The Effect of Regulatory Harmonization on Cross-border Labor Migration: Evidence from the Accounting Profession*

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Abstract

The paper examines whether international regulatory harmonization increases cross-border labor migration. To study this question, we analyze European Union (EU) initiatives that harmonized accounting and auditing standards. Regulatory harmonization should reduce economic mobility barriers, essentially making it easier for accounting professionals to move across countries. Our research design compares the cross-border migration of accounting professionals relative to tightly-matched other professionals before and after regulatory harmonization. We find that international labor migration in the accounting profession increases significantly relative to other professions. We provide evidence that this effect is due to harmonization, rather than increases in the demand for accounting services during the implementation of the rule changes. The findings illustrate that diversity in rules constitutes an economic barrier to cross-border labor mobility and, more specifically, that accounting harmonization can have a meaningful effect on cross-border migration.

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1. Introduction

In recent years, we have witnessed a strong push towards global convergence of rules in many areas of regulation. These initiatives are often intended to ease cross-border investments and to improve the allocation of capital (e.g., FSAP, 1999). Research in accounting and finance has analyzed whether regulatory harmonization indeed increases cross-border capital flows and has associated benefits such as increased liquidity and lower cost of capital (see Leuz and Wysocki, 2016, for an overview). Capital, however, is not the only factor of production for which diversity in rules could create economic barriers to mobility. Regulatory harmonization should also make it easier for professionals to seek employment outside of their home country, which in turn should improve the efficiency of labor markets. Indeed, labor mobility could be an important adjustment mechanism through which regions adjust to asymmetric economic shocks, especially in a currency union such as the Eurozone (Mundell, 1961; Farhi and Werning, 2014). However, there is no evidence on the role and potential benefits of regulatory harmonization for cross-border labor mobility.

In this paper, we analyze the effects of regulatory harmonization affecting the accounting profession in the European Union (EU) on cross-border labor migration. This setting has several desirable features from a research-design perspective. First, the accounting profession generally has a higher level of standardization than comparable occupations (Madsen, 2011), and regulatory harmonization has typically taken the form of adopting identical rules (or standards). Both factors should make it easier to detect an effect of regulatory harmonization on labor migration in the accounting profession, if there is one. Second, there is free movement of labor in the EU. Free movement of labor ensures that we can focus on regulatory harmonization and economic barriers, rather than immigration policies and other explicit restrictions. Third, there has been a relatively sharp increase in regulatory harmonization for the accounting profession in

the EU in recent years. In particular, two EU initiatives have substantially harmonized the rules relevant to those working in the accounting profession: (1) after mandatory reporting under IFRS publicly traded firms use identical accounting standards; (2) Directive 2006/43/EC harmonized statutory audits of companies' annual accounts and consolidated financial statements. We analyze changes in cross-border labor mobility around these regulatory changes. Henceforth, we refer to these changes also as "the treatment".

Our data are based on the EU's annual Labour Force Survey (LFS). The LFS is meant to generate a representative sample for each country using a standardized methodology, which substantially improves the comparability of mobility statistics across countries. In addition, the LFS data are collected at the individual level, giving us a rich set of demographics to control for other factors that affect migration. Following the literature, our main analysis is based on changes in a stock measure of migrants, i.e., the number of individuals that have a foreign nationality and were born abroad (Martí and Ródenas 2007; Bonin et al., 2008). To get closer to migration flows around regulatory changes, we also present analyses using a novel quasi-flow measure that starts from the stock measure but counts only foreigners that recently changed jobs, who are more likely to have moved in response to accounting and auditing harmonization.

Our identification strategy exploits that the regulatory changes primarily affect the accounting profession. Thus, we perform a difference-in-differences estimation comparing changes in cross-border mobility of accounting professionals with changes in mobility of other professions around regulatory harmonization. We estimate the effects relative to three separate control groups: legal professionals, all other professionals, and a combination of business people. We control for demographic characteristics known to determine migration (i.e., gender, marital status, age, education level, and the presence of children), including all possible interactions of these characteristics in order to account for non-linearities in these categorical demographics. In

addition, we estimate the effects *within country and year* to account for unrelated changes and shocks affecting labor mobility of professionals (e.g., changes in economic growth, unemployment benefits, national adjustments to survey methodology, etc.). To further tighten our design, we perform a double-matched difference-in-differences analysis. We pair accounting and control professionals from a given country by the exact same characteristics (e.g., single males, in Germany, between 25-29 years old, without children, with a university degree) for a year in the pre-treatment period *and* a year in the post-treatment period, creating a quadruplet. We then compute the relative change in mobility rates *within each quadruplet*. This double-matched approach assures perfect overlap in characteristics across treatment and control groups as well as across time and hence controls for composition changes in the survey sample.

Using the above setting and design, we find that cross-border labor migration increases for accounting professionals relative to matched professionals around the EU harmonization of accounting and auditing standards. The estimated increase in labor mobility is fairly similar across specifications and control groups. In our preferred (and most restrictive) specifications, the magnitude is between 20% and 22% of the pre-treatment mobility rate. This percentage increase implies that 10,000 to 16,000 accounting professionals moved within the EU as a result of regulatory harmonization, which is economically significant.

An important challenge for estimating the cross-border mobility effects of regulatory harmonization is that even regulatory changes without harmonization could increase labor mobility simply by changing the demand for accounting services. To see this, consider the Sarbanes-Oxley Act (SOX), which is not intended to harmonize accounting and auditing in the U.S.[1] Nevertheless, SOX likely increases labor mobility in the U.S. due to changes in the

[1] We thank one of our reviewers for providing this example. We note that regulatory harmonization can also change demand (e.g., by lowering the wages for accountants). As such knock-on effects are still the result of harmonization, we do not attempt to separate them in our empirical analyses (see Section 2).

demand for accounting services leading to new matches between employers and accountants (e.g., with respect to internal control skills). Such demand effects could confound our analysis and overstate the cross-border mobility effects due to harmonization.

However, a non-harmonizing regulatory change such as SOX should affect both the demand for domestic and foreign accountants. For example, new matching of accountants and employers can take place across borders but also domestically. In contrast, cross-border *harmonization* effects should be specific to foreigners. Thus, we introduce controls for changes in *domestic* job mobility to absorb demand effects. Doing so hardly changes the estimated treatment coefficient, which is inconsistent with a demand explanation for our results.

Next, we perform a series of robustness tests that evaluate other design assumptions and data challenges. First, our analysis assumes that the mobility trends in the accounting profession would have been parallel to those in the control groups had there been no regulatory harmonization in accounting. To assess the validity of this assumption, we provide graphical evidence that the pre- period trends are similar. We also show that pre-treatment mobility rates are close once we control for demographic characteristics. In addition, we consider several potential violations of the parallel-trends assumption, including differential changes in cross-border student mobility as well as licensing rules, and find that they cannot explain our results.

Second, we face two major data constraints. One constraint is that the LFS dataset does not provide researchers with job codes at the most granular level. Hence, we cannot perfectly identify accountants and auditors. For instance, the treatment group also contains personnel and career professionals. However, as long as the fraction of non-accountants in the treatment group does not systematically change around harmonization, which we verify to be the case, the difference-in-differences analysis takes care of this data issue.

The other LFS data constraint is that it is difficult to construct reliable flow measures of cross-border migration: low incidence rates cause a mini-domain problem (Purcell and Kish, 1980) and sampling techniques cause inconsistencies with population registers (Martí and Ródenas, 2007). We therefore base our main analysis on a stock measure and a novel quasi-flow measure using information about recent job changes. However, for these two measures, the timing of migration cannot be determined, yet naturally matters given our research question. Again, this data limitation should not affect the difference-in-differences analysis as long as the rate of earlier migration outside the relevant analysis window does not systematically change over time. We further mitigate this issue by using a quasi-flow measure, which has a time dimension based on job changes. In addition, we provide sensitivity tests using two flow measures, for which we can determine the exact timing of migration. While these tests confirm that flow measures have low incidence rates resulting in noisier estimates, we still find significantly positive mobility effects after regulatory harmonization among those individuals who are expected to be most responsive (i.e., singles without children, especially when they are young or work for large employers), which corroborates our main results.

Finally, we examine cross-country variation in the migration effects. We find that our results continue to hold when we restrict the analysis to EU-15 source and destination countries. The estimates are smaller but still significant, which is noteworthy because prior research finds that the migration response in the EU-15 is generally low, for instance, when it comes to labor market shocks (Bonin et al., 2008; Dao et al., 2014; OECD, 2014). Showing the results hold within the EU-15 also alleviates the concern that EU enlargement unduly influences our estimates. We also examine other sources of cross-country variation, such as the degree to which audit standards are harmonized, the fraction of public firms that adopt IFRS, licensing rules, and the market share of Big-4 auditors. While it is difficult to ex ante sign predictions for these

factors as well as to isolate their effects empirically, the evidence suggests that cross-border migration effects are stronger when there is more harmonization, licensing rules are less strict, and Big-4 auditors have a larger market share, consistent with the interpretation that regulatory harmonization drives our results.

Our paper makes several contributions to the literature. First, the literature on accounting harmonization focuses almost exclusively on informational effects in capital markets.[2] However, accounting harmonization potentially also affects the efficiency of labor markets, which would be economically important. Our study is the first to examine and document this effect. Our findings, which demonstrate relatively strong effects from accounting and auditing harmonization on cross-border labor mobility, may appear inconsistent with recent evidence suggesting that the capital-market effects attributable to accounting harmonization via IFRS adoption are fairly modest or even non-existent (e.g., Daske et al., 2008; Christensen et al., 2013). A potential explanation for weak capital-market results is that reporting standards grant managers discretion with respect to their application. Hence, capital-market effects hinge critically on whether harmonized standards alter managers' reporting incentives and the extent to which standards are being enforced (e.g., Ball et al., 2003; Burgstahler et al., 2006; Daske et al., 2013). The role of these forces is less obvious in a labor market setting. For instance, accountants and auditors need to know the relevant accounting and auditing rules to perform their jobs even if the standards grant managers discretion. Moreover, formal harmonization could have effects even when enforcement is weak. Therefore, we do not view our findings as inconsistent with those in the capital-market literature.

[2] See, e.g., Barth (2006), Soderstrom and Sun (2007), Hail et al. (2010) and Brüggemann et al. (2013) for reviews. There are two exceptions. Wu and Zhang (2009) document an increase in the sensitivity of CEO turnover to net earnings after voluntary IFRS adoption. Balsam et al. (2015) find an increase in CFO pay around IFRS adoption consistent with better monitoring and increased responsibility. There is also an accounting literature examining labor market outcomes for executives (see Armstrong et al., 2010) and for analysts (see Healy and Palepu, 2001).

Second, we contribute to the economics literature on cross-border labor migration. Much of this prior literature has focused on the effect of wage and unemployment differentials as well as legal barriers in the form of immigration laws (Zaiceva and Zimmermann, 2008; Skupnik, 2013) or occupational licensing rules (Holen, 1965; Kleiner et al., 1982). Immigration laws and occupational licensing rules are explicit government-enforced rules restricting who can move into a particular country or who can offer services in a particular market. The general result in this literature is that explicit restrictions create mobility barriers (see Kleiner, 2000, for an overview). International differences in the rules governing work practices or professions, which are the focus of our study, are different in that they constitute an *implicit* economic barrier, rather than an explicit government intervention aimed at restricting entry. Disparate professional rules are more akin to frictions that impede the portability of social security (D'Addio and Cavalleri, 2015) and tax differentials that encourage migration (e.g., Conway and Houtenville, 1998, 2001; Bakija and Slemrod, 2004; Coomes and Hoyt, 2008; Kleven et al., 2014). However, access to social security and tax differentials create direct monetary incentives to migrate (or not), whereas disparate rules create economic barriers via the required human capital investments by potential migrants.

Showing that differential professional rules indeed constitute a substantial barrier to cross-border labor mobility illustrates that the costs of learning and practicing other standards are economically significant. It further suggests that regulatory harmonization can be a policy instrument to improve cross-border mobility and labor market efficiency. Indeed, creating and improving the EU's "internal market," in which goods, services, capital, and people can move freely, is the main motivation for regulatory harmonization (e.g., FSAP, 1999). Our evidence suggests that disparate rules can be an economic barrier to cross-border migration and that regulatory harmonization can have economically large effects on mobility (even within the EU-

15 where migration responses are typically low). We acknowledge, however, that our findings are limited to the accounting profession, for which harmonization could arguably play a greater role. Hence, the magnitude of our estimates needs to be interpreted carefully.

2. Conceptual Underpinnings and Institutional Setting

In their migration decision, individuals trade off the initial costs of migration against the expected increase in income (Roy, 1951; Sjaastad, 1962; Borjas, 1987) as well as other potential benefits from moving, including better educational and job opportunities for their children. The costs of migration include transportation costs, income losses during migration, non-portable social rights losses, and psychological costs (Stark and Bloom, 1985; Massey et al. 1993; D'Addio and Cavalleri, 2015).

Another obstacle and hence cost of migration could come from differential rules and regulations of which knowledge is relevant when working in a particular profession. For instance, an auditor, lawyer, or building engineer who wants to move abroad needs to learn and know the accounting and auditing standards, laws, and building codes of the destination country, respectively, in order to perform the job. Thus, diversity in rules could act as an implicit economic barrier to labor migration, even when entry into the profession is unrestricted.

Harmonization of professional rules across countries should, ceteris paribus, reduce this mobility barrier and hence could increase cross-border labor migration. Consistent with this argument, the EU's regulatory harmonization intends to improve the functioning of the internal market so that goods, services, capital, and people can move freely. For instance, the Financial Services Action Plan (FSAP), which was established in 1999 with the goal to improve and harmonize EU financial market regulation through a series of legislative initiatives, explicitly cites the plan's potential to increase labor migration as one of the motivations for regulatory

reform.[3] Providing some anecdotal evidence, comment letters sent to the European Commission for its "Consultation on the Impact of IAS Regulation in the EU" in 2014 cite increased labor migration as one of the benefits from the IFRS mandate (see Internet Appendix, Table IA1).

