ** (5%). All time-variant explanatory variables are lagged at t-1.

Table A1.2.5 Additional robustness checks IMF loan-quota ratio

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Loan-quota ratio	0.004***	0.003***	0.003***	0.002*	0.004***	0.005***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Trade	0.009	0.006	0.002	-0.007	0.000		
	(0.011)	(0.011)	(0.011)	(0.015)	(0.011)		
Democracy Index	-0.000	-0.000	-0.000*	-0.000	-0.000		
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)		
GDP per capita (logs)	-0.120*	-0.133**					
	(0.062)	(0.059)					
Population growth	-0.839**	-0.661*	-1.055**	-0.429	-0.545*		
	(0.404)	(0.391)	(0.404)	(0.308)	(0.316)		
School enrollment	-0.008	-0.011	0.009	0.015	-0.008		
	(0.016)	(0.017)	(0.017)	(0.024)	(0.017)		
Investment share		-0.184***					
		(0.035)					
Nominal GDP (logs)			-0.015***	-0.009	-0.018***		
			(0.006)	(0.008)	(0.006)		
GR	0.059***	0.025***	0.056***	0.035***	0.047***		0.046***
	(0.011)	(0.008)	(0.010)	(0.011)	(0.010)		(0.010)
Observations	1,825	1,825	1,583	1,044	1,421	2,195	2,195
Adjusted R^2	0.227	0.275	0.253	0.168	0.253	0.121	0.163

Notes: Estimation results are obtained by estimating Equation (1.2.5), controlling for selection bias. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses. Significance level is denoted by *** (1%), ** (5%) and * (10%).
Model 1 uses instead of Nominal GDP (logs) the GDP per capita (logs) as control variable.
Model 2 adds investment share as an additional control variable.
Model 3 displays results when restricting the sample to country-years after 1991 which marks the end of the cold war with the Soviet Union. The Soviet Union ceased to exist on 26 December 1991.
Model 4 displays results when regressing only developing countries (65 countries in the sample).
Model 5 displays results excluding high exporting countries (China, United States, Germany, Japan, South Korea, France, Russian Federation, Netherlands, Italy, United Kingdom, Canada, Mexico, Switzerland, Belgium, Spain, India and Saudi Arabia).
For Model 3, 4 and 5 the results remain the same even if we use the set of regressors of Model 1 and 2.
Estimation results for Model 6 are obtained by estimating: $Y_{it} = \xi + \kappa_i + \lambda_t + \delta\ IMFq_{it} + \varepsilon_{it}$, to check if the relationship between the unemployment rate and IMF quota lending is not driven from other variables.
Estimation results for Model 7 are obtained by estimating: $Y_{it} = \xi + \kappa_i + \lambda_t + \delta\ IMFq_{it} + \breve{\eta}\hat{u}_{it} + \varepsilon_{it}$, similar to Model 6, but controlling for selection bias.

Table A1.2.6 Probit model for IMF participation

Dependent variable: *IMF program participation*	
Independent Variables	Marginal effects
Investment share	-5.886***
	(1.851)
Trade	-0.184
	(0.258)
Government share	-2.759
	(2.742)
Current account	-4.008**
	(1.738)
Debt	0.099
	(0.157)
Democracy index	-0.012**
	(0.005)
Inflation	-0.037
	(0.027)
Number of years under IMF	0.104***
	(0.014)
Observations	2,148

Notes: Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses. Significance level is denoted by *** (1%) and ** (5%).

The procedure we employ to address the issue of 'selection bias' follows the standard Heckman's (1979) two-step method. First, we run a Probit regression to predict IMF program participation:

$$IMFp_{it} = \pi\, Z_{it} + \nu_{it}, \tag{a1.2.1}$$

where IMF participation is assumed to be a linear function of a list of covariates, Z_{it}, and a stochastic component (*i.i.d.* error term), ν_{it}. In the presence of selection bias, e from Equation (1.2.6) (participation effects equation) and ν from Equation (a1.2.1) are correlated.

We then compute the so-called 'inverse-Mills ratio',$\hat{\Lambda}_{it}$, for each observation in the sample:

$$\hat{\Lambda}_{it} = \frac{\phi(Z_{it}\hat{\pi})}{\Phi(Z_{it}\hat{\pi})}, \tag{a1.2.2}$$

where ϕ denotes the standard normal density function, Φ the standard normal cumulative distribution function, and $\hat{\pi}$ is an estimated value taken from Equation (a1.2.1).

Second, we add the estimated 'inverse-Mills ratio' to the vector of controls in Equation (1.2.6). Its coefficient is interpreted as follows: if significantly negative, then unobserved variables that make IMF participation more likely are associated with lower levels of the unemployment rate; if significantly positive, then unobserved variables that make IMF participation more likely are associated with higher levels of the unemployment rate; if non-significant, then there is no association.

Table A1.2.7 Extended control variables IMF program participation

	(1)	(2)	(3)
IMF participation(*t*-1)	0.009**	0.009**	0.009**
	(0.004)	(0.004)	(0.004)
Trade(*t*-1)	-0.007	-0.007	-0.008
	(0.012)	(0.011)	(0.012)
Democracy index(*t*-1)	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Nominal GDP (logs) (*t*-1)	-0.014**	-0.014**	-0.014**
	(0.007)	(0.007)	(0.007)
Population growth(*t*-1)	-0.880**	-0.961**	-0.853**
	(0.385)	(0.396)	(0.371)
School enrollment(*t*-1)	-0.004	-0.001	0.001
	(0.016)	(0.016)	(0.016)
Part-time workers rights(*t*-1)	-0.028**		
	(0.012)		
Unionization (*t*-1)		0.024*	
		(0.013)	
Waiting period(*t*-1)			0.041***
			(0.011)
IMR	-0.074***	-0.072***	-0.079***
	(0.021)	(0.023)	(0.023)
Observations	1,623	1,623	1,623
Adjusted R^2	0.193	0.187	0.199

Notes: Estimation results are obtained by estimating Equation (1.2.6), controlling for selection bias. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses. Significance level is denoted by *** (1%) and ** (5%). All time-variant explanatory variables are lagged at t-1.

Table A1.2.8 Additional robustness checks IMF program participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
IMF participation	0.015***	0.013***	0.012***	0.010***	0.013***	0.017***	0.014***
	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
Trade	0.017*	0.013	0.014	-0.001	0.007		
	(0.010)	(0.011)	(0.010)	(0.016)	(0.011)		
Democracy Index	0.000	0.000	0.000	-0.000	0.000		
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)		
GDP per capita (logs)	-0.087	-0.134**					
	(0.070)	(0.065)					
Population growth	-1.076**	-0.750*	-1.365***	-0.464	-0.668*		
	(0.428)	(0.406)	(0.435)	(0.289)	(0.337)		
School enrollment	-0.017	-0.015	0.000	0.012	-0.017		
	(0.018)	(0.020)	(0.018)	(0.026)	(0.019)		
Investment share		-0.227***					
		(0.036)					
Nominal GDP (logs)			-0.016**	-0.010	-0.024***		
			(0.007)	(0.008)	(0.008)		
IMR	-0.080***	-0.036***	-0.088***	-0.041**	-0.075***		-0.069***
	(0.020)	(0.013)	(0.027)	(0.015)	(0.021)		(0.015)
Observations	1,793	1,793	1,499	1,005	1,382	2,148	2,148
Adjusted R^2	0.211	0.292	0.259	0.188	0.249	0.092	0.145

Notes: Estimation results are obtained by estimating Equation (1.2.6), controlling for selection bias (including the inverse-Mills ratio). Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses. Significance level is denoted by *** (1%), ** (5%) and * (10%).

Model 1 uses instead of Nominal GDP (logs) the GDP per capita (logs) as control variable.

Model 2 adds investment share as an additional control variable.

Model 3 displays results when restricting the sample to country-years after 1991 which marks the end of the cold war with the Soviet Union. The Soviet Union ceased to exist on 26 December 1991.

Model 4 displays results when regressing only developing countries.

Model 5 displays results excluding high exporting countries (China, United States, Germany, Japan, South Korea, France, Russian Federation, Netherlands, Italy, United Kingdom, Canada, Mexico, Switzerland, Belgium, Spain, India and Saudi Arabia).

For Model 3, 4 and 5 the results remain the same even if we use the set of regressors of Model 1 and 2.

Estimation results for Model 6 are obtained by estimating: $\Upsilon_{it} = \xi + \kappa_i + \lambda_t + \delta\ IMFp_{it} + e_{it}$, to check if the relationship between the unemployment rate and IMF program participation is not driven from other variables.

Estimation results for Model 7 are obtained by estimating: $\Upsilon_{it} = \xi + \kappa_i + \lambda_t + \delta\ IMFp_{it} + \rho\ IMR_{it} + e_{it}$, similar to Model 6, but controlling for selection bias.

Table A1.2.9 2SLS IV First stage results

Dependent variable: *IMF program participation*	
IMF liquidity ratio × Probability	-0.297***
	(0.063)
Probability	2.513***
	(0.311)
Investment share	-1.329**
	(0.652)
Trade	0.029
	(0.108)
Government share	0.688
	(0.868)
Current account	-0.191
	(0.400)
Debt	0.028
	(0.062)
Democracy index	-0.002**
	(0.001)
Inflation	-0.018**
	(0.007)
Number of years under IMF	0.052***
	(0.006)
Observations	1,785

Notes: Estimation results are obtained by estimating Equation (1.2.8). Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses. Significance level is denoted by *** (1%) and ** (5%).

Table A1.2.10 First stage results - MLE

Dependent variable: *IMF program participation*	
IMF liquidity ratio × Probability	-0.663**
	(0.319)
Probability	2.261
	(1.390)
Investment share	-2.568*
	(1.370)
Trade	0.277
	(0.171)
Government share	-0.780
	(1.778)
Current account	-2.387**
	(0.987)
Debt	0.036
	(0.045)
Democracy index	-0.011**
	(0.005)
Inflation	-0.033***
	(0.011)
Number of years under IMF	0.131***
	(0.025)
Dependent variable: *IMF conditionality*	
$\overline{Cond}$ × IMF liquidity ratio	-0.315***
	(0.094)
Trade	-0.676
	(2.418)
Democracy index	0.018
	(0.031)
Nominal GDP (logs)	-5.101**
	(2.399)
Population growth	-26.229
	(126.556)
School enrollment	-1.559
	(4.313)
Observations	1,717

Notes: Estimation results are obtained by estimating Equation (1.2.10) and (1.2.11). Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses. Significance level is denoted by *** (1%), ** (5%) and * (10%).

Subchapter I.III: The effects of IMF programs on income inequality: A semi-parametric treatment effects approach

1.3.1 Introduction

The rise of income inequality becomes the general concern among politicians, social scientists and the press. As we live in a world that displays disturbing trends, income inequality and poverty trends appear to be the most unstable (Sala-i-Martin, 2002). Furthermore, rising of inequality and poverty are not ubiquitous, in many countries it has gone up and has fallen in many others, as well as there being different trends regarding the level of inequality and poverty across countries.

The societal impact of economic globalization has been mentioned by many scholars. However, the impact of international organizations on wealth distribution is still ongoing. Our focus is the impact of the IMF, an international organization initially designed to promote global monetary cooperation among industrial countries. In the late decades the IMF has become an almost universal financial institution, providing financial loan arrangement to its members. To have access on the loan funds of the IMF, recipient countries have to implement a series of policy reforms, such as reduce government spending to bring down national debt, and fortify the stock of foreign reserves or safeguard the currency. In this way, economic austerity and accompanying reform are expected to be a part of IMF's loan package (Farnsworth and Irving, 2017; Nooruddin and Vreeland, 2010). While the IMF has often been blamed for failures in carrying out assistance focusing on development, in the wake of the effect of economic downturns, various calls have been made for an extended role of the IMF. It is difficult to imagine more important development issues than poverty and growth. Nevertheless, the IMF states that is a "monetary" not a "development" institution (Bird, 2004).

To provide evidence on the impact of IMF programs, in this subchapter we examine eleven types of loan arrangements that the IMF has been offering: the Stand-By Arrangement (SBA), the Extended Fund Facility (EFF), the Structural Adjustment Facility (SAF), the Enhanced Structural Adjustment Facility (ESAF), subsequently replaced by the Poverty Reduction and Growth Facility (PRGF), later was relabelled as the Extended Credit Facility (ECF), the Exogenous Shock Facility (ESF), the Standby Credit Facility (SCF), the Flexible Credit Line (FCL), the Precautionary Credit Line (PCL), which replaced by the Precautionary and Liquidity Line (PLL). [52] The SBA designed in 1952 to help countries addressing short-time balance of payment problems, covering up to three years duration. The EFF was set up in 1974 to help countries encountering long-term balance of payments issues, arrangements approved for a period up to four years. The SAF has been used since 1986 providing concessional financing to low-income countries (LICs) for a period of three to five years. Similar to SAF, the ESAF designed in 1987 providing concessional loans to LICs with higher loan amounts and stricter conditions under a three-year arrangement. In 1999 the ESAF replaced by the PRGF providing poverty reduction to LICs, covering up to four years. [53] The ESF was set up in 2005 to assist LICs facing sudden and exogenous shocks, arrangements approved for a period up to two years. The SCF designed in 2009 and provides financial assistance to LICs that may experience episodic, short-term financing and adjustment needs, under a two-year arrangement. The FCL was introduced in 2009 providing assistance for members with balance of payments needs, approved for up to two years. The PCL was introduced in 2010 providing members an effective crisis prevention window for a period up to two years. [54]

Using a novel methodological approach proposed by Acemoglu et al. (2018) and a sample of annual data for 147 countries for the time period 1963 to 2015, we compile evidence that

[52] For more details about IMF lending programs see, e.g., IMF (2019, 2020) and Chletsos and Sintos (2020).

[53] In 2009, the PRGF was relabelled to ECF.

[54] In 2011, the PLL program was introduced replacing PCL.

IMF programs increase income inequality. The results using the Gini index of the Estimated Household Income Inequality (EHII) Data Set built by the University of Texas Inequality Project (UTIP) show a clear increase in income inequality. Also, we conclude that the effect of PRGF and ECF programs on income inequality shows no statistically significant results. These findings may be interpreted as supporting previous empirical work, including Forster et al. (2019), Garuda (2000), Lang (2020) and Oberdabending (2013).

Our analysis contributes to the existing literature by applying a different identification strategy, namely, a potential outcomes framework (Acemoglu et al., 2018). The major advantage of this analysis is that can deal with selection bias and potential endogeneity arises between IMF signed programs and countries' income inequality. The semiparametric methods employed in this study explicitly deal with the dynamics of the dependent variable, income inequality. Specifically, following the proposed methodology, we model the counterfactual process for the outcome variables using (1) the regression adjustment, (2) the inverse probability weighting and (3) the doubly robust estimator, which combines (1) and (2). According to Adam and Tsarsitalidou (2018), the advantage of all three methods is that they control for time and country fixed effects and, additionally, as they are semiparametric, they do not rely on the selection of instruments or matching variables, but instead use lagged values of the variables, by estimating, in our case, the time path of outcome variable after the year the program started.

The remainder of the subchapter is structured as follows: Section 1.3.2 provides the theoretical considerations that motivate our empirical analysis. Section 1.3.3 presents our data and the empirical methodology employed. Section 1.3.4 reports the empirical results. Finally, Section 1.3.5 concludes. Details concerning summary statistics of the data used, countries list and additional estimations are reported in the Appendix at the end of this subchapter.

1.3.2 Theoretical considerations

Trade liberation can affect wealth distribution in many ways. On the one hand, based on Heckscher–Ohlin model greater openness should increase the relative demand and the prices for unskilled labour and lead to a more equal distribution of wages in the low-skilled-labour abundant countries (Stolper–Samuelson theorem). On the other hand, competitiveness from trade openness may worsen some protected sectors. In response to these primary well-known theoretical approaches, there exists a vast empirical research on the relationship between trade openness and income inequality with mixed results. Some scholars argue that there is a detrimental effect of trade liberation on income inequality. [55] Edwards (1997) finds that openness to trade leads to increased income inequality in more developed economies, but not in less developed countries. Nevertheless, the rise of income inequality has been observed robustly in the case of developing countries (e.g., Attanasio et al., 2004; Han et al., 2012; Harrison et al., 2010; Lee and Wei, 2015; Savvides, 2008). On contrary, previous research shows that trade openness may reduce income inequality. Jaumotte et al. (2013) examine a panel of 51 developed and developing countries, and find that trade globalization is associated with a reduction in income inequality. Wu and Hsu (2012) suggest an equalizing effect of international trade on income distribution. Lin and Fu (2016) examine a sample of small developing countries and argue that trade leads to a reduction in income inequality in autocracies, while the effect in democracies is positive, this difference presents based on different trade pattern of each regime.

[55] While this side of research concludes with adverse distributional effects, the majority of scholars who focused on the effects of trade openness on poverty demonstrated that trade liberation may contribute to a conditional poverty reduction (Winters et al., 2004).

The IMF is often considered “the most powerful international institution in history” (Stone, 2002). Providing loan facilities, in recent decades the IMF evolved into the “crisis manager” for developing and developed countries. Nevertheless, it is undeniable that international organizations, therefore the IMF, can take decisions that bind on member states through predictable mechanism and pursuing their interests (Wouters and de Man, 2009). Several studies have observed such behavior within the IMF and argue that its officials push for longer programs, larger loans and more far-reaching conditionality than what is economically optimal (Barnett and Finnemore, 2004; Copelovitch, 2010; Vaubel, 1996).

Many IMF loan conditionalities demand from the eligible country a decrease in budget deficit. This can achieve through: 1) augmented fiscal revenue and 2) decrease in public expenditure. Fiscal revenues can be increased with privatization of state-owned enterprises (SOEs) or restructuring tax systems. [56] However, tax reforms often imply a bigger focus on income taxes, resulting in a deterioration of the after-tax income distribution (Oberdaberning, 2013). Additionally, privatization of SOEs may yield to public sector layoffs. Public expenditure decreases may yield to a decrease in public employment level and wage bills (Abouharb and Cingranelli, 2007; Blanton et al., 2015; Garuda, 2000; Rickard and Caraway, 2014), which may affect poverty levels and worsen income distribution. Furthermore, the fact that many IMF loan programs request reductions in pensions, employment protection and more flexible labour market, can also have adverse distributional effects (Kentikelenis et al., 2016). Nevertheless, the evidence of the literature regarding the association of IMF program participation and social expenditure is ambiguous (Clements et al., 2013; Handa and King, 1997; Martin and Segura-Ubiergo, 2004; Nooruddin and Simmon, 2006).

[56] Crivelli and Gupta (2015) argue that IMF conditionality on countries’ tax structure has a positive impact on tax revenues.

Investigating the impacts of IMF programs on income distribution most studies find that program participation is connected to higher inequality (Forster, 2019; Garuda, 2000; Lang, 2020; Oberdaberning, 2013). More recent studies investigate the effects of IMF financial arrangements on poverty (Estearly, 2003; Garuda, 2000; Makedonas et al., 2015; Oberdaberning, 2013). This literature provides conflicting evidence depending on the sample and the methodology used. Some scholars also examine a related issue, the impact of IMF programs on labour's share of income. The results show a decline in income share (Pastor, 1987; Vreeland, 2002). Because of data limitation and their availability, some studies used alternatives measurements of poverty such as infant mortality and human development index. However, the current evidence shows no clear-cut conclusion about the relationship between IMF engagement and poverty proxies (Hajro and Joyce, 2009; Makedonas et al., 2015; Shandra et al., 2004; Shandra et al., 2012). A detailed summary of the studies evaluating the effects of IMF programs on income inequality and poverty is provided in Table 1.3.1.

Table 1.3.1 *Summary of the empirical literature on the impact of IMF programs*

Outcome variable	Gini coefficient	Poverty	Infant mortality	Human development index	Income share of labour	Period	Countries	Type of programs	Selection correction; method
Garuda (2000)	+*/-* [a]	−*/+* [α]				1975-1991	39 countries	Mixed	Yes; PSM
Estearly (2003)		−* [β]				1980-1998	65 developing countries	Mixed	Yes; IV
Oberdabernig (2013)	+* [c]	+* [c]				1982-2009	LICs and MICs	Mixed	Yes; IV
Lang (2020)	+*/- [d]					1973-2013	155 countries	Mixed	Yes; IV
Forster et al. (2019)	+* [e]					1980-2014	135 countries	Mixed	Yes; IV
Pastor (1987)					−*	1965-1981	18 Latin America countries	SBA & EFF	No; "before-after" approach
Vreeland (2002)					−*	1961-1993	110 countries	Mixed	Yes; Dynamic Heckman
Shandra et al. (2004)			+/+* [f]			1980-1997	59 developing countries	Mixed	No; Lagged dependent panel regression
Hajro and Joyce (2009)			−/−* [γ]	−/−* [γ]		1985-2000	82 developing countries	Mixed	Yes; FE OLS
Shandra et al. (2012)			+*			1990-2005	32 African countries	Mixed	Yes; IV
Makedonas et al. (2015)		+*	+*	−*		1985-2009	97 developing countries	Mixed	No; FE OLS

Notes: Draws on Steinwand and Stone (2008), Bal Gunduz (2016) and Thomson et al. (2017) (selecting studies on income inequality and poverty) as well as recent literature.
Heckman = Heckman two-step estimator for correcting selection bias; IV = Instrumental variable estimator; PSM = Propensity Score Matching; LICs = Low-Income Countries; MICs = Middle Income Countries; EFF = Extended Fund Facility; SBA = Stand-By Arrangement.
+*, Significantly positive; -*, Significantly negative; +, Positive but insignificant; -, Negative but insignificant.
[a] Countries with low propensity scores show improvement, while for those with high propensity score show statistically significant relative worsening of income share of the poorest quintile and increases in Gini coefficients.
[b] This study concludes that the main result of IMF condition is that they lower the "growth of elasticity of poverty"; that is, the proportional change in poverty rates for a given amount of growth. This means that economic expansion benefits the poor less, but at the same time economic contractions hurt the poor less.
[c] The findings are reversed for the period 2000–09 with IMF programs leading to lower poverty and lower inequality.
[d] This study finds that IMF programs substantially increase income inequality in democracies, while having no such effect in non-democracies. Also, the effects of PRGF programs are less positive and statistically insignificant.
[e] Adverse distributional effects also reported by disaggregating IMF programs conditionality by issue area for four policy areas: fiscal policy reforms that restrain government expenditure, external sector reforms stipulating trade and capital account liberalization, financial sector reforms entailing inflation-control measures, and reforms that restrict external debt.
[f] Detrimental significant effects on infant mortality rate when interacted IMF structural adjustment with political democracy (greater at lower levels of democracy).
[g] Significant negative effects on infant mortality (when interacted concessional programs with growth) and human development index (when interacted non-concessional programs with growth). This study claims that its two-way fixed effects approach adequately addresses selection bias concerns.

As mentioned above, the influence of IMF can create winners and losers. Hence, following the above theoretical predictions, we express our testable hypothesis as follows:

Hypothesis 1 IMF programs increase the level of income inequality within countries.

It is also important to mention that the main goal of some IMF loans is "to include poverty reduction and to grant governments larger scope in negotiating the policy conditions". These programs are: PRGF [57] and ECF (IMF, 2001). Hence, the involvement of civil society to the design of PRGF and ECF programs could affect the potential distributional effects of these programs, e.g., governments may have the opportunity to choose reforms that are less harmfull for the poor, and thereby these policies can reshape the gap between the rich and the poor. In response to this, the theoretical considerations suggest the following hypothesis:

Hypothesis 2 The effect of PRGF and ECF programs on income inequality is lower.

[57] The PRGF was designed with sharply focus on poverty reduction and greater degrees of participation by civil society and national ownership, which in turn would lead to more consistent policy implementation. The PRGF was relabeled as the ECF in 2009.

The following section presents the empirical strategy to test these hypotheses.

1.3.3 Data and empirical identification

1.3.3.1 Data

For the dependent variable, income inequality, we use the inequality index (Gini coefficient) from the University of Texas Inequality Project [the Estimated Household Income Inequality Data Set (EHII)], which combines data from Deininger and Squire (1996) and the United Nations Industrial Development Organization data (UNIDO). [58]

The explanatory variable of interest, IMF program participation, is a dummy variable that equals to 1 if country i was under an IMF program for at least five months in year t (defined based on Dreher (2006)). [59]

1.3.3.2 Empirical strategy

An ordinary fixed effects model will be helpful in uncovering a relationship between income inequality and IMF program participation, however it fails to consider the fact that the sample includes only those cases in which an IMF program were imposed. The performance outcomes of participants' countries may systematically differ from non-participants, a country's participation in IMF loan programs may causes the issue of selection bias. Hence, the estimation using OLS fixed effects could be biased. [60]

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[58]The data set has been revised and is available at: http://utip.lbj.utexas.edu/data.html.

[59]The data for IMF program participation was constructed based on IMF Lending Arrangements, available at: https://www.imf.org/external/np/fin/tad/extarr1.aspx.

[60] In Table A1.3.3 and A1.3.4 of the Appendix we report results using OLS estimation.

To overcome this issue, we use the semi-parametric method of Acemoglu et al. (2018) to model the counterfactual scenario, i.e., cases in which IMF programs are not imposed to change the path of income inequality.

The first estimation follows Jorda (2005) and Kline (2011) by splitting the observations / country-year pair into two groups: one with IMF program participations and one without IMF. It assumes that the path of Gini coefficient for both groups can be modelled by their lagged values and time effects. Hence, the effect of the IMF participation can be computed by the difference between the OLS-predicted dependent variable for the two groups (with and without IMF programs). We model the change in Gini coefficients at each year $t = -5, -4, \ldots, 0, 1, \ldots, 19$ from the IMF participation to estimate the counterfactual at $t = 0$ using the regression adjustment.

The second approach follows Angrist et al. (2018) and estimates the effect of IMF participation on income inequality conditioning on the propensity score for transitions to IMF via a probit regression of the probability of transitioning to IMF at *t*, conditional on not having an IMF program at *t-1*, on Gini coefficient lags and time fixed effects. In this case, effect of treatment is a weighted average of changes across observations. The propensity score determines the weights given the different observations, according to their pre-IMF dynamics, providing lower weight to observations that are expected to receive IMF lending, whereas country-year pairs that not experiencing adverse Gini coefficient dynamics prior to *t=0* receive a greater weight. Similar to the regression adjustment, the inverse probability weighting estimation is performed on the change in Gini coefficients for each year from its value at *t=0*.

The third estimation combines the above two estimators (regression adjustment and inverse probability weighting) into a doubly robust estimator which simultaneously reweights observations in the control group by their propensity score and adjusts the counterfactual outcome using a linear regression model. Consistency of the estimator is ensured if either the linear model for potential outcomes or the probit model for the IMF participation is valid (Imbens and Wooldridge, 2009).

1.3.4 Empirical results

In Table 1.3.2 we present our empirical results for the treatment variable *IMF* using Texas inequality index (Gini coefficient). The different panels report the estimates of the regression adjustment, the inverse probability weighting, and the doubly robust methods. Each column summarizes these estimates by reporting the average effect over different time horizons, for $t = -5$ up to $t = -1$ and, for the next 20 years, in 5-year intervals.

Panel A of Table 1.3.2 summarizes estimates using the regression adjustment method. The lack of significant effects before the transition to IMF participation ($t = -5, \ldots, -1$) is reassuring (Acemoglu et al., 2018). They show as well that between 10 to 14 years after IMF participation, Gini coefficient increased by about 1.7% (significant at the 1% level), where in the previous 5-year interval (5 to 9 years) it is increased by about 0.86% (significant at the 5% level). As we can observe, for countries that remain more than 10 years in an IMF program, the increase in income inequality is much higher. Panel B and C of Table 1.3.2 summarize estimates using inverse probability weighting and the doubly robust estimates, respectively; both confirm the results from Panel A. The longer the participation of IMF, the higher the income inequality.

Table 1.3.2 Semi-parametric estimates of the effect of IMF participation on Gini coefficient (Texas inequality index)

	-5 to -1 years (1)	0 to 4 years (2)	5 to 9 years (3)	10 to 14 years (4)	15 to 19 years (5)
			Panel A		
Regression adjustment					
ATET on Gini coefficient	-0.019	0.673***	0.860**	1.729***	1.873***
	(0.027)	(0.206)	(0.369)	(0.499)	(0.619)
			Panel B		
Inverse probability weighting					
ATET on Gini coefficient	-0.016	0.677***	0.819**	1.565***	1.590***

	(0.039)	(0.208)	(0.350)	(0.529)	(0.617)
			Panel C		
Doubly robust					
ATET on Gini coefficient	-0.009	0.678***	0.765**	1.481***	1.587***
	(0.028)	(0.210)	(0.352)	(0.495)	(0.614)

Notes: This table reports average treatment effects on the treated (ATET) using the regression adjustment, inverse probability weighting and the doubly robust estimation procedure, which combines the two previous methods, to compute the counterfactual. Standard errors obtained using 100 bootstraps are reported in parentheses.
Significance level is denoted by *** (1%) and ** (5%).

To obtain a clearer image about the effect of IMF participation over time (for $t = -15, -14, \ldots, 20$, with $t = 0$ corresponding the beginning year of IMF participation), we visualize the above three estimation methods in Figures 1.3.1, 1.3.2 and 1.3.3, respectively. For each figure, the solid line plots the estimated effects of IMF participation on Gini coefficients over time, and the dotted lines plots its 95% confidence interval. As we mentioned, there is a sharply increase in Gini coefficient after 10 years of IMF participation, which is clearly showed in each figure.

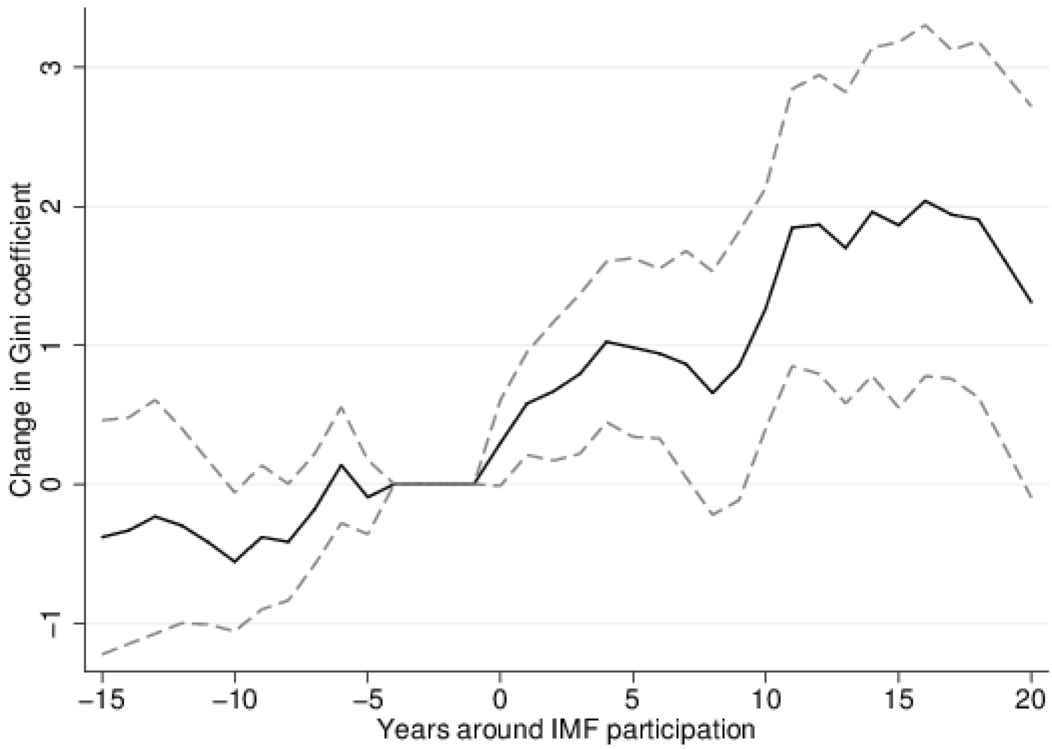

Figure 1.3.1 ATET of IMF at $t = 0$ on Gini coefficient. Estimates obtained using regression adjustment.

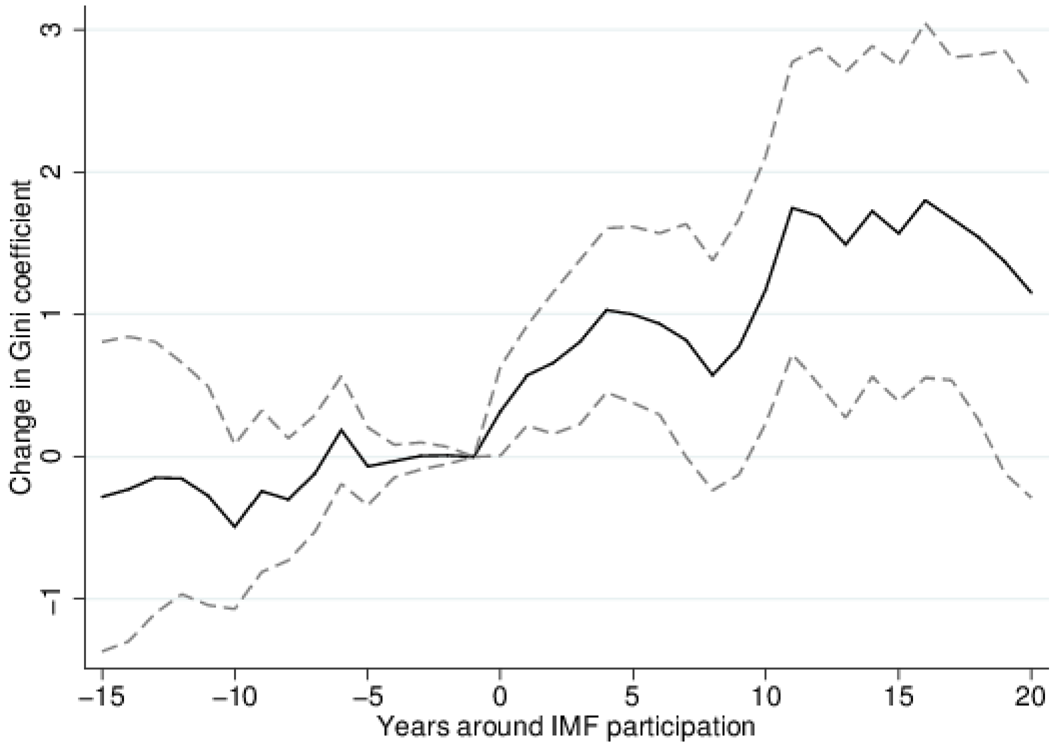

Figure 1.3.2 ATET of IMF at $t = 0$ on Gini coefficient. Estimates obtained using inverse-propensity score reweighting.

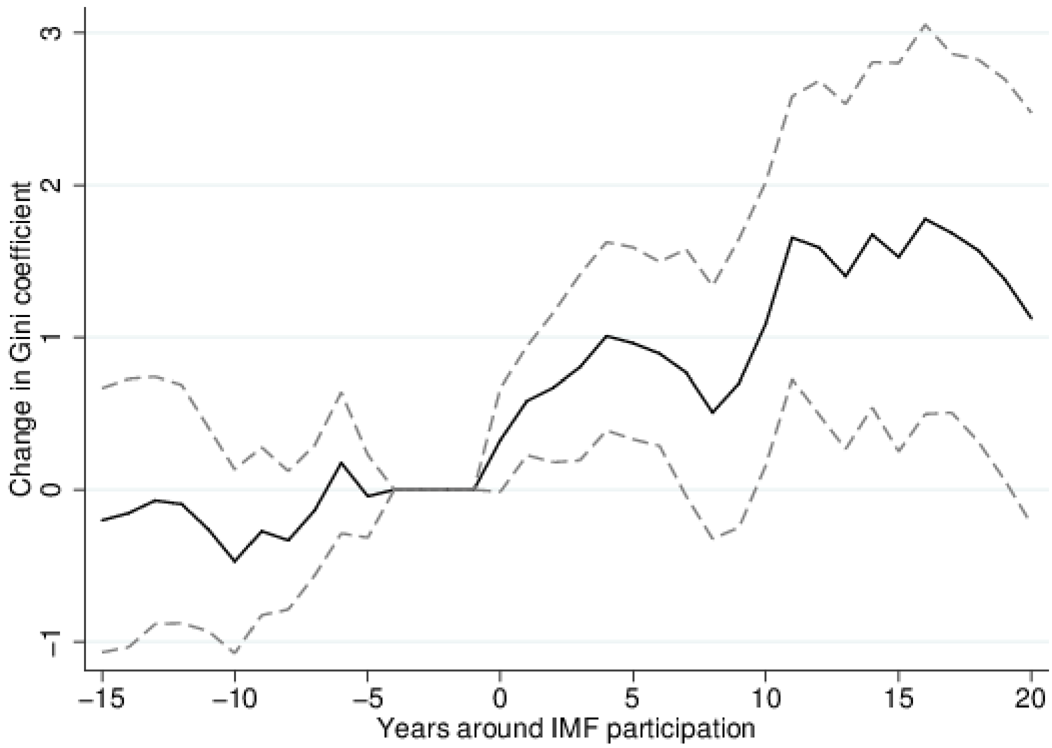

Figure 1.3.3 ATET of IMF at $t = 0$ on Gini coefficient. Doubly robust estimates.

In Table 1.3.3 we test our second hypothesis by separately examine the effect of PRGF and ECF programs. The structure of Table 1.3.3 follows this of Table 1.3.2, where now the treatment variable *IMF* takes into account only PRGF and ECF programs. The results obtained from the three different estimations show no significant results, [61] indicating that the effect of PRGF and ECF programs show no significant evidence on income inequality. As we already mentioned, the structure of PRGF and ECF programs with emphasis on public participation, national ownership and strong poverty reduction can explain why this type of concessional programs have no significant adverse distributional effects (Lang, 2020). Hence, the IMF could revise the policy agenda of lending facilities and include more programs that are designed on the basis of PRGF/ECF programs, which in turn can confine the adverse effect on income inequality.

Table 1.3.3 Semi-parametric estimates of the effect of IMF participation (only PRGF and ECF) on Gini coefficient (Texas inequality index)

	-5 to -1 years (1)	0 to 4 years (2)	5 to 9 years (3)	10 to 14 years (4)	15 to 19 years (5)
			Panel A		
Regression adjustment					
ATET on Gini coefficient	0.040	0.115	0.184	1.753**	0.798
	(0.114)	(0.659)	(1.328)	(0.761)	(1.324)
			Panel B		
Inverse probability weighting					
ATET on Gini coefficient	-0.003	-0.047	0.032	1.111	0.151
	(0.142)	(0.527)	(1.062)	(0.728)	(1.409)
			Panel C		
Doubly robust					
ATET on Gini coefficient	0.022	-0.095	-0.468	0.383	0.582
	(0.112)	(0.565)	(0.885)	(1.268)	(1.353)

Notes: This table reports average treatment effects on the treated (ATET) using the regression adjustment, inverse probability weighting and the doubly robust estimation procedure, which combines the two previous methods, to compute the counterfactual. IMF participation only considers PRGF and ECF (concessional) programs. Standard errors obtained using 100 bootstraps are reported in parentheses. Significance level is denoted by ** (5%).

[61] In Panel A, using regression adjustment, between 10 to 14 years after IMF participation we receive a positive significant sign at the 5% level. However, the results from inverse probability weighting and doubly robust estimates show no significant effects.

1.3.5 Conclusion

In this subchapter we examine the effect of IMF, an international organization, on distributional effects within countries. We find that the participation of the IMF on average leads to redistribution of income from the poor to the rich in the participating countries. Additionally, we do find that some targeted IMF programs, specifically PRGF and ECF, have no significant effects on income inequality.

From a policy perspective, we argue that the IMF could revise its policy advise and conditionality with regards to their distributional implications. Specifically, our results indicate that programs designed in combination with strong poverty reduction, public participation and increased national ownership do not exert adverse effects on income inequality. A new policy agenda on the basis of PRGF and ECF programs design is highly recommended.

Appendix 1.3

Table A1.3.1 List of countries

Afghanistan	Costa Rica	Iran	Myanmar	Spain
Albania	Croatia	Iraq	Nepal	Sri Lanka
Algeria	Cuba	Ireland	Netherlands	Sudan
Angola	Cyprus	Israel	New Zealand	Suriname
Argentina	Czech Republic	Italy	Nicaragua	Swaziland
Armenia	Denmark	Ivory Coast	Nigeria	Sweden
Australia	Dominican Republic	Jamaica	Norway	Switzerland
Austria	Ecuador	Japan	Oman	Syrian Arab Republic
Azerbaijan	Egypt	Jordan	Pakistan	Taiwan
Bahamas	El Salvador	Kazakhstan	Panama	Thailand
Bangladesh	Eritrea	Kenya	Papua New Guinea	Togo
Barbados	Estonia	Kuwait	Paraguay	Tonga
Belgium	Ethiopia	Kyrgyzstan	Peru	Trinidad and Tobago
Belize	Fiji	Latvia	Philippines	Tunisia
Benin	Finland	Lesotho	Poland	Turkey
Bolivia	France	Libya	Portugal	Uganda
Bosnia and Herzegovina	Gabon	Lithuania	Qatar	Ukraine
Botswana	Gambia	Luxembourg	Republic of Korea	United Arab Emirates
Brazil	Georgia	Macao	Republic of Moldova	United Kingdom
Bulgaria	Germany	Macedonia	Romania	United Republic of Tanzania
Burkina Faso	Ghana	Madagascar	Russian Federation	United States of America
Burundi	Greece	Malawi	Rwanda	Uruguay
Cambodia	Guatemala	Malaysia	Saudi Arabia	Venezuela
Cameroon	Haiti	Malta	Senegal	Vietnam
Canada	Honduras	Mauritius	Seychelles	Yemen
Central African Republic	Hong Kong	Mexico	Singapore	Zambia
Chile	Hungary	Mongolia	Slovakia	Zimbabwe
China	Iceland	Montenegro	Slovenia	
Colombia	India	Morocco	Somalia	
Congo	Indonesia	Mozambique	South Africa	

Countries without IMF program participation:

Australia, Austria, Bahamas, Belgium, Botswana, Canada, Cuba, Denmark, Eritrea, Germany, Hong Kong, Iran, Kuwait, Libya, Luxembourg, Macao, Malaysia, Malta, Montenegro, Netherlands, Norway, Oman, Qatar, Saudi Arabia, Singapore, Slovenia, Suriname, Swaziland, Sweden, Switzerland, Taiwan, Tonga, United Arab Emirates.

Table A1.3.2 Summary statistics

Variable	Mean	SD	Min	Max
Gini coefficient (Texas inequality index)	42.667	7.231	20.578	62.85
IMF (all) programs	0.273	0.446	0	1
PRGF and ECF programs	0.059	0.235	0	1

Table A1.3.3 Fixed effects model, dependent variable: Gini coefficient (Texas inequality index)

	All IMF programs			*PRGF and ECF*		
	(1)	(2)	(3)	(4)	(5)	(6)
IMF	0.241***	0.219***	0.222***	-0.100	-0.125	-0.121
	(0.069)	(0.072)	(0.079)	(0.177)	(0.159)	(0.199)
Gini coefficient first lag	0.853***	0.756***	0.741***	0.857***	0.758***	0.743***
	(0.022)	(0.033)	(0.039)	(0.022)	(0.033)	(0.040)
Gini coefficient second lag		0.121***	0.084**		0.123***	0.084**
		(0.030)	(0.041)		(0.030)	(0.041)
Gini coefficient third lag			0.010			0.010
			(0.036)			(0.036)
Gini coefficient fourth lag			0.042			0.042
			(0.035)			(0.035)
Country and Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,057	3,756	3,239	4,057	3,756	3,239
Adjusted R-squared	0.800	0.818	0.812	0.799	0.817	0.811

This table presents results using fixed effects estimation. Estimated equation: $y_{it} = \beta D_{it} + \sum_{j=1}^{p} \gamma_j y_{it-j} + \kappa_i + \lambda_t + \varepsilon_{it}$, where y_{it} is Gini coefficient in country i at time t, and D_{it} is our dichotomous measure of IMF participation in country i at time t. The specification includes p lags (where $p = 1, 2, 4$) of the dependent variable on the right hand side, κ_i and λ_t represent country and year fixed effects, respectively, and ε_{it} is *i.i.d.* error term. Columns 1-3 present results for all IMF programs. In columns 4-6 IMF participation only considers PRGF and ECF programs. In all specifications we control for a full set of country and year fixed effects. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses. Significance level is denoted by *** (1%) and ** (5%).

Chapter II: Temporary work and employment stability: Evidence from the Dutch labour market

2.1 Introduction

Temporary work is a significant and growing component of total employment in many developed countries (Vosko, 2008). A job may be considered as temporary employment if it is understood by both the employer and employee that the termination of the job is determined by objective conditions such as reaching a certain date, completion of an assignment or return of another employee who has been temporarily replaced (Hipp et al., 2015; Hoekstra et al., 2016; OECD, 2002). Hence, temporary employment can also be described as 'dependent employment of limited duration', in the sense that it is subject to the hiring organization with respect to promises of long-term employment (De Jong et al., 2007). In general, employees tend to have a strong preference for permanent employment (Silla et al., 2005). In many cases, temporary arrangements can be used to escape from unemployment or to return to work after a period of economic and labour market inactivity (Heinrich et al., 2005).

In the literature there are two opposing scenarios - theories for the ongoing employment effects of temporary work. Specifically, the debate in this research refers to whether temporary employment functions as a port of entry into stable employment positions-employability or as a dead end, a job that leads to multiple unemployment spells. On the one hand, the stepping stone function of temporary work is based on human capital theory (Mincer, 1974), which suggests that it is preferable for individuals to work on a non-standard form of employment (including temporary work) (Hipp et al., 2015) as they can develop skills, experience and access to social networks which subsequently improve their career prospects, instead of remaining unemployed (De Graaf-Zijl et al., 2009). Additionally, signalling theory, which incorporates imperfect information into human capital theory (Becker, 1993; Spence, 1973), supports the view that temporary work can function as a stepping stone, as employers use flexible forms of employment during the probation period to draw imperfect information on the productivity of new hire employees (Weiss, 1995). If an employee meets the requirements and expectations of the employer, the latter offers a permanent contract of job (Booth et al., 2002; Faccini, 2014; McGinnity et al., 2005; Reichelt, 2015). Therefore, according to signalling theory, the pathway through which temporary work can function as a portal to ongoing employment is through the signalling of high motivation. According to Browning et al. (2007), temporary jobs may be of help in obtaining more stable and better positions through the channel of the accumulation of financial assets to sponsor a longer and better job search in the eventual subsequent unemployment spell. On the other hand, according to dual labour market theory (Doeringer and Piore, 1971) temporary work does not allow for the prospect of ongoing employment. It is used as an instrument for employers to adjust their workforce in a way that depends on the demand (Kalleberg et al., 2000; Kalleberg, 2003). At the same time, because of the short-term nature of the work, temporary employees are less likely to be committed to the organization and the knowledge they obtained is lost when they find employment elsewhere (Aldrich and Ruef, 2006), thus employers have fewer incentives to invest in workers' human capital (Mattijssen and Pavlopoulos, 2019). A large participation in temporary jobs can function as a bad signal indicating of lower productivity for future employers, making them less likely to offer the worker a permanent contract (Berton et al., 2011; Esteban-Pretel et al., 2011; Hopp et al., 2015; Hudson, 2007).

The empirical literature regarding the effects of temporary work on the employability of individuals is bidirectional. Some studies argue that temporary work functions as a stepping stone to regular jobs (e.g., Booth et al., 2002; Picchio, 2008; Steijn et al., 2006), while others connect temporary employment to a dead end (e.g., De Graaf-Zijl et al., 2009; D'Addio and Rosholm, 2005; Giesecke and Groß, 2003).[62]

Our study contributes to the debate regarding the function of temporary contracts. It is still controversial in the literature whether temporary employment is linked to 'dead-end' jobs, or a port of entry into stable employment positions and career prospects. On this point of view, we also test whether participation in a series of training programs, through which workers acquire new skills that are important for their work or profession, can have a direct effect on the employment perspective of temporary employees. As such, our main contribution to the literature is on analysing the effect of temporary work conditional on the number of training programs. We focus on the Netherlands because of (1) its relatively high shares of temporary employment among the EU countries (Eurostat, 2018a), (2) its unique labour market institutions (De Lange, 2013; De Jong et al., 2007; Fagan and Ward, 2003) and (3) country's political debate for the use of temporary work and its consequences to the Dutch labour market (CBS and TNO, 2015; Delsen, 2016).

To empirically examine our research questions, we use annual survey data from the Longitudinal Internet Studies for the Social Sciences (LISS) for the years 2008 through 2018. The main advantages of LISS panel data are that it tracks the same individuals over time and has unique information at the individual level about the main variables we want to test. As the surveys are implemented on an annual basis, we are able to define our key dependent variable, employment stability, over time.

Using several methods of estimation, we find that the effect of temporary work on employment stability is negative and statistically significant. The impact of temporary work is

[62] See Filomena and Picchio (2021) for a meta-analysis on the debate about the "stepping stone vs. dead end" hypothesis related to the causal effect of temporary jobs on future labour market performances.

stronger for unmarried temporary employees compared to married, and for foreign compared to Dutch temporary workers. However, this negative effect can be mitigated for temporary employees who have developed their professional skills through training programs. Our initial results are found to be robust across alternative empirical specifications, including a novel instrumental variables strategy proposed by Lewbel (2012) that generates internal instrumental variables in the presence of heteroscedasticity.

The rest of the chapter proceeds as follows. Section 2.2 gives a short outlook of the institutional setting of temporary work in the Netherlands. In Section 2.3, we present the empirical methodology and the data used in our analyses. The empirical results are presented in Section 2.4. Finally, Section 2.5 offers our conclusions.

2.2 Institutional background

The Netherlands has undertaken a continuous reform process creating a more flexible labour market. An important and unique characteristic of the Dutch labour market, regarding temporary employment, is the approach of 'flexicurity' (Bekker and Mailand, 2018; Houwing, 2010; Van Oorschot, 2004). It is a combination of labour market flexibility and worker security, according to which, temporary employees have legal rights that approximate those of regular employees. Specifically, since the law of equal treatment of temporary and permanent employees, which is effective since 2002, the conditions of the collective agreements (e.g., pay, holiday- and pension rights), social security and labour laws and rights should have been the same[63] for temporary and permanent employees (De Lange, 2013). The Dutch government

[63] Deviation of equivalent treatment is conceivable under certain objective criteria. For the full text, see: College voor de Rechten van de Mens (2016): Wet onderscheid bepaalde en onbepaalde tijd (WOBOT).

considers the implementation of laws and regulations to prevent the unequal treatment of temporary employees as desirable, proper, and appropriate (Van De Arbeid, 2005).

The main difference between temporary and permanent contracts is their level of Employment Protection Legislation (EPL); the total of procedures and costs involved in dismissing workers (OECD, 2016). Nevertheless, policy makers in many countries have loosened restrictions on hiring temporary workers but retained the protection for permanent employees (Cahuc and Postel-Vinay, 2002; Giesecke and Groß, 2003; DiPrete et al. 2006; Barbieri, 2009). The Netherlands can be seen as one of the least restricting countries with respect to temporary contracts (CPB, 2015). The Dutch labour market has a long history of temporary employment, a relatively large percentage of workforce is employed on temporary arrangements, and an extensive framework of (recently implemented) laws and regulations protects temporary employees. The development of temporary employment in the EU15 started in two waves, between the late 1960s and the late 1970s and between the late 1980 and the early 2000s (Arrowsmith, 2006; Vosko, 1997). Companies called for the relaxing rules with respect to labour contracts and more opportunities for flexible labour agreements (Looise et al., 1998). In the Netherlands started in the beginning of the 1970s (1965) (Delsen, 1995)[64] with a process that introduced licensing and registry systems for temporary employment (Dunnewijk, 2001; Jacobs, 1999, 2005; Zaal, 2005).

Collective bargaining and social dialogue play an important role in determining the working and labour conditions of temporary employees in the Netherlands, often have an effect

[64] Pre-war legislation from 1930, includes the Job Placement Act (Arbeidsbemiddelingswet), had prohibited for-profit employment services (Finn, 2016), but left several aspects unregulated allowing private employment agencies to operate. Later was replaced by the 1965 Temporary Act (Wet op de terbeschikking stelling van arbeidskracht) and a licensing system in 1972 (Van Liemt, 2013).

on legal frameworks and regulations (Voss et al., 2013). Recently, comprehensive rules and regulations are also in use providing a more easily qualify of a temporary employee to a permanent work.[65]

In the Netherlands, the share of temporary employment has considerably increased during the last 15 years: while in 2004 14.6% of the working population was employed in a temporary contract, this percentage increased to 21.5% in 2018 (Eurostat, 2018a). In contrast to other European countries, this increase has persisted even after the peak of the recent economic crisis (Eurostat, 2018b; Euwals et al., 2016). By 2013, the Netherlands ranked third among the EU-15 countries regarding the share of temporary contracts, after Spain and Portugal (Kosters and Smits, 2015).

A possible explanation for the large increase and share of temporary employment is high levels of legal protection against dismissal – in countries where legal protection against individual dismissal is high, organizations achieve labour flexibility by hiring employees on temporary contracts (Kosters and Smits, 2015). When the difference in legal protection of permanent and temporary employees is smaller, the share of temporary employees is often smaller (Waaijer et al., 2017).

Figure 2.1 outlines trends in temporary employees as percentage of the total number of employees for the EU15 countries. The increasing trend of temporary employment throughout the years is illustrated for France, Belgium, Ireland, Italy, Luxembourg and the Netherlands. As we already mentioned above, to date the Netherlands remains at the top 3 among the EU-

[65] See e.g., the introduction of the Work and Security Act in 2015 (Wet Werk en Zekerheid) (Buiskol et al., 2015). For the legal text, see: http://wetten.overheid.nl/BWBR0035254/geldigheidsdatum_03-10-2015; https://zoek.officielebekendmakingen.nl/stb-2014-216.html.

15 countries regarding the share of temporary contracts. Throughout the years, only Austria and the UK maintain the share of temporary employment on a low and stable level.

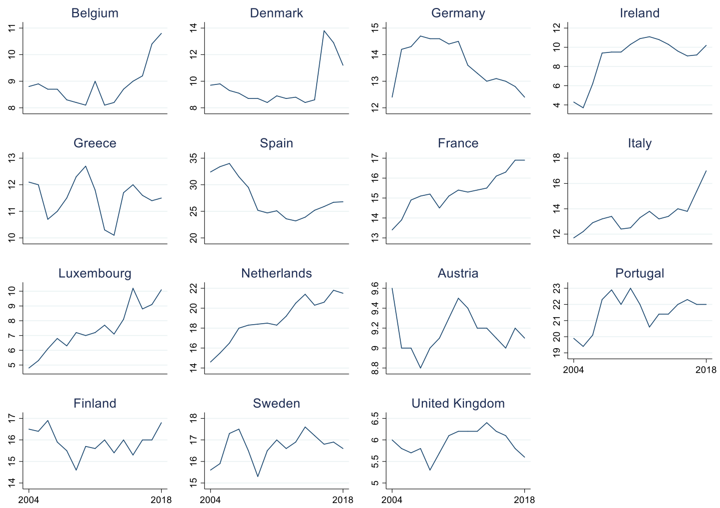

Figure 2.1 Temporary employment (as percentage of the total number of employees) in the EU15

As it is shown in Figure 2.2, gender distribution in temporary employment is quite balanced, about 50% in the Netherlands, while the situation in other countries of the EU15 shows prevalence of women like in Denmark, Finland, Sweden or the UK or men, like in Austria, Belgium, France or Germany (Eurostat, 2018a). Except Europe, in Canada studies have shown that women are more likely to hold temporary jobs than men (Cranford et al., 2003; Vosko, 2008).

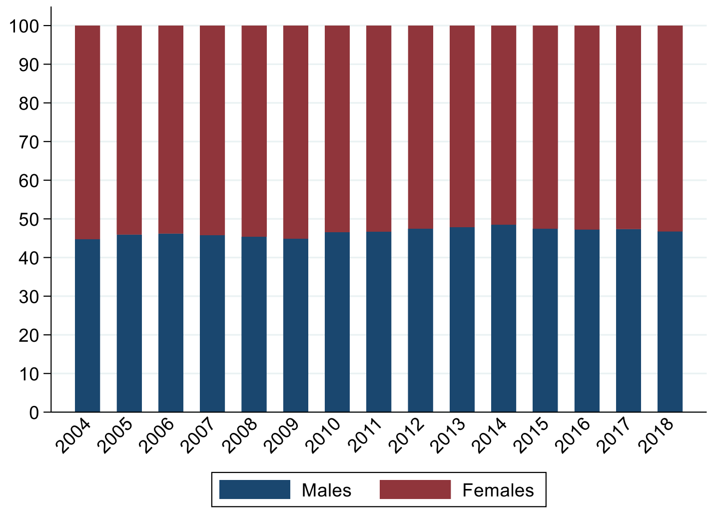

Figure 2.2 Temporary employment – gender distribution

Figure 2.3 represents age distribution of temporary employees (share of total employment) in the Netherlands. In particular, the majority of temporary employees are below 25 years old. For older workers, the proportion of temporary contracts for workers up to 49 years old ranges between 15% of total employment of this age group, while for temporary workers over 49 years old is less than 10% of total employment.

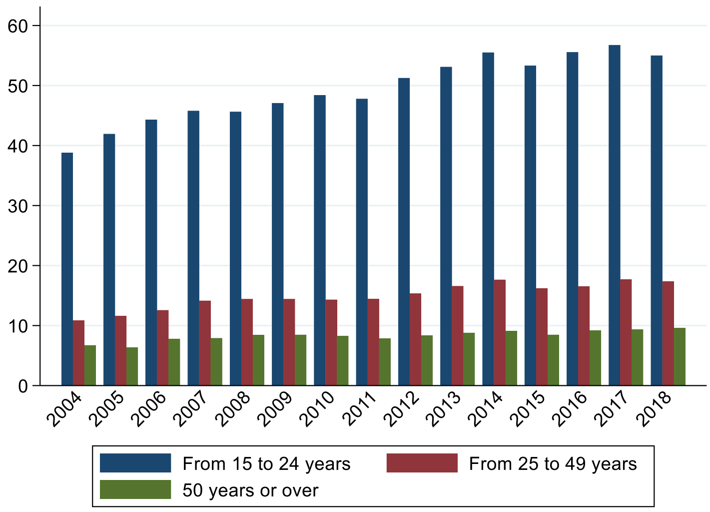

Figure 2.3 Temporary employees (share of total employment) – age distribution

In most of the EU15 countries, temporary employment is a disproportionate share of low-skilled workers (Eurostat, 2018c; Maurin and Postel-Vinay, 20005). As it is shown in Figure 2.4, which classifies temporary employees based on educational level in the Netherlands, the largest group of temporary employees is the one that has completed secondary education.

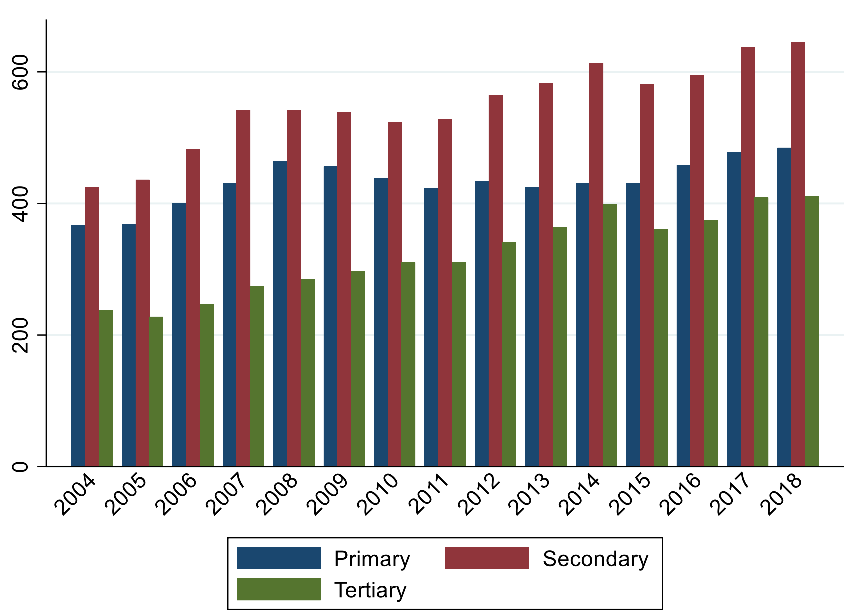

Figure 2.4 Temporary employees (1,000) based on educational level

Lastly, in Figure 2.5, we classify temporary employees based on the length of contracts. The majority of temporary employment in the Netherlands, throughout the years, works for a duration of 7 months to 1 year (7-12 months).