Nevertheless, it is not obvious that regulatory harmonization significantly increases labor mobility. The benefits from harmonizing professional rules could be too small relative to other costs involved in migrating to another country to have a meaningful effect on cross-border labor mobility. In addition, it is possible that local accounting and auditing practices persist after the formal harmonization of the rules (e.g., Kvaal and Nobes 2010, 2012). To the extent these local traditions make it difficult for foreigners to practice in the country even when the rules are the same, regulatory harmonization is much less effective.

Thus, our study aims to analyze whether differential rules indeed constitute a substantial economic barrier and to shed light on the effectiveness of (formal) regulatory harmonization as a policy instrument to increase labor migration. We study the impact of regulatory harmonization on labor migration in the context of the accounting profession for several reasons. First, accounting and auditing standards play an important role in the profession, and learning how to apply them likely is a significant human capital investment. For instance, knowledge of accounting standards is required for any accountant involved in the production of general-purpose financial statements, regardless of whether they work as preparers or auditors. Second, there has been substantial regulatory harmonization in accounting and auditing, which affected virtually all aspects of the profession. Third, accounting harmonization has generally taken the form of explicitly adopting a common set of standards issued in English by an international

[3] The FSAP's motivation discusses among other things that "lack of a Community framework can also discourage labour mobility." The discussion, however, is framed in terms of reforms to the EU pension systems. So far, the EU has passed Directive 2003/41/EC, which facilitates the operation of pension *funds* across member states. The European Commission is also proposing regulation that would make pensions portable across member states. Such regulation would also remove an economic mobility barrier (but its effects would occur after our sample period).

organization. Thus, there is almost complete formal harmonization of the accounting rules and the remaining country-level variation in the rules after harmonization is relatively minor. Prior to regulatory harmonization, however, there were substantial differences in countries' accounting and auditing standards.[4] Thus, harmonization has the potential to eliminate economic barriers for auditors and accountants. Fourth, the large auditing firms are set up as international networks, enabling them to take advantage of accounting and auditing harmonization.

In addition, there are several advantages to studying labor migration in the EU. In principle, labor can move freely among EU member states. Free movement of labor is a fundamental principle enshrined in Article 45 of the "Treaty on the Functioning of the European Union," which grants EU citizens the right to work in another EU country without a work permit. The absence of explicit immigration restrictions makes it easier for us to examine economic barriers and to measure the effect of regulatory harmonization on labor migration.[5] Furthermore, the EU has been on the forefront of international regulatory harmonization in the accounting profession and changes in recent years have been substantial. In 2005, the application of IFRS became mandatory in the consolidated financial statements for almost all publicly traded firms in the EU. In addition, private companies can voluntarily adopt IFRS in many member states. Hence, IFRS adoption affects all accountants working for publicly-traded firms, voluntary IFRS adopters as well as for audit firms that have any of these as clients.

Importantly, IFRS adoption is not the only source of regulatory harmonization in the EU's accounting profession. With Directive 2006/43, later amended by Directive 2008/30, the

[4] For evidence on prior accounting standard differences, see Bae et al. (2008). There is no comparable study for differences in auditing standards. However, about half the EU countries did not even translate national auditing standards into English prior to regulatory harmonization. In addition, seven (eleven) countries had additional national reporting (procedural) requirements that ISA did not have. This comparison understates the actual differences in auditing rules because we do not consider legal audit requirements or more minor differences that have also been harmonized (see Internet Appendix, Table IA2, for details).

[5] Initially, some EU member states imposed labor mobility restrictions for citizens of new member states after its enlargement in 2004. In principle, such restrictions should affect all professions, not just accountants. Nevertheless, we provide sensitivity analyses, in which we restrict the sample and mobility to EU-15 countries only. See Table 5.

EU harmonized statutory auditing requirements. The main purpose of Directive 2006/43 was to harmonize the audit process and establish a single market for audit services across the EU member states (Heß and Stefani, 2012). Its provisions were substantial and ranged from harmonization of educational requirements and ethical standards to granting the European Commission the option to mandate the adoption of International Standards on Auditing (ISA) throughout the EU. Auditing standards specify procedural and reporting requirements for auditors regarding issues such as independence, documentation, certification, and sampling, regardless of whether they audit private or public firms. As such the harmonization of auditing standards has an even wider reach in terms of firms than IFRS adoption, affecting all external auditors in the profession, but it does not directly affect accountants outside of auditing. The European Commission has not yet mandated ISA adoption but, in anticipation of a mandate, all member states have adopted ISA in some form—many around the time of IFRS adoption.[6]

In sum, the accounting profession in the EU provides a powerful setting to estimate the effect of regulatory harmonization on labor migration. Several of the above factors should make it easier to identify and establish an effect of regulatory harmonization on labor migration, if one exists. At the same time, these factors imply that the magnitude of the treatment effect for the accounting profession may not generalize to other professions. Moreover, while the setting is well-suited, it also poses a number of research-design challenges.

First, accounting harmonization in the EU has been an ongoing process for many years. In fact, harmonization of national accounting standards and audit regulation began in corporate law long before IFRS and ISA adoption with the 4th, 7th, and 8th Company Law Directives in 1978, 1983, and 1984, respectively. These early initiatives to harmonize accounting regulation could reduce the effect of the more recent initiatives and hence reduce the power of the setting.

[6] By 2012, only seven EU countries had not fully adopted ISA. See Internet Appendix, Table IA9a, for details.

However, it is important to recognize that national accounting and auditing standards were formally different until IFRS and ISA adoption. From a labor market perspective, it is likely that formal harmonization and knowledge of the detailed standards themselves matter a great deal, even if national standards are similar in spirit or yield similar reporting outcomes.

Second, determining the timing of the potential treatment effect is challenging. For one, labor mobility is likely to be a relatively slow moving construct, which should make any response to harmonization more gradual. In addition, it is not obvious when regulatory harmonization begins to affect labor mobility, despite the fact that the effective dates themselves are sharply defined.[7] For instance, the first mandated IFRS financials were not disclosed until 2006, but accountants and auditors would already have done much of the work that goes into the preparation of the financial statements and the audits in 2005. Thus, labor mobility could increase even ahead of regulatory harmonization, especially if preparers, audit firms, and universities train people in anticipation of IFRS and ISA adoption. At the same time, it may take considerable time before people with the required knowledge are able to take advantage of the reduction in economic barriers. Based on these institutional considerations, it seems reasonable to expect effects from regulatory harmonization to begin at some point in 2005 but to gradually increase in subsequent years. As we are unsure about the start date and as several of our analyses require symmetric pre- and post-windows, we exclude the years 2005 to 2007 and compute the average treatment effects from 2008 and until the end of our sample period in 2010. In untabulated sensitivity analyses, we use the years 2006 to 2008 as well as 2006 to 2010 as the post-treatment period, and find similar results.

[7] Mandatory IFRS adoption applies to fiscal years beginning in or after January 2005. The adoption of Audit Directive 2006/43 followed one year later in 2006.

A third design challenge is that the regulatory changes we study can have demand effects that are unrelated to harmonization as well as "knock-on" demand effects that stem from increased labor migration. The former are demand effects that would arise after regulatory changes even when there is no harmonization (e.g., after the introduction of SOX in the U.S.). We discuss these effects, and how we control for them, in Section 4.2. In addition, regulatory harmonization can also have demand effects. For instance, increased cross-border migration should contribute to the equalization of wages across EU countries, which in turn could spur the demand for accounting services and further increase labor mobility. We do not attempt to separate such knock-on demand effects in our analysis as their source is harmonization and hence they should be included in the estimates.

3. Data and Descriptive Statistics

3.1 LFS Dataset

We base our analysis on the EU's Labour Force Survey (LFS). The LFS dataset is compiled by Eurostat, the statistical office of the EU.[8] The group of participating countries comprises the 28 EU member states, three EFTA countries (Iceland, Norway and Switzerland) and two EU candidate countries (Macedonia and Turkey).[9] The LFS dataset is based on quarterly or annual interviews that are conducted by the national statistical offices of the participating countries. The national statistical offices follow strict guidelines laid out in EU Regulation when

[8] Researchers at academic institutions can gain access to the LFS data for scientific purposes after an approval process. Eurostat provides detailed information on the LFS data and the application process for researchers on its website: http://ec.europa.eu/eurostat/en/web/microdata/european-union-labour-force-survey.

[9] We include Iceland, Norway, and Switzerland in the sample even though they are not EU members. Citizens of these European Free Trade Association (EFTA) countries have the right of free movement within the European Economic Area (EEA), which includes 28 EU members, Iceland, and Norway. Switzerland is not a member of the EEA but its citizens have the same free movement rights through bilateral agreements. Iceland and Norway have adopted EU regulation that harmonizes accounting and auditing standards. In Switzerland, IFRS is not mandatory (Nobes and Zeff, 2015) but audit and accounting standards have effectively been harmonized with the EU. The results are not sensitive to including Switzerland in the sample. For simplicity, we refer to the three additional countries as member states or EU countries. The dataset is reduced to 29 countries because the annual LFS files provide no or very limited information from Croatia, Malta and the two EU candidates during our sample period.

they survey their populations. The guidelines ensure that the sample is representative of the populations in each country and that the collection methods, questions, definitions, and classifications are (almost) identical across countries. The standardized methodology substantially improves the comparability of statistics across countries compared to data used in previous studies. Indeed, lack of comparability has often hampered international migration studies in the past, as they had to rely on disparate data sources with different definitions and collection methods, e.g., population registers, border control, permit, or census data (Rendall et al., 2003). In contrast, the LFS data allow us to estimate consistent migration measures across all EU member states through time, which to our knowledge makes it unique.

Despite these important advantages, the LFS dataset also has drawbacks. In particular, it is not a panel dataset that follows the same individuals through time but instead is a combination of separate cross-sections, raising the concern that changes in sample composition over time could affect our inferences. This concern is mitigated by Eurostat's sampling techniques that are specifically designed to ensure representativeness and comparability across years. In addition, the LFS dataset offers a large set of demographic characteristics, which allow us to match individuals on these characteristics over time (see Section 4.3 for details on our double-matched analysis). Another potential issue with the dataset is that certain variables are provided to researchers in an aggregated form only. For instance, the LFS dataset does not provide the specific country of origin for migrants but instead provides regional information, which prevents us from examining bilateral migration flows between countries.

LFS data are provided in quarterly and annual files. We conduct our analysis on the annual files due to the limited availability of quarterly data in the first half of the sample period. Our sample period starts in 2002, three years before the first fiscal year for which reporting under IFRS was mandatory and four years before the EU adopted Directive 2006/43. Our sample

period ends in 2010. We do not include years before 2002 and after 2010 because the coding of several key variables, notably the job codes, was different before and after these years. As several migration metrics require an analysis with symmetric pre- and post-windows (see also Footnote 14), our sample period focuses on the years 2002 to 2004 and 2008 to 2010, respectively. We further restrict the sample to LFS respondents who are between 20 and 59 years old, because this group is likely to be active in the workforce.

The resulting dataset comprises 10.3 million respondents from 29 countries with yearly totals varying between 1.1 and 2.5 million. Eurostat computes a weighting factor for each respondent based on his/her representativeness in the country's population. The total weighted number of respondents is about 1,672 million, when adding over all countries and all sample years. The weighted number of respondents in a given year roughly maps into the countries' total population between the ages of 20 and 59 (see first two columns of Table 1 for further details).

3.2 Treatment and Control Samples

We identify our treatment and control groups through the LFS item ISCO3D, which indicates the respondents' job based on the current version of the International Standard Classification of Occupations (ISCO-88) at the *three*-digit level.[10] Our treatment group ("accountants") consists of all respondents with ISCO3D equal to 241 which includes accountants (ISCO-88 = 2411) but also personnel and career professionals (ISCO-88 = 2412) and other business professionals such as account executives or market research analysts (ISCO-88 = 2419). Hence, the treatment group includes non-accountants who are not directly affected by regulatory harmonization in the accounting profession. The inclusion of non-accountants does not pose a problem in our research design provided their fraction remains roughly constant

[10] Item ISCO3D focuses on people who are in employment and is set to missing for respondents who are unemployed, inactive, in military service, or younger than 15 years old.

through time. To gauge concerns about measurement error and the validity of this assumption, we obtain aggregate statistics on the fraction of accountants (ISCO-88 = 2411) within the group of professionals with ISCO3D = 241 through a special request to Eurostat. The statistics are based on a sample of 16 countries over the period 2002 to 2010 and show that the fraction of accountants in ISCO3D = 241 is by far the largest and amounts to roughly 50% and, more importantly, varies little over time. Thus, the inclusion of non-accountants does not appear to be an issue and, if anything, is likely to attenuate the observed treatment effect.

We construct three control groups. The first control group comprises legal professionals ("legal pros"), which we define as all respondents with ISCO3D equal to 242. This group includes lawyers (ISCO-88 = 2421), judges (ISCO-88 = 2422), and other legal professionals such as coroners or notaries (ISCO-88 = 2429). Legal professionals are comparable to accountants in that both professions require substantial education and expert knowledge to apply a certain set of rules. As there are risks to choosing one benchmark profession based on conceptual comparability, the second control group ("all pros") consists of all other respondents in the job code group "professionals," which are all at the same professional level (as indicated by the first digit of ISCO-88 = 2). This group includes lawyers, physicists, engineers, computing professionals, and teachers, among others. We exclude architects, veterinary surgeons, and healthcare professionals (ISCO3D = 214, 222, 223) from the "all pros" group because Directive 2005/36 entered into force in 2005 and granted these three professions automatic recognition of their licenses to practice in all EU countries. Including them would likely violate the parallel trends assumption.[11] The third control group ("biz people") consists of respondents with other business jobs, but at different professional levels. We use the following ISCO3D job codes to

[11] Like other professions with national licensing requirements, accountants and auditors can apply for recognition of a foreign license in any EU country. Recognition is, however, not automatic and may require taking a test. We examine the potentially confounding effect of changes in licensing and recognition rules in the Internet Appendix (see Sections IA6 and IA9).

define the group of business people: 121 (directors and chief executives), 122 (production and operations department managers), 123 (other department managers), 131 (general managers), 341 (finance and sales associates), and 342 (business service agents and trade brokers).

Our treatment group of accounting professionals comprises 105,940 respondents from 26 countries over the sample period.[12] The number of respondents in the control group of legal professionals (all professionals) [business people] is 39,480 (600,982) [748,313], when adding over all countries and sample years (see Table 1 for further details).

We recognize various trade-offs in choosing a control group. For instance, legal professionals are conceptually appealing but the group is relatively small, which limits matching. In contrast, the group of all professionals provides a large sample of people working at the same professional level. The latter is desirable but their jobs are in some cases quite different from the jobs of accountants. The third group, in turn, comprises people that all work in business jobs but at higher or lower job levels, which is not ideal. Since there are pros and cons to each group, we estimate treatment effects using all three control groups.

In the Internet Appendix (Table IA3), we present the distribution of demographic characteristics of accounting professionals and the three control groups. We focus on demographic characteristics that prior literature has shown to affect migration, i.e., gender, age, marital status, number of children, and education level (see Krieger, 2004, for an overview). The demographic distributions for the accounting professionals and the three control groups are remarkably similar, except for the educational level. Based on these statistics, legal professionals and all professionals appear to be most comparable to the accounting professionals in terms of demographic characteristics that previous research shows determine cross-border migration

[12] The final sample comprises only 26 countries because Bulgaria, Poland and Slovenia do not provide ISCO3D information at the three-digit level (i.e., we cannot distinguish treatment and control groups).

decisions. Hence, those two are our preferred control groups. But even the distributions for accountants and business people are still quite similar.[13]

3.3 Measuring Migration

For our first migration metric (*NATBIRTH*), we code respondents that have a foreign nationality *and* were born outside the host country in which the survey was conducted as a migrant (or mobile). This simple stock measure of migration is reliable and refers to data items that are widely available in the LFS dataset. It is also the preferred migration metric in prior studies using the LFS database (Martí and Ródenas, 2007; Bonin et al., 2008). Note that this definition does not count individuals that moved to the host country a long time ago and in the meantime have adopted its nationality, which is favorable in our setting considering that we intend to study relatively recent mobility decisions around regulatory harmonization.

However, as a stock measure, *NATBIRTH* does not indicate *when* migration occurred. This data limitation should not affect our difference-in-differences analysis as long as the rate of earlier migration outside the analysis window does not systematically change over time. Although we have no reason to believe that such systematic changes occurred, we construct a second quasi-flow measure that mitigates this concern. Specifically, we use NATBIRTH but count only migrants who recently changed their jobs (*NATBIRTH_CHG*). We identify job changes through LFS item STARTIME ("time in months since the person started current employment") and define recent job changes as those that occurred (a) in/after 1999 for the pre-treatment period, and (b) in/after 2005 for the post-treatment period.

The idea behind this refinement is that migrations due to accounting and auditing harmonization typically involve a change of employment and hence we attempt to preclude

[13] The larger discrepancies likely reflect the fact that business people work at higher (e.g., managers) or lower (e.g., associates) job levels than the accounting professionals, as indicated by the first digit of the ISCO3D.

earlier migrations from the stock measure. However, *NATBIRTH_CHG* is not perfect and not necessarily better than *NATBIRTH*. The former still counts people as migrants who moved many years ago but recently changed jobs within the host country. At the same time, *NATBIRTH_CHG* does not count people that move to a different country within the same firm (e.g., on cross-border assignments, which are quite common in large audit firms).

The LFS dataset also allows the construction of metrics that amount to flow measures. While these flow measures are conceptually desirable given the time dimension of our research question, they have severe drawbacks, which we discuss in Section 5.1. Our main analysis therefore focuses on the established stock measure, *NATBIRTH*, and our novel migration measure, *NATBIRTH_CHG*, which has a time dimension to it. We also present sensitivity analyses using two flow metrics despite their severe shortcomings (see Section 5.1).

3.4 Descriptive Statistics

In Table 2, we report descriptive statistics for the sample used in the regression analysis. For this analysis, we impose two more sample restrictions relative to Table 1. First, as discussed in Section 2, we exclude years 2005 to 2007 because (a) the exact starting point of regulatory harmonization is ambiguous and (b) *NATBIRTH_CHG* requires *symmetric* pre- and post-treatment periods.[14] Second, we restrict the sample to respondents with at least an upper-secondary education. Respondents that have not obtained this educational level are rare among the accounting professionals but also within most control groups, except for business people (see Internet Appendix, Table IA3).

The analysis naturally restricts the sample to observations with non-missing information on all control variables as well as on the mobility metrics. As shown in Table 2, information

[14] Symmetry is necessary because, by construction, *NATBIRTH_CHG* increases over time. For instance, a foreigner who is surveyed in 2008 counts as mobile in the post-treatment period only if the person has changed jobs in the last three years. In 2009, however, a person counts as mobile if she has changed jobs in the last four years, resulting in an upward trend as the window expands (relative to a fixed starting point).

indicating whether an individual has children is missing with some frequency in the LFS dataset, as some countries do not provide this information in all survey years. To preserve a relatively balanced sample across time, we treat missing information on the number of children as a separate category when matching on demographics or creating fixed effects for them.[15] We also combine upper-secondary and post-secondary education levels as well as tertiary and doctoral education levels. The fraction of individuals with education at the post-secondary level and at the doctoral level is very small (see Internet Appendix, Table IA3) and the data are too sparse to create separate categories and the full set of interactions for these education levels. All of our statistical analyses require that both *NATBIRTH* and *NATBIRTH_CHG* be non-missing. The samples for the analyses using LFS weights are slightly smaller because the weighing factor is missing for some individuals.

Taken together, the final sample for our main analyses comprises individuals from 26 countries for the years 2002 to 2004 and 2008 to 2010, who are between 20 and 59 years old, and whose highest degree of education is at least at the upper secondary level. Table 2 reports the number of observations for accounting professionals and the three control groups that meet the above criteria and have non-missing control variables. Table 2 also reports the mean mobility rates of accounting professionals, legal professionals, all professionals, and business people using our main mobility metrics *NATBIRTH* and *NATBIRTH_CHG*. Accounting professionals exhibit the highest mobility rates. However, these statistics include sample years after regulatory harmonization and do not yet control for (or match on) demographic characteristics. We provide a formal comparison of pre-treatment mobility rates in Section 4.

[15] As we conduct the analysis within bins of certain demographics and within country, this design choice should be innocuous and primarily help sample representativeness as well as power. For Ireland, we have the number of children only for the post-treatment period, which we set to missing to allow matching with observations in the pre-treatment period.

Table IA4 in the Internet Appendix also presents descriptive by-country migration statistics for *NATBIRTH* and *NATBIRTH_CHG*. We provide these statistics separately for the pre- and the post-treatment periods and based on a matched sample to facilitate comparisons with our Section 4 analyses. Table IA4 shows considerable variation in both the fraction of migrants as well as the changes in this fraction across host countries. We note that the fraction of migrants is not higher in the post-treatment period for all countries, as one would probably expect. Comparing the change in the migration rate for accountants with the rate for all professionals, 58 (58) percent of the countries exhibit a positive change for NATBIRTH (NATBIRTH_CHG). However, most of the negative changes are close to zero, and when we count only absolute changes that exceed 0.5 percentage points, then 76 (80) percent of the countries exhibit positive changes for NATBIRTH (NATBIRTH_CHG).

4. Effect of Regulatory Harmonization on Migration

4.1 Difference-in-Differences Analysis

We begin with a difference-in-differences analysis using individual-level regressions. This design is useful in that many potential confounds "wash out" in one of the two differences. For example, contemporaneous but unrelated regulatory changes that affect both the treatment and the control groups wash out in the first difference. Similarly, time-invariant measurement problems for one group wash out in the second difference.

Our identification strategy rests on the assumption that mobility trends for the treatment group would have been parallel to those in the control groups had there been no regulatory harmonization in the accounting profession. We therefore begin by graphing mobility rates over time. Figure 1 compares aggregate mobility rates based on the *NATBIRTH* measure across treatment and control groups over time (before matching). The graphs suggest that the mobility

rates of all groups move in concert during the years 2002 to 2004. In other words, the mobility rates of accounting professionals and the three control groups behave similarly prior to regulatory harmonization. The *levels* of the mobility rates are also similar for three of the groups before harmonization. The mobility rates for the legal professionals are considerably lower throughout the sample period (in the unmatched sample), but they still move remarkably in parallel with the accounting professionals over the pre-treatment period. Thus, mobility patterns over the pre-treatment period lend support to the parallel-trend assumption. After 2005, the mobility rate of the accountants increases and does so more strongly than the mobility rates of the three control groups. The relative increase is strongest against legal professionals and all professionals, and less pronounced against business people. By 2010, the mobility rate of accounting professionals is substantially above the rates of the control groups, which is descriptively consistent with a mobility effect from regulatory harmonization.[16]

We formally test for a mobility effect at the individual level. The granularity of this analysis is a major advantage as it allows us to control for demographic characteristics of the respondents that are unrelated to treatment but predicted to affect cross-border mobility. Specifically, we include gender, marital status, age, education level, and the presence of one or more children under the age of 15 living in the household, all measured at the time of the survey. As these characteristics are all categorical, we control for them with fixed effects for all possible combinations of the variables. In total, we include 192 fixed effects (one for each bin). The fixed effects imply that we estimate the treatment effect within bin, i.e., for individuals with the *same* demographic characteristics. Aside from finely controlling for individual characteristics that affect mobility, the fully interacted fixed-effect structure avoids extrapolation and functional-

[16] In the Internet Appendix, we graph the time trends in the aggregate mobility rates for the four groups from 2002 to 2013 (see Section IA5). Figure IA5 shows that the differences in mobility rates between the accountants and the three control groups persist until 2013. We do not use the extended period due to the job code changes in 2011.

form assumptions for the control variables. As a result, the estimation is less susceptible to non-linearities in the data (e.g., Cochran and Rubin, 1973; Rubin, 1973 and 1979), which is a particular concern when working with categorical variables as in our setting.

We also include country-accountant and country-year fixed effects. The purpose of the country-accountant fixed effects is to eliminate differences in mobility rates for accounting professionals and the respective control groups across countries as well as to account for differential frequencies in accounting professionals across countries. The country-year fixed effects eliminate country-specific shocks as well as trends in mobility common to all professions in a given country (e.g., due to shocks to economic growth or changes to the survey methodology). Thus, in this regression design, the treatment effect is identified by differences in the time-series variation in mobility rates between professions within countries and within bins combining various demographic characteristics. We draw statistical inferences based on standard errors clustered by country-job group with job group indicating either accounting professionals or the respective control group. Since our sample comprises 26 countries, this approach yields 52 clusters, which strikes us as conservative.[17]

In Table 3 Panel A, we present OLS regression results for each of the three different control groups.[18] In the first six columns, we use *NATBIRTH* as our migration measure and present regressions with and without weighing observations by the statistical weight provided for

[17] However, the homogeneity assumption required for clustering standard errors becomes more important yet can be tenuous as the number of clusters becomes small. We therefore follow the suggestions in Conley et al. (2016) and also draw inferences using a Fama-MacBeth procedure as a robustness check. We group countries into six European regions by geography and language. This grouping allows for even more cross-sectional dependence than the country-job clustering. We then draw inferences in a Fama-MacBeth fashion based on six separate diff-in-diff regressions, one for each region, and find that our inferences are similar (albeit at lower significance levels as is expected with only five degrees of freedom). See also Internet Appendix IA4 for more details on the region results.

[18] We estimate OLS regressions rather than logit or probit models to avoid an incidental parameter problem given the heavy use of fixed effects. However, OLS regressions may be biased with a binary dependent variable. The double-matched approach presented in Section 4.3 does not suffer from this potential problem. As a sensitivity test we also estimate probit models. The inferences are consistent across the two estimation methods holding the sample constant. The probit analysis yields statistically weaker results for the legal professionals but slightly stronger results for the other two control groups.

each individual in the LFS dataset. In the last six columns, we present the results using our refined migration measure, *NATBIRTH_CHG*. The relevant coefficient estimates for the interaction term *Accountant * Post* are positive and always statistically significant for the first two control groups. The estimates are also positive but only marginally significant in two of four specifications when using business people as the control group. Overall, these findings are consistent with the hypothesis that regulatory harmonization increases cross-border labor migration.

In gauging the magnitude of the coefficients, we focus on regression estimates using LFS weights, as the weights likely make the sample and hence the estimates more representative of the EU population. The estimated treatment effect with LFS weights is generally around 70 basis points and even larger when benchmarking against legal professionals. For *NATBIRTH*, these estimates imply an 18% increase in cross-border migration of accounting professionals relative to their pre-treatment migration rate and benchmarking against the two large control groups. Using *NATBIRTH_CHG*, the corresponding percentage effect is around 30 percent given the refined measure exhibits a lower pre-treatment migration rate. The fact that the percentage effects are similar when benchmarking against all professionals and against business people indicates that the statistically weaker results against business people are primarily a matter of statistical power. The coefficient magnitudes and percentage treatment effects are similar albeit slightly weaker in the regressions without LFS weights.