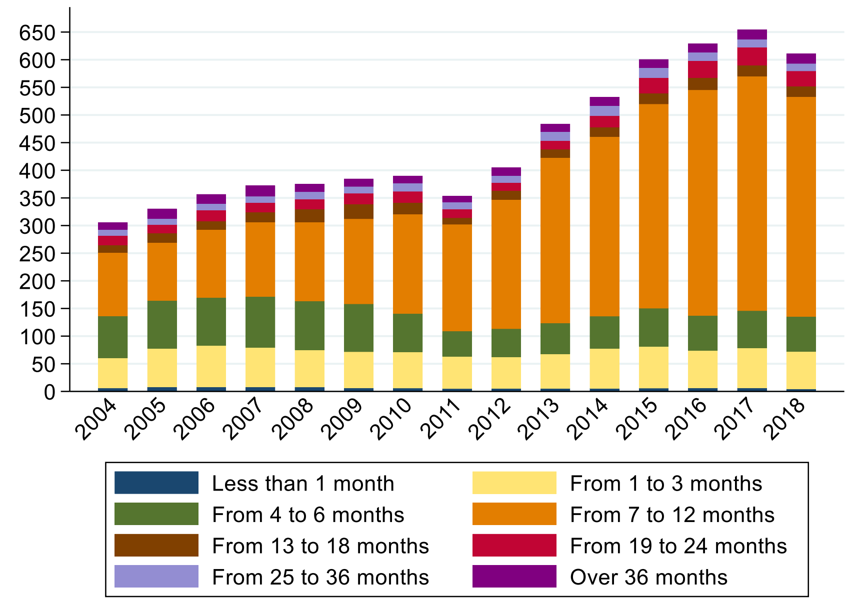

Figure 2.5 Temporary employees (1,000) based on the duration of contracts.

2.3 Data and empirical methodology

2.3.1 Empirical strategy

The empirical model we use to estimate the benchmark relationship between temporary work and employment stability takes the following form:

$$ES_{it} = \beta Controls_{it} + \gamma TW_{it} + \varepsilon_{it} \tag{2.1}$$

where ES represents employment stability the study's dependent variable, TW is the explanatory variable – temporary work. $Controls$ is a vector of control variables and ε is the error term. Subscript i indexes individuals, whereas t indexes time (years). The parameter of interest is γ, which measures the responsiveness of employment stability to temporary work. A detailed description of the variables and the structure of the panel are provided in the following subsection.

2.3.2 Data sample and covariates

To estimate Equation (1), we draw data from the LISS (Longitudinal Internet Studies for the Social Sciences) panel, which is the core element of the project entitled Measurement and Experimentation in the Social Sciences, undertaken by the CentER research institute at Tilburg University in the Netherlands. It is a survey panel dataset consisting of 5000 households and 7500 individuals in the Netherlands (both native and immigrants (individuals reporting to be born outside the Netherlands)), offering a true probability sample of the Dutch population (Scherpenzeel, 2009). Recruitment for the LISS panel occurred via mail, phone and face-to-face visits, special efforts were taken to maximize participation from sampled households, including a 10-euro payment to participants and provision of a free PC and internet access for those without. A survey is fielded in the panel every year, covering a large variety of domains including work, education, income, housing, time use, political views, values and personality.

We use all eleven available waves for the years 2008 through 2018, including a rich number of individual's general characteristics, such as age, education, family composition, income level and work status. We explicitly define each variable used in our empirical analysis in Table 2.1 and provide some further discussion in the text below. Table 2.2 reports basic summary statistics for these variables.

Our sample includes individuals i of working age, i.e., people aged between 16 and 64 years, excluding full-time students and self-employed individuals. The structure of the panel captures the same person i over the period 2008 to 2018 ($t = 1, \dots, 11$). As in most empirical studies, our panel is unbalanced, i.e., there are missing values for some years and individuals within the sample period. After omission of records with missing data, the final sample contains 32682 observations from 6447 individual people. On average, $t = 5.07$.

Table 2.1 Definition of variables

Variables	Definition
Employment stability (ES)	The fraction of years the individual i has been employed (with a paid work) since the start of the observation period. $$ES_{it} = \frac{\sum_t^T \mathrm{I}(Employed_{it} = 1)}{(T - t) + 1}$$
Temporary work (TW)	The fraction of years the individual i has been a temporary worker since the start of the observation period. $$TW_{it} = \frac{\sum_t^T \mathrm{I}(Temporary\_worker_{it} = 1)}{(T - t) + 1}$$
Age	The age (in years) of the respondent.
Education	Variable indicating the highest level of education with diploma of the respondents. Recoded into 7 dummies: 1=none, 2=elementary, 3=middle school, 4=secondary, 5=post-secondary, non-tertiary, 6=tertiary and 7=post-tertiary.
Children in household	Dummy variable that takes value one if the respondent has children and zero otherwise.
Income	Variable indicating the approximate taxable income of the respondents and takes a value equal to 1 if income is less than €2500, 2 if income is between €2500 and €5000, 3 if income is between €5000 and €10,000, 4 if income is between €10,000 and €15,000, 5 if income is between €15,000 and €20,000, 6 if income is between €20,000 and €30,000, 7 if income is between €30,000

	and €40,000, 8 if income is between €40,000 and €50,000, 9 if income is between €50,000 and €75,000, and 10 if income is €75,000 or more.
Unemployment benefit	Dummy variable that takes value one if the respondent receives unemployment benefit in year t and zero otherwise.
Training programs	Number of training-educational programs or courses the respondent followed over the past 12 months.

Table 2.2 Summary Statistics

Variables	Mean	St.Dev.	Min	Max
Employment stability	0.785	0.361	0	1
Temporary work	0.148	0.298	0	1
Age	42.275	12.152	16	64
Education	4.900	1.590	1	7
Children in household	0.610	0.488	0	1
Income	5.901	2.656	1	10
Unemployment benefit	0.196	0.397	0	1
Training programs	0.863	1.002	0	20

Before defining our key variables used for econometric analysis, we can observe the following alternative cases with regards to the employment status of respondents:

- The individual i can be either employed ($Employed_{it} = 1$) or unemployed ($Employed_{it} = 0$) in year t.
- The employed individual i can be either a temporary ($Temporary\_worker_{it} = 1$) or a permanent worker ($Temporary\_worker_{it} = 0$) in year t.[66]

The variable that measures employment stability (our study's dependent variable) reflects individuals' employment status throughout the years. It is defined as the fraction of years the individual i has been employed (with a paid work) since the start of the observation period, as follows:

$$ES_{it} = \frac{\sum_t^T \mathrm{I}(Employed_{it} = 1)}{(T - t) + 1} \tag{2.2}$$

[66] Based on the LISS panel questionnaires, we consider as temporary employees those individuals-respondents who in the corresponding question regarding their employment type answered "employee in temporary employment". Individual people in our sample can be unemployed, permanent or temporary employees in a given year t.

Alternatively, it measures the probability that the individual remains employed (with a paid work) throughout the examined period. The mean value of the variable is 0.785 with a standard deviation of 0.361.

Our key explanatory variable, temporary work, is also defined as the fraction of years the individual i has been a temporary worker since the start of the observation period, as follows:

$$TW_{it} = \frac{\sum_t^T \mathrm{I}(Temporary\_worker_{it} = 1)}{(T - t) + 1} \tag{2.3}$$

The mean value of the explanatory variable is 0.148 with a standard deviation of 0.298.

The covariates we use that may influence individual's employment stability are a set of socioeconomic and sociodemographic variables. Specifically, the control variables include age (and age squared), children living at home, educational level, total net income and unemployment benefit. Summary statistics for all the above variables are reported in Table 2.2 and an extensive definition for each of them is provided in Table 2.1.

2.4 Empirical results

2.4.1 Baseline results

In Table 2.3, we report our baseline results that assess the benchmark relationship between temporary work and employment stability. The models' goodness of fit is measured using the adjusted R-squared (with higher values indicating in general a better fit) and the F test of all coefficients. The latter rejects the null hypothesis that the dependent and independent variables are not related, if the F-statistic is of a significant value. As can been seen from the lower panel of Table 2.3, all alternative specifications are highly significant in this respect. We report coefficient effects and clustered robust standard errors at the individual level (same person over time) to deal with possible heteroscedasticity and serial correlation.

In column 1 of Table 2.3, we report results using a pooled OLS incorporating only the explanatory variable (TW) with clustered robust standard errors at the individual level. Results in column 2 are obtained using a pooled OLS, but we also include the set of control variables along with temporary work. In column 3, we incorporate the explanatory variable (TW), the set of control variables and year dummies, and estimate our model using a pooled OLS. In these three columns, we find a strong negative and statistically significant ($p<0.01$) effect of exposure to temporary work on employment stability. For the control variables (columns 2 and 3), we find that income and education are positive and statistically significant ($p<0.01$) associated with employment stability, while children in household and unemployment benefit are negative and statistically significant correlated with employment stability. Regarding the age of individuals, we find that (based on column 3) individuals' employment stability tends to increase until the age of 48 and decreases from this age onward.[67]

In the following three columns (columns 4 to 6), we replace the analysis of previous columns (columns 1 to 3) which uses a pooled OLS but now using fixed effects (FE) regressions. We observe that the estimated coefficient on exposure to temporary work remains negative and statistically significant ($p<0.01$) throughout. Results on the control variables maintain their direction of effect and we refrain from discussing these from now on. Turning to the interpretation of the effect of temporary work (column 6), an increase in exposure to temporary work by one standard deviation (0.298) results in a decrease in employment stability by 0.026 (=0.086*0.298) which corresponds to approximately 7% of its standard deviation.

Table 2.3 Employment stability and temporary work

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Employment stability (ES)					
	Pooled OLS			Fixed effects		

[67] We calculate this finding by taking the first derivative of the regression and solving with respect to age – for example, $\frac{-0.077}{2(-0.001)} = 48.445$.

Temporary work (TW)	-0.461***	-0.202***	-0.203***	-0.109***	-0.084***	-0.086***
	(0.016)	(0.014)	(0.014)	(0.018)	(0.017)	(0.016)
Age		0.078***	0.077***		0.021***	0.033***
		(0.002)	(0.002)		(0.003)	(0.004)
Age squared		-0.001***	-0.001***		-0.000***	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
Education		0.005***	0.005***		0.003***	0.003***
		(0.001)	(0.001)		(0.000)	(0.000)
Children in household		-0.030***	-0.034***		-0.009	-0.010*
		(0.007)	(0.007)		(0.006)	(0.006)
Income		0.008***	0.009***		0.004***	0.004***
		(0.001)	(0.001)		(0.001)	(0.001)
Unemployment benefit		-0.044***	-0.043***		-0.014***	-0.013***
		(0.005)	(0.005)		(0.002)	(0.002)
Year FE	No	No	Yes	No	No	Yes
Number of observations	32682	32682	32682	32682	32682	32682
Number of individuals					6447	
F-test	867.593***	1052.617***	488.302***	36.924***	33.181***	31.485***
Adjusted R-squared	0.145	0.445	0.455	0.013	0.057	0.071

Notes: Clustered robust standard errors at the individual level in parentheses. The overall significance of each model is assessed using adjusted R-squared and the F-test. Significance level is denoted by *** (1%), ** (5%) and * (10%).

To test the existing theoretical considerations and assess the long-run effects of temporary work on employment stability, we re-estimate our baseline model (Equation (2.1)) using FE regressions with year effects and include deeper lags in all predictors. We find that exposure to temporary work maintains its detrimental effect in the long-term (across all lags, temporary work ($\boldsymbol{TW}$) retains a negative and statistically significant sign ($p<0.01$)). Adding deeper lags in our models (columns 1 to 4), we obtain a smaller (and perhaps more plausible) detrimental long-run effect of temporary work ($\boldsymbol{TW}$), however it remains statistically significant throughout. The results of this exercise are reported in Table 2.4.

Table 2.4 Long-run effects: Employment stability and temporary work

	(1)	(2)	(3)	(4)
	Dependent variable: Employment stability (ES)			
	t-1	t-2	t-3	t-4

Temporary work (TW)	-0.079***	-0.070***	-0.049***	-0.043***
	(0.011)	(0.010)	(0.009)	(0.008)
Control variables	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Number of observations	24461	20324	16739	13643
Number of individuals	4563	3922	3369	2880
F-test	30.151***	33.655***	33.169***	37.709***
Adjusted R-squared	0.124	0.143	0.161	0.201

Notes: Reported results for the outcome equation (Equation (1)) using deeper lags. All predictors are included at the lag specified in the column header. All models are estimated using a FE regression with year effects. Clustered robust standard errors at the individual level in parentheses. The overall significance of each model is assessed using adjusted R-squared and the F-test. Significance level is denoted by *** (1%).

In the following table, we test whether our baseline results differ across subgroups at the individual-level. Specifically, in Table 2.5, we show the results when the last model (Table 2.3, column 6) is fully interacted with dummy variables indicating i) individual's gender (women and men) (column 1), ii) marital status (married and unmarried[68] individuals) (column 2) and iii) person's origin (i.e., foreign and Dutch individuals) (column 3).[69] We perform our analyses using FE regressions. In column 1, while the results show that the negative coefficient on exposure to temporary work is stronger for female compared to male individuals, the equity test (Chow test) for the difference in the coefficients between the two subgroups is statistically insignificant. Next, in column 2, we show that the negative effect of exposure to temporary work is stronger and significant for unmarried compared to married individuals. The equity test for the difference in the coefficients between unmarried and married workers shows that the coefficients of temporary work (TW) are significantly different (p<0.01). Lastly, in column 3, we find that the negative effect of exposure to temporary work is stronger for the subgroup of foreign compared to Dutch individuals. The difference in the coefficients of temporary work (TW) between foreign and Dutch individuals is also statistically significant (p<0.01).

[68] This dummy variable includes individuals who are not now (in year t) or previously married, or are divorced or widowed.

[69] We describe these subgroups in detail in Appendix Table A2.1.

Table 2.5 Results across subgroups

	(1)	(2)	(3)
		Dependent variable: Employment stability (ES)	
	Female vs. Male	Unmarried vs. Married	Foreign vs. Dutch
Temporary work (TW)*Female	-0.094***		
	(0.015)		
Temporary work (TW)*Male	-0.076***		
	(0.016)		
Temporary work (TW)*Unmarried		-0.129***	
		(0.018)	
Temporary work (TW)*Married		-0.048**	
		(0.021)	
Temporary work (TW)*Foreign			-0.095***
			(0.017)
Temporary work (TW)*Dutch			-0.051***
			(0.018)
Equity test	0.697	10.923	9.480
[P-value]	[0.404]	[0.001]	[0.002]
Age	0.030***	0.030***	0.031***
	(0.004)	(0.004)	(0.004)
Age squared	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)
Education	0.002***	0.002***	0.002***
	(0.000)	(0.000)	(0.000)
Children in household	-0.009	-0.009	-0.009
	(0.006)	(0.006)	(0.006)
Income	0.004***	0.004***	0.004***
	(0.001)	(0.001)	(0.001)
Unemployment benefit	-0.013***	-0.013***	-0.013***
	(0.002)	(0.002)	(0.002)
Year FE	Yes	Yes	Yes
Number of observations		32682	
Number of individuals		6447	
F-test	31.173***	31.294***	30.028***
Adjusted R-squared	0.077	0.077	0.077

Notes: All models are estimated using a FE regression with year effects. The equity test (Chow test) tests the hypothesis that the difference in the coefficient of the interactions is equal to zero. The p-value of the equity test is presented in brackets []. Clustered robust standard errors at the individual level in parentheses. The overall significance of each model is assessed using adjusted R-squared and the F-test. Significance level is denoted by *** (1%), ** (5%) and * (10%).

2.4.2 Introducing interaction effects – the role of training programs

It is well-documented in the literature that training programs can have a direct impact on the employment status of individuals (e.g., Aakvik, 2001; Bergemann et al., 2009; Gritz, 1993; Fitzenberger et al., 2010; Torp, 1994). Based on this evidence, in this subsection, we turn our analysis by examining how training programs with temporary work co-determine employment stability. The human capital theory (Becker, 1964) suggests that investments in personnel (such as training and education) should yield returns in the future. Autor (2001) argues that temporary workers may be benefited from training programs provided through their jobs and might be able to accumulate more human capital than job-seekers who stayed in open unemployment. Gagliarducci (2005) suggests that the introduction of public training programs could facilitate workers re-employability and alleviate the problem of short disruptions by providing effective support to individuals while searching for a new job. Previous studies highlight the positive external effects of training provided by staffing agencies (e.g., Acemoglu and Pischke, 1998; De Grip and Sauermann, 2012; Spermann, 2011, 2016). The support of skill development and the provision of basic qualification through targeted training programs are regarded crucial factors of improving the prospects of temporary employees' transitions into direct employment and supporting (upward) their professional mobility (Voss et al., 2013). If employers provide no specific training to these employees, their position on the labour market becomes structurally weakened, leading to a 'dead-end trap' – out of the labour market (De Jong et al., 2007).

Having this in mind, in this subsection we delve deeper in the nexus between temporary work and employment stability by considering temporary employee's participation in training programs. As we discussed above, we conjecture that training programs strongly influence the effect of temporary work on employment stability. For this purpose, we examine the effect of temporary work on employment stability conditional on the number of training programs the respondent has participated.

We use a variable from the LISS data to account for participation in training programs. The variable of training programs measures the number of training-educational programs or courses the respondent have followed over the past 12 months, which are important for respondent's work or profession. Definitions and summary statistics of the variable are also provided in Table 2.1 and 2.2, respectively. Using this measure of training programs, we interact the explanatory variable of temporary work (TW) with the measure of training programs and evaluate their interaction effect on employment stability. We would expect to see the effect of the interaction between temporary work and training programs be positive (or at least less negative).

Table 2.6 offers evidence on the relationship between employment stability, temporary work and training programs. Throughout the specifications, exposure to temporary work maintains its negative and statistically significant coefficient. Training programs have a positive and statistically significant sign ($p<0.01$) throughout. Regarding the interaction term (TW*Training programs), it exerts a positive and statistically significant effect ($p<0.01$) on employment stability in both specifications (columns 1 and 2), suggesting that the number of training programs increases the correlation between being exposure to temporary work and the likelihood of being employed. Taking together, this finding indicates that temporary work exerts a beneficial effect on employment stability only for temporary employees who have participated in training programs.

Table 2.6 Employment stability, temporary work and training programs

	(1) Pooled LPM	(2) Fixed Effects
Temporary work (TW)	-0.226***	-0.131***
	(0.010)	(0.011)
Training programs	0.014***	0.011***
	(0.002)	(0.001)
Temporary work (TW)*Training programs	0.075***	0.040***
	(0.009)	(0.007)
Age	0.076***	0.033***
	(0.002)	(0.004)
Age squared	-0.001***	-0.000***
	(0.000)	(0.000)
Education	0.005***	0.002***
	(0.001)	(0.000)
Children in household	-0.033***	-0.009
	(0.007)	(0.006)
Income	0.009***	0.004***

	(0.001)	(0.001)
Unemployment benefit	-0.043***	-0.013***
	(0.005)	(0.002)
Year FE	Yes	Yes
Number of observations	32682	32682
Number of individuals		6447
F-test	448.716***	29.775***
Adjusted R-squared	0.458	0.073

Notes: Clustered robust standard errors at the individual level in parentheses. The overall significance of each model is assessed using adjusted R-squared and the F-test. Significance level is denoted by *** (1%).

Figure 2.6 visualizes the marginal effect of temporary work on employment stability for different values of training programs with the associated 90% confident intervals (red dashed lines).[70] The magnitude of the marginal effect increases as the values of training programs are increasing. Evidently, the marginal effect of temporary work on employment stability becomes positive for values of training programs that are 3 and more.

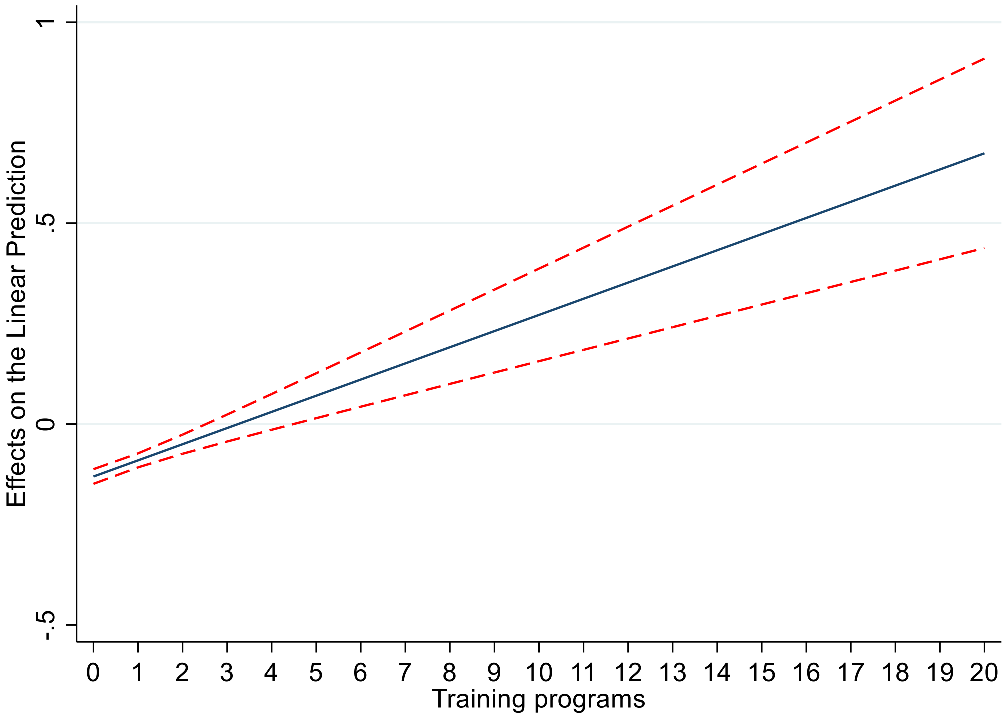

Figure 2.6 Marginal effect of temporary work on employment stability at different values of training programs

[70] We visualize the marginal effect using the FE model (Table 2.5, column 4).

2.4.3 *Accounting for endogeneity*

To support our previous strong evidence, we focus on additional tests that can address the problem of potential endogeneity. For instance, selection may be a concern in our estimated results, because workers may have systematic preferences affecting the selection into temporary work, and the unobserved factors of this preference may affect both self-sorting into temporary work and employment stability. In other words, self-selection into temporary work is endogenous and depends on both observed and unobserved factors. While we account for the former through the inclusion of control variables, failing to account for the latter will cause omitted variable bias.

To account for potential endogeneity, we employ Lewbel's (2012) two-stage least squares (2SLS) approach in our baseline model (Equation (2.1)), which utilizes a heteroskedastic covariance restriction to construct an internal instrument with the presence of heteroskedasticity as a precondition for identification. This approach is often used in the literature in the absence of external instruments (see, e.g., Baranowska-Rataj and Matysiak, 2016; Denny and Oppedisano, 2013; Mishra and Smyth, 2015). Following Lewbel (2012), we use Breusch-Pagan test of heteroskedasticity to check the presence of heteroskedasticity in our models as a precondition for identification. The results show that the null of homoskedastic errors is clearly rejected in each case with a P-value equal to 0.00. Diagnostic statistics, reported at the bottom of Table 2.7, indicate that the instrumentation strategy (2SLS approach with internal instruments) is relevant.[71] Although the results from this exercise (Table 2.7) need to be interpreted with caution, they seem to verify our previous findings. Specifically, accounting for endogeneity, throughout the specifications (columns 1 and 2), we find a negative and

[71] The appropriateness of internal instruments is confirmed with the under-identification test (UIT), the weak-identification test (WIT) and the over-identification test (OIT).

statistically significant coefficient (p<0.01) on temporary work (TW), confirming our baseline results on the relationship between temporary work and employment stability.

Table 2.7 IV estimates

	(1) Pooled IV	(2) FE IV
Temporary work (TW)	-0.140***	-0.093***
	(0.023)	(0.021)
Age	0.079***	0.032***
	(0.002)	(0.004)
Age squared	-0.001***	-0.000***
	(0.000)	(0.000)
Education	0.005***	0.002***
	(0.001)	(0.000)
Children in household	-0.034***	-0.009
	(0.007)	(0.006)
Income	0.009***	0.004***
	(0.001)	(0.001)
Unemployment benefit	-0.049***	-0.013***
	(0.005)	(0.002)
Year FE	Yes	Yes
Number of observations	32682	32682
Number of individuals		6447
F-test	1105.175***	36.911***
UIT	599.814***	325.776***
WIT	39.540***	240.154***
OIT	0.147	0.158

Notes: Clustered robust standard errors at the individual level in parentheses. The overall significance of each model is assessed using the F-test. Under-identification test (UIT) reports the Kleibergen-Paap rk LM statistic; Weak-identification test (WIT) reports the Kleibergen-Paap Wald rk F statistic (Staiger and Stock's (1997) rule of thumb suggests rejecting the null hypothesis of a weak instrument when F exceeds 10); Over-identification test (OIT) reports the p-value of Hansen J statistic. Significance level is denoted by *** (1%).

2.5 Conclusion

This chapter aimed to provide an evaluation of the use of temporary contracts on the employment perspective of individuals. Using longitudinal data on workers in the Netherlands exploited from the LISS panel, which is representative of the Dutch population and employing several models of estimation, controlling for personal, job and firm characteristics, we show that temporary work is significant negatively related to employment stability. A stronger effect of temporary work on employment stability is reported for unmarried and foreign individuals.

Furthermore, we investigate a potential channel through which temporary work can function as a stepping stone providing prospects for employment stability. As such, we show that the negative effect of temporary work on employment stability can be mitigated for temporary employees who have developed their professional skills through training programs.

Our findings have important implications for the Dutch labour market. They suggest that the employment prospects of temporary employees are poor unless they are highly skilled. Thus, the act of providing training opportunities to job-seekers, with the joint involvement of all actors (the Dutch government, employers and employees), can improve the development of skills of temporary workers and in the future provide employment prospects for better quality jobs.

Appendix 2

Table A2.1 Subgroups

Marital status	Married	Includes individuals i who in year t are married
	Unmarried	Includes individuals i who in year t belong to one of the following groups: i) single (not married) ii) divorced, iii) widowed
Gender	Female	Includes female (women) individuals
	Male	Includes male (men) individuals
Origin	Foreign	Includes individuals with foreign (non-Dutch) background
	Dutch	Includes individuals with Dutch background

Chapter III: Performance pay and job satisfaction: Does sleep quality and exhaustion affect worker's effort?

3.1 Introduction

The importance of differences in payment schemes[72] and how these differences are affecting employees, employers and firms is not a new topic, but is one that has recently attracted increasing attention from academic scholars and compensation specialists.

It is well-documented in the previous literature that performance-related pay can have multi-level effects on employees (e.g., wage inequality (Lemieux et al., 2009) and worker training (Gielen, 2007; Koffarnus et al., 2013)), the relationship between employers and employees (e.g., effects on the quality of relations of employees with the boss (Green and Heywood, 2010; Heywood et al., 2005)), and the firm as whole (e.g., effects on output and productivity (Dohmen and Falk, 2011; Lazear, 2000), profits (Bhargava, 1994), innovation (Harden et al., 2010) and employment growth (Gielen et al., 2009)).

In this chapter, we place the spotlight on the effect of performance pay schemes on individuals' job satisfaction. As such, we investigate the impact of performance pay schemes on job satisfaction which proxy not only the general economic behavior of individuals, but also helps to explain job mobility and the connection between job and demographic characteristics (Artz, 2008; Freeman, 1978; Hamermesh, 2001; 2004). In this study we center our attention on a sample of countries from the European Working Conditions Surveys 2010–2015 (EWCS), analyzing individual's job and socio-economic characteristics. In addition, we account for a variety of health-related issues of individuals since the literature suggests that sleep quality and

[72] According to McLaughlin (1986), to enhance the effectiveness of the Roman army, it is said that Julius Caesar had implemented a performance-based salary system in lieu of booty.

exhaustion from work (work-related exhaustion)[73] can exert a direct effect on overall job satisfaction (Brewer and Clippard, 2002; Lizano and Barak, 2015; Karagozoglu and Bingöl, 2008; Scott and Judge, 2006). As such, our main contribution to the literature is on analyzing the job satisfaction effects of performance pay conditional on the sleep quality and work-related exhaustion among those receiving performance pay.

The empirical evidence currently available in the literature regarding the effect of performance-related pay on job satisfaction is rather mixed. For instance, Green and Heywood (2008) find that performance-related pay is associated with increased job satisfaction, while Heywood and Wei (2006) document that for some specific performance pay schemes the effect on job satisfaction is negative. Other studies indicate that there is no significant effect on job satisfaction (Allen et al., 2017; Kruse et al., 2010).

Using data from the EWCS 2010–2015 for a maximum of 36 countries, overall, we find that performance-related pay is associated with increased job satisfaction even after controlling for earnings. This result appears to be robust for all disaggregated performance pay schemes: department, company and individual. We also show that this positive effect is stronger for those individuals who have better sleep quality and experience less work-related exhaustion. A series of robustness checks (including proper econometric techniques to account for selection bias) confirm these general patterns but show that results vary by gender, by sector (private and public) and by firm size.

Based on our results, we posit three pathways that link performance-related pay to job satisfaction. According to Brown and Sessions (2003) workers prefer employment environments that reward hard work, effort and productivity, and such environments increase

[73] Work-related exhaustion refers to the exhaustion-fatigue an individual experiences-feels after work.

worker optimism about future employment, thus increasing job satisfaction. A performance pay system can be seen as a rewarding pay system, thus employees who work under performance-related pay may feel more satisfied. Second, employees' job satisfaction will increase if they feel closer to management and more engaged in the decision-making process within the organization (Artz, 2008). Under a performance-related pay system, management can effectively reward workers for their productivity rather than overlook it, thus reducing the distance between management and workers, aligning the interests of both sides, while generating a job satisfaction enhancing link between effort and reward. Third, performance-related pay schemes can influence the job satisfaction of workers through higher earnings and by providing better working conditions (Godard, 2001). Bryson et al. (2016) argue that the extra payment of workers in the firm can be seen as a form of gift exchange. This may increase worker wellbeing through the 'doing good' or 'warm glow' from the employer created by this type of gift. One might argue that the effect on worker wellbeing may be related to the value of the gift, however according to Kosfeld and Neckermann (2011) even small value payments can increase wellbeing as they have been shown to influence worker performance.

We also explore two additional possible channels for which our data is well-suited. First, we examine the potential role of individuals' sleep quality by focusing on two separate factors which are related to sleep problems – insomnia symptoms (i.e., trouble falling and staying asleep), both of them with performance-related pay may co-determine the overall job satisfaction of individuals. Totterdell et al. (1994) suggest that sleep loss causes decreased motivation and thus more difficulty in maintaining performance levels. Additionally, poor sleep quality – insomnia has been linked to decreased task interest (Kjellberg, 1977), decreased social cooperation (Pasnau et al., 1968), and greater job difficulty and lower achievement (Kageyama et al., 1998). Studies also highlight that sleep problems are associated with greater fatigue and anger, as well as reduced feelings of well-being (Akerstedt, 1988; Lavidor, 2003;

Pilcher et al., 1997; Pilcher and Ott, 1998). Thus, individuals are possible to experience increased fatigue as a result of insomnia. Watson (2000) argues that fatigue is negatively related to job satisfaction. We expect that workers who have low quality of sleep will report being less satisfied with their job due to decreased motivation, greater fatigue and reduced feelings of well-being. It is also possible that individuals may attribute their poor sleep to their job (e.g., their work schedule or stressors experienced at work), thus reducing their job satisfaction. Second, we ask whether workers in performance pay schemes report low levels of job satisfaction when exposed to increased exhaustion from their job. Common feelings associated with physical and emotional exhaustion include frustration, powerlessness, inability to meet work goals (Valent, 2002), as well as being less engaged in work (Schaufeli et al., 2009). Also, studies have shown that physical and emotional exhaustion have linked to a series of serious negative outcomes, such as deterioration in the quality of service, higher job turnover and absenteeism, and low morale (Brotheridge and Lee, 2002; Grandey, 2003). Lizano and Barak (2015) state that exhaustion can lead to job behaviors including withdrawal, which diminishes the opportunities to have satisfying work experiences. In extreme cases when the exhaustion becomes too overwhelming for individuals, they detach from the work, become cynical and disconnected from clients and co-workers. In an analogous way with sleep quality, we assume that individuals who experience increased exhaustion from their job will have a lower level of job satisfaction due to the negative outcomes caused from the work-related exhaustion.

The rest of the chapter proceeds as follows. Section 3.2 reviews the related literature. In Section 3.3, we introduce the data and the empirical methodology. The empirical results are presented in Section 3.4. Finally, Section 3.5 provides some concluding remarks.

3.2 Related literature

3.2.1 Performance-related pay and job satisfaction

The effect of performance pay schemes on job satisfaction remains theoretically unresolved. In order to link the relationship between performance-related pay and job satisfaction, scholars have performed a series of empirical studies. Using survey data from 565 private sector employees in Australia, Drago et al. (1993) find that individual and group performance pay as well as ownership of company shares significantly increase job satisfaction. Heywood and Wei (2006) examine US data and find that, in general, while both profit sharing and individual performance pay tend to increase job satisfaction, a within measure of individual performance pay, piece rates, may reduce overall satisfaction.[74] Ledić (2018) uses Korean data and after controlling for the level of earnings, attitudes towards risk, and other personal and job-related characteristics, he shows that workers who are employed on either individual or group or company performance pay job schemes are more satisfied on their job than workers in the non-performance pay job schemes.

Results on the relationship between performance-related pay and job satisfaction from studies which use data from the British Household Panel Survey (BHPS) vary. Specifically, while Green and Heywood (2008) clear show that performance-related pay is a positive determinant of job satisfaction, McCausland et al. (2005) argue that the influence of

[74] As they point out, there are various explanations for the negative effect of piece rates. Drago and Garvey (1998) state that piece rates have been shown to undermine valuable teamwork and co-operation, increasing the competition among co-workers which directly leads to lower job satisfaction for these workers. Kennedy (1995) explains that performance-related pay schemes may erode the morale of the less productive workers, and consequently they would reduce their effort (productivity). Also, Gibbons (1987) formalized the traditional union fear of ‘ratcheting’, according to which while workers responded with additional efforts, rewards and incentives would be lower.

performance pay tends to increase satisfaction for the more highly paid but reduce it for the less highly paid, and Pouliakas and Theodossiou (2009) find an insignificant influence.

Bryson et al. (2017) use employer–employee data for Britain and posit that performance pay is linked to positive job attitudes (job satisfaction and organisational commitment) among private sector employees, while in the public sector performance pay has no such effect. As they point out, this result is expected because while in private sector performance pay schemes can be seen as financial incentives that are associated with greater job satisfaction, this act cannot translate the same for public sector employees who have traditionally relied upon a good total reward package, including pensions, to attract high-calibre candidates (Danzer and Dolton, 2012) and have used career incentives based on promotion opportunities, rather than performance-related pay, to motivate workers (Prendergast, 1999). Using a sample of West German private sector workers, Cornelissen et al. (2011) find that even after controlling for earnings, workers in performance pay jobs have higher job satisfaction. They also point out that workers in performance pay schemes who have greater risk tolerance routinely report greater job satisfaction. Bryson et al. (2016) show that group-based performance pay is associated with higher levels of job satisfaction and this effect is larger for bigger performance payments. They also argue that this positive effect is due to the channels of greater organisational loyalty and feelings of fair pay engendered by such payment methods, and that group performance pay schemes can mitigate the negative satisfaction effects of exposure to poor working conditions. These findings hold across three quite different data sets. In a sample of British workers, Artz (2008) highlights that the effect of performance pay schemes on job satisfaction varies with the size of the establishment.

A study by Kruse et al. (2010) examines the effect of a variety of share-capitalist type compensation schemes on worker outcomes, including job satisfaction, using two US-based data sets, the General Social Survey (GSS), which is a representative sample of employees at for-profit organisations, and an NBER survey of 14 companies which have at least one group-performance scheme. After controlling for job and demographic characteristics and company fixed effects, the results for both data sets show no relationship between share-capitalist schemes and job satisfaction.[75] A similar result (no significant effect) is also reported in Allen et al. (2010) who examine the effect of pay-for-performance schemes on job satisfaction of general practitioners in the UK.

3.2.2 Sleep quality and job satisfaction

Another strand of the literature highlights that job satisfaction is positively associated with individual's sleep quality. Doi et al. (2003) investigate the case of Japanese white-collar employees (telecommunications workers) and show that poor sleep quality is associated with dissatisfied with job. Léger et al. (2006) examine a sample of managers, white- and blue-collar workers in France and find that insomniacs are less satisfied with their jobs, compared to good sleepers. Lavie (1981) also reports lower job satisfaction and job productivity in insomniacs in a sample of industrial workers in Israel. In a cross-sectional study in Sweden, Rosmond et al. (1998) find that insomnia is related to high degrees of work stress and dissatisfaction with work. Scott and Judge (2006) indicate that poor sleep quality is associated with significant lower job satisfaction of insurance company workers in US. Studies which also examine the impact of insomnia/insomnia symptoms on job satisfaction for health care assistants, find a negative and significant association between poor sleep quality and job satisfaction (Brossoit et al., 2020; Karagozoglu and Bingöl, 2008).

[75] Additionally, this study provides a review of 12 studies which examine the relationship between performance pay and worker wellbeing, and concludes that the effect is bidirectional.

3.2.3 Exhaustion and job satisfaction

The literature also points out that job satisfaction depends on individuals' exhaustion from their job. In a sample of white- and blue-collar workers in Finland, Kauppinen-Toropainen (1983) investigates the relationship between job dissatisfaction and work-related exhaustion and show that work-related exhaustion is positively associated with job dissatisfaction as a result of the joint effects of lack of self-determination and time pressure. Skaalvik and Skaalvik (2011) examine the effect of exhaustion from work on job satisfaction using a sample of teachers in Norway. They find that exhaustion is negatively associated with job satisfaction. Moon and Hur (2011) use data from retail sales employees in South Korea to show that emotional exhaustion, depersonalization and diminished personal accomplishment have a negative effect on organizational commitment and job satisfaction. Other studies which examine healthcare professionals and assistants also indicate that work-related exhaustion negatively affects job satisfaction (Dolan, 1987; Ozyurt et al., 2006; Shanafelt et al., 2012; Tarcan et al., 2017).

3.3 Data and empirical methodology

3.3.1 Data

The empirical analysis makes use of data from the European Working Conditions Survey (EWCS).[76] We use data from the two most recent waves of the survey, referring to the years 2010 and 2015 respectively, in 36 countries (from European Union, candidate countries and other countries).[77] Face-to-face interviews were carried out between January and August 2010 and between February and September 2015. In most countries the targeted sample was 1,000 individuals, and the overall response rate for the surveys was nearly 44% in 2010 and 43% in

[76] See http://eurofound.europa.eu/european-working-conditions-surveys-ewcs for more on this survey.

[77] Table A3.1 of the Appendix lists all countries included in the study.

2015.[78] We include all workers aged 18-64 years. Individuals being self-employed are excluded from the sample. Respondents in the 2010 survey are not the same as those in 2015 wave, so that the dataset is not a panel but rather as a pooled cross-section. After omission of records with missing data, the final sample contains up to 47,969 workers.

The dependent variable, job satisfaction, is a categorical variable indicator ranging between 0 and 3. The participants in the EWCS were asked to rank their overall job satisfaction with the working conditions in their main job on the following four-point scale: 0 if not at all satisfied, 1 if not very satisfied, 2 if fairly satisfied, and 3 if very satisfied. Freeman (1978) argues that job satisfaction not only represents the general behavior of individuals, but also helps to explain job mobility[79] and the connection between job and demographic characteristics (Artz, 2008). According to Hamermesh (2001), job satisfaction can be characterized as the only measure which reflects "how (employees) react to the entire panoply of job characteristics" and additionally "it can be viewed as a single metric that allows the worker to compare the current job to other labour market opportunities".[80]

Identification of workers in the performance pay jobs comes from three questions:

1) *Earnings from main job include - Payments based on the performance of your team/department?* – where individuals indicate if they receive department performance-related pay (wage is decided based on team's or department's output) or not.

[78] A more detailed description of the methodology can be found in the technical reports (Eurofound, 2010, 2015).

[79] Workers with low levels of job satisfaction tend to move to different jobs until they feel more satisfied.

[80] Studies have also shown that job satisfaction scores predict future job quits (Clark, 2001; Green, 2010) and retirement (Clark et al., 2015).

2) *Earnings from main job include - Payments based on the performance of the company where you work?* – where individuals indicate if they receive performance-related pay at the company level (wage is decided based on the company's overall performance) or not.
3) *Earnings from main job include - Payments based on your individual performance?* – where individuals indicate if they receive performance-related pay at the individual level (wage is decided based on an individual's output) or not.[81]

All of the previously researched personal and job controls are included based on the availability of our dataset. The surveys allow us to control for demographic variables such as age, gender, education and the size of the household. We also control for various job characteristics such as usual hours worked per week, work at night (days per month the respondent usually works at night), work on Sundays (days per month the respondent usually works on Sundays), if workers have a fixed job contract, monotonous tasks (respondent's job involves monotonous tasks), learning new things (respondent's job involves learning new things) and firm size. We provide an explicit description of the variables in Table 3.1 and report summary statistics in Table 3.2.

Table 3.1 Description of variables

Variable	Description
Job satisfaction	Ordinal variable that equals 0 if respondent is not at all satisfied, 1 if he is not very satisfied, 2 if he is fairly satisfied, and 3 if he is very satisfied with the working conditions in the main job.
Performance pay (department)	Dummy variable that equals 1 if the respondent's earnings from main job include payments based on the performance of his team/department, 0 otherwise.
Performance pay (company)	Dummy variable that equals 1 if the respondent's earnings from main job include payments based on the performance of the company where he works, 0 otherwise.
Performance pay (individual)	Dummy variable that equals 1 if the respondent's earnings from main job include payments based his individual performance, 0 otherwise.

[81] The variables of department and company performance-related pay are available for both 2010 and 2015 surveys, while individual performance pay is available only for the 2015 wave.

Age	Age of respondent measured in years.
Household size	The number of people who live in respondent's household.
Gender	Dummy variable that equals 1 if the respondent is male, 0 otherwise (female).
Public sector	Dummy variable that equals to 1 if the respondent is employed on the public sector, 0 otherwise (private sector).
Tenure	Number of years a respondent has been employed in his/her present main job (job tenure).
Working hours	Number of hours the respondent usually works per week.
Work at night	Number of times (days) per month the respondent usually works at night (for at least 2 hours between 10.00 pm and 05.00 am).
Work on Sundays	Number of times (days) per month the respondent usually works on Sundays.
Fixed contract	Dummy variable that equals 1 if the respondent is employed on a fixed-term contract, 0 otherwise.
Monotonous tasks	Dummy variable that equals 1 if respondent's main job involves monotonous tasks, 0 otherwise.
Learning new things	Dummy variable that equals 1 if respondent's main job involves learning new things, 0 otherwise.
Educational level	Highest level of education or training that the respondent has successfully completed, seven ISCED codes: 0 = 'Pre-primary education', 1 = 'Primary education or first stage of basic education', 2 = 'Lower secondary or second stage of basic education', 3 = '(Upper) secondary education', 4= 'Post-secondary non-tertiary education', 5 = 'First stage of tertiary education' and 6 = 'Second stage of tertiary education'.
Firm size	Categorical variable that: equals 2 if the respondent is employed in an establishment with 2 to 4 workers, equals 3 if the respondent is employed in an establishment with 5 to 9 workers, equals 4 if the respondent is employed in an establishment with 10 to 49 workers, equals 5 if the respondent is employed in an establishment with 50 to 499 workers and equals 6 if the respondent is employed in an establishment with 500 workers or over.
Log(pay)	Logarithm of net monthly earnings of respondent's main paid job
Waking up repeatedly during the sleep	Sleep related problems – Waking up repeatedly during the sleep. Five categories: 1 = 'Daily', 2 = 'Several times a week, 3 = 'Several times a month', 4 = 'Less often' and 5 = 'Never'.
Difficulty falling asleep	Sleep related problems – difficulty falling asleep. Five categories: 1 = 'Daily', 2 = 'Several times a week, 3 = 'Several times a month', 4 = 'Less often' and 5 = 'Never'.
Exhausted after work	Respondent feels exhausted at the end of the working day. Five categories: 1 = 'Always', 2 = 'Most of the time', 3 = 'Sometimes', 4 = 'Rarely' and 5 = 'Never'.
Political/trade union activity	Respondent's involvement in political/trade union activity. Five categories: 1 = 'Daily', 2 = 'Several times a week', 3 = 'Several times a month', 4 = 'Less often' and 5 = 'Never'.
Influence decisions	Respondent's involvement in decision influence that are important for work. Five categories: 1 = 'Always', 2 =

	'Most of the time', 3 = 'Sometimes', 4 = 'Rarely' and 5 = 'Never'.
Involvement in processes	Respondent's involvement in improving the organisation or processes. Five categories: 1 = 'Always', 2 = 'Most of the time', 3 = 'Sometimes', 4 = 'Rarely' and 5 = 'Never'.

Table 3.2 Summary statistics

Variable	Mean	Std. Dev.	Min	Max
Job satisfaction	2.0619	0.7032	0	3
Performance pay (department)	0.1130	0.3167	0	1
Performance pay (company)	0.1512	0.3583	0	1
Performance pay (individual)	0.1819	0.3858	0	1
Age	42.0737	11.3568	18	64
Household size	2.8719	1.2921	1	6
Gender	0.4934	0.5000	0	1
Public sector	0.6498	0.4771	0	1
Tenure	10.8230	9.9686	1	46
Working hours	37.7930	10.2826	1	98
Work at night	1.4136	3.7312	0	31
Work on Sundays	0.6932	1.1551	0	5
Fixed contract	0.6696	0.4704	0	1
Monotonous tasks	0.4716	0.4992	0	1
Learning new things	0.7774	0.4160	0	1
Educational level	3.7709	1.2849	0	6
Firm size	4.3421	1.0849	2	6
Log(pay)	9.6217	4.2377	1.3863	18.4207
Waking up repeatedly during the sleep	3.7701	1.2869	1	5
Difficulty falling asleep	3.9986	1.1547	1	5
Exhausted after work	2.8407	1.0405	1	5
Political/trade union activity	4.8374	0.5614	1	5
Influence decisions	3.0335	1.3040	1	5
Involvement in processes	2.8550	1.4400	1	5

3.3.2 Empirical strategy

The general form of the empirical model to be estimated is:

$$JS_{it} = \alpha PP_{it} + \beta Controls_{it} + \varepsilon_{it} \tag{3.1}$$

where JS represents individual's job satisfaction and PP is a measure of performance related pay. $Controls$ is a vector of control variables and ε is the error term. Subscript i indexes workers-individuals, whereas t indexes time. The coefficient of interest is α, which measures the responsiveness of job satisfaction for workers receiving performance pay schemes.

Because of the ordinal nature of the dependent variable, an ordered probit regression is used[82] with job satisfaction as the dependent variable, participation on performance pay schemes is the variable of interest (explanatory variable) and numerous personal and job characteristics as control variables. These controls include the variables commonly used in previous job satisfaction literature. Also, all regressions include country, survey year, industry and occupational fixed effects.[83] Standard errors are calculated using the robust Sandwich estimator, which adjusts for heteroscedasticity.

3.4 Empirical results

3.4.1 Baseline results

In Table 3.3, we report our baseline results on the relationship between performance-related pay and job satisfaction, using an ordered probit estimation method. The models' goodness of fit is measured using McFadden's pseudo R-squared (with higher values indicating in general a better fit) and a chi-squared Wald test of all coefficients. As can been seen from the lower panel of Table 3.3, all alternative specifications are highly significant in this respect. We report

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[82] Following previous studies, the values of job satisfaction are fitted to the cumulative normal distribution through ordered probit estimates (see Clark and Oswald, 1996; Clark et al., 1997 among others). Such ordered probit estimation is designed for dependent variables with a natural ordering, such as least to most satisfied (see McKelvey and Zavonia, 1975).

[83] A list of all industries (Table A3.2) and occupations (Table A3.3) is reported at the Appendix.

marginal effects on the probability of reporting the highest level of satisfaction ('Very satisfied').[84]

Before we begin to analyze and interpret our results, it is important to mention that according to the standard microeconomic theory, after controlling for the level of earnings, the positive effect of performance-related pay on worker's job utility should disappear (Gazioglu and Tansel, 2006). To test if this hypothesis holds in our case, we examine the effect of performance pay on job satisfaction without (columns 1 to 3) and with earnings (columns 4 to 6).[85] Even after controlling for the level of earnings, the variable of department performance pay enters the specifications (columns 1 and 4) with a positive sign and it is statistically significant ($p<0.01$). This is not a small effect, as the mean level is around 11%. Thus, a one percentage point increase represents an increase of (0.0345/0.1130)th relative to the mean, or a finding that those who receive performance pay at the department level are 30% more likely to report being in the most satisfied category (column 4). Also, the effect of the other group-performance pay scheme (at the company level) is also highly significant ($p<0.01$). Turning to the interpretation of the effect of company performance pay (column 5), at its mean of around 15%, those who receive company performance-related pay are about 37% more likely to report being in the most satisfied category. Lastly, the effect of individual performance pay is also

[84] Coefficient effects are reported in Table A3.4 of the Appendix. In the main body of the chapter, we report the marginal effects (instead of coefficients), which allows us to interpret the magnitude of the effects (e.g., based on the reported coefficient on department performance pay in Table A3.4, we can only argue that workers receiving department performance-related pay are more likely to be in the higher categories of job satisfaction). To save space, we have not presented a full set of marginal effects, which includes four satisfaction categories for all three payment schemes variables, but they are available from the authors upon request.

[85] To control for the level of earnings, in columns 4 to 6 we include the logarithm of worker net monthly earnings (log(pay)). Description and summary statistics of the variable are reported in Table 3.1 and 3.2 respectively.

positive and highly significant (p<0.01) in both cases (columns 3 and 6). The interpretation in column 6 indicates that, at the mean of individual performance pay of around 18%, workers who receive individual performance-related pay are about 14% more likely to report being in the most satisfied category.

Table 3.3 Job satisfaction and performance pay

	(1)	(2)	(3)	(4)	(5)	(6)
			Marginal effects			
Performance pay (department)	0.0354***			0.0345***		
	(0.0039)			(0.0039)		
Performance pay (company)		0.0569***			0.0559***	
		(0.0047)			(0.0047)	
Performance pay (individual)			0.0474***			0.0454***
			(0.0076)			(0.0076)
Age	-0.0000	-0.0000	0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0000)	(0.0001)
Household size	-0.0000	-0.0001	0.0002	-0.0000	-0.0000	0.0003
	(0.0004)	(0.0004)	(0.0005)	(0.0004)	(0.0004)	(0.0005)
Gender	0.0069**	0.0071**	0.0081	0.0062**	0.0064**	0.0069
	(0.0029)	(0.0029)	(0.0057)	(0.0029)	(0.0029)	(0.0057)
Public sector	0.0000	-0.0000	0.0013	0.0000	0.0000	0.0012
	(0.0006)	(0.0006)	(0.0012)	(0.0006)	(0.0006)	(0.0012)
Tenure	0.0001	0.0001	0.0003	0.0000	0.0000	0.0002
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0002)
Working hours	0.0000	0.0000	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Work at night	-0.0011***	-0.0011***	-0.0007**	-0.0011***	-0.0011***	-0.0008***
	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0003)
Work on Sundays	-0.0003*	-0.0003*	-0.0001	-0.0003*	-0.0003*	-0.0001
	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0003)
Fixed contract	0.0091***	0.0090***	0.0160***	0.0093***	0.0092***	0.0164***
	(0.0025)	(0.0025)	(0.0055)	(0.0025)	(0.0025)	(0.0055)
Monotonous tasks	-0.0253***	-0.0254***	-0.0447***	-0.0251***	-0.0252***	-0.0439***
	(0.0018)	(0.0018)	(0.0046)	(0.0018)	(0.0018)	(0.0045)
Learning new things	0.0208***	0.0207***	0.0380***	0.0204***	0.0204***	0.0371***
	(0.0020)	(0.0020)	(0.0053)	(0.0020)	(0.0020)	(0.0054)
Educational level	0.0033***	0.0033***	0.0014*	0.0032***	0.0032***	0.0013*
	(0.0007)	(0.0007)	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Firm size	-0.0002*	-0.0002*	0.0001	-0.0002**	-0.0002**	0.0001
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0002)
Log(pay)				0.0029***	0.0029***	0.0047***
				(0.0003)	(0.0003)	(0.0007)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	-	Yes	Yes	-
Observations	47,969	47,969	15,089	47,969	47,969	15,089

Pseudo R-squared	0.048	0.048	0.036	0.049	0.049	0.038
Wald chi-squared model fit	4269.648***	4329.022***	1012.376***	4364.237***	4423.579***	1058.206***

Notes: Robust standard errors in parentheses. The overall significance of each model is assessed using McFadden's pseudo R-squared and the Wald-test. Significance level is denoted by *** (1%), ** (5%) and * (10%).

The results on the effect of control variables largely conform to established previous studies. Job satisfaction declines with work at night and monotonous tasks throughout the specifications. Fixed contract and more educated workers are more satisfied with their jobs, and learning new things at work also increases job satisfaction. The level of earnings exerts a significant beneficial effect on job satisfaction. In specifications where group-performance pay schemes are examined (columns 1, 2, 5 and 6), job satisfaction is reported to be lower for workers who work more on Sundays and in larger establishments, while males appear to have markedly higher job satisfaction than females.

3.4.2 Introducing interaction terms

In this subsection, we delve deeper in the nexus between performance-related pay and job satisfaction by considering individual's health related issues. Based on the literature reviewed in Section 3.2, we conjecture that worker's health related issues, specifically sleep quality and work-related exhaustion, strongly influences the effectiveness of performance pay schemes. For this purpose, we examine the effect of performance-related pay on job satisfaction conditional upon worker's sleep quality and work-related exhaustion.

The data we use to capture individual's sleep quality is based on two survey questions ('*Waking up repeatedly during the sleep?*' and '*Difficulty falling asleep?*'). The questions ask workers to evaluate the intensity (how many times they experience some of these insomnia symptoms) of sleep problems – insomnia symptoms (i.e., trouble falling and staying asleep) on a scale from 1 to 5 (where 1 corresponds to 'Daily' and 5 corresponds to 'Never'). In an analogous way, the measurement of work-related exhaustion is made using the question '*Do you feel exhausted at the end of the working day?*', where respondents evaluate work-related exhaustion on a scale from 1 to 5 (where 1 corresponds to 'Always' and 5 corresponds to 'Never').[86] Description and summary statistics of these variables are also provided in Table 3.1 and 3.2 respectively.

To explore the relationship between job satisfaction, performance-related pay conditional on the sleep quality and work-related exhaustion, each time we interact the dichotomous variable of performance-related pay with one of the three variables ($PP^{(j)} * Waking\ up\ repeatedly\ during\ the\ sleep$, $PP^{(j)} * Difficulty\ falling\ asleep$ and $PP^{(j)} * Exhausted\ at\ the\ end\ of\ the\ working\ day$, where j can take three values – based on the level of performance pay: department, company and individual).[87] In the following figures (Figure 3.1 to 3.9), we visualize the marginal effects of performance pay conditional on sleep quality and work-related exhaustion on the highest level of job satisfaction ('Very satisfied'). Figure 3.1 visualizes the marginal effects of department performance pay on the predicted value of the 'Very satisfied' category of the outcome variable (job satisfaction=3), for different values of $Waking\ up\ repeatedly\ during\ the\ sleep = 1, 2, 3, 4, 5$, with the associated 95% confidence intervals. The figure clearly shows that the positive effect of marginal effects become stronger and significant for higher values of $Waking\ up\ repeatedly\ during\ the\ sleep$, that is, for values that indicates better sleep quality. In addition, Figure 3.2 visualizes the marginal effects of department performance pay on job satisfaction at different values of $Difficulty\ falling\ asleep$. Evidently, we observe

[86] These variables are available only for the 2015 survey.

[87] Coefficient effects are reported in Table A3.5, A3.6 and A3.7. Based on these results, we can observe that in all specifications the interaction terms are positive and statistically significant. Also, the effect of $Waking\ up\ repeatedly$, $Difficulty\ falling\ asleep$ and $Exhausted\ at\ the\ end\ of\ the\ working\ day$ is positive and statistically significant (those workers who have better sleep quality and experience less work related exhaustion are more likely to be in the higher categories of job satisfaction) in all tables.

stronger and significant marginal effects for higher values – better sleep quality. In Figure 3.3, we plot the marginal effects of department performance pay on job satisfaction at different values of $Exhausted\ at\ the\ end\ of\ the\ working\ day$. Clearly, marginal effects increase with higher values of $Exhausted\ at\ the\ end\ of\ the\ working\ day$, i.e., those who experience less work-related exhaustion. These effects remain substantively the same for company (Figures 3.4, 3.5 and 3.6) and individual performance pay (Figures 3.7, 3.8 and 3.9). Thus, we can argue that all performance pay schemes have a stronger effect on job satisfaction of workers who have better sleep quality, and experience less work-related exhaustion.