Arguably, an even better way to gauge the economic magnitude of our estimates is to compute the increase in the total number of accounting professionals that migrate as a result of regulatory harmonization.[19] Towards this end, we determine the average number of survey

[19] For instance, this translation mitigates downward bias in the percentage effects that can arise from the fact that the treatment group contains non-accountants, which presumably do not increase in mobility.

respondents from the accounting profession in any given year, invert the LFS weights to obtain an estimate for the population of accountants, and then multiply this number with the regression estimate for the increase in migration. Using coefficients from the LFS-weighted regressions in Panel A, we estimate that the increase in the number of migrating accountants is between 16,000 and 18,000 individuals, but could be as large as 30,000 individuals when using the legal professionals as a control group.[20] Such increases seem economically significant. We obtain very similar estimates for the increase in migrating accounting professionals using both migration measures. The consistency across these two measures is reassuring and supports our argument that the difference-in-differences design strips out "stale" mobility that occurred earlier in life and is unrelated to regulatory harmonization. Considering that the primary distinction between *NATBIRTH* and *NATBIRTH_CHG* is that the latter has been better purged of stale mobility, the two measures should provide similar treatment effects.[21]

In sum, the results in Panel A provide consistent evidence that regulatory harmonization in the accounting profession led to a substantial increase in cross-border migration. In the remainder of Section 4, we focus on two issues that could confound this interpretation. First, we examine to what extent demand effects induce an upward bias in our estimates. Second, we address the concern that our analysis is not based on a panel, raising the possibility that changes in sample composition drive our results. In the Internet Appendix, we discuss two further concerns, which could amount to a violation of the parallel-trends assumption: differential

[20] The population of accountants comprises about 2.3 million people in the sample countries according to our estimates based on the number of respondents and their LFS weights. Multiplying this number with the relevant coefficient estimate on the interaction term *Accountant * Post* yields an estimate for the increase in migration due to regulatory harmonization over the post-treatment effect. For example, the second specification in Table 3, Panel A (NATBIRTH, with LFS weights and all pros as control group), shows a coefficient estimate of 0.723. This translates into an increase of about 16,600 migrating accountants (0.723 * 2.3 million / 100).

[21] One could also consider the incremental explanatory power of the harmonization variable in explaining migration. However, as many individual-level factors play into a migration decision, most of the variation comes from individual characteristics, which we control for with an extensive fixed-effect structure. Hence, the incremental R-squared for the harmonization indicator is not surprisingly close to zero.

changes in cross-border student mobility as well as in the recognition of occupational qualifications. We find no evidence that such changes explain our results (see Section IA6).

4.2 Separating Regulatory Harmonization and Demand Effects

An important concern about the findings in Section 4.1 is that the documented increase in cross-border migration reflects demand effects stemming from new regulation, rather than regulatory harmonization per se. Regulatory changes can affect the demand for accounting services and as a result increase job mobility even without harmonization. For instance, the implementation of SOX likely increased the demand for accounting and auditing services for some time. In addition, it is conceivable that SOX created new opportunities for accounting professionals with certain skills (e.g., related to internal controls) and that these professionals move to new firms if their services are of greater value elsewhere. This new matching of employees and firms can also increase job mobility for some time.[22]

We first note that we scale our migration measures by the number of professionals in the country (and year). Such scaling captures at least some of the demand effects. For instance, if a (non-harmonizing) regulatory shock like SOX increases the demand for accountants and does so symmetrically for domestic and foreign accountants, then the construction of the mobility measure already controls for the demand effect. However, demand shocks could be asymmetric. Furthermore, the new matching need not imply an influx of people into the accounting profession (i.e., the denominator does not have to increase). Thus, demand effects from regulatory changes could still affect our results.

[22] Consistent with these arguments and the notion that IFRS adoption also increases the demand for accounting services, there is evidence that the cost of preparing financial statements (ICAEW, 2007) and audit fees (De George et al. 2010) increase around IFRS adoption, although most of the effects are limited to the year of adoption. To limit the influence of such demand effects, we exclude the sample years 2005 to 2007, which is the period over which EU countries adopted IFRS and harmonized national audit standards with ISA.

To gauge this concern, we use the insight that demand effects also imply an increase in *domestic* job mobility. For instance, the new matching of employees and firms after a SOX-like regulatory change can take place across borders but also domestically. In contrast, supply-side effects due to cross-border harmonization should primarily pertain to foreigners. We therefore introduce a variable to control for *domestic* job mobility. If our results largely reflect demand effects, then controlling for domestic job mobility should attenuate the coefficient of interest.

We create a variable that measures *Domestic job mobility* at the country-year-profession level. The variable is the proportion of domestic people who recently changed their jobs in a given year, country, and profession, which essentially captures disruptions of existing matches and re-matching within a profession. We identify job changes through LFS item STARTIME ("time in months since the person started current employment") and define recent job changes as those that occurred (a) in/after 1999 for the pre-treatment period, and (b) in/after 2005 for the post-treatment period.

In Table 3, Panel B, we report the result controlling for *Domestic job mobility* for each of the three control groups, both migration measures as well as with and without using the LFS weights. The coefficients on *Domestic job mobility* are positive in all and statistically significant in some specifications—consistent with concurrent increases in domestic job mobility and hence demand effects around regulatory changes. More importantly, however, introducing this control variable barely changes the estimated treatment coefficients and, if anything, strengthens the results when using business people as a control group. Thus, the findings in Panel B are *not* consistent with the concern that the results reflect demand effects. Such effects may be present in the data but they do not appear to affect our coefficients of interest in a material way.

While the regressions controlling for domestic job mobility go a long way towards separating demand and harmonization effects, we conduct additional tests using the country-

level correlations of labor migration inflows and outflows to specifically gauge the presence of asymmetric demand shocks *across countries* (see Internet Appendix, Section IA7). If the increase in migration around harmonization is driven by asymmetric demand shocks, countries with relatively small demand shocks should act as "sources," experiencing an increase in outflows and decline in inflows, while countries with relatively large demand shocks act as "sinks" having an increase in inflows and decrease in outflows, leading to a negative correlation of country-level changes in inflows and outflows. Contrary to this, we find that changes in country-level in- and outflows are positively associated for accountants relative to all professionals. Indeed, using (without using) the LFS weights, the relative changes in in- and outflows have the same sign for 83 (83) percent of the countries for which we have sufficient data to calculate pre- and post-harmonization migration flows. The positive correlation is more consistent with a harmonization effect than an asymmetric demand effect (see Tables IA7a and IA7b for details).[23]

Nevertheless, we acknowledge that it is difficult to separate supply and demand effects entirely. They can be endogenously connected. As noted earlier, it is possible that increased cross-border migration due to regulatory harmonization contributes to the equalization of wages across EU countries, which would likely lower wages and in turn could spur the demand for accounting services. In our view, it would be appropriate to include such knock-on demand effects in the estimation of the treatment effect (as the source is harmonization). For this reason, including domestic job mobility in the model could over-control and is not without costs.

[23] Finally, we note that demand effects due to implementation or new matching should be of limited duration. Once the new rules are implemented and the re-matching has taken place, labor migration rates should decrease. However, when we extend Figure 1 to 2013, we find that the migration effect persists after 2010 and does not look like a temporary demand effect (see Section IA5).

4.3 Double-Matched Difference-in-Differences Estimates

The LFS dataset consists of separate annual cross-sections of survey respondents. It is not a panel dataset that follows individuals through time. Thus, changes in sample composition over time can potentially bias the treatment effect and produce spurious inferences. For example, suppose that, relative to the control group, accountants living in Sweden exhibit persistently high mobility rates. If for some reason the sample composition changes over time such that Sweden is overrepresented in the post-treatment period relative to the pre-treatment period, then such changes could upward bias the estimated treatment effects in the regression analysis presented in Sections 4.1 and 4.2. We note that the LFS weights mitigate this possibility but we perform a "double-matched" difference-in-differences analysis to further alleviate this concern. The idea is to form quadruplets of individuals with identical characteristics and then to compute the difference-in-differences within each matched quadruplet, which eliminates sample composition effects.

We implement this approach as follows: Within each country and year, we first match all accounting professionals and all control group observations with the exact same characteristics for gender, marital status, age, education level, and the presence of one or more children under the age of 15 living in the household. We then match across time, linking pre-treatment accounting professionals to post-treatment accounting professionals with the exact same characteristics, and do the same for the control observations. We drop all observations that cannot be matched. This double-matching yields a collection of quadruplets, each consisting of accounting professionals and controls before and after harmonization that are jointly identical except for their occupation (some are accountants and some are in control professions) and survey year (some are pre-treatment and some are post-treatment). Thus, we have precisely the

variation required for a difference-in-differences analysis, but hold everything else constant. We illustrate this design in Figure 2.

In order to implement the double-matched approach, we must specify a "year-pair" linking a pre-treatment year with a particular post-treatment year. For each year *t* of the pre-treatment period (2002-2004) we define year *t+6* as the corresponding year (e.g., observations in 2002 are matched to observations in 2008).[24] For example, an accountant living in Germany in 2002 with a particular combination of demographic characteristics is included in the double-matched sample if and only if there is at least one lawyer (control) living in Germany in 2002 with identical characteristics *and* there is at least one lawyer *and* one accountant living in Germany in 2008 with those same characteristics. For instance, as the LFS does not contain data for Italy in the pre-treatment period, all observations for Italy drop out of this analysis. This example illustrates why the double-matched approach eliminates sample composition changes.

Within-quadruplet estimation of the treatment effect requires that we equally weigh the four components of a quadruplet. However, not all quadruplets are equally informative. There is substantial heterogeneity in the number of individuals contained within each quadruplet. Small countries and unusual demographic combinations (e.g., single men in their late 50s with young children living in the household) produce potentially less informative difference-in-differences estimates. To address this issue, we give each quadruplet a weight equal to the minimum sample size in the quadruplet's four components. The idea is that the information of the quadruplet is ultimately constrained by the sample size of the four components. By defining the weight at the level of the quadruplet, we ensure that the within-quadruplet estimate reflects an equal-weighted

[24] In so doing, we again exclude years 2005-2007. By focusing on later post-treatment years, our results should be less susceptible to temporary demand effects in mobility (e.g., if audit firms import workers to help with IFRS implementation for a limited time). As a robustness test, we alternatively define year t+4 as the corresponding year and hence exclude years 2009 and 2010 (as well as 2005). This alternative definition yields only slightly smaller treatment effects that are statistically significant in most specifications. These attenuated results are consistent with the gradual increase in the mobility rates of accounting professionals after 2005 documented in Figure 1.

difference-in-differences, but allow the informational value of each difference-in-differences to vary across quadruplets. The reported treatment effect is hence a weighted average across the effects for the quadruplets. We again draw statistical inferences based on standard errors clustered by country-job group (48 clusters).

The double-matched approach is obviously very demanding. Comparing panels in Table 3 shows that we lose a substantial fraction of our sample due to double-matching. For this reason, we focus on the control group of all professionals, which offers a large sample of individuals at the same professional level and with similar demographic characteristics as the accountants (see Internet Appendix, Table IA3).[25] At the same time, double-matching provides tighter identification and also makes the pre-treatment mobility rates quite similar across treatment and control groups.

Table 3, Panel C, presents the estimated treatment effects from the double-matched difference-in-differences analyses. Again, we find positive and statistically significant increases in cross-border labor migration after regulatory harmonization using both *NATBIRTH* and *NATBIRTH_CHG*. Our estimates using the LFS weights indicate that cross-border migration of accountants increases by 42 to 69 basis points relative to all professionals. The estimates without LFS weights are similar (59 to 65 basis points). Weighted and unweighted estimates imply fairly similar percentage increases in cross-border migration of accounting professionals relative to the pre-treatment migration rates (20% and 11% for NATBIRTH; 22% and 19% for NATBIRTH_CHG, respectively). These percentage effects are quite comparable to those reported for the regression analysis in Table 3, Panels A and B, which is reassuring.[26] As before,

[25] We report findings for the other control groups in the Internet Appendix, Table IA8. The effects are generally larger in magnitude when using legal professionals, and smaller and less significant using business people.

[26] In unreported tests, we gauge the sensitivity of the results to the inclusion of specific countries by implementing a jackknife procedure, dropping each country in turn and computing separate treatment effects each time. The

we gauge the economic magnitude of the treatment effects by translating them into an absolute increase in the number of accounting professionals that migrate as a result of regulatory harmonization. We proceed analogously and invert the LFS weights to obtain population estimates. The treatment effects imply an increase of roughly 10,000 to 16,000 accountants.[27] These numbers are slightly smaller than those reported in Section 4.1, but conceptually superior.

Finally, as isolating harmonization effects is a central challenge of our paper, we conclude the double-matched analysis with an additional test that further mitigates the concern that our results reflect demand effects that would arise even for non-harmonizing (SOX-like) regulatory changes. In the spirit of the tests reported in Section 4.2, we estimate the effects solely among people that recently changed their jobs. As a result of this sample restriction, we essentially estimate the cross-border migration effects relative to domestic job mobility, which should control for demand effects. We present the results in the last column of Table 3, Panel C.

The estimated treatment effects remain statistically significant and are even larger in magnitude within the subsample of workers who recently changed jobs. Specifically, using the LFS weights we find that cross-border migration of accountants increases by 137 basis points after regulatory harmonization, relative to all other professionals.[28] Finding significant treatment effects even in the subsample of domestic and foreign people that recently changed their jobs further increases our confidence that regulatory harmonization caused the increase in cross-border labor migration for accounting professionals.

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strongest attenuation occurs when we drop the Switzerland but the treatment effect decreases by only 20% and remains statistically significant at the 1%-level.

[27] For details on how to compute these numbers, see Footnote 20.

[28] Since the population of accountants comprises about 942,000 people who recently changed their jobs according to the number of respondents and their LFS weights, the estimated treatment effect implies an increase of roughly 13,000 migrating accountants (1.374 * 0.942 million / 100). This is in line with the estimates reported in our main analysis based on the full sample

5. Sensitivity Analyses and Cross-Sectional Heterogeneity in Treatment Effects

In this section, we examine alternative flow measures of migration as well as cross-sectional heterogeneity in the treatment effects. The flow measures also allow us to examine cross-sectional variation in the treatment effects based on the demographic characteristics of the respondents because for the flow measures these characteristics are by design measured in close proximity to the time of migration. For the stock-based measures, we examine cross-sectional variation in the treatment effects based on institutional (country-level) characteristics.

5.1 Flow Labor Migration Measures and Cross-Sectional Differences by Demographics

Prior work using the LFS dataset prefers to use stock measures of migration due to major concerns about the flow measures that can be constructed based on the LFS dataset (Martí and Ródenas, 2007; Bonin et al., 2008; Skupnik, 2013). Our earlier analyses therefore follow the prior literature and use a stock measure based on nationality and country of birth (*NATBIRTH*). We also construct a novel quasi-flow measure (*NATBIRTH_CHG)*, which starts from the stock measure but counts only foreigners that recently changed jobs, making it more likely that migration occurred recently. We have discussed the pros and cons of the two measures in Section 3.3 and noted that the difference-in-differences design addresses shortcomings of these measures, provided these shortcomings are constant through time (or do not change systematically with regulatory harmonization). While we have no indication of systematic changes in the direction of our results, this section presents sensitivity analyses using two flow measures that ensure migration occurs in a window around regulatory harmonization. Importantly, we do not construct these two measures simply as changes in the stock of foreigners but instead rely on different items from the LFS dataset to determine the time of migration.

We base the first flow measure (*YEARESID*) on the number of years for which an individual who was born abroad (LFS item COUNTRYB) has been a resident in the host country

(LFS item YEARESID). *YEARESID* defines individuals as mobile if they were born abroad and moved to the host country in/after 1999 (for sample years 2002 to 2004) or in/after 2005 (for sample years 2008 to 2010). This measure allows us to determine the exact time of the migration, both relative to the treatment (i.e., before or after regulatory harmonization) and relative to the respondent's age (i.e., we can rule out migrations during childhood). While these features of the *YEARESID* measure are desirable for our identification strategy, there are also major drawbacks. First, the measure mechanically increases over time and hence requires a symmetric pre- and post-estimation window (see Footnote 14). Second, its construction implies relatively low incidence rates, meaning that the number of survey participants who are foreigners *and* have moved within the analysis window is low. The low incidence rate can cause a mini-domain problem making survey-based estimates unreliable (Purcell and Kish, 1980). This issue arises in our setting when we form bins of observations matched on demographic characteristics, especially in countries with few observations in the LFS dataset (e.g., Luxembourg). Bins with few observations might indicate a migration rate of zero in a given profession (and country), even when the true migration rate for the profession is positive, simply because none of the respondents in that profession (with certain characteristics) migrated in recent years. Similarly, with low base migration rates, adding a single migrant could produce a large percentage change, which in turn can make estimates of changes in the migration rates noisy.

For the second flow measure (*COUNTR1Y*), we use the question regarding the place of residence one year before the survey (LFS item COUNTR1Y).[29] This measure also allows us to determine the exact time of the migration and it has the desirable feature that respondent

[29] We note that some of the mobility captured by *COUNTR1Y* is repatriation, that is, workers moving back to their home countries after spending time abroad. However, harmonization could increase the number of accountants returning to their home countries (e.g., after IFRS adoption, expatriates of German companies may no longer be needed to help with German GAAP reporting in foreign subsidiaries). This return migration should be counted as harmonization-induced cross-border migration. We obtain smaller but still economically meaningful treatment effects when excluding repatriations from the *COUNTR1Y* analysis.

characteristics are measured close to the time of the move. The latter feature facilitates cross-sectional tests using variation in demographic characteristics. As LFS data measure demographic variables at the time the person is surveyed, rather than the time of the move, stock-based measures do not necessarily have this feature, which makes cross-sectional analyses using demographic characteristics problematic.[30]

While *COUNTR1Y* may seem ideally suited for our analysis, it has several severe drawbacks. First, it counts only very recent migration; a foreign resident who moved two or three years prior to the survey is coded as non-mobile when using *COUNTR1Y*. As a consequence, *COUNTR1Y* has even lower incidence (or migration) rates than *YEARESID*, exacerbating the problems that we described earlier.[31] Second, and perhaps more importantly, the LFS sampling techniques can systematically bias *COUNTR1Y*, which results in major discrepancies with population registers (Martí and Ródenas, 2007). One reason for the discrepancies is that some countries survey the same individuals for several quarters reaching into adjacent years. The lack of sample replacement implies that a certain fraction of the answers regarding *COUNTR1Y* change mechanically. For instance, an individual that moved in the first quarter of 2003 and is surveyed for 6 quarters starting in the third quarter of 2003 has to change the answer to *COUNTR1Y* by the second quarter of 2004. Martí and Ródenas (2007) show that the sample replacement rates differ across countries and that the problem can be quite severe in some countries (e.g., in Austria, Germany and Sweden). Third, in some countries and sample years, the

[30] This is particularly true for people that moved a long time ago. When computing average treatment effects, the design difference-out those that moved a long time ago. However, they could still skew cross-sectional tests. For instance, if migrants are more likely to have children and marry *after* they move to a new foreign country relative to the average respondent, then cross-sectional splits for *NATBIRTH* may not yield meaningful results. For this reason, we perform cross-sectional splits based on demographic characteristics for the two flow measures only.

[31] Illustrating the problem, we find that if we form bins for accountants and lawyers by country, year and personal characteristics, 98% contain a migration rate of zero using COUNTR1Y.

data item to code *COUNTR1Y* has not been collected (see Table 1), creating gaps in the time series, which is problematic for a study like ours that relies on changes over time.

Given the shortcomings of the flow measures, we use them for sensitivity tests only. For the same reasons, we do not necessarily expect the coefficient estimates from these tests to map into the earlier analyses using stock-based measures. To mitigate the mini-domain problem, we use the control group of all professionals, which provides a large sample of individuals at the same professional level as accountants and with similar demographics (Internet Appendix, Table IA3). We also conduct several cross-sectional splits by demographic characteristics. These splits serve two purposes. First, by showing that increases in migration occur in sub-populations that should be more responsive to regulatory harmonization, we increase the confidence in our findings. Second, by splitting into subsamples for which mobility rates are larger, we increase power and hence mitigate the problems due to low incidence rates.

Based on prior empirical evidence, we expect single respondents without children to be more mobile (Mincer, 1978) and therefore to be more likely to respond to regulatory harmonization. We also expect young people (20-39 years old) and people that work for large employers (50+ employees, in local unit) to be more responsive. Young people do most of the technical work in audit firms and thus their jobs require the most detailed knowledge of rules. Similarly, large audit firms have a higher fraction of large clients relative to small audit firms and large clients are most likely to report under IFRS, both mandatorily and voluntarily (e.g., Le Vourc'h and Morand, 2011). These considerations determine our split variables.

In Table 4, we present difference-in-differences estimates for various combinations of these splits. We obtain each estimate using the same double-matched approach described in Section 4.3. As expected, the mobility rates for the flow measures are lower than for *NATBIRTH*, especially for *COUNTR1Y*. Consistent with our prediction that cross-border mobility is higher

for (1) singles without young children, (2) younger workers and (3) workers at larger firms, we find that pre-treatment mobility rates increase as we introduce the three restrictions cumulatively. Using pre-treatment averages for *YEARESID* (*COUNTR1Y*) with LFS weights, the mobility rate climbs from 133 (18) basis points for the full sample to as high as 254 (58) basis points for the most restricted subsample. Thus, we expect these restrictions to be effective in mitigating the mini domain problem and increasing power.

In interpreting the treatment effects, we focus on LFS-weighted estimates as they adjust for discrepancies in sample size across countries and/or across time by anchoring the weights in countries' populations. We find that the estimated treatment effects are insignificant for the full sample, although for *YEARESID* the estimates are positive and sizeable relative to the pre-treatment mobility rates. Thus, the statistical insignificance is likely explained by the mini-domain problem, which reduces the reliability of the estimated treatment effects and is most severe in the full sample. Indeed, when we restrict the analysis to subsamples with higher migration base rates, treatment effects and statistical significance begin to increase. Using *YEARESID* (*COUNTR1Y*), the estimated coefficients are significant in all subsamples and indicate, for instance, that labor migration of single accountants without children increases by roughly 46 (52) basis points relative to all other (matched) professionals. Further restricting to younger, single workers without children working at large firms, the treatment effects indicate an increase in cross-border labor migration by 173 and 77 basis points for *YEARESID* and *COUNTR1Y*, respectively.

The estimated effects are quite large relative to the pre-treatment mobility rates. We also gauge the economic magnitude of the estimates by computing the implied increase in the number of migrants over the post-treatment period. Restricting the computations to statistically significant coefficients, the flow measures yield an estimate between 3,000 and 5,000 individuals

using *YEARESID* and 6,000 and 10,000 for *COUNTR1Y*.[32] While these estimates are smaller than those in Section 4.3, they are still economically meaningful. It is also important to keep in mind that they apply to restrictive subsamples and hence have to be interpreted in that context.

In sum, the results for the flow measure in Table 4 corroborate our earlier findings using stock-based migration measures. As expected, statistical power of the flow measures is low but the relatively large percentage effects in subsamples that should be more responsive to regulatory harmonization are reassuring. Overall, the evidence is consistent with the interpretation that regulatory harmonization has a sizeable effect on cross-border migration of accountants.

5.2 Cross-Country Variation in Treatment Effects

Institutional differences across countries likely affect migration patterns. For instance, prior research finds that East European citizens respond more strongly to economic shocks that should lead to migration than Western European citizens (Dao et al., 2014). Such cross-country differences may also shape migration responses to regulatory harmonization. In addition, country-level differences in the *extent* of harmonization as well as with respect to explicit entry barriers such as licensing rules can lead to cross-sectional variation in the treatment effects. We explore such cross-country differences in this section.

5.2.1 Treatment Effects in East and West Europe

In 2004 and 2007, the EU was enlarged by in total twelve new, mainly Eastern European member states (EU-10 and EU-2).[33] EU membership gave the citizens of the twelve new

[32] As before, we gauge the economic magnitude by multiplying the treatment effect with an estimate for the relevant population of accountants. For example, the population of accountants who are young singles without kids and working at larger firms comprises about 275,000 people in the sample countries according to our estimates based on the number of respondents and their LFS weights. Multiplying this number with the relevant treatment effect for YEARESID (with LFS weights) yields an increase of about 4,750 migrating accountants due to regulatory harmonization over the post-treatment period (1.731 * 275,000 / 100). Multiplying the population estimate with the relevant treatment effect for COUNTR1Y (with LFS weights) yields an increase of about 2,100 migrating accountants per year (0.771 * 275,000 / 100). This yearly effect translates into an increase of about 6,300 migrating accountants over the post-treatment period (2,100 people * 3 years).

member states access to the labor markets of the EU-15 countries with certain restrictions for a transition period (for details, see Zaiceva and Zimmermann, 2008; Skupnik, 2013). Prior research finds that labor mobility among the mainly Western European countries that became EU members before 2004 (EU-15) is relatively low compared to, for instance, the United States (Bonin et al., 2008; OECD, 2014). Since the EU-15 has not had formal restrictions on labor mobility for decades, a widespread conjecture is that EU-15 mobility rates are low because of implicit barriers, such as language or culture, which are difficult to change.[34]

It is therefore interesting to examine whether regulatory harmonization increased labor mobility even within EU-15 countries. We present results from two specifications in Table 5. In the first specification (EU-15 host countries only), we use the same design as in Table 3, Panel A, but focus on the subsample of individuals surveyed in the EU-15. In the second specification (EU-15 host and EU-15 source countries only), we also focus on respondents from EU-15 but in addition, we count as migrants only those individuals who were born in the EU-15 and have an EU-15 nationality. In both specifications, we focus on the more restrictive design that uses *NATBIRTH_CHG* and controls for domestic job mobility.

We find that the treatment effects are very similar when we restrict the sample to EU-15 host countries (first three columns of Table 5). Restricting to EU-15 host and source countries, we still find significant treatment effects, but they are slightly smaller in magnitude, consistent with prior literature indicating lower within-EU-15 mobility and migration responses (cited above). In light of this work, it is noteworthy that we find significant within EU-15 migration effects after regulatory harmonization. This finding also eases the concern that EU enlargement

[33] In 2004, the following countries became EU members: Cyprus, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia, and Slovenia (EU10). In 2007, Bulgaria and Romania became members (EU2).
[34] Related to the role of culture and language, we estimate treatment effects within six regions that are defined based on language and geography. The results show substantial regional differences in the response to accounting harmonization. See Internet Appendix IA4.

and the removal of labor migration restrictions on workers from EU-10 and EU-2 countries are responsible for the documented migration effects of accounting professionals.

5.2.2 Variation in Treatment Effects by the Extent of Harmonization and Licensing Rules

In this section, we explore cross-sectional variation in the estimated treatment effects. We focus on variables related to the extent of harmonization and professional entry barriers. However, we note that it is difficult to ex ante sign cross-sectional differences in the treatment effects. One reason is that cross-sectional variation also affects the size of the profession in the first place. In addition, cross-border migration is not the only margin along which the system can adjust after the regulatory change. Consider, for instance, the prediction that countries with a larger fraction of publicly-listed firms that need to report under IFRS should see a larger inflow of migrants. While sensible, it is conceivable that these countries already had a much larger accounting profession to begin with and hence even if they receive a larger number of migrants, the relative effect of migration could be larger in countries with fewer publicly-traded firms reporting under IFRS (and a smaller audit profession). Furthermore, countries with a larger fraction of publicly-traded firms could also see a larger *domestic* inflow into the accounting and auditing profession after IFRS adoption. As a result, it is again not clear that the inflow of migrants (scaled by the size of the profession) is necessarily larger in countries with more publicly-traded firms reporting under IFRS. It is also worth noting that at the country-level, many institutional variables are highly correlated and difficult to separate.

With these limitations in mind, we explore cross-sectional variation along two dimensions. We view the evidence as descriptive only. First, we explore proxies for the extent of regulatory harmonization, i.e., the extent to which audit standards are being harmonized and the size of public equity markets to GDP (which is related to how widespread IFRS adoption is in the economy). Second, we explore proxies for the extent to which regulatory harmonization is

likely to affect migration to a country, i.e., the market share of Big 4 auditors and remaining professional entry barriers in the form of licensing requirements. Due to their descriptive nature, we present these analyses in the Internet Appendix only (see Section IA9).