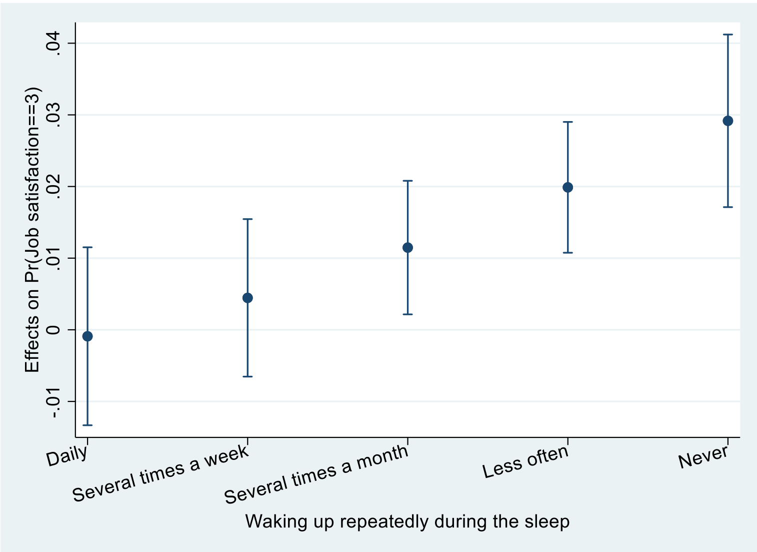

Figure 3.1 Marginal effect of department performance pay on job satisfaction at different values of $Waking\ up\ repeatedly\ during\ the\ sleep$

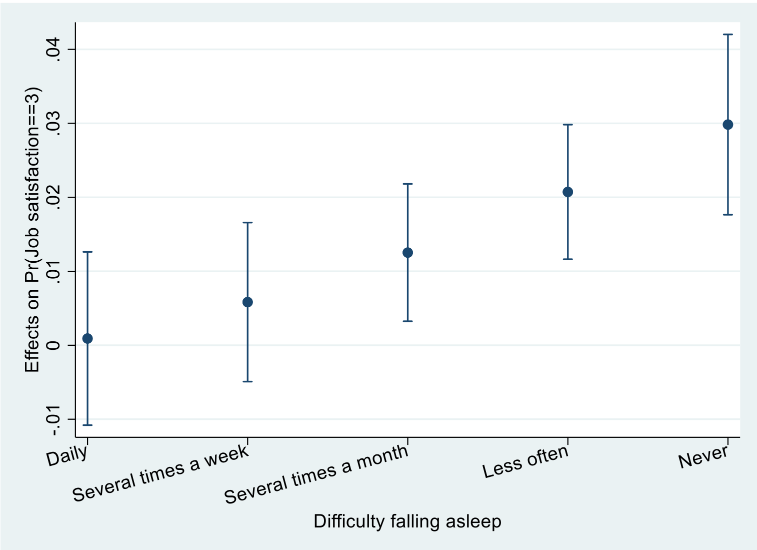

Figure 3.2 Marginal effect of department performance pay on job satisfaction at different values of $Difficulty\ falling\ asleep$

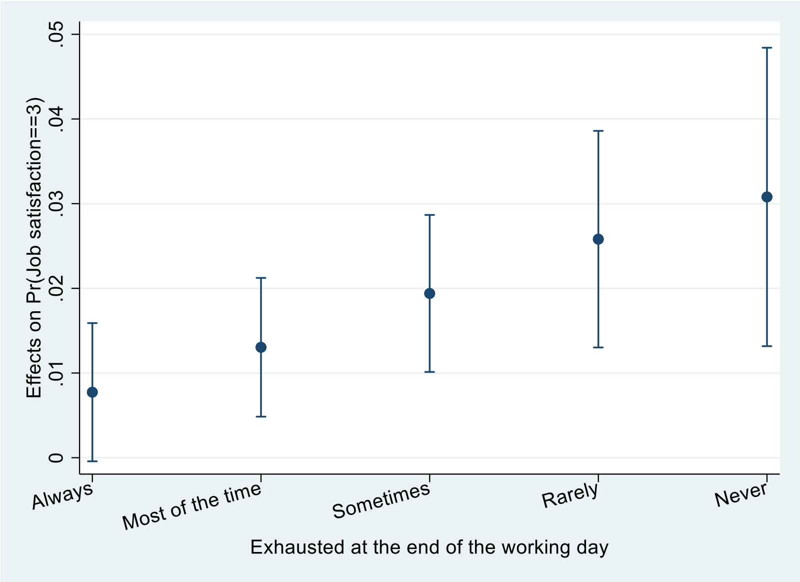

Figure 3.3 Marginal effect of department performance pay on job satisfaction at different values of *Exhausted at the end of the working day*

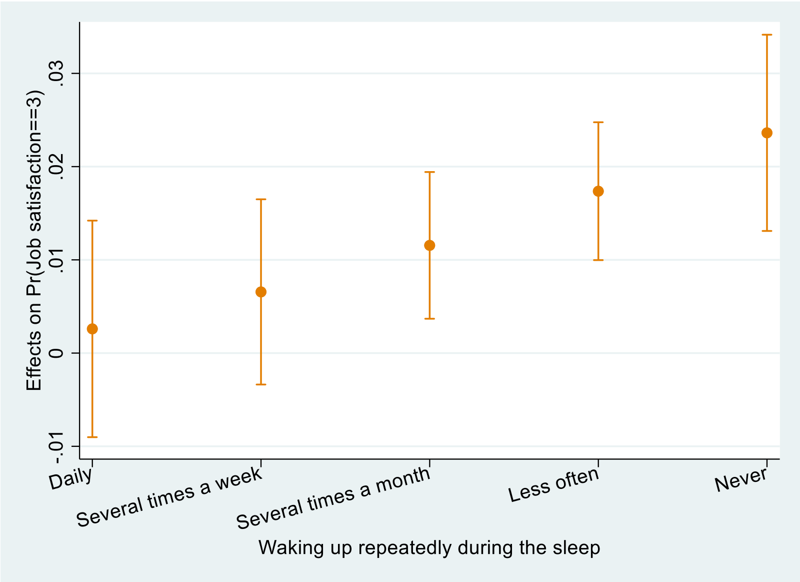

Figure 3.4 Marginal effect of company performance pay on job satisfaction at different values of *Waking up repeatedly during the sleep*

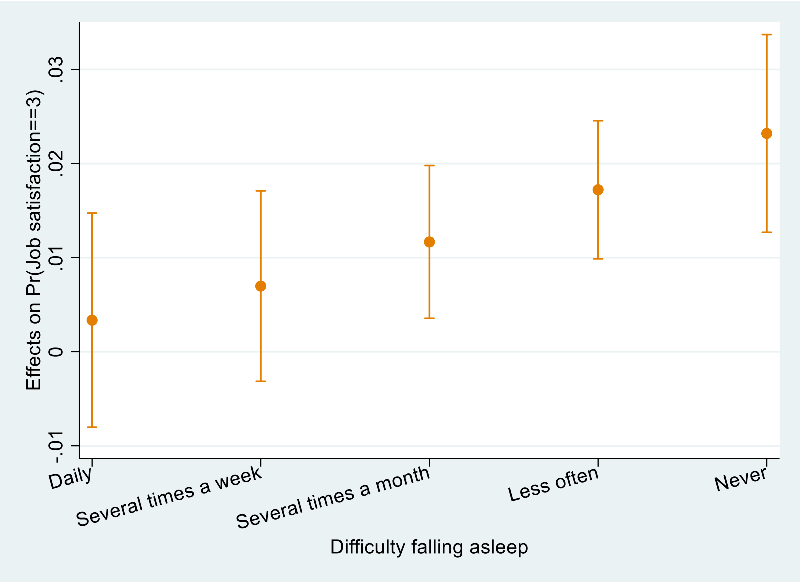

Figure 3.5 Marginal effect of company performance pay on job satisfaction at different values of *Difficulty falling asleep*

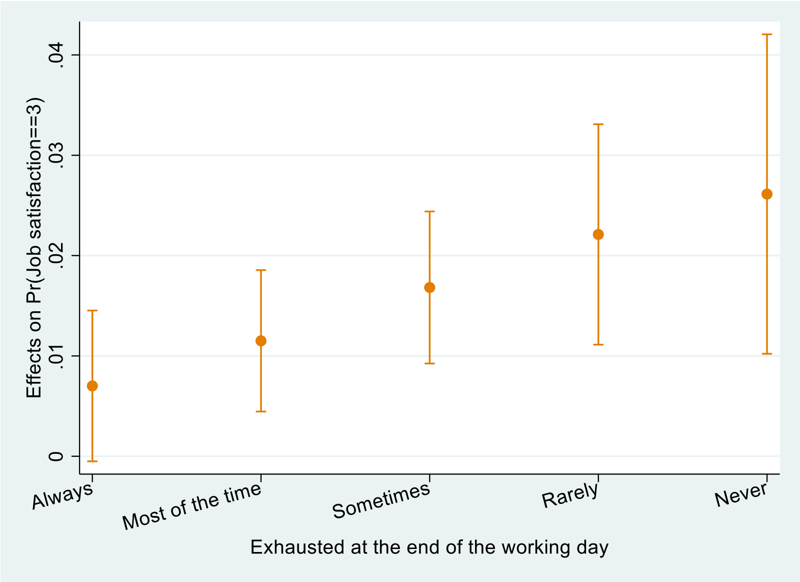

Figure 3.6 Marginal effect of company performance pay on job satisfaction at different values of $Exhausted\ at\ the\ end\ of\ the\ working\ day$

Figure 3.7 Marginal effect of individual performance pay on job satisfaction at different values of $Waking\ up\ repeatedly\ during\ the\ sleep$

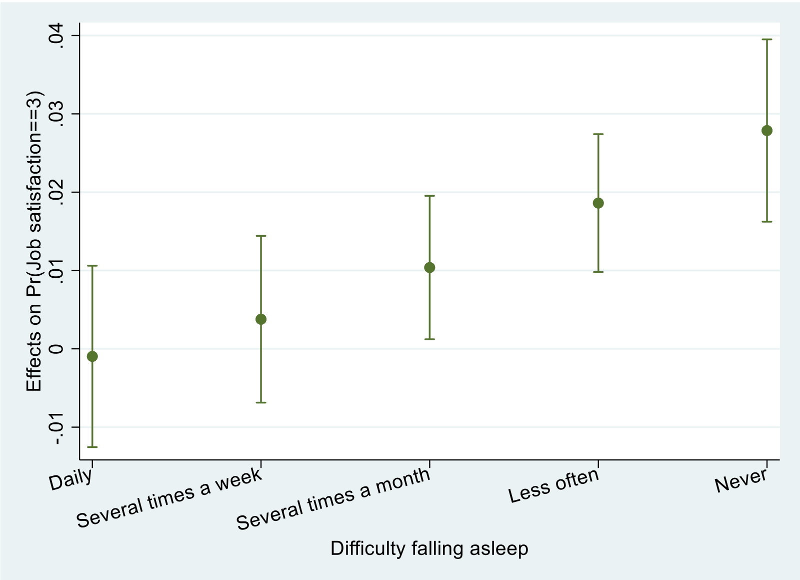

Figure 3.8 Marginal effect of individual performance pay on job satisfaction at different values of $Difficulty\ falling\ asleep$

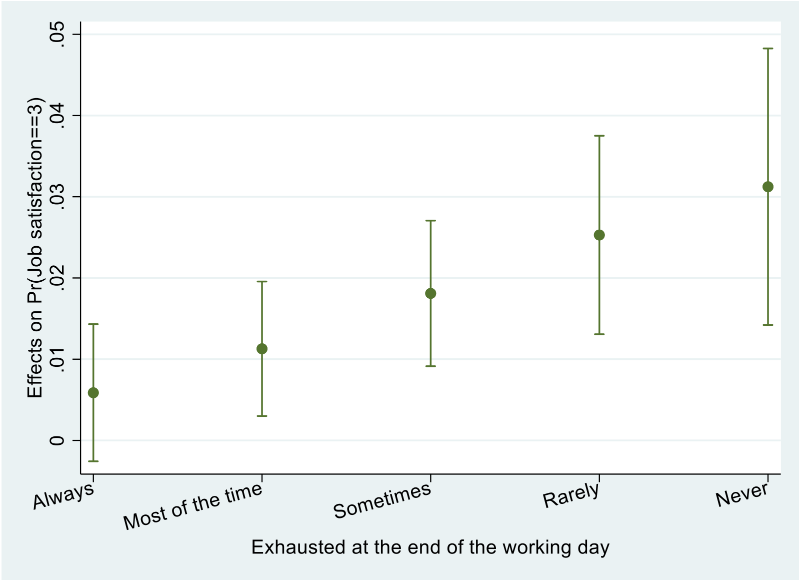

Figure 3.9 Marginal effect of company performance pay on job satisfaction at different values of ***Exhausted at the end of the working day***

3.4.3 Additional estimates

In the following three tables (Table 3.4, 3.5 and 3.6), we conduct a subsample analysis. We split our sample into i) men and women employees (Table 3.4), ii) private and public sector (Table 3.5) and iii) firms with up to 49 employees, firms with 50-499 employees and firms with 500 or more employees (Table 3.6). In Table 3.4, we show that the effect of all performance-related pay schemes (department, company and individual) on job satisfaction are relative larger for women compared to men employees.[88] This contrasts with Bender's et al.

[88] At the mean of department performance-related pay for women employees, women who receive department performance pay are about 27% (=0.0458/0.1683) more likely to report being in the most satisfied category, while men employees who receive department performance pay are on average about 17% (=0.0356/0.2131) more likely to report being in the most satisfied category. Women employees under a company performance pay scheme are on average about 65% (=0.0586/0.0907) more likely to be in the 'very satisfied' outcome, while male employees under a company performance pay are on average about 33% (=0.0492/0.1484) more likely to be in the 'very satisfied' outcome. Women employees who receive individual performance pay are on average about 34% (=0.0527/0.1548) more likely to report being in the most satisfied category, compared to 20% (=0.0407/0.2083) for men employees. All magnitude effects are interpreted based on the average of performance pay schemes (reported in Table A3.8 of the Appendix) for each subgroup.

(2005) claim that women feel a higher peer pressure when operating in the groups (i.e., department and company performance pay schemes), and as a consequence they acquire a lower level of job satisfaction while working in such environments. The results also vary when we split the sample between private and public sector (Table 3.5), the effect of all performance-related pay schemes is relative larger and significant for workers employed in the private sector compared to the public sector.[89] The finding that performance-related pay is a significant positive determinant of job satisfaction for private sector employees is in line with the evidence found by Bryson et al. (2017), who argue that performance pay has a positive and significant effect on job satisfaction among private sector employees, while the effect in the public sector is insignificant. In Table 3.6, where we split the sample based on the size of establishments, we show that the effect of individual performance pay on job satisfaction is higher in larger firms (the largest effect is highlighted in the group of firms with 500 or more employees)[90], while a greater effect is reported for group performance pay schemes (department and

[89] We interpret the results based on the average performance pay scheme for each subgroup. Workers who are employed in the private sector and receive department performance pay are on average about 24% (=0.0462/0.1891) more likely to report being in the most satisfied category, those in private sector and under a company performance pay scheme are on average about 33% (=0.0518/0.1555) more likely to be in the 'very satisfied' outcome (the effect of both group performance pay is insignificant for public sector employees). Under an individual performance pay scheme, private sector employees are on average about 26% (=0.0536/0.2046) more likely to report being in the most satisfied category, compared to 18% (=0.0272/0.1471) in the public sector.

[90] Under an individual performance pay scheme workers that they are employed in firms with 500 or more employees are on average about 29% (=0.0637/0.2212) more likely to report being in the most satisfied category, compared to 25% (=0.0493/0.1962) in firms with 50-499 employees and 25% (=0.0407/0.1645) in firms with up to 49 employees.

company) in smaller firms (firms with up to 49 employees have the largest effect).[91] The result that the effect of individual performance pay scheme on job satisfaction is higher in larger firms goes in line with the findings of Artz (2008) who have found that performance-related pay has a positive and statistically significant effect on job satisfaction in larger firms, while in smaller firms there is no significant relationship. However, we do find evidence that group-performance pay has a relative larger effect on job satisfaction in smaller compared to larger firms.

Table 3.4 Job satisfaction and performance pay – Subgroup: men and women

	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
	Marginal effects					
Performance pay (department)	0.0356***			0.0458***		
	(0.0052)			(0.0058)		
Performance pay (company)		0.0492***			0.0586***	
		(0.0074)			(0.0060)	
Performance pay (individual)			0.0407***			0.0527***
			(0.0104)			(0.0113)
Age	0.0000	0.0000	0.0000	-0.0001*	-0.0001**	-0.0000
	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0000)	(0.0001)
Household size	-0.0001	-0.0000	0.0002	0.0001	0.0001	0.0004
	(0.0005)	(0.0005)	(0.0007)	(0.0005)	(0.0005)	(0.0007)
Public sector	-0.0007	-0.0007	0.0006	0.0006	0.0005	0.0014
	(0.0009)	(0.0009)	(0.0018)	(0.0009)	(0.0009)	(0.0017)
Tenure	-0.0000	-0.0000	0.0002	0.0002	0.0002	0.0001
	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0003)
Working hours	0.0000	0.0000	-0.0000	-0.0000	-0.0000	-0.0001*
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Work at night	-0.0012***	-0.0012***	-0.0008**	-0.0009**	-0.0009**	-0.0004
	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0005)
Work on Sundays	-0.0002	-0.0002	-0.0000	-0.0005*	-0.0005*	-0.0002
	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0004)
Fixed contract	0.0037	0.0036	0.0076	0.0158***	0.0157***	0.0256***
	(0.0033)	(0.0033)	(0.0072)	(0.0039)	(0.0039)	(0.0085)
Monotonous tasks	-0.0270***	-0.0272***	-0.0459***	-0.0236***	-0.0236***	-0.0418***
	(0.0025)	(0.0025)	(0.0064)	(0.0025)	(0.0025)	(0.0065)
Learning new things	0.0265***	0.0263***	0.0401***	0.0143***	0.0145***	0.0352***
	(0.0029)	(0.0029)	(0.0077)	(0.0028)	(0.0028)	(0.0074)

[91] Workers in firms with up to 49 employees and who receive department individual pay are on average about 24% (=0.0428/0.1750) more likely to be in the ‘very satisfied’ outcome, workers in companies with up to 49 employees and under a company performance pay scheme are on average about 55% (=0.0525/0.0958) more likely to report being in the most satisfied category.

Educational level	0.0037***	0.0037***	0.0019*	0.0024***	0.0025***	0.0007
	(0.0010)	(0.0010)	(0.0011)	(0.0009)	(0.0009)	(0.0010)
Firm size	0.0000	-0.0000	0.0003	-0.0004***	-0.0004***	-0.0000
	(0.0001)	(0.0001)	(0.0003)	(0.0001)	(0.0001)	(0.0003)
Log(pay)	0.0023***	0.0023***	0.0043***	0.0034***	0.0034***	0.0050***
	(0.0004)	(0.0004)	(0.0009)	(0.0004)	(0.0004)	(0.0009)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	-	Yes	Yes	-
Observations	23,509	23,509	7,454	24,457	24,457	7,632
Pseudo R-squared	0.056	0.057	0.045	0.045	0.045	0.036
Wald chi-squared model fit	2439.971***	2475.441***	618.440***	2072.249***	2090.894***	526.206***

Notes: Robust standard errors in parentheses. The overall significance of each model is assessed using McFadden's pseudo R-squared and the Wald-test. Significance level is denoted by *** (1%), ** (5%) and * (10%).

Table 3.5 Job satisfaction and performance pay – Subgroup: private and public

	Private			Public		
	(1)	(2)	(3)	(4)	(5)	(6)
			Marginal effects			
Performance pay (department)	0.0462***			0.0082		
	(0.0047)			(0.0075)		
Performance pay (company)		0.0518***			0.0126	
		(0.0051)			(0.0128)	
Performance pay (individual)			0.0536***			0.0272*
			(0.0089)			(0.0157)
Age	-0.0000	-0.0000	0.0001	-0.0001	-0.0001	-0.0001
	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0000)	(0.0001)
Household size	-0.0003	-0.0003	0.0002	0.0013*	0.0013*	0.0009
	(0.0004)	(0.0004)	(0.0005)	(0.0007)	(0.0007)	(0.0008)
Gender	0.0033	0.0035	0.0034	0.0087	0.0087	0.0082
	(0.0035)	(0.0035)	(0.0070)	(0.0059)	(0.0059)	(0.0109)
Tenure	0.0002	0.0002	0.0003	-0.0003	-0.0003	-0.0002
	(0.0001)	(0.0001)	(0.0003)	(0.0002)	(0.0002)	(0.0004)
Working hours	0.0000	0.0000	-0.0001	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0001)
Work at night	-0.0014***	-0.0014***	-0.0011***	-0.0002	-0.0002	-0.0001
	(0.0003)	(0.0003)	(0.0003)	(0.0005)	(0.0005)	(0.0007)
Work on Sundays	-0.0000	-0.0000	0.0001	-0.0008***	-0.0008***	-0.0001
	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0005)
Fixed contract	0.0081***	0.0080***	0.0171***	0.0104**	0.0104**	0.0147
	(0.0030)	(0.0030)	(0.0064)	(0.0045)	(0.0045)	(0.0100)
Monotonous tasks	-0.0263***	-0.0264***	-0.0493***	-0.0204***	-0.0205***	-0.0393***
	(0.0021)	(0.0021)	(0.0057)	(0.0033)	(0.0033)	(0.0076)
Learning new things	0.0203***	0.0204***	0.0381***	0.0212***	0.0211***	0.0318***
	(0.0024)	(0.0024)	(0.0062)	(0.0043)	(0.0043)	(0.0097)
Educational level	0.0026***	0.0026***	0.0008	0.0035**	0.0035**	0.0016
	(0.0007)	(0.0007)	(0.0007)	(0.0017)	(0.0017)	(0.0020)
Firm size	-0.0004***	-0.0004***	-0.0002	0.0001	0.0001	0.0004
	(0.0001)	(0.0001)	(0.0003)	(0.0002)	(0.0002)	(0.0003)
Log(pay)	0.0029***	0.0029***	0.0049***	0.0025***	0.0025***	0.0035***
	(0.0004)	(0.0004)	(0.0008)	(0.0006)	(0.0006)	(0.0012)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes

Year FE	Yes	Yes	-	Yes	Yes	-
Observations	29,138	29,138	9,057	15,282	15,282	4,773
Pseudo R-squared	0.057	0.057	0.049	0.035	0.036	0.029
Wald chi-squared model fit	3150.275***	3164.954***	831.840***	1035.418***	1057.707***	295.176***

Notes: Robust standard errors in parentheses. The overall significance of each model is assessed using McFadden's pseudo R-squared and the Wald-test. Significance level is denoted by *** (1%), ** (5%) and * (10%).

Table 3.6 Job satisfaction and performance pay – Subgroup: firm size

	Up to 49			50-499			500 or more		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Marginal effects								
Performance pay (department)	0.0428***			0.0256***			0.0201		
	(0.0050)			(0.0073)			(0.0128)		
Performance pay (company)		0.0525***			0.0388***			0.0197	
		(0.0063)			(0.0086)			(0.0137)	
Performance pay (individual)			0.0407***			0.0493***			0.0637***
			(0.0109)			(0.0125)			(0.0218)
Age	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000	-0.0001	0.0001	0.0001	-0.0001
	(0.0000)	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Household size	-0.0004	-0.0003	-0.0004	0.0005	0.0005	0.0014**	0.0022	0.0022	0.0022*
	(0.0005)	(0.0005)	(0.0006)	(0.0005)	(0.0005)	(0.0005)	(0.0014)	(0.0014)	(0.0013)
Gender	0.0048	0.0050	0.0114	0.0084	0.0085	0.0043	0.0052	0.0052	-0.0103
	(0.0037)	(0.0037)	(0.0079)	(0.0058)	(0.0058)	(0.0101)	(0.0105)	(0.0105)	(0.0175)
Public sector	0.0001	0.0001	0.0037**	0.0010	0.0010	-0.0015	-0.0011	-0.0011	-0.0014
	(0.0008)	(0.0008)	(0.0017)	(0.0012)	(0.0012)	(0.0023)	(0.0020)	(0.0020)	(0.0032)
Tenure	-0.0000	-0.0000	-0.0001	-0.0001	-0.0001	-0.0003	0.0002	0.0002	0.0010
	(0.0001)	(0.0001)	(0.0003)	(0.0002)	(0.0002)	(0.0004)	(0.0004)	(0.0004)	(0.0007)
Working hours	-0.0000	-0.0000	-0.0001*	0.0000	0.0000	0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Work at night	-0.0012***	-0.0012***	-0.0009**	-0.0018***	-0.0018***	-0.0014***	-0.0001	-0.0001	0.0007
	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.0006)	(0.0006)	(0.0007)
Work on Sundays	-0.0006***	-0.0006***	-0.0005	-0.0003	-0.0003	0.0002	0.0005	0.0005	0.0003
	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0005)	(0.0006)	(0.0006)	(0.0007)
Fixed contract	0.0083***	0.0082***	0.0118*	0.0042	0.0041	0.0072	0.0132	0.0130	0.0377**
	(0.0030)	(0.0030)	(0.0071)	(0.0053)	(0.0053)	(0.0091)	(0.0085)	(0.0085)	(0.0168)
Monotonous tasks	-0.0214***	-0.0215***	-0.0367***	-0.0378***	-0.0377***	-0.0559***	-0.0317***	-0.0320***	-0.0639***
	(0.0021)	(0.0021)	(0.0056)	(0.0041)	(0.0041)	(0.0092)	(0.0071)	(0.0071)	(0.0176)
Learning new things	0.0175***	0.0176***	0.0322***	0.0289***	0.0288***	0.0384***	0.0290***	0.0289***	0.0616***
	(0.0024)	(0.0024)	(0.0070)	(0.0050)	(0.0050)	(0.0099)	(0.0102)	(0.0102)	(0.0214)
Educational level	0.0030***	0.0030***	0.0012	0.0030***	0.0030***	0.0014	0.0070***	0.0070***	0.0045**
	(0.0009)	(0.0009)	(0.0010)	(0.0011)	(0.0011)	(0.0010)	(0.0023)	(0.0023)	(0.0021)
Log(pay)	0.0026***	0.0026***	0.0052***	0.0032***	0.0031***	0.0051***	0.0041***	0.0041***	0.0021
	(0.0004)	(0.0004)	(0.0009)	(0.0006)	(0.0006)	(0.0012)	(0.0011)	(0.0011)	(0.0020)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	-	Yes	Yes	-	Yes	Yes	-
Observations	28,963	28,963	7,817	13,281	13,281	4,985	4,197	4,197	1,810
Pseudo R-squared	0.052	0.052	0.043	0.047	0.049	0.038	0.059	0.061	0.065
Wald chi-squared model fit	2844.828***	2841.793***	641.680***	1161.294***	1200.374***	353.299***	522.683***	539.915***	1699.882***

Notes: Robust standard errors in parentheses. The overall significance of each model is assessed using McFadden's pseudo R-squared and the Wald-test. Significance level is denoted by *** (1%), ** (5%) and * (10%).

In this context, selection may bias the estimated results (McCausland et al., 2005). Workers may have systematic preferences as to how they get paid, and the unmeasured-unobserved factors of this preference may affect both self-sorting into performance pay and job satisfaction. In other words, self-selection into performance pay schemes is endogenous and depends on both observed and unobserved factors. While we account for the former through the inclusion of control variables, failing to account for the latter will cause omitted variable bias. Two techniques were followed to examine whether or not selection bias seems to be a concern in the estimations. First, the procedure we employ to correct for selection bias (non-random worker self-selection into performance-related pay) uses the standard Heckman (1979) two-stage methodology by including the inverse-Mills ratio in our model (Equation 1).[92] These values are generated in a separate probit model predicting performance-related pay (Table A3.9).[93] Table 3.7 reports results using Heckman's (1979) methodology. Using this procedure to account for selection bias does not substantively alter our baseline results. The effect of all performance-related pay variables remains positive and statistically significant (although the marginal effects are slightly smaller in magnitude.). The estimated coefficient on inverse-Mills ratio (reported in the bottom of the Table 3.7) is highly significant in all specifications, thus verifying the presence of selection bias which is properly accounted for by the Heckman's two-stage procedure. Second, propensity score matching (PSM) is also employed to account for selection bias.[94] Results using the PSM method are reported in Table 3.8. All performance-related pay schemes have a positive and statistically significant effect on job satisfaction for all alternative PSM methods, indicating that after matching treated and control individuals, workers who received performance-related pay are more satisfied with their job than those that did not received performance-related pay.

[92] Heckman's (1979) two-stage procedure which accounts for selection bias is described in detail in the Appendix.

[93] A significant negative coefficient on the inverse-Mills ratio indicates that unobserved variables that make performance pay more likely makes workers more likely to be in the lower categories of job satisfaction; a significantly positive coefficient indicates that unobserved variables that make performance pay more likely makes workers more likely to be in the higher categories of job satisfaction.

[94] The PSM procedure which correct for selection bias is described in detail in the Appendix.

Table 3.7 Job satisfaction and performance pay – Heckman's estimates

	(1)	(2)	(3)
		Marginal effects	
Performance pay (department)	0.0219***		
	(0.0038)		
Performance pay (company)		0.0339***	
		(0.0046)	
Performance pay (individual)			0.0228***
			(0.0075)
Age	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0001)
Household size	-0.0002	-0.0002	0.0002
	(0.0004)	(0.0004)	(0.0004)
Gender	0.0070***	0.0036***	0.0052***
	(0.0038)	(0.0034)	(0.0066)
Public sector	-0.0007	-0.0008	0.0010
	(0.0007)	(0.0007)	(0.0013)
Tenure	-0.0004***	-0.0008***	0.0001
	(0.0001)	(0.0001)	(0.0002)
Working hours	-0.0000**	-0.0000**	-0.0001**
	(0.0000)	(0.0000)	(0.0000)
Work at night	-0.0012***	-0.0012***	-0.0008***
	(0.0002)	(0.0002)	(0.0003)
Work on Sundays	-0.0004***	-0.0004**	-0.0003
	(0.0002)	(0.0002)	(0.0003)
Fixed contract	0.0137***	0.0140***	0.0186***
	(0.0027)	(0.0027)	(0.0052)
Monotonous tasks	-0.0260***	-0.0260***	-0.0410***
	(0.0017)	(0.0017)	(0.0044)
Learning new things	0.0126***	0.0124***	0.0183***
	(0.0020)	(0.0020)	(0.0046)
Educational level	0.0050***	0.0013*	-0.0003
	(0.0007)	(0.0007)	(0.0007)
Firm size	-0.0004***	0.0001	-0.0001
	(0.0001)	(0.0001)	(0.0002)
Log(pay)	0.0025***	0.0026***	0.0042***
	(0.0003)	(0.0003)	(0.0007)
Inverse Mills Ratio (IMR)	-1.7426***	-1.0037***	-1.4261***
	(0.0467)	(0.0254)	(0.0594)
Country FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes
Year FE	Yes	Yes	-
Observations	47,969	47,969	15,089
Pseudo R-squared	0.065	0.068	0.060
Wald chi-squared model fit	5506.075***	5833.839***	1481.440***

Notes: Robust standard errors in parentheses. The overall significance of each model is assessed using McFadden's pseudo R-squared and the Wald-test. Significance level is denoted by *** (1%), ** (5%) and * (10%).

Table 3.8 Propensity score matching methods – Average treatment effects on treated (ATET)

	Differences		
Outcome variable:	Job satisfaction		
Explanatory variable:	Performance pay (department)	Performance pay (company)	Performance pay (individual)
Estimation method			
ATET nearest neighbor	0.0373***	0.0492***	0.0445**
	(0.0130)	(0.0139)	(0.0211)
ATET radius matching	0.0448***	0.0635***	0.0543***
	(0.0112)	(0.0103)	(0.0142)
ATET kernel matching	0.0370***	0.0550***	0.0440***
	(0.0114)	(0.0106)	(0.0143)

Notes: Coefficients show the average treatment effects on the treated (ATET). We present the ATET, using the nearest neighbor, radius and kernel matching. The number of observations is the same as in columns (4) to (6) in Table 3.3. Standard errors obtained using 100 bootstraps are presented in the parentheses. Significance level is denoted by *** (1%) and ** (5%).