The analyses indicate that the treatment effects tend to be larger in magnitude when accounting and auditing harmonization is greater, i.e., in countries that fully adopted ISA and have large equity markets (see Table IA9b, Panels A and B). The estimated treatment effects are also larger in magnitude in countries with low licensing requirements and a high Big 4 auditor market share (see Table IA9b, Panels C and D). In all cases and for all control groups, the results directionally make sense but the differences in the treatment effects between the high and the low groups are often not statistically significant. As the differences are often large in magnitude, we find this evidence suggestive and descriptively useful.

6. Conclusion

This paper examines the effect of international regulatory harmonization on cross-border labor migration. The idea is that diversity in professional rules presents an economic barrier to cross-border mobility and hence harmonization makes it easier for people to migrate. To analyze this idea, we exploit an increase in harmonization for the accounting profession in the EU in recent years, namely the mandate to report under IFRS and a directive that harmonized statutory audits of companies' financial statements.

Our identification strategy relies on the idea that regulatory harmonization primarily affected the accounting profession. Thus, we perform a difference-in-differences analysis comparing changes in mobility of accounting professionals around regulatory harmonization with changes in mobility of other professions using three separate control groups: legal professionals, all (other) professionals, and a combination of (other) business people. We control

for demographic characteristics that prior studies have shown as related to migration decisions (i.e., gender, marital status, age, education level, and the presence of children), including all possible interactions of these characteristics. In addition, we estimate the effects within country and year to account for unrelated changes and shocks affecting labor mobility of professionals. To further tighten our design, we perform a double-matched difference-in-differences analysis that pairs individuals with the same characteristics from the same country before and after harmonization.

We find that, after regulatory harmonization, cross-border labor migration in the accounting profession increases by roughly 20% to 22% of the pre-treatment mobility rate, relative to all other professionals. This percentage increase translates into an increase in the total number of migrants by 10,000 to 16,000 accounting professionals in our double-matched specifications. We document this increase using several mobility measures and show that it is sustained over time.

Importantly, we obtain significant treatment effects even after controlling for changes in domestic job mobility as well as among individuals that recently changed their jobs. It is therefore unlikely that our results merely reflect changes in the demand for accounting services, rather than regulatory harmonization. Demand effects due to new regulation should be limited in time and affect both domestic and foreign professionals. We also show that the increase in cross-border migration exists when we limit the analysis to EU-15 host and source countries. Thus, our results do not reflect EU enlargement or a migration wave from Eastern to Western Europe. Finally, we provide descriptive evidence suggesting that the migration effects are stronger when there is more harmonization, licensing rules are less strict, and Big-4 auditors have a larger market share, consistent with regulatory harmonization driving our results.

Overall, we conclude that diversity in rules and regulations constitutes an important economic barrier to cross-border labor migration. More specifically, our results imply that accounting and auditing harmonization can reduce this barrier and have a meaningful effect on cross-border migration. Regulatory harmonization could therefore be an important policy instrument to improve cross-border mobility and, ultimately, the efficiency of international labor markets. However, we acknowledge that our findings are limited to the accounting profession, which may be more responsive to regulatory harmonization than other professions. It is also possible that the financial crisis, which partially overlaps with our sample period, exacerbated unemployment and/or wage differentials across the EU, which in turn increased the magnitude of the treatment effect of regulatory harmonization (relative to what it would have been without the crisis). Finally, we emphasize that the LFS dataset has several drawbacks in measuring cross-border mobility around regulatory harmonization and in identifying accounting professionals. For all these reasons, readers should interpret the magnitude of the estimated treatment effects carefully.

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TABLE 1

Description of LFS dataset

Host country	LFS Dataset (2002-2004, 2008-2010)		Treatment (Accountants)	Control (Legal pros)	Control (All pros)	Control (Biz people)	Coverage of mobility metrics (years available)	
	Number of respondents	LFS weighted (in '000)	Number of respondents	Number of respondents	Number of respondents	Number of respondents	Stock measures	Flow measures
Austria	394,442	27,436	2,880	1,710	22,771	40,448	all	all
Belgium	223,058	34,622	4,206	1,293	20,396	20,048	all	all
Bulgaria	126,625	25,379	0	0	0	0	2006-2010	2006-2010
Cyprus	86,654	2,491	1,567	485	5,877	5,917	all	all
Czech Republic	486,237	36,274	5,411	1,429	22,916	35,988	all	all
Denmark	190,995	17,640	4,678	554	17,205	16,351	all	all
Estonia	42,673	4,442	732	103	2,826	4,339	all	all
Finland	167,458	17,078	1,126	313	10,539	11,578	all	all [2002-2007]
France	833,687	193,451	876	2,390	57,215	71,881	all	all [2002-2010^{+}]
Germany	607,135	273,650	6,240	2,592	38,573	34,830	all	all
Greece	586,674	35,829	3,904	3,584	37,626	43,844	all	all
Hungary	601,543	33,850	5,111	1,552	29,546	37,487	all	all
Iceland	35,521	971	735	256	3,899	3,815	all	all [2002]
Ireland	480,646	14,305	7,900	1,790	34,390	54,700	all	2002-2010^{+} [none]
Italy	1,313,874	195,315	7,775	6,732	54,524	95,848	2005-2010	all
Latvia	71,291	7,645	1,227	308	4,706	6,255	2004-2010	all
Lithuania	125,379	11,262	3,656	473	8,549	10,811	all	all
Luxembourg	56,960	1,594	2,202	530	5,367	3,923	all	all
Netherlands	353,578	54,360	8,604	2,243	36,428	44,193	all	all [2006-2010]
Norway	91,248	15,270	1,079	290	5,501	8,766	all	all [2002-2005]
Poland	559,474	130,259	0	0	0	0	2004-2010	2004-2010 [all]
Portugal	331,237	35,521	1,676	976	13,275	19,919	all	all
Romania	481,245	74,745	1,791	1,641	21,564	9,554	2004-2010	2004-2010 [2002-2010^{+}]
Slovakia	225,808	19,083	2,074	596	11,317	12,548	2003-2010	2003-2010
Slovenia	151,935	7,108	0	0	0	0	all	all [2008-2010]
Spain	473,571	152,741	1,858	2,448	26,578	32,834	all	all
Sweden	627,015	28,778	17,437	2,381	61,941	54,818	all	all [2006-2010]
Switzerland	202,076	25,438	5,249	1,071	19,170	9,446	all	all [2010]
United Kingdom	398,166	195,196	5,946	1,740	28,283	58,172	all	all [2002-2010^{+}]
Total	10,326,205	1,671,730	105,940	39,480	600,982	748,313		

Table 1 presents details on the composition of our dataset at the country level. Our analysis is based on the EU's Labor Force Survey (LFS). The LFS dataset provides information from 29 European countries over the years 2002 to 2010. Our sample period focuses on the years 2002 to 2004 and 2008 to 2010, respectively. In the second column, we report the total number of survey respondents (raw and LFS weighted) in the LFS dataset who are between 20 and 59 years old. Our sample focuses on the following professions: The treatment group comprises respondents with job code 241 (business professionals primarily accountants). The control group comprises legal professionals (job code 242), all professionals (all job codes with first digit 2, except 241, 214, 222 and 223) or business people (job codes 121, 122, 123, 131, 341 and 342). We report the raw number of respondents for treatment and control groups in the middle of the table. Our sample is reduced to 26 countries, because Bulgaria, Poland and Slovenia do not provide job codes at the three digit level (i.e., respondents with job codes 241 cannot be identified). In the last two columns of the table, we report country-level statistics on the coverage of our mobility metrics. Stock-based measures are *NATBIRTH* and *NATBIRTH_CHG* (see Table 2 for details). Flow measures are *YEARESID* and *COUNTR1Y* (see Table 4 for details). Coverage details for *COUNTR1Y* are presented in parentheses if they differ from the coverage details for *YEARESID*. The superscript $^{+}$ denotes gaps in the coverage time series (e.g., 2002-2010^{+} indicates that the mobility metric is available for years 2002 and 2010 but not for all years in between).

TABLE 2

Descriptive statistics for the variables in regression analysis

Variables	LFS weighted (yes)		LFS weighted (no)	
	Obs.	Mean	Obs.	Mean
Mobility yes/no (NATBIRTH)				
Treatment (Accountants)	97,821	4.96%	99,061	5.40%
Control (Legal pros)	35,541	2.46%	37,182	2.65%
Control (All pros)	526,678	4.42%	560,701	3.98%
Control (Biz people)	560,804	4.43%	600,772	3.56%
Mobility yes/no (NATBIRTH_CHG)				
Treatment (Accountants)	97,821	2.86%	99,061	2.82%
Control (Legal pros)	35,541	1.36%	37,182	1.36%
Control (All pros)	526,678	2.24%	560,701	1.88%
Control (Biz people)	560,804	2.09%	600,772	1.60%
Control variables (Accountants)				
Domestic job mobility	97,821	39.62%	99,061	37.83%
Female yes/no	97,821	0.465	99,061	0.499
Age	97,821	38.927	99,061	39.861
Has kids yes/no	62,552	0.340	63,792	0.353
Single yes/no	97,821	0.369	99,061	0.342
Higher education yes/no	97,821	0.720	99,061	0.729

Table 2 presents descriptive statistics for the variables in the regression analysis in Table 3. In the second column, we present LFS-weighted statistics (i.e., each observation is weighted with the statistical weight for the individual provided in the LFS dataset). In the last column, we report statistics based on the raw number of survey respondents (i.e., not LFS weighted). Our sample comprises individuals from 26 countries for the years 2002 to 2004 and 2008 to 2010, respectively. We focus on individuals who are between 20 and 59 years old, and whose highest degree of education is at least at the upper secondary level. In addition, we require non-missing information on all control variables as well as on the mobility metrics *NATBIRTH* and *NATBIRTH_CHG*. *NATBIRTH* defines respondents as migrants (or mobile) if they have a foreign nationality and were born outside the host country in which the survey was conducted. *NATBIRTH_CHG* is based on *NATBIRTH* but, as a further requirement, defines only those respondents as migrants who recently changed their jobs. We define recent job changes as those that occurred (a) in/after 1999 for the pre-treatment period (2002 to 2004), and (b) in/after 2005 for the post-treatment period (2008 to 2010). The treatment group comprises accountants. The control group comprises legal professionals, all professionals or business people. For details on the composition of treatment and control groups, see Table 1. The upper (middle) part of Table 2 shows statistics on the mobility metric *NATBIRTH* (*NATBIRTH_CHG*). The lower part presents statistics on the following control variables for the treatment group of accountants: *Domestic job mobility* is the proportion of domestic people in a particular job code who recently changed their jobs in a given year, country, and profession. Recent job changes are again defined as those that occurred (a) in/after 1999 for the pre-treatment period, and (b) in/after 2005 for the post-treatment period. *Female yes/no* is a binary variable and equals one if the individual is female, and zero otherwise. *Age* is the age of the individual measured by the middle value of 5-year bins. *Has kids yes/no* is a binary variable and equals one if the individual has one or more children under the age of 15 living in the same household at the time of the survey, and zero otherwise. *Single yes/no* is a binary variable and equals one if the individual is single, and zero otherwise (married, divorced or widowed). *Higher education yes/no* is a binary variable and equals one if the highest degree of the individual is at the tertiary or doctoral level, and zero if it is at the upper or post-secondary level.

TABLE 3

Difference-in-differences analysis of cross-border mobility effects from regulatory harmonization

Panel A: Baseline regressions

Independent variables	Dependent variable: Mobility yes/no (NATBIRTH)						Dependent variable: Mobility yes/no (NATBIRTH_CHG)					
	LFS weighted (yes)			LFS weighted (no)			LFS weighted (yes)			LFS weighted (no)		
Accountant * Post	1.309**	0.738**	0.716	0.694**	0.830***	0.452*	1.127**	0.767**	0.669*	0.622**	0.703***	0.330
	(2.34)	(2.05)	(1.64)	(2.53)	(3.76)	(1.75)	(2.05)	(2.53)	(1.92)	(2.42)	(2.86)	(1.44)
Control group	Legal pros	All pros	Biz people	Legal pros	All pros	Biz people	Legal pros	All pros	Biz people	Legal pros	All pros	Biz people
Fixed effects	Bin			Bin			Bin			Bin		
	Country * Accountant			Country * Accountant			Country * Accountant			Country * Accountant		
	Country * Year			Country * Year			Country * Year			Country * Year		
Observations	133,362	624,499	658,625	136,243	659,762	699,833	133,362	624,499	658,625	136,243	659,762	699,833
R-squared	0.10	0.05	0.04	0.24	0.13	0.10	0.06	0.04	0.03	0.12	0.07	0.06

Panel B: Baseline regressions with domestic job mobility control

Independent variables	Dependent variable: Mobility yes/no (NATBIRTH)						Dependent variable: Mobility yes/no (NATBIRTH_CHG)					
	LFS weighted (yes)			LFS weighted (no)			LFS weighted (yes)			LFS weighted (no)		
Accountant * Post	1.232**	0.730**	0.776*	0.670**	0.810***	0.550**	1.083*	0.753**	0.747**	0.608**	0.695***	0.447*
	(2.19)	(2.07)	(1.80)	(2.45)	(3.88)	(2.04)	(1.99)	(2.58)	(2.32)	(2.47)	(2.98)	(1.91)
Domestic job mobility	0.047	0.013	0.078*	0.046	0.020	0.060*	0.029	0.023	0.102***	0.046	0.008	0.072**
	(1.19)	(0.39)	(1.97)	(1.09)	(0.72)	(1.86)	(0.87)	(0.72)	(3.15)	(1.17)	(0.25)	(2.40)
Control group	Legal pros	All pros	Biz people	Legal pros	All pros	Biz people	Legal pros	All pros	Biz people	Legal pros	All pros	Biz people
Fixed effects	Bin			Bin			Bin			Bin		
	Country * Accountant			Country * Accountant			Country * Accountant			Country * Accountant		
	Country * Year			Country * Year			Country * Year			Country * Year		
Observations	133,362	624,499	658,625	136,243	659,762	699,833	133,362	624,499	658,625	136,243	659,762	699,833
R-squared	0.10	0.05	0.04	0.24	0.13	0.10	0.06	0.04	0.03	0.12	0.07	0.06