3.5 Concluding remarks

Differences in payment systems can have multilevel effects on employee and organizational outcomes. Using a sample of countries from the EWCS 2010–2015 the object of this study has been to provide new insights regarding the direct impact of performance-related pay systems on job satisfaction. After controlling for personal, job and firm characteristics our baseline results suggest that performance pay exerts a beneficial effect on job satisfaction. Specifically, our findings show that workers who are employed on either department or company or individual performance pay job schemes are more satisfied on their job than workers who are paid by the fixed amount. The results using sub-samples also indicate that the job satisfaction effects of performance pay vary by gender, by sector and by firm size. As we explained, women and private sector employees experience a greater satisfaction with their jobs under a performance pay scheme. The effect of individual performance pay on job satisfaction is higher in larger firms, while a greater effect is reported for group performance pay schemes in smaller firms.

We investigate two channels through which performance pay schemes produce a greater impact on job satisfaction. First, we show that workers under a performance pay scheme feel more satisfied with their jobs when they have better sleep quality. Second, we point out that the receipts of performance-related pay have a greater job satisfaction when they experience less work-related exhaustion. Thus, our findings are driven by all type of performance pay schemes to individuals who have better sleep quality and experience less work-related exhaustion.

When workers' payment is based on the performance, they make a greater effort to reach the 'required' level of performance which enables a greater payment. As we argue, workers tend to prefer employment environments that rewards hard work, effort and productivity. This is one of the discussed pathways through which performance pay schemes positively affect job satisfaction. Nevertheless, health-related issues directly affect worker's effort. In this study, we center our attention to insomnia symptoms and work-related exhaustion. As we show, they both negatively affect job satisfaction, and one potential pathway is through lower effort (e.g., decreased motivation and inability to meet work goals). As such, we find that two potential channels through which worker's effort is negatively affected are poor sleep quality and increased work-related exhaustion, which in turn will change worker's job satisfaction.

Appendix 3

Table A3.1 List of countries

Albania	Lithuania
Austria	Luxembourg
Belgium	Malta
Bulgaria	Montenegro
Croatia	Netherlands
Cyprus	North Macedonia
Czech Republic	Norway
Denmark	Poland
Estonia	Portugal
Finland	Romania
France	Serbia
Germany	Slovakia
Greece	Slovenia
Hungary	Spain
Ireland	Sweden
Italy	Switzerland
Kosovo	Turkey
Latvia	United Kingdom

Table A3.2 List of industries (NACE Rev. 1 - Statistical classification of economic activities)

Agriculture, forestry and fishing
Mining and quarrying
Manufacturing
Electricity, gas, steam and air conditioning supply
Water supply; sewerage, waste management and remediation activities
Construction
Wholesale and retail trade; repair of motor vehicles and motorcycles
Transportation and storage
Accommodation and food service activities
Information and communication
Financial and insurance activities
Real estate activities
Professional, scientific and technical activities
Administrative and support service activities
Public administration and defence; compulsory social security
Education
Human health and social work activities
Arts, entertainment and recreation
Other service activities
Activities of households
Activities of extraterritorial organisations and bodies

Table A3.3 List of occupations (International Standard Classification of Occupations 2008 (ISCO-08))

Armed forces occupations
Managers
Professionals
Technicians and associate professionals
Clerical support workers
Service and sales workers
Skilled agricultural, forestry and fishery workers
Craft and related trades workers
Plant and machine operators, and assemblers
Elementary occupations

Table A3.4 Job satisfaction and performance pay – Covariate estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Performance pay (department)	0.1250***			0.1221***		
	(0.0137)			(0.0137)		
Performance pay (company)		0.2013***			0.1977***	
		(0.0165)			(0.0165)	
Performance pay (individual)			0.1555***			0.1494***
			(0.0250)			(0.0250)
Age	-0.0001	-0.0001	-0.0000	-0.0001	-0.0001	-0.0001
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0002)
Household size	-0.0006	-0.0005	0.0005	-0.0006	-0.0004	0.0007
	(0.0013)	(0.0013)	(0.0014)	(0.0013)	(0.0013)	(0.0014)
Gender	0.0281**	0.0260**	0.0330*	0.0254**	0.0233**	0.0289
	(0.0114)	(0.0114)	(0.0195)	(0.0114)	(0.0114)	(0.0196)
Public sector	-0.0021	-0.0026	0.0033	-0.0020	-0.0025	0.0030
	(0.0024)	(0.0024)	(0.0042)	(0.0024)	(0.0024)	(0.0042)
Tenure	-0.0002	-0.0003	0.0001	-0.0004	-0.0005	-0.0002
	(0.0004)	(0.0004)	(0.0008)	(0.0004)	(0.0004)	(0.0008)
Working hours	-0.0001**	-0.0001**	-0.0002**	-0.0001***	-0.0001***	-0.0003***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Work at night	-0.0051***	-0.0050***	-0.0031***	-0.0051***	-0.0050***	-0.0031***
	(0.0008)	(0.0009)	(0.0010)	(0.0008)	(0.0008)	(0.0010)
Work on Sundays	-0.0016***	-0.0014**	-0.0006	-0.0017***	-0.0015***	-0.0008
	(0.0006)	(0.0006)	(0.0008)	(0.0006)	(0.0006)	(0.0008)
Fixed contract	0.0363***	0.0377***	0.0501***	0.0371***	0.0384***	0.0509***
	(0.0093)	(0.0094)	(0.0169)	(0.0094)	(0.0094)	(0.0171)
Monotonous tasks	-0.1159***	-0.1172***	-0.1713***	-0.1155***	-0.1169***	-0.1695***
	(0.0063)	(0.0063)	(0.0155)	(0.0063)	(0.0063)	(0.0155)
Learning new things	0.0765***	0.0763***	0.1115***	0.0750***	0.0748***	0.1083***
	(0.0073)	(0.0074)	(0.0165)	(0.0073)	(0.0074)	(0.0166)
Educational level	0.0130***	0.0128***	0.0042*	0.0124***	0.0122***	0.0038
	(0.0031)	(0.0031)	(0.0025)	(0.0030)	(0.0030)	(0.0025)
Firm size	-0.0012***	-0.0011***	-0.0000	-0.0013***	-0.0012***	-0.0002
	(0.0004)	(0.0004)	(0.0006)	(0.0004)	(0.0004)	(0.0006)
Log(pay)				0.0108***	0.0108***	0.0166***
				(0.0011)	(0.0011)	(0.0022)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	-	Yes	Yes	-
Observations	47,969	47,969	15,089	47,969	47,969	15,089
Pseudo R-squared	0.048	0.048	0.036	0.049	0.049	0.038
Wald chi-squared model fit	4269.648***	4329.022***	1012.376***	4364.237***	4423.579***	1058.206***

Notes: Robust standard errors in parentheses. The overall significance of each model is assessed using McFadden's pseudo R-squared and the Wald-test. Significance level is denoted by *** (1%), ** (5%) and * (10%).

Table A3.5 Department performance pay, sleep quality, exhaustion and job satisfaction

	(1)	(2)	(3)
Performance pay (department)	0.2476**	0.2684**	0.1017*
	(0.1079)	(0.1152)	(0.0781)
Waking up repeatedly	0.1747***		
	(0.0083)		
Difficulty falling asleep		0.1979***	
		(0.0091)	
Exhausted at the end of the working day			0.2761***
			(0.0111)
PP (department)*Waking up repeatedly	0.0237**		
	(0.0098)		
PP (department)*Difficulty falling asleep		0.0218**	
		(0.0103)	
PP (department)*Exhausted			0.0230*
			(0.0154)
Age	0.0000	-0.0000	-0.0003
	(0.0002)	(0.0002)	(0.0002)
Household size	0.0001	0.0002	-0.0000
	(0.0014)	(0.0014)	(0.0014)
Gender	-0.0159	-0.0095	-0.0137
	(0.0200)	(0.0199)	(0.0203)
Public sector	0.0029	0.0032	0.0034
	(0.0043)	(0.0043)	(0.0043)
Tenure	0.0008	0.0001	-0.0010
	(0.0008)	(0.0008)	(0.0008)
Working hours	-0.0003***	-0.0003***	-0.0002**
	(0.0001)	(0.0001)	(0.0001)
Work at night	-0.0027***	-0.0023**	-0.0024***
	(0.0009)	(0.0010)	(0.0009)
Work on Sundays	-0.0011	-0.0011	-0.0009
	(0.0008)	(0.0008)	(0.0009)
Fixed contract	0.0378**	0.0343**	0.0319*
	(0.0166)	(0.0169)	(0.0166)
Monotonous tasks	-0.1600***	-0.1555***	-0.1489***
	(0.0149)	(0.0150)	(0.0149)
Learning new things	0.1136***	0.1125***	0.1101***
	(0.0171)	(0.0167)	(0.0170)
Educational level	0.0033	0.0034	0.0023
	(0.0025)	(0.0026)	(0.0026)
Firm size	-0.0003	-0.0003	-0.0003
	(0.0006)	(0.0006)	(0.0006)
Log(pay)	0.0147***	0.0150***	0.0165***
	(0.0022)	(0.0022)	(0.0023)
Country FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes
Year FE	-	-	-
Observations	15,089	15,089	15,089
Pseudo R-squared	0.052	0.053	0.064
Wald chi-squared model fit	1438.892***	1495.731***	1725.728***

Notes: Robust standard errors in parentheses. The overall significance of each model is assessed using McFadden's pseudo R-squared and the Wald-test. Significance level is denoted by *** (1%), ** (5%) and * (10%).

Table A3.6 Company performance pay, sleep quality, exhaustion and job satisfaction

	(1)	(2)	(3)
Performance pay (company)	0.2736***	0.2746***	0.2019**
	(0.0864)	(0.0967)	(0.0839)
Waking up repeatedly	0.1752***		
	(0.0087)		
Difficulty falling asleep		0.1974***	
		(0.0094)	
Exhausted at the end of the working day			0.2781***
			(0.0113)
PP (company)*Waking up repeatedly	0.0150**		
	(0.0055)		
PP (company)*Difficulty falling asleep		0.0132*	
		(0.073)	
PP (company)*Exhausted			0.0093*
			(0.0061)
Age	0.0001	0.0001	-0.0002
	(0.0002)	(0.0002)	(0.0002)
Household size	0.0005	0.0006	-0.0000
	(0.0016)	(0.0016)	(0.0015)
Gender	-0.0171	-0.0113	-0.0146
	(0.0197)	(0.0196)	(0.0200)
Public sector	0.0042	0.0047	0.0047
	(0.0042)	(0.0042)	(0.0042)
Tenure	0.0014*	0.0008	-0.0004
	(0.0008)	(0.0008)	(0.0008)
Working hours	-0.0002*	-0.0002	-0.0001
	(0.0001)	(0.0001)	(0.0001)
Work at night	-0.0023**	-0.0019*	-0.0021**
	(0.0010)	(0.0010)	(0.0010)
Work on Sundays	-0.0009	-0.0009	-0.0008
	(0.0009)	(0.0009)	(0.0009)
Fixed contract	0.0390**	0.0351**	0.0305*
	(0.0172)	(0.0174)	(0.0169)
Monotonous tasks	-0.1446***	-0.1407***	-0.1364***
	(0.0145)	(0.0146)	(0.0145)
Learning new things	0.1250***	0.1242***	0.1199***
	(0.0174)	(0.0171)	(0.0171)
Educational level	0.0031	0.0031	0.0019
	(0.0024)	(0.0024)	(0.0024)
Firm size	0.0002	0.0003	0.0002
	(0.0007)	(0.0007)	(0.0007)
Log(pay)	0.0140***	0.0143***	0.0157***
	(0.0022)	(0.0022)	(0.0022)
Country FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes
Year FE	-	-	-
Observations	15,089	15,089	15,089
Pseudo R-squared	0.052	0.053	0.064
Wald chi-squared model fit	1436.842***	1491.792***	1729.989***

Notes: Robust standard errors in parentheses. The overall significance of each model is assessed using McFadden's pseudo R-squared and the Wald-test. Significance level is denoted by *** (1%), ** (5%) and * (10%).

Table A3.7 Individual performance pay, sleep quality, exhaustion and job satisfaction

	(1)	(2)	(3)
Performance pay (individual)	0.1393*	0.1211*	0.0359
	(0.0820)	(0.0723)	(0.0807)
Waking up repeatedly	0.1719***		
	(0.0087)		
Difficulty falling asleep		0.1936***	
		(0.0095)	
Exhausted at the end of the working day			0.2691***
			(0.0107)
PP (individual)*Waking up repeatedly	0.0253***		
	(0.0093)		
PP (individual)*Difficulty falling asleep		0.0229**	
		(0.0101)	
PP (individual)*Exhausted			0.0145*
			(0.0082)
Age	0.0001	0.0001	-0.0002
	(0.0002)	(0.0002)	(0.0002)
Household size	0.0004	0.0005	-0.0001
	(0.0016)	(0.0016)	(0.0015)
Gender	-0.0167	-0.0110	-0.0149
	(0.0196)	(0.0195)	(0.0199)
Public sector	0.0043	0.0050	0.0049
	(0.0042)	(0.0042)	(0.0042)
Tenure	0.0015*	0.0009	-0.0004
	(0.0008)	(0.0008)	(0.0008)
Working hours	-0.0002*	-0.0002*	-0.0001
	(0.0001)	(0.0001)	(0.0001)
Work at night	-0.0025**	-0.0021**	-0.0022**
	(0.0010)	(0.0010)	(0.0010)
Work on Sundays	-0.0008	-0.0008	-0.0007
	(0.0009)	(0.0009)	(0.0009)
Fixed contract	0.0379**	0.0349**	0.0299*
	(0.0171)	(0.0172)	(0.0167)
Monotonous tasks	-0.1425***	-0.1390***	-0.1348***
	(0.0144)	(0.0145)	(0.0144)
Learning new things	0.1237***	0.1228***	0.1192***
	(0.0173)	(0.0170)	(0.0170)
Educational level	0.0027	0.0027	0.0016
	(0.0024)	(0.0024)	(0.0024)
Firm size	0.0002	0.0003	0.0003
	(0.0007)	(0.0007)	(0.0007)
Log(pay)	0.0140***	0.0143***	0.0156***
	(0.0022)	(0.0022)	(0.0022)
Country FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes
Year FE	-	-	-
Observations	15,089	15,089	15,089
Pseudo R-squared	0.052	0.053	0.064
Wald chi-squared model fit	1441.531***	1498.470***	1728.983***

Notes: Robust standard errors in parentheses. The overall significance of each model is assessed using McFadden's pseudo R-squared and the Wald-test. Significance level is denoted by *** (1%), ** (5%) and * (10%).

Table A3.8 Summary statistics of performance pay – Subgroups

Variable	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Subgroup: men and women	Men		Women			
Performance pay (department)	0.2131	0.4090	0.1683	0.3742		
Performance pay (company)	0.1484	0.3555	0.0907	0.2872		
Performance pay (individual)	0.2083	0.4061	0.1548	0.3617		
Subgroup: private and public	Private		Public			
Performance pay (department)	0.1891	0.3916	0.1906	0.3928		
Performance pay (company)	0.1555	0.3624	0.0519	0.2218		
Performance pay (individual)	0.2046	0.4034	0.1471	0.3542		
Subgroup: firm size	Up to 49		50-499		500 or more	
Performance pay (department)	0.1750	0.3799	0.2043	0.4032	0.2505	0.4333
Performance pay (company)	0.0958	0.2944	0.1431	0.3502	0.2187	0.4134
Performance pay (individual)	0.1645	0.3707	0.1962	0.3972	0.2212	0.4152

The procedure we employ to address the issue of 'selection bias' follows the standard Heckman's (1979) two-step method. First, we run a probit regression to predict performance-related pay:

$$PP_{it} = \pi Z_{it} + v_{it} \tag{a3.1}$$

where performance-related pay is assumed to be a linear function of a list of covariates, Z_{it},[95] and an independent identically distributed random error, v_{it}. In the presence of selection bias, ε from Equation (3.1) and v from Equation (a3.1) are correlated.

We then compute the so-called 'inverse-Mills ratio', $\hat{\Lambda}_{it}$, for each observation in the sample:

$$\hat{\Lambda}_{it} = \frac{\varphi(Z_{it}\hat{\pi})}{\Phi(Z_{it}\hat{\pi})} \tag{a3.2}$$

where φ denotes the standard normal density function, Φ the standard normal cumulative distribution function, and $\hat{\pi}$ is an estimated value taken from Equation (a3.1).

Second, we add the estimated 'inverse-Mills ratio' to the vector of controls in Equation (3.1).

[95] The set of regressors we include are possible determinants (at the individual level) of performance-related pay (see Heywood and Jirjahn, 2006). These are: firm size, gender, tenure, education, fixed term contract, political/trade union activity, influence decisions in the workplace and involvement in improving processes.

Table A3.9 Predicting performance-related pay

	(1)	(2)	(3)
	Marginal effects		
Dependent variable	Performance pay (department)	Performance pay (company)	Performance pay (individual)
Firm size	0.0002**	-0.0001	0.0002
	(0.0001)	(0.0001)	(0.0002)
Political/trade union activity	-0.0195***	-0.0075***	-0.0162***
	(0.0027)	(0.0023)	(0.0052)
Gender	0.0422***	0.0259***	0.0373***
	(0.0037)	(0.0030)	(0.0062)
Tenure	-0.0001	0.0002**	-0.0006**
	(0.0001)	(0.0001)	(0.0003)
Educational level	0.0041***	0.0022***	0.0005
	(0.0006)	(0.0004)	(0.0005)
Fixed contract	-0.0364***	-0.0356***	-0.0512***
	(0.0059)	(0.0051)	(0.0108)
Influence decisions	-0.0143***	-0.0150***	-0.0198***
	(0.0014)	(0.0012)	(0.0026)
Involvement in processes	-0.0128***	-0.0186***	-0.0186***
	(0.0013)	(0.0011)	(0.0024)
Country FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes
Year FE	Yes	Yes	-
Observations	47,969	47,969	15,089
Pseudo R-squared	0.083	0.167	0.101
Wald chi-squared model fit	3384.308***	3986.668***	1333.815***

Notes: Robust standard errors in parentheses. The overall significance of each model is assessed using McFadden's pseudo R-squared and the Wald-test. Significance level is denoted by *** (1%) and ** (5%).

Propensity score matching (PSM) approach estimates the effect of a treatment (e.g., participation in a training/government program) by accounting for the covariates that predict receiving the treatment. The first step in our analysis is therefore to estimate a model for the propensity of observations to be assigned into the treated group, using a set of covariates that may affect the likelihood of being assigned into the treated group. The second step uses these probabilities, or propensity scores to match the treated to control group, conditioned on their having similar participation propensities, and thereby, construct a statistical control group.

Having predicted performance-related pay (participation or not in the payment scheme) (Equation a1),[96] we divide our sample into treatment and control groups to calculate the impact of performance pay schemes on job satisfaction. This we do by taking the difference between the outcomes of treated and the outcomes of the treated observations if they had not been treated:

$$ATET(JS) = E[\Delta|PP_{it} = 1, p(x_{it})] = E[JS_{it}|PP_{it1} = 1, p(x_{it})] - E[JS_{it}|PP_{it0} = 0, p(x_{it})] \qquad \text{(a3.3)}$$

where JS refers to job satisfaction, PP indicates the existence of a performance-related pay scheme, and $p(x_{it})$ stands for the propensity score.[97]

We employ three alternative PSM methods, namely, i) nearest neighbor matching, ii) radius matching and iii) kernel matching.[98]

[96] We are saving the propensity scores (predicted probabilities) from the probit model and using them to find matches for the treated observations.

[97] Two necessary assumptions for identification of the performance-related schemes effects are (i) conditional independence and (ii) presence of a common support. Conditional independence, also called confoundedness, implies that performance-related pay schemes are based entirely on observed characteristics of individuals. If unobserved characteristics determine performance-relates pay, conditional independence will be violated, and PSM would not be an appropriate method. A well-specified and comprehensive model predicting performance pay schemes helps support the conditional independence assumption. The second condition, i.e., presence of a common support, ensures that treatment observations have comparison observations “nearby” in the propensity score distribution.

[98] See e.g., Rosenbaum and Rubin (1985), Dehejia and Wahba (2002), and Heckman et al. (1998).

Chapter IV: Financial development and income inequality: A meta-analysis

4.1 Introduction

A well-developed financial system mobilizes savings, diversifies risk, facilitates transactions, produces information about investment opportunities and attracts foreign investments. Such markets are able to create an environment through which can allocate efficiently financial resources, and bolster risk management, transparency and corporate governance practices, improving the productivity of financed investments (Deidda and Fattouh, 2008). Thus, financial development is instrumental in attaining sustainable and balanced growth. Ample previous literature point outs that financial development fosters economic growth (e.g., Christopoulos and Tsionas, 2004; Abu-Bader and Abu-Qarn, 2008; Yang and Yi, 2008), although some scholars report more ambiguous results (e.g., Zang and Kim, 2007; Law and Singh, 2014; Arcand et al., 2015). Moreover, three available meta-analyses in this literature (Arestis et al., 2015; Valickova et al., 2015; Bijlsma et al., 2018) identify an authentic positive relation between financial development and economic growth.

Arguably, the development of financial markets and institutions has been shown to improve the growth prospects, it can also affect the distribution of economic opportunities (Ayadi et al., 2015). According to Demirgüç-Kunt and Levine (2009) '*[f]inancial development may affect the degree to which a person's economic opportunities are determined by individual skill and initiative, or whether parental wealth, social status, and political connections largely shape economic horizons*'. The financial system can influence the ability of individuals to start a business, pay for education and realize their economic aspirations. Thus, the development of financial markets and institutions can shape the gap between the rich and the poor and the degree to which that gap persists across generations.

Although the impact of financial development on income inequality has attracted the attention of many scholars, the current literature lacks a clear view on its impact. The magnitude of the estimated effect regarding the relationship between financial development and income inequality varies greatly. Various factors mentioned in the literature influence the relation between financial development and income inequality, such as the measurement of the examined variables, data and estimation differences, and the composition of countries. A strand of the empirical literature highlights robust significant effects of financial development on income inequality (e.g., Beck et al., 2007; de Haan and Sturm, 2017; Zhang and Naceur, 2019), while some studies indicate tenuous or no effects (e.g., Huang et al., 2009; Park and Shin, 2017; Adeleye et al., 2019; De Vita and Luo, 2020). Furthermore, the literature also points out mixed empirical results on the relationship between financial development and inequality across different aspects, such as the measurement of financial development and income inequality, estimation methods, time span and the composition of countries (e.g., Asongu, 2013; Asongu and Tchamyou, 2014; Altunbas and Thornton, 2020; Chakroun, 2020). Given the diversity of findings in this specific literature, we perform a meta-analysis of 1,329 estimates reported in 88 studies to assess the effects of financial development on inequality.

Meta-analysis has the advantages to provide a reliable and consistent way of summarizing research findings of previous empirical literature (Stanley and Doucouliagos, 2012). Recent high-quality meta-analyses regarding the distributional effect of economic policies include Heimberger (2020) on the impact of economic globalization on income inequality, Anderson et al. (2017) on the impact of government spending on income inequality, Huang et al. (2020) on the distributional effect of foreign direct investment (FDI) and Ni and Liu (2019) on the nexus between financial liberalization and income inequality. But to the best of our knowledge, the current literature lacks a meta-analysis on the impact of financial development on income inequality. We use meta-analysis techniques to answer the following questions that previous literature lacks to address: How does financial development affect income inequality? Does the current literature suffer from publication bias? To what extent do research study characteristics (e.g., data and estimation methods) systematically influence the heterogeneity of the estimates?

Following some recently developed sophisticated techniques that meta-analysis literature provides in order to correct for publication bias, our results suggest that the mean effect of financial development on income inequality is close to zero. To explain the important systematic heterogeneity, we utilize Bayesian model averaging estimation and address model uncertainty. The heterogeneity analysis indicates that various study characteristics, such as controlling for endogeneity, the characteristics of data and estimation methods, the different measurement of financial development and the composition of countries matter significantly for the effect of financial development on inequality. The results of this study could provide a reference for evaluating and improving the income distribution effects of financial development policies.

The body of this chapter proceeds as follows. The following section provides a compendious review of previous studies. Section 4.3 describes our approach to data collection and presents an overview of our dataset. Section 4.4 tests for publication bias in the literature. In Section 4.5, we investigate the sources of heterogeneity. Finally, Section 4.6 provides concluding remarks.

4.2 Review of literature

This section presents a compendious review of the current literature. Below in Section 4.5, we describe in detail the studies incorporated in our data sample, as such our purpose in this section is not to be exhaustive. On this point, we refer the readers to broad reviews by Claessens and Perotti (2007), Demirgüç-Kunt and Levine (2009), Zhuang et al. (2009), Isah and Soliu (2016), and de Haan and Sturm (2017). In addition, a thorough definition and measurement of financial development can be found in King and Levine (1993), Levine (1999), Beck et al. (2000), Čihák et al. (2013), and Valickova et al. (2015).

The current theories available in the literature provide conflicting predictions on the link between financial development and income inequality. One strand of literature proposes that financial development tends to reduce income inequality. Two theories developed separately by Banerjee and Newman (1993) and Galor and Zeira (1993) belong to this theoretical strand. Financial imperfections such as transaction costs, financial asymmetries, and costly contract enforcement could be important obligations for low-income individuals who lack collateral, networks and credit histories. The above models argue that any mitigation of the credit constraints will favor the poor by improving the efficient allocation of capital and reducing income inequality. As a result, financial development facilitates the funding of poor individuals with productive investments. Consistent with this part of the theory, empirical studies also find a robust negative effect of financial development on inequality (e.g., Beck et al., 2007; Kai and Hamori, 2009; Zhang and Naceur, 2019).

Greenwood and Jovanovic (1990) are the first who have predicted a nonlinear relationship between financial development, income inequality, and economic development. Their model indicates the existence of a positive impact of financial development on capital allocation and economic growth at all stages of economic development, which therefore improves the life of the poor through these channels. However, the distributional effect of financial development is not constant throughout, but depends on the level of economic development. At early stages of development, only the rich directly benefit from financial development because they have easier access to financial markets. As the level of economic development increases, many people access financial markets so that financial development directly helps a larger proportion of society. The empirical literature also supports a nonlinear relationship between financial development and inequality. For example, Kim and Lin (2011) find that the benefits of financial development on income distribution occur only if the country has reached a threshold level of financial development. Below this critical value, financial development counteracts income inequality. Law et al. (2014) find that the reduction of income inequality due to financial development happens only after a certain threshold level of institutional quality has been achieved. In addition, the model developed by Galor and Moav (2004) which is based on inequality and growth dynamics suggests the existence of an inverted U-shaped relationship between financial development and inequality. The inverted U-shaped relationship between financial development and inequality is empirically supported by Clarke et al. (2006), Nasreddine and Mensi (2016), Gravina and Lanzafame (2021), and others.

Contrastingly, some theories anticipate that financial development has a positive effect on inequality (i.e., financial development leads to increased income inequality). The study by Rajan and Zingales (2003) highlights that due to constraints such as collateral, only the rich conduct transactions with financial intermediaries, while the poor are unable to participate in these transactions. Even with development in the financial sector, the rich would disproportionately benefit and would still have upper-hand in the financial transactions, which further widens the rich-poor gap. The findings of this side of the theory are also reported in many empirical works which observe a positive relationship between financial development and inequality (e.g., de Haan and Sturm, 2017; Altunbas and Thornton, 2020; Ngangué, 2020).

Given the mixed evidence that the current literature provides, we need an empirical tool to systematically evaluate the true effect of financial development on income inequality. The meta-analysis is able to answer the above questions.

4.3 The meta-data set

In the first stage of our research, we collected estimates from the current literature. The search strategy in this chapter followed the updated reporting guidelines proposed by Havranek et al. (2020). Numerous electronic databases and search engines are used: ScienceDirect, Scopus, RePec/Ideas and Google Scholar. The following key words are combined: ‘finance’, ‘financial development’, ‘financial depth’, ‘financial deepening’, ‘inequality’, and ‘distribution’. We also used references cited in prior literature reviews and empirical papers to search for more studies. Our research process ended at the end of February 2021.

Our selection criteria for the sample of studies were as follows. First, we requested macro-level and cross-country studies, therefore we eliminated micro-based and single-country studies. Second, to be included, studies had to report sufficient information from which we could quantify a comparable size effect, i.e., report common metric characteristics such as regression coefficients, sample sizes, and t-statistics or standard errors or p-values. Hence, we excluded any study that did not provide this information. Third, studies had to include a numerical estimate for the effect of an indicator of financial development on income inequality derived from regression results. This restriction excludes papers that provided descriptive statistics only, theoretical studies or systematic reviews. Fourth, we only consider published papers in peer-reviewed journals, which in comparison to unpublished manuscripts, they have passed a certain ‘quality’ check by reviewers. Thus, we eliminated unpublished articles on the finance–inequality nexus (e.g., Master theses, Doctoral dissertations, working papers, conference papers, book chapters).[99] Fifth, we eliminated studies that utilize interaction and

[99] When the current literature is well-established, mature and large, exclusion of unpublished studies is unlikely to affect the results (Stanley and Doucouliagos, 2012). Having this in mind, we include only published papers in

quadratic terms of financial development due to the difficulty of deriving the partial effects of financial development on income inequality.[100] Sixth, we requested a single-effect estimate, therefore we eliminated vector autoregression (VAR) and Granger causality studies.[101] Finally, for practical reasons, we excluded papers in languages other than English. In the end, we found 88 studies, with 1,357 estimates, that meet all of the aforementioned criteria. The list of studies that our current study utilizes in the meta-analysis is provided in Appendix. All estimates included in our sample were gleaned from regression models that examine the effect of at least one indicator of financial development (*FD*) on income inequality (*I*), conditional on a vector of control variables, W:

$$I_{ct} = \delta_0 + \beta_1 FD_{ct} + \sum_{k=1}^{K} \gamma_{kct} W_{kct} + \varepsilon_{ct}, \tag{4.1}$$

where *c* indexes the *c*th country and *t* indexes the time period *t* when panel data are used (the time dimension is removed in cross-sectional studies). We are interested in estimates of β_1 which measures the responsiveness of income inequality (*I*) to financial development (*FD*).

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peer-reviewed journals. Thus, 88 published studies are included in the final dataset, which is considered to be a large number in meta-analysis.

[100] We eliminate 14 studies using interaction terms and quadratic specifications from our data sample. In the following specifications (1): $I = \beta_0 + \beta_1 FD + \beta_2 (FD \times Z) + error$ and (2): $I = \beta_0 + \beta_1 FD + \beta_2 FD^2 + error$, I denotes income inequality and FD denotes financial development, the associated marginal effects are (1): $\frac{\partial I}{\partial FD} = \beta_1 + \beta_2 Z$ and (2): $\frac{\partial I}{\partial FD} = \beta_1 + \beta_2 FD$, we observe that neither produces the true partial effects of financial development on income inequality.

[101] According to Gunby et al. (2017, p.243): '*the empirical analysis in those studies [VAR and Granger causality studies] was often constructed for the purpose of developing impulse response functions, and thus did not report a cumulative, long-run impact with corresponding standard error'*.

However, measures of financial development and income inequality differ between studies. For instance, inequality measures include the Gini index, top and bottom income share, and other inequality indicators. The same is true for financial development indicators, which can be measured based on liquid liabilities, bank, stock market and other financial development measures. Therefore, to ensure comparability of estimates between diverse studies, we transform the estimates into partial correlations coefficients (PCCs) (Stanley and Doucouliagos, 2012). The PCC is calculated as follows:

$$PCC_{is} = \frac{t_{is}}{\sqrt{t_{is}^2 + df_{is}}} \tag{4.2}$$

where, PCC_{is} is the partial correlation coefficient from regression *i* in study *s*, t_{is} represents the corresponding *t*-statistic, and df_{is} corresponds to the number of degrees of freedom. Hence, the strength and direction of the relationship between financial development and income inequality, ceteris paribus, are represented by the partial correlation coefficient which can takes values between -1 and 1 ($-1 \leq PCC_{is} \leq 1$).[102]

We use the following formula to obtain the standard error for each PCC:

$$SE(PCC_{is}) = \sqrt{\frac{1 - PCC_{is}^2}{df_{is}}} = \frac{PCC_{is}}{t_{is}} \tag{4.3}$$

[102] When the estimate is based on an income inequality measure which is inversely related to inequality (i.e., less inequality would imply a greater value of that measure, e.g., bottom income share), we transformed the partial correlations and their corresponding t-statistics by multiplying them with (-1). With this transformation we are able to make all estimates comparable (no matter which income inequality measure they use), and thus the general interpretation of a positive partial correlation coefficient suggests a positive relationship between financial development and income inequality.

where $SE(PCC_{is})$ denotes the standard error of the partial correlation coefficient from regression i in study s, df_{is} corresponds to the number of degrees of freedom and t_{is} is the corresponding t-statistic.

The initial number of observations in our sample is 1,357 extracted from 88 studies. As we pointed out above, to calculate partial correlation coefficients we use *t*-values and *df*. The first two columns of Table 4.1 represent basic summary statistics of these two variables (*t*-values and *df*). The full sample of *t*-values has mean and median values of -0.071 and -0.22, respectively, and we note the minimum and maximum values of -22 and 8.979. Regarding the *df* variable, it has mean and median values of 306.512 and 167, with minimum and maximum values of 8 and 2857. This raises concern with outliers.

Table 4.1 Detailed summary statistics for t-values, *df* and PCCs

	t-values		*df*		PCCs	
	Full	Truncated	Full	Truncated	Full	Truncated
Mean	-0.071	-0.054	306.512	312.481	-0.007	-0.007
Median	-0.22	-0.22	167	172	-0.014	-0.014
Minimum	-22	-13.91	8	8	-0.914	-0.656
Maximum	8.979	8.825	2857	2857	0.916	0.582
Std. Dev.	2.971	2.811	400.84	402.893	0.25	0.227
1%	-7.13	-6.66	13	15	-0.664	-0.533
5%	-4.4	-4.321	21	24	-0.428	-0.409
10%	-3.361	-3.313	31	37	-0.312	-0.295
90%	3.64	3.6	662	683	0.307	0.299
95%	4.39	4.278	1454	1454	0.377	0.352
99%	6.871	6	1731	1731	0.589	0.499
Observations	1,357	1,329	1,357	1,329	1,357	1,329

Notes: This table provides detailed summary statistics for t-values, *df* and PCCs, as obtained from the full (n=1,357) and truncated (n=1,329) sample. The truncated sample eliminates from the full sample 28 observations having the top and bottom 1% of PCCs.

From Table 4.1, we also obtain the min (-0.914), max (0.916), mean (-0.007) and median (-0.014) of PCC. Nevertheless, large (absolute) values of PCC are potentially a problem because of the key role that PCC plays in determining the standard error, and hence, the weights used in the empirical analysis:

$SE(PCC_{is}) = \sqrt{\frac{1-PCC_{is}^2}{df_{is}}} = \frac{PCC_{is}}{t_{is}}$, with weights increasing in the absolute value of PCC. To account for this issue, we proceed by truncating the top and bottom 1% of PCC values, leaving 1,329 observations from 88 studies. The truncated distributions of *t*-statistic, *df*, and PCC values are also reported in Table 4.1, immediately to the right of the full sample statistics.

In Table 4.2, we provide basic summary statistics for the PCC. According to this table, the simple average of PCC regarding the relationship between financial development and income inequality equals to -0.007. Nevertheless, the above simple estimator for the underlying effect of financial development on inequality suffers from two main caveats. First, for the calculation of the simple average, each PCC carries the same weight regardless of the sample size from which it was extracted, therefore it does not take into account the precision of the estimate. Second, this simple estimator does not account for potential publication selection, which can bias the reported effect. Borenstein et al. (2011) suggest that it is more appropriate to make use of the fixed effects and random effects models to obtain the mean of PCCs in a more accurate way. The fixed effects approach weights the partial correlation coefficients by the inverse of their estimated variance. The obtained average is -0.005. The use of random-effects approach has an additional advantage as it additionally accounts for between-study heterogeneity (i.e., it takes into consideration the different methodology that studies apply to estimate the effect of financial development on income inequality). The random effects model yields the estimates of -0.005, which broadly confirm the findings of the previous two methods.

Table 4.2 Partial correlation coefficients for the effect of financial development on income inequality

Number of estimates		1,329	
Averages	PCC	95% CI	
Simple average	-0.007	-0.019	0.006
Fixed effects	-0.005	-0.008	-0.002
Random effects	-0.005	-0.014	0.004

Notes: The table provides basic summary statistics for the PCC. PCC represents the estimated partial correlation coefficient for the effect of financial development on income inequality. Simple average: the arithmetic mean, Fixed effects: weights the PCCs by the inverse of their variance, Random effects: weights the PCCs by the inverse of their variance and accounts for between-study heterogeneity, 95% CI: 95% Confidence Interval.

Table 4.2 displays that the mean of PCCs for the effect of financial development on income inequality is negative and significantly different from zero at the 5% level only in the case of the fixed-effects estimate. Doucouliagos (2011) provides guidelines for interpreting partial correlations in economics and suggests a strong effect if $|PCC| > 0.327$, a medium effect if $0.173 < |PCC| \leq 0.327$, a small effect if $0.070 < |PCC| \leq 0.173$, and no effect at all if $|PCC| \leq 0.070$. Thus, the above results (Table 4.2) demonstrate no effect of financial development on income inequality.

Nevertheless, there are two main drawbacks in the above preliminary assessments (Table 4.2) and need to be treated with caution. First, these simple estimators do not take into consideration the possibility that our findings are influenced by publication bias. Second, these estimates do not properly account for the heterogenous methodologies of primary studies. Although the random effects model accounts for between-study heterogeneity, it assumes that the differences among the underlying effects are random, which does not have to be realistic. In the following two sections we discuss both issues, where we further develop our estimation approach towards identifying the underlying effect of financial development on income inequality.

4.4 Testing for publication bias

The issue of publication bias, also known as the 'file drawer problem' (Rosenthal, 1979), arises when among researchers, referees, or editors there is a preference for publishing results that either support a particular theory or are statistically significant. Consequently, publication bias threatens the economic interpretation and the validity of statistical findings.

The funnel plot represents a simple visual tool for the evaluation of publication bias. The funnel graph plots precision (i.e., the inverse of standard error) in the vertical axis and the partial correlation in the horizontal axis (Stanley and Doucouliagos, 2012). The funnel plot will be symmetrical if the reported estimates are free from publication bias. In the existence of publication bias the funnel plot will have an asymmetrical shape. The estimates with a smaller standard error (more precision) will be spread at the top of the graph, while estimates that are less precise form the bottom of the funnel.

In Figure 4.1, we show that the association between financial development and income inequality is heterogenous by plotting a ‘funnel’ graph of PCC against the inverse of its standard error (precision). Here, we can see that there is substantial disagreement among the primary studies about the size and direction of the effect of financial development on inequality. The distribution of results appears to be symmetrical; both positive and negative estimates are reported. Symmetry is an important characteristic in a funnel plot as it indicates no clear visible sign of publication selection bias in the econometric studies of the effect of financial development on inequality. However, a funnel plot provides a visual examination that is largely subjective. Thus, the presence of publication bias in the underlying effect of financial development on inequality should be tested more formally.

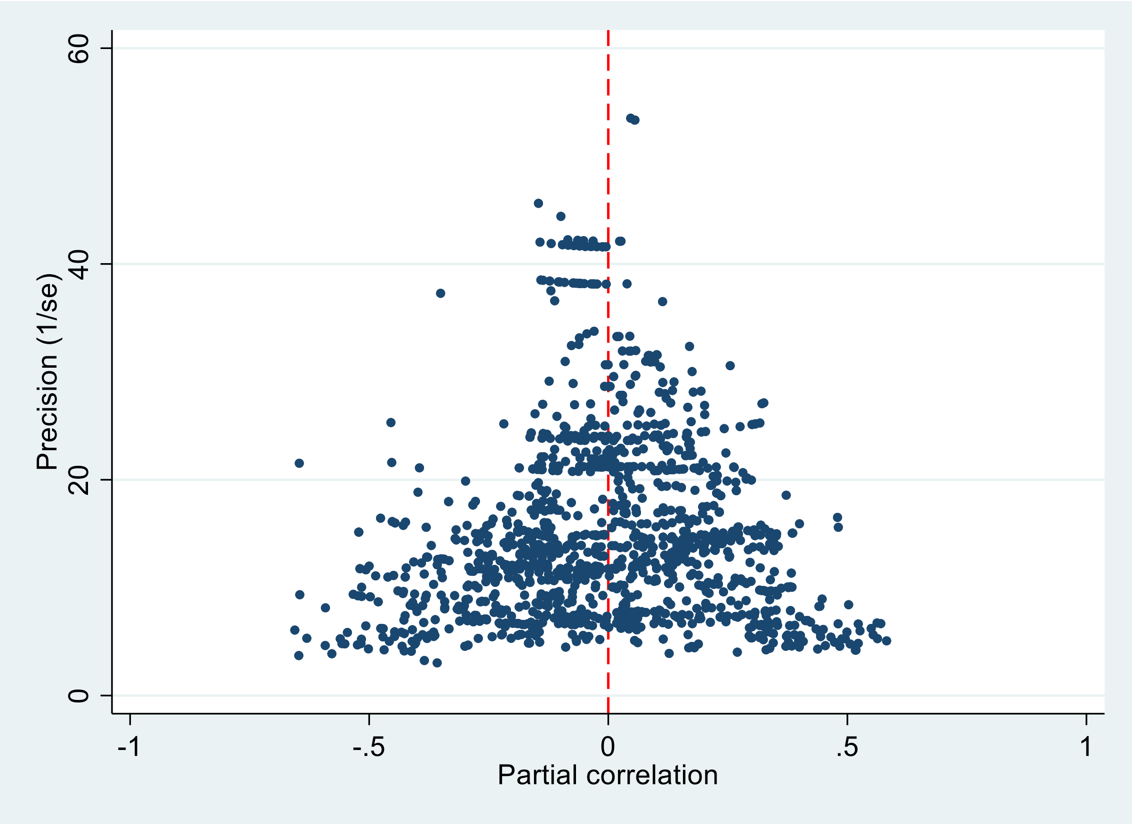

Figure 4.1 Funnel plot, partial correlations of financial development and inequality (*n* = 1,329).
Notes: Precision is calculated as 1/standard error of the partial correlation coefficient. Dotted line indicates position of a zero partial correlation.

To test for publication bias formally, we follow (Stanley, 2005, 2008) and conduct the funnel-asymmetry and precision-effect (FAT-PET) test, which is a simple meta-regression of the PCCs on their standard errors, as follows:

$$PCC_{is} = \alpha_0 + \alpha_1 SE(PCC_{is}) + e_{is} \tag{4.4}$$

where PCC_{is} is the partial correlation coefficients, $SE(PCC_{is})$ represents the corresponding standards errors and e_{is} denotes the regression error term. α_1 assesses the severity of publication bias. If publication bias exists in equation (4), there will be correlation between PCC and $SE(PCC)$. α_0 is the measure of mean, 'true' effect after correcting for publication bias. Accordingly, if α_0 is statistically significant, there is evidence that a genuine effect of financial development on inequality exists (Doucouliagos and Stanley, 2013).

Nevertheless, an issue may arise in equation (4.4), that is when the null hypothesis is rejected ($\alpha_0 \neq 0$), α_0 may be biased downward (Stanley, 2008). To address this issue, we follow Stanley and Doucouliagos (2014) who propose a non-linear version and conduct the precision effect estimate with standard error (PEESE) test, which regresses the PCCs on the square of the corresponding standard errors ($SE(PCC_{is})^2$), as follows:

$$PCC_{is} = \alpha_0 + \alpha_1 SE(PCC_{is})^2 + u_{is} \tag{4.5}$$

To address concerns of the apparent heteroscedasticity in equations (4.4) and (4.5), we follow the well-established literature and apply weighted least squares (WLS). To estimate the above equations, we use the Fixed Effects (FE) and Random Effects (RE) model. In the WLS-FE model, the main hypothesis is that the only reason the PCC values differ is because of sampling error. In this case, WLS-FE estimates can be obtained by dividing equations (4.4) and (4.5) by $1/\{SE(PCC_{is})^2\}$. Contrastingly, the WLS-RE procedure assumes that an additional source of differences is heterogeneity, along with sampling error. Thus, to utilize the WLS-RE model, we divide equations (4.4) and (4.5) by $1/\{SE(PCC_{is})^2 + \tau^2\}$, where the variance of the true effect is measured by τ^2.

Another issue that may arise when estimating equations (4.4) and (4.5) is potential endogeneity of the standard error, that is some method choices may affect both the estimate and the standard error in systematic way, thus introducing a form of bias in the coefficient of publication bias. To mitigate this issue, we follow previous meta-analyses (e.g., Cazachevici et al., 2020; Xue et al., 2021; Zigraiova et al., 2021) and utilize an instrumental variables (IV) approach using the inverse of the square root of the degrees of freedom as an instrument for standard error. The identification condition is that this quantity is by definition directly associated with the standard errors but is unlikely to be correlated with the choice of methodology applied.

Panel A of Table 4.3 presents the results of the FAT-PET test (equation (4.4)) using WLS-FE, WLS-RE and IV models. All the reported specifications utilize cluster-robust at the study level standard errors and the inverse of the number of estimates reported by each study as the weight. In the first two columns ('WLS-FE' and 'WLS-RE'), the null hypothesis: $\alpha_0 = 0$ cannot be rejected. Next, in the 'IV' regression, the reported coefficient equals to 0.062 and it is significantly different from zero at the 10% level. However, according to the classification of Doucouliagos (2011), this value (0.062<0.070) suggests no effect at all. The first two columns reject the null hypothesis: $\alpha_1 = 0$ at the 10% significance level and the third column rejects the null hypothesis at the 5% level, demonstrating the existence of publication bias. The negative publication bias coefficients suggest downward publication bias, demonstrating that the current literature favors the publication of negative impacts of financial development on income inequality (i.e., financial development is associated with lower income inequality). For the first two columns, the estimated coefficient of α_1 in absolute values is slightly below 1 (0.859 and 0.842, respectively) and according to the classification proposed by Doucouliagos and Stanley (2013), the magnitude of the selectivity is little to modest. Nevertheless, in the third column ('IV'), where we account for endogeneity bias, the absolute value of publication bias coefficient (α_1) is above 1 (1.049), which according to Doucouliagos and Stanley (2013) suggests that the selectivity is substantial.

Table 4.3 Linear and nonlinear techniques detect publication bias

Panel A: FAT-PET	WLS-FE	WLS-RE	IV
True effect (α_0)	0.050	0.045	0.062*
	(0.033)	(0.034)	(0.032)
Publication bias (α_1)	-0.859*	-0.842*	-1.049**
	(0.491)	(0.482)	(0.488)
Observations	1,329	1,329	1,329
Panel B: PEESE	**WLS-FE**	**WLS-RE**	**IV**
True effect (α_0)	0.025	0.003	0.026
	(0.020)	(0.019)	(0.020)
Publication bias (α_1)	-5.408**	-2.833	-5.794**
	(2.711)	(2.244)	(2.816)
Observations	1,329	1,329	1,329

Panel C: Advanced	Kinked model	AK1 (symmetric)	AK2 (asymmetric)	Stem method
Mean effect	-0.008	-0.004	0.002	0.003
	(0.023)	(0.004)	(0.012)	(0.074)
Observations	1,329	1,329	1,329	1,329

Notes: Panel A & B: All models utilize the inverse of the number of estimates reported by each study as the weight. WLS = weighted least squares, FE = fixed effects, RE = random effects, IV = instrumental variables estimation, where the instrument for the standard error is the inverse of the square root of the degree of freedom. Panel C: Kinked model (Bom and Rachinger, 2019), AK1 and AK2 (Andrews & Kasy, 2019), stem method (Furukawa, 2020). Robust standard errors clustered at study level are shown in parentheses. Significance level is denoted by ** (5%) and * (10%).

Panel B of Table 4.3 displays the results of the PEESE test (equation (4.5)) using WLS-FE, WLS-RE and IV models. Once again, all the reported specifications utilize cluster-robust at the study level standard errors and the inverse of the number of estimates reported by each study as the weight. In all the reported specifications, the null hypothesis: $\alpha_0 = 0$ cannot be rejected. Nevertheless, in columns 'WLS-FE' and 'IV', we reject the null hypothesis: $\alpha_1 = 0$ at the 5% significance level, suggesting the existence of publication bias. In both cases, the absolute value of publication bias coefficient (α_1) is above 2 (5.408 and 5.794, respectively), which according to Doucouliagos and Stanley (2013) suggests that the selectivity is severe.[103]

In conjunction with the FAT-PET-PEESE tests employed above, we also conduct a number of recently established procedures by meta-analysts. In order to get the bias-adjusted true effect of financial development on income inequality, Panel C of Table 4.3 reports the results for three advanced methods. First, we perform the endogenous Kink (EK) technique

[103] In addition, we re-estimate equations (4.4) and (4.5) using WLS-FE, WLS-RE and IV with robust standard errors clustered at study level and an alternative weighting scheme: equal weights for each estimate. In all specifications the true effect (α_0) is statistically insignificant and close to zero. Nevertheless, the estimated publication bias coefficient (α_1) is negative in all cases, suggesting downward publication bias. In all specifications, according to the classification proposed by Doucouliagos and Stanley (2013), the magnitude of the selectivity is little to modest. The results of this exercise are available upon request.

(Bom and Rachinger, 2019) which constitutes a 'refinement' of the PET-PEESE test, as it tries to better fit the non-linearity of the relationship between the estimated effect and the SE in the existence of publication bias. In this technique, when the SE is very small there is no selective publication, while publication bias usually increases as the SE increases. In our case, the EK estimate equals to -0.008 and is statistically insignificant. Second, two advanced approaches (symmetric and asymmetric) to correct for publication bias are proposed by Andrews and Kasy (2019). The main difference between the symmetric and asymmetric estimator is that the former accounts for the selective publication on statistical significance, while the latter addresses the selective publication caused by both statistical significance and the sign of the estimates. In both cases, the findings of the AK estimators indicate that the true effect of financial development on income inequality is statistically insignificant and close to zero. Last, we make use of the stem-based method developed by Furukawa (2019), which is non-parametric estimator that focuses on the most precise studies. This method constitutes a generally conservative approach able to create a bias-corrected estimate that can operate under many different publication selection processes. The estimation of stem-based method yields a statistically insignificant mean effect of 0.003. In summary, the results of these advanced methods confirm that once the correction for publication bias is executed, the underlying effect of financial development on income inequality is close to zero in all of the methodological approaches: none passes Doucouliagos' (2011) bar for at least a small effect.

4.5 Heterogeneity analysis

4.5.1 Description of variables

Table 4.4 describes all the variables included for heterogeneity analysis that we gather from the primary studies. We group the variables as follows: the measure of income inequality, the measure of financial development, data characteristics, estimation methods, control variables, countries examined and publication characteristics. In Table 4.4, for each variable we provide the mean, the standard deviation, and the mean weighted by the inverse of the number of estimates reported per study. Before proceeding with the heterogeneity analysis, we performed multicollinearity diagnostic tests, which indicated no particular problems.

Table 4.4 Description and summary statistics of the regressors used in the heterogeneity analysis

Variable	Definition	Mean	SD	WM
	Effect size			
PCC	Partial correlation coefficient	-0.007	0.227	-0.022
SE	Standard error of the PCC	0.088	0.049	0.082
	Income inequality measures			
Gini	Dummy, 1 if dependent variable in primary regression is the Gini index, 0 otherwise	0.710	0.454	0.784
Top income	Dummy, 1 if dependent variable in primary regression is income share of the top quintile, 0 otherwise	0.045	0.208	0.028
Bottom income (Ref.)	Dummy, 1 if dependent variable in primary regression is income share of the bottom quintile, 0 otherwise	0.105	0.307	0.063
Other inequality	Dummy, 1 if other income inequality measure used (e.g., Theil index, Atkinson index, EHII Gini coefficient or income share of quintiles other than top and bottom), 0 otherwise	0.139	0.346	0.126
	Financial development measures			
Depth	Dummy, 1 if financial depth is used as a measure of financial development, 0 otherwise	0.202	0.401	0.228
Market	Dummy, 1 if a market-based variable is used as a measure of financial development, 0 otherwise	0.086	0.280	0.058
Bank (Ref.)	Dummy, 1 if a variable based on bank credit is used as a measure of financial development, 0 otherwise	0.645	0.479	0.590
Complex	Dummy, 1 if other indicator of financial development is used as indicator of financial development, 0 otherwise	0.068	0.251	0.124
Joint	Dummy, 1 if more than one financial development indicator is included in regression, 0 otherwise	0.130	0.337	0.070
	Data characteristics			
Cross section	Dummy, 1 if dataset is cross-section, 0 otherwise	0.189	0.392	0.158
Panel data (Ref.)	Dummy, 1 if dataset is panel, 0 otherwise	0.811	0.392	0.842
Time span	Logarithm of number of years in the sample	2.371	1.046	2.377
Number of variables	Logarithm of number of explanatory variables	2.007	0.364	2.043
Number of countries	Logarithm of number of countries in the sample	3.707	0.712	3.731
Log transformation	Dummy, 1 if logarithm transformation of dependent variable is applied, 0 otherwise	0.287	0.452	0.291
Control for endogeneity	Dummy, 1 if the primary study controls for endogeneity, 0 otherwise	0.581	0.494	0.487
	Estimation methods			
OLS	Dummy, 1 if OLS is used in the estimation, 0 otherwise	0.246	0.431	0.292
Fixed effects	Dummy, 1 if fixed effects is used in the estimation, 0 otherwise	0.186	0.389	0.231

Random effects	Dummy, 1 if random effects is used in the estimation, 0 otherwise	0.028	0.165	0.043
GMM	Dummy, 1 if GMM is used in the estimation, 0 otherwise	0.319	0.466	0.285
TSLS (Ref.)	Dummy, 1 if two-stage least squares is used in the estimation, 0 otherwise	0.262	0.440	0.203
	Control variables			
Lagged term	Dummy, 1 if the regression specification contains income inequality lagged, 0 otherwise	0.341	0.474	0.266
Liberalization	Dummy, 1 if liberalization (e.g., trade, openness, and foreign direct investment) included as explanatory variable, 0 otherwise	0.591	0.492	0.607
Income level	Dummy, 1 if income level (or GDP) included as explanatory variable, 0 otherwise	0.488	0.500	0.533
Inflation	Dummy, 1 if inflation included as explanatory variable, 0 otherwise	0.454	0.498	0.509
Democracy	Dummy, 1 if democracy variable included as explanatory variable, 0 otherwise	0.137	0.344	0.174
Government spending	Dummy, 1 if government expenditures included as explanatory variable, 0 otherwise	0.479	0.500	0.426
Education	Dummy, 1 if education variable included as explanatory variable, 0 otherwise	0.547	0.498	0.549
	Countries examined			
Developed	Dummy, 1 if only developed are included in the sample, 0 otherwise	0.062	0.241	0.088
Mixed	Dummy, 1 if both developing and developed countries are included in the sample, 0 otherwise	0.544	0.498	0.556
Developing (Ref.)	Dummy, 1 if only developing are included in the sample, 0 otherwise	0.394	0.489	0.357
	Publication characteristics			
Publication year	Logarithm of publication year	7.609	0.003	7.609
Impact factor	Recursive impact factor of journal from RePEc	0.218	0.411	0.188
Non-economic journal (Ref.)	Dummy, 1 if published in non-economic journals (demography, development, sociology, etc.), 0 otherwise	0.149	0.356	0.136
Economic journal	Dummy, 1 if published in economics journals, 0 otherwise	0.851	0.356	0.864
Citations	Logarithm of number of Google Scholar citations	3.690	1.849	3.217

Notes: The recursive impact factor of the outlet from RePEc was collected in March 2021. SD = standard deviation; WM = mean weighted by the inverse of the number of estimates per study; Ref. = reference category.

First, the category 'Income inequality measures' classifies the different dependent variables (inequality measures) that the primary studies use. The Gini coefficient is utilized in 71% of the cases in our sample. Income inequality is also measured using income share of top (10.5% of cases) and bottom quantile (4.5% of cases). The last measure of inequality 'Other inequality' captures other inequality measures that are used in the literature, such as Theil index, Atkinson index, EHII Gini coefficient and income share of quintiles other than top and bottom.

Since financial development is a multidimensional concept (Beck et al., 2000; Asongu, 2014), the current studies use various proxies to capture financial development. The simplest proxies of financial development that the literature uses are the money supply (% of GDP) and the ratio of liquid liabilities to GDP, which are commonly expressed as financial depth. The first dummy we use corresponds to studies that use financial depth as a proxy of financial development. To capture information about the efficiency, activity and size of the banking sector we use a dummy for studies that use a bank measure (e.g., bank credit and domestic/private credit to GDP) to capture bank development. A 64.5% of the estimates use a bank measure as a proxy of financial development. Additionally, some studies use market-based measures of financial development, such as stock market capitalization and turnover. Therefore, we create a third dummy for studies that utilize market-based proxies. Finally, we utilize a fourth dummy ('Complex') for the studies that incorporate proxies that are not included in the above three dummies.

The first variables that we use in the category of data characteristics are whether the data that the primary study makes use is cross section or panel data, and the number of years in the sample. To account for data characteristics in the regression model of different studies, we incorporate the number of countries, the number of explanatory variables (excluding dummy variables used for fixed effects), countries included in the regression, and whether the primary study uses a log transformation of the dependent variable. Additionally, we utilize a dummy variable that takes value one if the study control for the endogeneity in the regression. In our sample, 58% of the estimates utilize methods that account for endogeneity.[104]

In the category estimation methods, we account for the different estimation models that the primary studies utilize in order to estimate the effect of financial development on income inequality. While many studies use the GMM (generalized method-of-moments) or two-stage least squares (TSLS), some studies employ panel fixed effects, random effects and OLS. Therefore, we classify the following categories: OLS, Fixed effects, Random effects, GMM and TSLS.

To account for various control variables (e.g., macroeconomic, socioeconomic, political, and country context) that previous literature utilizes in the regression model, in the 'Control variables' category we create a list of dummies for the most-used control variables in our sample. In more than one-half of the cases in our sample, primary studies use control variables to capture the liberalization process and educational level. In addition, income level (49%), inflation (45%) and government spending (48%) are also used. The lagged term of the dependent variable is used in 34% of the cases. Finally, researchers sometimes include a variable to capture the level of democracy.

The underlying country sample and its level of development are possible to play a role on the finance-inequality relationship. For instance, the characteristics of the banking sector differ greatly between developed and developing countries, which can affect the results of primary studies. We thus account for whether an estimate is based on a country sample of

[104] According to our sample, two methods are designed to correct for endogeneity, the generalized method of moments (GMM) estimator and two stages least squares (TSLS). As highlighted by Claessens and Perotti (2007), endogeneity (e.g., reverse causality between financial development and income inequality) is an important issue in this literature, along with issues related to measurement and statistical power.

advanced (developed) economies, developing/emerging economies or a mix of advanced and developing countries according to the International Monetary Fund (IMF) classification (IMF, 2018).

In the last category ('Publication characteristics'), as it is shown in Table 4.4, we create a set of variables to evaluate if differences on publication characteristics between primary studies matters significantly for the effect of financial development on income inequality. First, to ascertain if there is a potential time trend, we use a variable which captures the year of publication of the study ('Publication year'). Additionally, to account for journal quality we use the following two variables: 'Impact factor', which represents the recursive impact factor from RePec and 'Citations' as measured by Google Scholar. Finally, we utilize the dummy variable 'Economic journal' to assess if there is a systematic difference in the reported estimates of published papers in economics journals compared to non-economics journals.