Panels A and B of Table 3 present results from OLS regressions that test for differences in mobility trends between the treatment group (accountants) and the control groups (legal professionals, all professionals or business people). Our sample comprises individuals from 26 countries for the years 2002 to 2004 and 2008 to 2010, respectively. The dependent variable is a binary variable and equals one if the individual is mobile according to the relevant mobility metric (*NATBIRTH* or *NATBIRTH_CHG*), and zero otherwise. The independent variables are defined as follows: *Accountant* is a binary variable equal to one for individuals from the treatment group (job code 241), and zero for individuals from the relevant control group. *Post* is a binary variable equal to one for years 2008 to 2010, and zero for years 2002 to 2004. We include fixed effects to control for differences in individuals' characteristics (*Bin*), country-specific mobility differences between the treatment and the control group (*Country*Accountant*) and country-specific mobility trends (*Country*Year*). *Bin* introduces 192 separate fixed effects, one for each combination of the control variables: gender, age, presence of children, marital status, and education level (see Table 2 for details). These fixed effects control for the full set of interactions between all control variables. Panel A (Panel B) present results from the baseline regressions (with *Domestic job mobility* as additional control). *Domestic job mobility* is the proportion of domestic people in a particular job code who recently changed their jobs in a given

TABLE 3 (continued)

year and country (see Table 2 for details). In the first six columns of Panels A and B, we use *NATBIRTH* as migration measure and present regression results with and without weighing observations by the statistical weight provided for each individual in the LFS dataset. In the last six columns of Panels A and B, we present the results using *NATBIRTH_CHG*, both with and without LFS weights. *NATBIRTH* defines respondents as mobile if they have a foreign nationality and were born outside the host country in which the survey was conducted. *NATBIRTH_CHG* is based on *NATBIRTH* but, as a further requirement, defines only those respondents as migrants who recently changed their jobs (see Table 2 for details). The table reports OLS coefficient estimates and t-statistics (in parentheses). We multiply the coefficient estimates by 100 for expositional purposes. The t-statistics are based on robust standard errors with one-way clustering by country-job group. ***, **, * indicate statistical significance at the 1%, 5% and 10% levels (two-sided), respectively.

TABLE 3 (continued)

Panel C: Double matched diff-in-diff analysis

Double matched diff-in-diff analysis	Full sample						Only recent job changers		
	Mobility rate: NATBIRTH			Mobility rate: NATBIRTH_CHG			Mobility rate: NATBIRTH		
	Pre-period (1)	Post-period (2)	Difference (2) - (1)	Pre-period (1)	Post-period (2)	Difference (2) - (1)	Pre-period (1)	Post-period (2)	Difference (2) - (1)
LFS weighted (yes)									
(a) Treatment (Accountants)	3.402	5.118	1.717	1.924	2.919	0.994	4.540	7.467	2.927
(b) Control (All pros)	3.940	4.965	1.026	2.050	2.621	0.571	5.388	6.941	1.553
Difference (a) - (b)	-0.538	0.153	**0.691*** (4.02)**	-0.126	0.297	**0.423** (2.83)**	-0.849	0.526	**1.374*** (3.06)**
Observations	482,603			482,603			135,776		
LFS weighted (no)									
(a) Treatment (Accountants)	6.013	7.605	1.592	3.098	4.205	1.107	7.808	10.205	2.397
(b) Control (All pros)	6.066	7.010	0.944	3.063	3.581	0.518	8.793	10.442	1.649
Difference (a) - (b)	-0.053	0.595	**0.648*** (2.66)**	0.035	0.624	**0.589* (1.89)**	-0.985	-0.237	**0.749* (1.97)**
Observations	498,950			498,950			137,827		

Panel C of Table 3 presents results from a double matched difference-in-differences analysis that tests for differences in mobility trends between the treatment group (accountants) and the control group of all professionals. The sample comprises individuals from 24 countries for the years 2002 to 2004 and 2008 to 2010 (Italy drops out due to missing mobility metrics in the pre-treatment period; Romania drops out because job codes are only available at the three digit level in the pre-treatment period). The sample is restricted to pairs of treatment and control group individuals with the same characteristics for a year in the pre-treatment period and a year in the post-treatment period (see Figure 2 for details on the double matched difference-in-differences approach). We match on gender, age, presence of children, marital status, and education level (see Table 2 for details). The left (middle) part of the table presents results based on the full sample using *NATBIRTH* (*NATBIRTH_CHG*) as mobility metric. *NATBIRTH* defines respondents as mobile if they have a foreign nationality and were born outside the host country in which the survey was conducted. *NATBIRTH_CHG* is based on *NATBIRTH* but, as a further requirement, defines only those respondents as migrants who recently changed their jobs (see Table 2 for details). The right part of the table also uses NATBIRTH as mobility metric but focuses on the subsample of people who recently changed jobs. We define recent job changes as those that occurred (a) in/after 1999 for the pre-treatment period (2002 to 2004), and (b) in/after 2005 for the post-treatment period (2008 to 2010). The table reports the mobility rate in percent for each cell in the difference-in-differences analysis (treatment and control; pre and post) and presents results both with and without LFS weights. The treatment effect is the weighted average of the quadruplet treatment effects using the minimum number of individuals from treatment or control entering the quadruplet as a weight. The t-statistics (in parentheses) are based on robust standard errors with one-way clustering by country-job group. ***, **, * indicate statistical significance at the 1%, 5% and 10% levels (two-sided), respectively. ^ indicates statistical significance at the 10% level (one-sided).

TABLE 4

Difference-in-differences analysis of cross-border mobility effects using flow-based metrics

Double matched diff-in-diff analysis	Full sample		Singles without kids		Young singles without kids		Young singles without kids at big employer	
	LFS weighted (yes)	LFS weighted (no)	LFS weighted (yes)	LFS weighted (no)	LFS weighted (yes)	LFS weighted (no)	LFS weighted (yes)	LFS weighted (no)
Mobility rate: YEARESID								
Pre-treatment mobility	1.330	2.351	1.621	2.314	1.674	2.483	2.538	3.652
Treatment effect	**0.267**	**0.107**	**0.458^**	**0.168**	**0.501***	**0.277^**	**1.731*****	**0.798****
	(1.10)	**(0.55)**	**(1.56)**	**(0.93)**	**(1.72)**	**(1.50)**	**(6.02)**	**(2.32)**
Observations	480,241	496,620	84,724	86,717	71,769	73,358	22,747	23,297
Mobility rate: COUNTR1Y								
Pre-treatment mobility	0.185	0.199	0.424	0.399	0.422	0.434	0.583	0.526
Treatment effect	**0.101**	**0.041**	**0.521*****	**0.367***	**0.566****	**0.417***	**0.771*****	**0.427**
	(1.48)	**(0.41)**	**(2.99)**	**(1.75)**	**(3.06)**	**(1.77)**	**(5.05)**	**(0.80)**
Observations	365,627	366,997	70,736	70,972	60,791	60,987	18,296	18,303

Table 4 presents results from the double matched difference-in-differences analysis with two alternative mobility metrics (*YEARESID* and *COUNTR1Y*). The sample comprises individuals from 26 countries for the years 2002 to 2004 and 2008 to 2010, respectively (25 countries for COUNTR1Y because this item is not available for Ireland). The research design is the same as in the analysis presented in Panel C of Table 3. All results in this table are based on tests that use all professionals as the control group. The alternative mobility metrics are as follows: *YEARESID* defines individuals as mobile if they were born abroad and moved to the host country in/after 1999 (for sample years 2002 to 2004) or in/after 2005 (for sample years 2008 to 2010). *COUNTR1Y* defines individuals as mobile if they moved to the host country within the last year. We report results for the full sample as well as for the following subsamples: *Singles without kids* focuses on individuals who are single (*Single yes/no* = 1) and who do not have children aged less than 15 years living in the same household (*Has kids yes/*no = 0). *Young singles without kids* focuses on singles without children who are less than 40 years old (20 <= *Age*< 40). *Young Singles without kids at big employer* focuses on young singles without children who work for a firm with more than 50 employees (LFS item *SIZEFIRM* measured at local unit). The table reports pre-treatment mobility rates in percent for the treatment group, minimum-sample weighted treatment effects as defined in Figure 2, t-statistics (in parentheses) as well as the number of quadruplets and observations in these quadruplets. We report these statistics both with and without LFS weights. The t-statistics are based on robust standard errors with one-way clustering by country-job group. ***, **, * indicate statistical significance at the 1%, 5% and 10% levels (two-sided), respectively. ^ indicates statistical significance at the 10% level (one-sided).

TABLE 5

Migrations to and within EU-15 around regulatory harmonization

Independent variables	Dependent variable: Mobility yes/no (NATBIRTH_CHG)					
	EU-15 host countries only			EU-15 host and EU-15 source countries only		
Accountant * Post	1.122*	0.775**	0.821**	0.640**	0.518**	0.541**
	(1.80)	(2.32)	(2.28)	(2.62)	(2.51)	(2.51)
Domestic job mobility	0.039	0.031	0.130***	0.029	-0.004	0.030
	(0.90)	(0.70)	(3.56)	(1.28)	(-0.21)	(1.40)
Control group	Legal pros	All pros	Biz people	Legal pros	All pros	Biz people
Fixed effects	Bin			Bin		
	Country * Accountant			Country * Accountant		
	Country * Year			Country * Year		
Observations	98,075	468,711	493,418	98,075	468,711	493,418
R-squared	0.06	0.03	0.03	0.07	0.02	0.02

Table 5 presents for two sets of specifications on migrations to and within EU-15 countries: The first set of regressions is restricted to the subsample of survey individuals from EU-15 host countries. The second set of regressions is restricted to individuals from EU-15 host countries and, in addition, to migrants from EU-15 source countries (i.e., the dependent variable equals one only if a mobile individual was born inside the EU-15 and has a EU-15 nationality). The research design is the same as in the regression analysis presented in Panel B of Table 3. All regression results in this table are based on LFS weighted observations. The table reports OLS coefficient estimates and t-statistics (in parentheses). We multiply the coefficient estimates by 100 for expositional purposes. The t-statistics are based on robust standard errors with one-way clustering by country-job group. ***, **, * indicate statistical significance at the 1%, 5% and 10% levels (two-sided), respectively.

FIGURE 1

Mobility trends at aggregate level

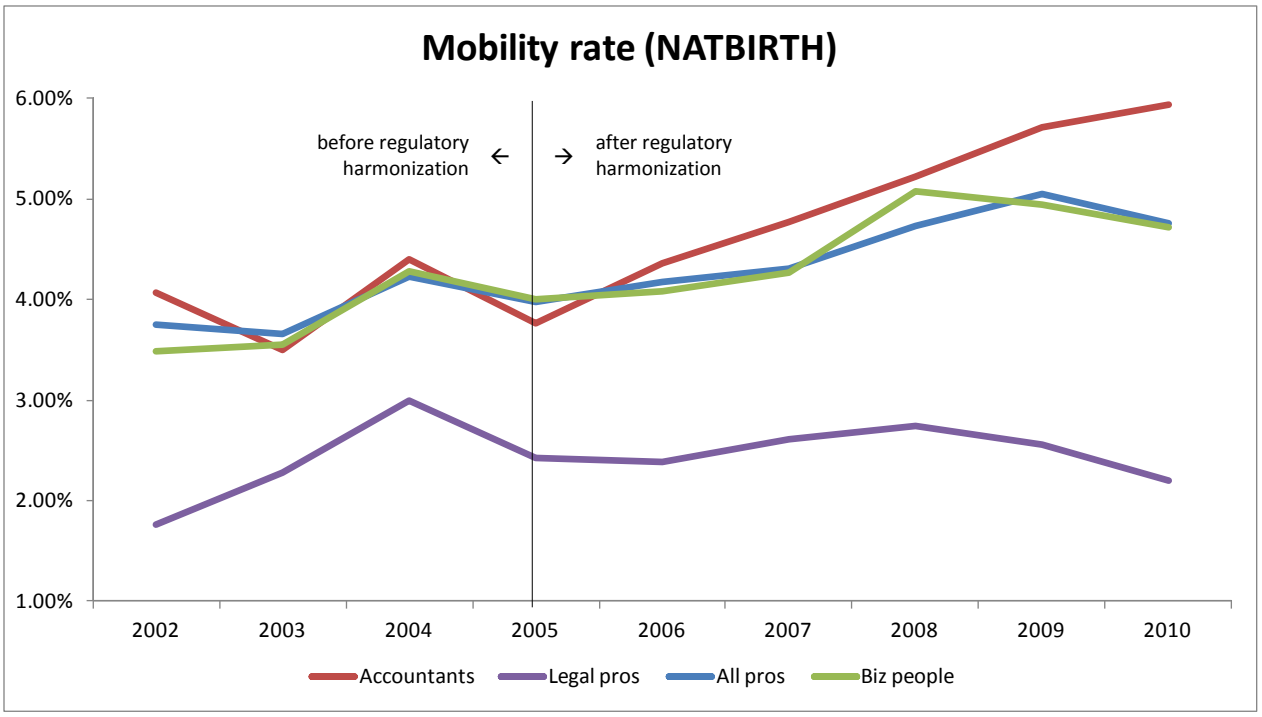

Figure 1 compares mobility rates between the treatment group (accountants) and the control groups (legal professionals, all professionals and business people) over time. The analysis is based on the mobility metric *NATBIRTH*. This metric defines individuals as migrants (or mobile) if they were born outside the host country and have a foreign nationality. The graph shows aggregate mobility rates for the sample used in the main regression analysis (see Table 3, Panel A) over the period 2002 to 2010. To make mobility rates representative of the population, the aggregate mobility rates in this graph are weighted averages using the statistical weight the LFS dataset provides for each individual.