4.5.2 Methods

To investigate if systematic differences between studies significantly matter for the relationship between financial development and income inequality, we expand equation (4.4) by incorporating various study characteristics which capture the heterogeneity among the primary studies:

$$PCC_{is} = \alpha_0 + \alpha_1 SE(PCC_{is}) + \sum_{k=1}^{K} \alpha_{k+1} X_{isk} + \varepsilon_{is} \tag{4.6}$$

where X_{isk} includes the regressors listed in Table 4.4 capturing specific characteristics from regression *i* in study *s*. To account for heteroskedasticity in the regression, we divide equation (6) by $SE(PCC_{is})$:

$$\frac{PCC_{is}}{SE(PCC_{is})} = \alpha_0 \frac{1}{SE(PCC_{is})} + \alpha_1 + \sum_{k=1}^{K} \alpha_{k+1} \cdot \frac{1}{SE(PCC_{is})} \cdot X_{isk} + \varepsilon_{is} \frac{1}{SE(PCC_{is})} \tag{4.7}$$

To conduct our heterogeneity analysis, we consider 36 potential regressors (see above, Table 4.4). However, a fundamental issue in estimating equation (4.7) using a simple OLS regression is model uncertainty associated with the included variables. That is, the inclusion of wrong variables in the above equation may lead to misspecification bias and invalid inference. To account for this aforementioned challenge, we follow previous meta-analyses (e.g., Havranek and Irsova, 2017; Havranek et al., 2017, 2018, 2018b, 2018c; Bajzik et al., 2020; Cazachevici et al., 2020; Zigraiova et al., 2021) and apply Bayesian model averaging (BMA; Hoeting et al., 1999).

4.5.3 Results

Figure 4.2 visualizes the graphical results of BMA estimation. The vertical axis displays the regressors in descending order according to their posterior inclusion probabilities (PIPs). Each column depicts a specific regression model sorted from left to right according to the posterior model probability (PMP). The sign of the corresponding regression coefficient is displayed based on the color of the individual cell. According to Figure 4.2, a cell with color blue illustrates that the included variable has a positive effect, while a cell with red color indicates that the included variable has a negative effect. The elimination of a regressor from the estimated model is depicted with a blank cell in Figure 4.2.

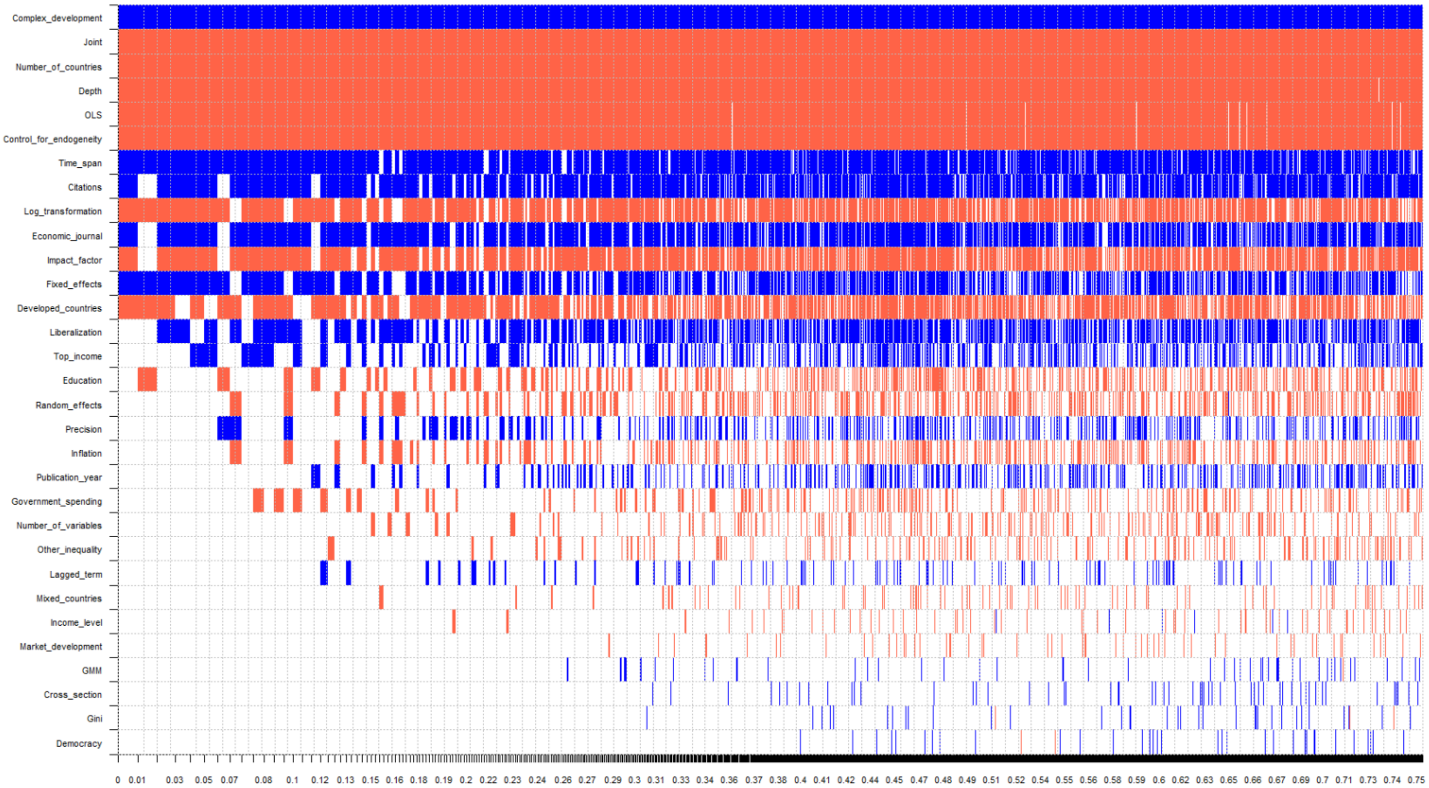

Figure 4.2 Model inclusion in Bayesian model averaging.

Notes: The figure visualizes the results of Bayesian Model Averaging (BMA). All regressors are listed and defined in Table 4.4. The vertical axis displays the regressors in descending order according to their posterior inclusion probabilities (PIPs). Cumulative posterior model probabilities (PMPs) are measured in the horizontal axis. Each column depicts a specific regression model sorted from left to right according to the PMP. The sign of the corresponding regression coefficient is displayed based on the color of the individual cell. A cell with color blue illustrates that the variable entails a positive effect, i.e., it demonstrates that the estimated effect of financial development on income inequality in primary studies is larger, while a cell with red color indicates that the included variable has a negative effect. An empty cell denotes that the variable is eliminated from the regression model. The results are based on the specifications that use the inverse of SE as the weight.

The first column ('BMA') of Table 4.5 reports the results of BMA estimation, i.e., the posterior mean, the standard deviation, and the PIP of each regressor. Fourteen variables are found to have PIPs above 0.50, indicating that they significantly matter for the estimated effect of financial development on income inequality in the primary studies.

Table 4.5 Explaining the heterogeneity in the effect of financial development on income inequality

	BMA			FMA			OLS		
	Post mean	Post St. Dev.	PIP	Coef.	St. Error	p-value	Coef.	St. Error	p-value
Gini	0.000	0.003	0.024	-0.009	0.021	0.664			
Top income	0.028	0.039	0.395	0.065	0.033	0.051			
Bottom income (Ref.)									
Other inequality	-0.003	0.011	0.107	-0.051	0.029	0.076			
Depth	-0.071	0.016	**1.000**	-0.066	0.015	0.000	-0.072	0.027	0.009

Market	-0.001	0.007	0.036	-0.024	0.023	0.282			
Bank (Ref.)									
Complex	0.115	0.023	**1.000**	0.126	0.026	0.000	0.123	0.040	0.003
Joint	-0.098	0.022	**1.000**	-0.099	0.023	0.000	-0.094	0.034	0.007
Cross section	0.001	0.007	0.028	0.041	0.032	0.207			
Panel data (Ref.)									
Time span	0.032	0.014	**0.906**	0.035	0.010	0.001	0.033	0.013	0.013
Number of variables	-0.005	0.014	0.125	-0.028	0.020	0.167			
Number of countries	-0.052	0.015	**1.000**	-0.059	0.015	0.000	-0.063	0.019	0.001
Log transformation	-0.042	0.025	**0.809**	-0.048	0.016	0.004	-0.049	0.024	0.046
Control for endogeneity	-0.114	0.040	**0.994**	-0.112	0.028	0.000	-0.099	0.051	0.054
OLS	-0.136	0.045	**0.995**	-0.128	0.033	0.000	-0.114	0.051	0.028
Fixed effects	0.060	0.043	**0.717**	0.072	0.030	0.017	0.079	0.051	0.124
Random effects	-0.029	0.048	0.307	-0.029	0.037	0.430			
GMM	0.001	0.005	0.032	-0.014	0.021	0.504			
TSLS (Ref.)									
Lagged term	0.003	0.010	0.098	0.034	0.021	0.109			
Liberalization	0.026	0.022	**0.645**	0.049	0.017	0.005	0.030	0.016	0.065
Income level	-0.001	0.004	0.040	-0.005	0.013	0.694			
Inflation	-0.008	0.016	0.260	-0.021	0.015	0.146			
Democracy	0.000	0.002	0.016	0.006	0.016	0.728			
Government spending	-0.005	0.011	0.174	-0.017	0.013	0.203			
Education	-0.010	0.016	0.323	-0.013	0.014	0.343			
Developed	-0.043	0.033	**0.708**	-0.076	0.026	0.004	-0.059	0.028	0.038
Mixed	-0.002	0.008	0.057	-0.024	0.018	0.172			
Developing (Ref.)									
Publication year	0.009	0.018	0.234	0.015	0.022	0.498			
Impact factor	-0.052	0.032	**0.796**	-0.074	0.024	0.002	-0.059	0.025	0.021
Non-economic journal (Ref.)									
Economic journal	0.037	0.022	**0.803**	0.044	0.017	0.010	0.050	0.016	0.002
Citations	0.013	0.007	**0.837**	0.017	0.005	0.002	0.017	0.007	0.014
Precision	0.009	0.016	0.285	0.018	0.018	0.306	0.015	0.015	0.308
Publication bias	0.237	NA	1.000	0.172	0.322	0.592	0.292	0.177	0.102
Number of observations		1,329			1,329			1,329	
Number of groups		88			88			88	

Notes: All models use the inverse of standard error as the weight. OLS includes variables that have a PIP of above 0.5, according to BMA, with robust standard errors clustered at study level. Regressors with PIP above 0.5 are highlighted in bold.

The rule of thumb provided by Kass and Raftery (1995) allows us to interpret the effect size of PIPs. The significance of each regressor is weak if $0.50 \leq PIP < 0.75$, positive if $0.75 \leq PIP < 0.95$, strong if $0.95 \leq PIP < 0.99$, or decisive if $PIP \geq 0.99$. Therefore, according to PIPs reported in the BMA results, we find a decisive effect for six regressors, namely: Depth, Complex, Joint, Number of countries, Control for endogeneity and OLS. Moreover, the results indicate a positive effect for five variables, namely: Time span, Log transformation, Impact factor, Economic journal and Citations. Last, we observe a weak effect for three variables, namely: Fixed effects, Liberalization and Developed. We provide a detailed discussion of these results below.

In conjunction with our baseline BMA estimation, we provide two robustness checks. First, we apply frequentist model averaging (FMA). In comparison to BMA, FMA have two main advantages. First, it does not require the necessary use of explicit priors. Second, it utilizes a more optimal process of estimation. We use Mallow's criteria as weights since they were shown to be asymptotically optimal (Hansen, 2007). In addition, we utilize Amini and Parmeter's (2012) orthogonalization of the covariate space to lessen the number of estimated models. The results of FMA are also reported in the second column ('FMA') of Table 4.5, immediately to the right of the BMA results. Second, we utilize ordinary least squares (OLS) and include the variables with PIPs above 0.5 from our BMA results. The OLS results are presented in the last column ('OLS') of Table 4.5. Both these additional results (FMA and OLS) generally support our baseline results of BMA.

The measure of financial development

Regarding the different measures of financial development, our BMA findings show that the studies that use complex measure as an indicator of financial development tend to report a more positive impact of financial development on income inequality. On the contrary, the studies that use financial depth to capture financial development and those that include more than one indicator of financial development seem to reduce the reported effect. This finding is in line with previous studies (Jeanneney and Kpodar, 2011; Asongu and Tchamyou, 2014; Tchamyou et al., 2019), which point out that compared to other financial development indicators, financial depth contributes to pushing down income inequality.

Data and estimation characteristics

With regards to data characteristics, our results highlight that the log transformation of the dependent variable and the number of countries used are associated with reporting lower estimated effects. However, a larger number of years examined in the primary studies is associated with reporting larger estimated effects. Correcting for endogeneity is also important ($PIP > 0.99$), our results indicate the studies that do not correct for endogeneity tend to overestimate the effect of financial development on income inequality. With regards to estimation characteristics, the results suggest that the studies that use fixed effects in the estimation are associated with reporting a greater effect of financial development on inequality. On the contrary, the studies that use OLS tend to report smaller effects of financial development on income inequality.

Control variables

Regarding the control variables that each study utilizes, our results highlight that studies that investigate the association between financial development and income inequality should include in their models a variable which captures the liberalization process. Thus, controlling for the liberalization process, the effect of financial development on income inequality becomes stronger (i.e., more positive). This finding is consistent with the meta-analysis conducted by Heimberger (2020), who finds that trade and financial globalization have a (small-to-moderate) inequality-increasing effect.

Countries examined

With regards to the composition of countries included in each study, we find that financial development has a greater beneficial effect on income inequality in developed compared to developing countries. This result is consistent with several empirical studies (Zhang and Nacuer, 2019; Altunbas and Thornton, 2020; Chakroun, 2020), which highlight that developed countries have more beneficial finance-inequality effects in comparison with the rest of the world.

Publication characteristics

As for publication characteristics, a higher recursive impact factor is associated with smaller reported estimates. Conversely, we find that the number of citations is associated with reporting larger estimated effects. We also show that, in comparison to non-economics journal, the primary studies that have been published in economics journals tend to report larger effects of financial development on income inequality.

Furthermore, in Table 4.6, we probe the robustness of our baseline BMA results. Specifically, we utilize BMA estimation using alternative weights, namely: no weights (unweighted), and using the inverse of the number of estimates reported by each study as the weight. These robustness checks are largely in line with our baseline results reported above (Table 4.5). It is also worthy to mention that the above results highlight that additional variables might also matter for the estimated effect of financial development on inequality as they have a PIP greater than 0.5, however, we choose to stay on the conservative side of the meta-analysis literature which overwhelmingly recommends the inverse of standard error as the weight (instead of the alternative ones), and we do not consider these additional variables as important regressors.

Table 4.6 Robustness checks: alternative weights

	BMA – Unweighted regressions			BMA – Weighted by number of equations within study		
	Post mean	Post St. Dev.	PIP	Post mean	Post St. Dev.	PIP
Gini	-0.000	0.004	0.020	-0.001	0.007	0.048
Top income	0.086	0.052	**0.810**	0.002	0.013	0.046
Bottom income (Ref.)						
Other inequality	-0.002	0.010	0.059	-0.049	0.028	**0.830**
Depth	-0.070	0.015	**1.000**	-0.032	0.023	**0.730**
Market	0.000	0.003	0.016	0.002	0.010	0.059
Bank (Ref.)						

Complex	0.044	0.043	**0.588**	0.101	0.021	**1.000**
Joint	-0.123	0.023	**1.000**	-0.077	0.035	**0.898**
Cross section	0.003	0.012	0.069	-0.002	0.009	0.045
Panel data (Ref.)						
Time span	0.012	0.012	**0.576**	0.028	0.008	**0.980**
Number of variables	-0.002	0.009	0.060	0.013	0.022	0.304
Number of countries	-0.067	0.010	**1.000**	-0.037	0.008	**1.000**
Log transformation	-0.068	0.015	**0.999**	-0.084	0.014	**1.000**
Control for endogeneity	-0.173	0.019	**1.000**	-0.047	0.036	**0.759**
OLS	-0.194	0.022	**1.000**	-0.025	0.034	0.410
Fixed effects	0.002	0.010	0.045	0.171	0.030	**1.000**
Random effects	-0.039	0.053	0.406	0.001	0.009	0.039
GMM	0.000	0.002	0.017	0.001	0.008	0.055
TSLS (Ref.)						
Lagged term	0.000	0.003	0.019	0.009	0.018	0.248
Liberalization	0.003	0.009	0.101	-0.001	0.005	0.046
Income level	0.000	0.002	0.016	-0.001	0.004	0.046
Inflation	-0.002	0.008	0.091	-0.006	0.014	0.208
Democracy	0.000	0.002	0.015	0.002	0.007	0.061
Government spending	0.000	0.004	0.030	0.000	0.002	0.016
Education	-0.015	0.019	0.445	-0.054	0.014	**0.999**
Developed	-0.044	0.043	**0.587**	-0.028	0.032	0.488
Mixed	-0.001	0.006	0.045	0.000	0.002	0.018
Developing (Ref.)						
Publication year	0.035	0.610	0.022	0.000	0.001	0.023
Impact factor	-0.024	0.026	**0.525**	-0.006	0.014	0.211
Non-economic journal (Ref.)						
Economic journal	0.006	0.015	0.151	0.001	0.005	0.038
Citations	0.001	0.004	0.151	0.000	0.001	0.040
Precision	0.149	NA	1.000	0.023	NA	1.000
Publication bias	0.011	0.078	0.046	0.001	0.024	0.020
Number of observations		1,329			1,329	
Number of groups		88			88	

Notes: The results are based on the specifications with no weight and with weight being the inverse of number of estimates per study. PIPs above 0.5 are highlighted in bold.

As a final test, the results of BMA are used to obtain a predicted estimate of the effect of financial development assuming 'best study' characteristics. We *a priori* decide on a set of characteristics that form a 'best study'; that is, a study that is ideally designed to reliably estimate the effect of financial development on income inequality. We designated these to be: a study that incorporates the Gini index for the outcome variable, uses bank measure as its financial development indicator, is based on panel data, utilizes the GMM estimator, corrects for endogeneity bias, is based on a country sample of a mix of developing and developed countries, is published in economics journals, and includes the lag of the dependent variable and the set of control variables (liberalization, income level, inflation, democracy, government spending and education). Sample mean values were assumed for the joint variable (if more than one financial development indicator is included), time span, number of variables and countries, publication year, citations and the impact factor. The coefficient of publication bias was set equal to zero. The associated prediction, which represents the model-weighted average across the models estimated using BMA, was 0.023, with a standard error of 0.073. Therefore, even using 'best study' characteristics, we find that the effect of financial development on income inequality is close to zero.

4.6 Concluding remarks

In this study, we perform the first meta-analysis on the effect of financial development on income inequality. Although the distributional effects of financial development have attracted attention in recent studies, the literature has not reached a consensus and continues to produce estimates that differ widely. Using a sample of 1,329 estimates from 88 published studies and after correcting for publication bias, our findings indicate that the distributional effect of financial development is practically zero. In addition, we show that the primary studies in our sample suffer from modest publication bias: studies reporting a negative effect of financial development on income inequality (i.e., financial development reduces income inequality) are preferentially reported. Moreover, we examine whether various research characteristics of the primary studies drive the heterogeneity in the estimated effect of financial development. To account for the aforementioned challenges, we investigate more than 30 candidate variables and make use of Bayesian model averaging to address the inherent uncertainty surrounding the choice of regression specifications. Our quantitative analyses show that several characteristics matter robustly and explain the existence of heterogeneity in the primary studies regarding the relationship between financial development and income inequality.

Specifically, our results indicate that is important to account for the liberalization process in order to estimate the effect of financial development on income inequality accurately. More generally, the results suggest that the effect of financial development on inequality depends on the measurement of financial development (e.g., financial depth tends to reduce the reported effect). Furthermore, accounting for endogeneity in the regression matters for the relationship between financial development and inequality. Studies that ignore endogeneity commonly produce larger estimates of the financial development effect. Similarly, our findings indicate that primary studies using fixed effects in the estimation tend to report larger positive effects, while the studies that use OLS tend to report smaller effects of financial development on inequality. Finally, our results indicate that the estimated effects of financial development on inequality depend on the composition of countries included in the sample: the effect of financial development is systematically smaller in developed than in developing countries.

The findings of this study do not offer typical policy formulations but rather formulate recommendations on how to conduct future policy-relevant empirical research, in particular how to estimate the effect of financial development on income inequality accurately. Although our quantitative analyses show that financial development generates no measurable distributional effects, scholars, policymakers, and civil society will still need to discover much more about which components have a crucial influence on income inequality. In this regard, we believe that this meta-analysis will inspire research scholars to investigate other candidate factors which can affect the distribution of income. Extensive literature on the candidate factors of income distribution can enable policymakers to evaluate the current distributional policies and to make the right policy decisions in the future.

Finally, this meta-analysis examines the relationship between financial development and income inequality. The available theories in literature emphasize that financial development can have direct effects on other policy-relevant factors, such as economic growth and FDI. Therefore, this study can stimulate more research for addressing various other research questions regarding the effects of financial development.

Appendix 4

List of primary studies included in the meta-analysis:

1. Abiad, A., Oomes, N., & Ueda, K. (2008). The quality effect: Does financial liberalization improve the allocation of capital? Journal of Development Economics, 87(2), 270–282. https://doi.org/10.1016/j.jdeveco.2007.12.002
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3. Agnello, L., & Sousa, R. M. (2012). How do banking crises impact on income inequality? Applied Economics Letters, 19(15), 1425–1429. https://doi.org/10.1080/13504851.2011.631885
4. Ajide, K. B., Alimi, O. Y., & Asongu, S. A. (2019). Ethnic Diversity and Inequality in Sub-Saharan Africa: Do Institutions Reduce the Noise? Social Indicators Research, 145(3), 1033–1062. https://doi.org/10.1007/s11205-019-02122-y
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7. Altunbaş, Y., & Thornton, J. (2019). The impact of financial development on income inequality: A quantile regression approach. Economics Letters, 175, 51–56. https://doi.org/10.1016/j.econlet.2018.12.030
8. Altunbaş, Y., & Thornton, J. (2020). Finance and income inequality revisited. Finance Research Letters, 37. https://doi.org/10.1016/j.frl.2019.101355
9. Andres, A. R., & Ramlogan-Dobson, C. (2011). Is Corruption really bad for inequality? evidence from Latin America. Journal of Development Studies, 47(7), 959–976. https://doi.org/10.1080/00220388.2010.509784
10. Asongu, S. A. (2013). How do financial reforms affect inequality through financial sector competition? Evidence from Africa. Economics Bulletin, 33(1), 401–414. https://doi.org/10.2139/ssrn.2493335
11. Asongu, S. A. (2013). Investment and inequality in Africa: Which financial channels are good for the poor? African Finance Journal, 15(2), 43–65. https://doi.org/10.2139/ssrn.2493146
12. Asongu, S. A. (2015). The impact of mobile phone penetration on African inequality. International Journal of Social Economics, 42(8), 706–716. https://doi.org/10.1108/IJSE-11-2012-0228
13. Asongu, S. A., & Tchamyou, V. S. (2014). Inequality, finance and pro-poor investment in Africa. Brussels Economic Review, 57(4), 517-547.
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Conclusion

Chapter I of the present thesis connects the literature of political economic of international organization with labour economics. First, we argue that both IMF program participation and conditions have a detrimental effect on the shadow economy of the recipient countries. A more detailed analysis which disaggregates IMF conditions into structural conditions, which concern a wider range of reforms in the domestic economy (microeconomic reforms) and afford governments less flexibility in the construction of policy reforms, and quantitative conditions, which take the form of quantitative targets that countries have to meet and provide governments more flexibility, shows that only structural conditions are significantly related to a larger shadow economy both in the short- and long-term. Second, we show that IMF loan-quota ratio, the sum of all IMF loan programs a country is eligible to as a share of its quota at the IMF, and program participation both have a detrimental effect on the unemployment rate. We provide evidence that significant short-run adverse effects hold robust in the long-run. Moreover, our findings point out that IMF conditions – policy reforms included within the program increase the unemployment rate of borrowing countries. In the last subchapter of Chapter I, we provide evidence that IMF lending programs worsen income distribution. However, our findings point out that this adverse effect does not hold for some targeted IMF programs, namely PRGF and ECF, which allow the involvement of civil society to their design and grant governments larger scope in negotiating the policy conditions.

In each of the above subchapters, we explicate the pathways through which IMF lending programs directly and indirectly affect the labour market outcomes of borrowing countries. More specifically, with regards to the unemployment rate, we show that IMF conditions, which are close related to policy reforms on the labour market, have a direct adverse effect on the unemployment rate of countries at least in the short-term. Moreover, our findings indicate that IMF programs can indirectly increase the unemployment rate through their effect on other channels, such as economic growth, labour rights, income inequality and political stability. In addition, in this case, a clearer indirect channel is through fiscal consolidation measures implemented through IMF loan programs. These measures, independent of IMF programs, have already been documented to increase unemployment (Agnello and Sousa, 2014; Agnello et al., 2014) via the decrease in government consumption or the cut in government investment. With regards to the shadow economy, we posit that the pathways through which IMF programs operate and affect the shadow economy of borrowing countries are mainly indirect and have to do with the structure of the economy (e.g., reduced state capacity), the labour market (e.g., reduced labour rights), fiscal balance pressures and other channels outside of the conditions, such as IMF policy advice, moral hazard and the domestic political environment of borrowing countries. Lastly, with regards to income distribution, our theoretical considerations that the indirect pathways through which IMF programs worsen income inequality are associated with budget deficit reductions (e.g., through augmented fiscal revenue or/and decrease in public expenditure), privatization of SOEs, reductions in pensions, employment protection and more flexible labour markets.

Furthermore, it is worth mentioning that some IMF programs, which afford governments more flexibility in the construction of policy reforms and allow the involvement of civil society to their design, do not exert a detrimental effect on the shadow economy and income inequality (as we show in subchapters 1.1 and 1.3). Thus, from a policy perspective, our findings point out that if the IMF is serious about the potential detrimental effects on the labour market outcomes (shadow economy and income inequality) that its lending programs may induce, then it needs to carefully consider the type of programs and the conditions included in these lending programs.

Chapter II investigates the effect of temporary work on individual's employment stability in the case of the Netherlands, a developed country with relatively high shares of temporary employment among the EU countries (Eurostat, 2018a) and unique labour market institutions (De Lange, 2013; De Jong et al., 2007; Fagan and Ward, 2003). The main finding of the second chapter suggest that temporary contracts are negatively associated with employment stability. Nevertheless, this adverse effect can be mitigated for temporary employees who have developed their professional skills through training programs.

The findings of the second chapter provide important social implications. First, they suggest that the employment prospects of temporary employees for job stability are poor. This finding is in line with the strand of the literature which argues that temporary work does not allow for the prospect of ongoing employment (Kalleberg et al., 2000; Kalleberg, 2003). As we argue, a possible explanation for this detrimental effect is because temporary employees are less likely to be committed to the organization and the knowledge they obtained is lost when they find employment elsewhere (Aldrich and Ruef, 2006), thus employers have fewer incentives to invest in the human capital of temporary workers (Mattijssen and Pavlopoulos, 2019). However, our analysis delves deeper on the nexus between temporary work and employment stability by considering the level of skills and knowledge of temporary employees. As such, we show that the adverse effect of temporary work on employment stability can be mitigated for temporary employees who have already developed their professional skills through training programs. This finding highlights the fundamental human capital theory (Becker, 1964), which suggests that investments in highly-skilled job-seekers can yield returns in the future. Thus, providing training opportunities to temporary employees, can improve their skills and knowledge and make them a 'good investment' for future employers.

In Chapter III, we place the spotlight on the effect of performance pay schemes on individuals' job satisfaction using a sample of countries from the EWCS 2010–2015. The main findings of the third chapter indicate that workers who are employed on either department or company or individual performance pay job schemes are more satisfied with their job than workers who are paid by the fixed amount. Furthermore, Chapter III links the literature of labour economics to health economics by examining how self-reported health-related issues, specifically sleep quality and work-related exhaustion, with participation in performance pay schemes co-determine job satisfaction. As such, we show that this positive effect is stronger for performance pay workers who have better sleep quality and experience less work-related exhaustion.

The main findings of the third chapter contribute to the empirical literature which argues that performance pay schemes are positively associated with job satisfaction. As we explicate in this chapter, one of the theoretical considerations that leads to this positive relationship is through the rewarding pay system and the extra form of gift they reach working under this scheme, thus generating a job satisfaction enhancing link between effort and reward. To empirically test the above theoretical consideration, we use two self-reported health-related indicators, specifically sleep quality and work-related exhaustion which both have been documented to cause decreased motivation and task interest, and thus make it difficult for employees to maintain performance levels (e.g., Totterdell et al., 1994; Valent, 2002). Thus, our analysis investigates how performance pay schemes affect job satisfaction conditional on self-reported sleep quality and work-related exhaustion. As we expected, the findings indicate that performance pay workers which have better sleep quality and experience less work-related exhaustion are more likely to be more satisfied with their jobs. Nevertheless, Chapter III points out that except from performance pay schemes, motivation, effort and task interest are some additional determinants that positively affect job satisfaction. Therefore, factors that can negatively affect one of these determinants (motivation, effort and task interest), such as poor sleep quality and increased work-related exhaustion, also lead to reduced job satisfaction.

The last chapter (Chapter IV) of the thesis provides for the first time a meta-analysis of the empirical literature which examines the effect of financial development on income inequality. The current literature on the relationship between financial development and income inequality lacks a clear view. Given the diversity of findings in this specific literature, we perform a meta-analysis of 1,329 estimates reported in 88 studies to assess the true effects of financial development on inequality. Our findings indicate that the distributional effect of financial development is practically zero. In addition, publication bias exists in the primary studies of this literature, indicating that studies reporting a negative effect of financial development on income inequality (i.e., financial development reduces income inequality) are preferentially reported. Moreover, Chapter IV examines whether various research characteristics of the primary studies drive the heterogeneity in the estimated effect of financial development.

Chapter IV provides various interesting practical implications. First, the findings of our meta-analysis indicate that a political strategy that involves changes in the development of financial markets and institutions is unlikely to bring beneficial effects on income distribution. Thus, from a policy perspective, our findings do not offer typical policy implications but rather formulate recommendations on how to conduct future policy-relevant empirical research, in particular how to estimate the effect of financial development on income inequality accurately. Furthermore, while this meta-analysis indicates that financial development generates no measurable distributional effects, there are many other factors that affect income inequality. Therefore, we believe that our research can inspire scholars to investigate other candidate factors which can affect the distribution of income, and thus allow policymakers to evaluate their role for future targeted income distribution policies.

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