FIGURE 2

Description of double matched difference-in-differences approach

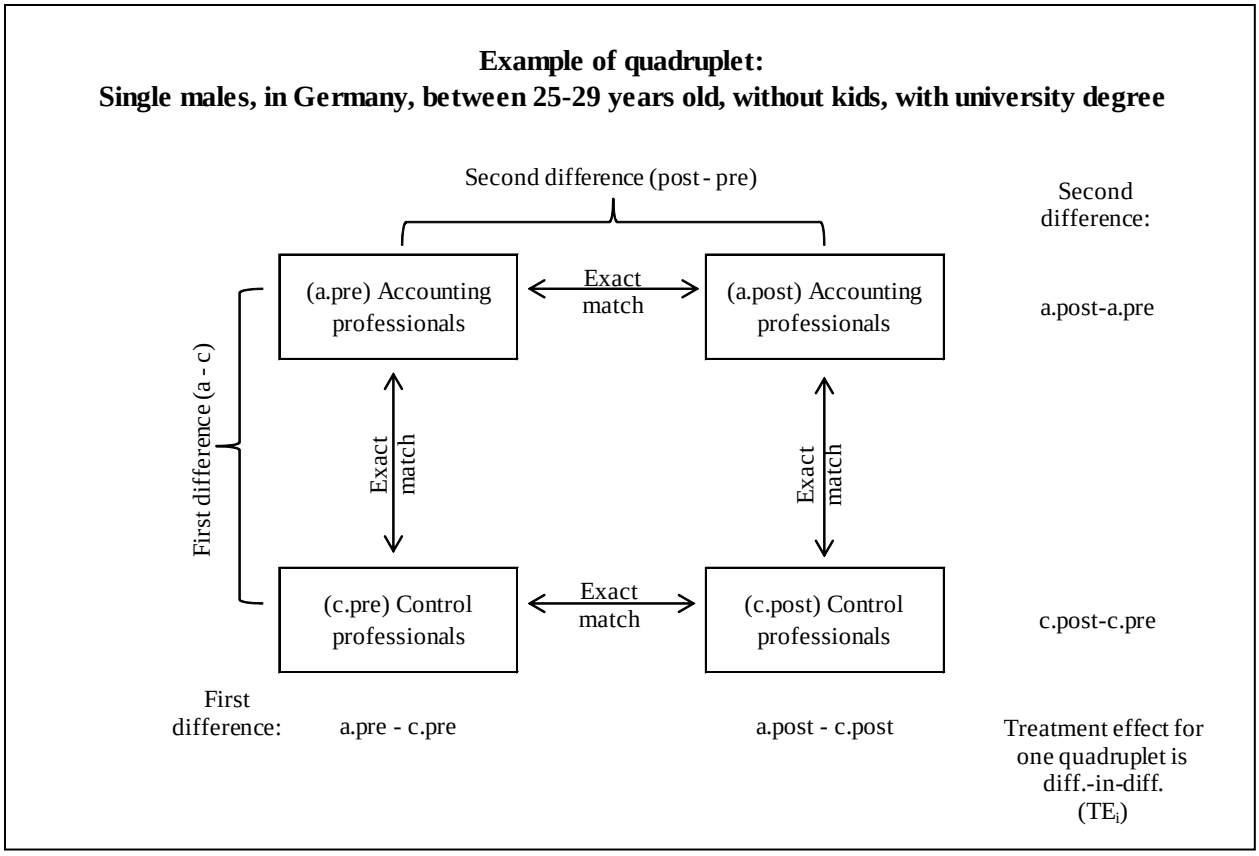

Figure 2 illustrates the double-matched difference-in-differences approach. Within each country and year, we first match all accounting professionals and all control observations. We further match pre-treatment observations in year t to post-treatment observations in year t+6 (e.g., observations in 2004 to observations in 2010). Along both dimensions, we perform an exact match on gender, marital status, age, education level, and the presence of one or more children under the age of 15 living in the household (see Table 2 for details). This approach yields two sets of accounting professionals and two sets of control professionals (one pre- and one post-treatment), which share the exact same characteristics (illustrated by the four boxes above). We define these four sets as a quadruplet and compute the difference-in-differences (or treatment effect) within each quadruplet. The total treatment effect is the weighted average over all quadruplets, i.e., $\Sigma w_i TE_i$, where TE_i is the treatment effect for quadruplet i and w_i is the weight assigned to quadruplet i. We use the minimum sample in any of the four boxes in each quadruplet i as weight because the information in the quadruplet is ultimately constrained by the smallest sample size of the four boxes.

For Online Publication

Internet Appendix for "The Effect of Regulatory Harmonization on Cross-border Labor Migration: Evidence from the Accounting Profession"

This appendix provides supplemental discussion and analysis for our manuscript "The Effect of Regulatory Harmonization on Cross-border Labor Migration: Evidence from the Accounting Profession." We summarize the content as follows:

Section IA1: Quotes Regarding Labor Mobility from Comment Letters to the European Commission's Consultation on the Impact of IAS Regulation in the EU

To provide anecdotal evidence that regulatory harmonization increases labor migration, we collected and read comment letters to the European Commission for its Consultation on the Impact of IAS Regulation in the EU. These letters, which were written in 2014, provide essentially an ex-post assessment of IFRS adoption. In Table IA1, we provide excerpts and quotes from these letters with respect to labor mobility. The table shows that various groups claim that the use of IFRS (or a common language) has helped the mobility of accountants in the EU. While this evidence is clearly more anecdotal in nature, it supports the plausibility of our findings and common standards being the mechanism.

TABLE IA1

Quotes from comment letters to the European Commission for its Consultation on the Impact of IAS Regulation

Organization	*Quote*
Panel A: Audit Firms	
EY	[T]he common language of IFRS has also helped mobility of accountants across the EU. EY and its clients are able to move people around and recruit from beyond our national boundaries, using people more effectively, reducing costs and helping to foster a more integrated community.
KPMG IFRG Limited	...we would list increased mobility of people knowledgeable in IFRS as an additional benefit of IFRS implementation.
BDO IFR Advisory Ltd	...the adoption of IFRS as the global accounting language has enhanced global mobility of employees within multinational organisations.
Deloitte and Associés	...the use of a single accounting language helps for the mobility of skilled workers within the the EU and globally.
Grant Thornton International Ltd	The Regulation is also directionally conducive to a free market in accounting services across the EU and enhances the mobility of qualified accountants.
Panel B: Non-audit Firms	
RSM International	...use of IFRS increases the global mobility of expertise and resources, thus contributing to quality of international groups' internal processes and control.
Nestle S.A.	[IFRS] has facilitated communication with lenders and investors. It has enhanced internal mobility and lowered training costs, as staff can access local training for IFRS basics.
HSBC Holdings PLC	There are tangible and intangible benefits to being able to apply IFRS to local and group reporting across a large, international group in terms of cost savings and in improved understanding and communication and finance staff mobility.

(continued)

TABLE IA1 (continued)

Panel C: Organizations of Auditors or Finance Professionals	
ACCA	…the development of IFRS as the single accounting language, has permitted greater mobility for accounting staff between companies and between countries. It has also allowed for easier and improved training and education of accountants.
FEE – Federation of European Accountants	FEE believes that the use of international standards increases the mobility of expertise and resources across different jurisdictions.
Financial Accounting and Reporting Special Interest Group (FARSIG	There is a major advantage in providing global education in accounting across EU universities and also in facilitating training of professional accountants across the EU, all of which is consistent with the EU aims of mobility of labour.
100 Group of Finance Directors	...the adoption of a single accounting language [i.e., IFRS] by listed companies has improved the mobility of finance professionals across the EU.
International Swaps and Derivatives Association Inc (ISDA)	It has also significantly increased the transferability and career mobility of accountants and other finance professionals across the EU and globally by providing them with a common language applicable for financial reporting of all EU companies.

Table IA1 presents selected quotes from comment letters sent to the European Commission for its Consultation on the Impact of IAS Regulation. The comment period began in August 2014 and ended in November 2014 (i.e., more than eight years after IFRS became mandatory). All selected quotes are from qualitative responses to the open ended question no. 19: "Do you see other benefits from applying IFRS as required under the IAS Regulation?"

Section IA2: Differences between National Audit Standards and ISA

In this section, we report descriptive evidence on country-level differences between national auditing standards and International Standards on Auditing (ISA) prior to harmonization. Column (1) in Table IA2 reports whether there was an English translation of national audit standards in 2005. Among the 29 European countries covered by the LFS dataset, an English translation was available in 12 countries. Yet, it is worth noting that the English translations in these 12 countries were not always up to date and that national standards in local language generally took precedence. Column (2) reports the number of additional reporting requirements included in national auditing standards that were not part of ISA prior to harmonization. Among the 29 countries, 7 countries had additional national reporting requirements. Column (3) reports the number of additional procedural requirements included in national auditing standards that were not part of ISA prior to harmonization. Among the 29 countries, 11 countries had additional national procedural requirements.

Note that the evidence in Table IA2 understates actual differences for two reasons. First, the table focuses on differences between auditing standards, yet audit requirements stem also from laws or regulation that have been subject to EU harmonization efforts. For simplicity, we do not tabulate differences that result from such laws or regulation. This table merely serves as evidence that there were significant differences (and is not meant to be a comprehensive dataset of these differences). Second, the source documents that we use to create Table IA2 were initially created to inform policy makers about the decision to mandate ISA in the EU and as such they focus only on significant differences. However, as we discuss in the main text of the study, formal (or more minor) differences could matter and impose barriers to migration as well.

TABLE IA2

National auditing standards versus ISA

Country	English in 2005? Yes=1 (1)	No.of Significant Additional National Reporting in 2003 (2)	No. of Significant Additional National Procedures in 2003 (3)
Austria	0	0	0
Belgium	0	1	0
Bulgaria	1	0	0
Cyprus	1	0	0
Czech Republic	1	0	3
Denmark	0	0	7
Estonia	1	0	0
Finland	1	0	0
France	0	0	0
Germany	0	4	8
Greece	0	0	2
Hungary	0	0	0
Iceland	1	0	0
Ireland	1	2	10
Italy	0	0	6
Latvia	1	0	0
Lithuania	0	0	0
Luxembourg	0	0	0
Netherlands	0	1	12
Norway	0	0	0
Poland	0	1	5
Portugal	0	1	2
Romania	1	0	0
Slovakia	1	0	0
Slovenia	1	0	0
Spain	0	0	0
Sweden	0	0	0
Switzerland	0	0	1
United Kingdom	1	2	10

Table IA2 presents descriptive statistics at the country level on the differences between national auditing standards and ISA prior to regulatory harmonization. Column (1) indicates countries with an English translation of national auditing standards in 2005. We obtain data on the language of national audit standards from answers to IFAC's Compliance Program Responses and Action Plans (Section 9A). Column (2) [(3)] reports the number of significant reporting [procedural] requirements in national auditing standards that do not exist in International Auditing Standards. We cannot publicly reveal the source of the data in Columns (2) and (3) for contractual reasons but have identified the source to the Editor in compliance with JAR's data policy.

Section IA3: Distribution of Key Characteristics for Treatment and Control Groups

In Table IA3, we provide descriptive statistics for key characteristics of the individuals in the treatment group (accounting professionals) and the control groups (legal professionals, all professionals and business people). The statistics are based on the raw number of survey respondents in the LFS dataset who are between 20 and 59 years old over the sample period (years 2002 to 2004 and 2008 to 2010, respectively). The LFS dataset provides information from 29 European countries. The final sample comprises a subset of 26 countries (see Table 1 for further details).

TABLE IA3

Distribution of key demographic characteristics for treatment and control groups

Panel A: Gender

Gender	Accountants		Legal pros		All pros		Biz people	
	Obs.	Share	Obs.	Share	Obs.	Share	Obs.	Share
Male	53,314	50%	19,915	50%	261,759	44%	478,454	64%
Female	52,626	50%	19,565	50%	339,223	56%	269,859	36%
Total	105,940	100%	39,480	100%	600,982	100%	748,313	100%

The LFS dataset provides information on the gender of the survey respondents (item *SEX*).

Panel B: Age

Age	Accountants		Legal pros		All pros		Biz people	
	Obs.	Share	Obs.	Share	Obs.	Share	Obs.	Share
20-24	4,572	4%	788	2%	20,344	3%	27,608	4%
25-29	13,762	13%	4,894	12%	69,734	12%	64,112	9%
30-34	17,049	16%	7,255	18%	86,222	14%	93,097	12%
35-39	17,816	17%	7,319	19%	91,212	15%	118,186	16%
40-44	16,469	16%	6,358	16%	91,532	15%	127,704	17%
45-49	14,196	13%	5,243	13%	89,746	15%	123,035	16%
50-54	12,026	11%	4,361	11%	85,253	14%	109,032	15%
55-59	10,050	9%	3,262	8%	66,939	11%	85,539	11%
Total	105,940	100%	39,480	100%	600,982	100%	748,313	100%

The LFS dataset provides information on the age (measured in bins of 5 years) of the survey respondents (item *AGE*).

TABLE IA3 (continued)

Panel C: Marital status

Marital status	Accountants		Legal pros		All pros		Biz people	
	Obs.	Share	Obs.	Share	Obs.	Share	Obs.	Share
Divorced	8,312	8%	2,480	6%	47,773	8%	63,647	9%
Single	36,060	34%	14,043	36%	199,718	33%	193,849	26%
Married	61,526	58%	22,941	58%	353,100	59%	490,579	66%
Missing	42	0%	16	0%	391	0%	238	0%
Total	105,940	100%	39,480	100%	600,982	100%	748,313	100%

The LFS dataset provides information on the marital status of the survey respondents (item *MARSTAT*).

Panel D: Number of children

Number of children	Accountants		Legal pros		All pros		Biz people	
	Obs.	Share	Obs.	Share	Obs.	Share	Obs.	Share
0	43,827	41%	20,589	52%	289,753	48%	364,656	49%
1	12,094	11%	5,926	15%	80,311	13%	115,246	15%
2	9,695	9%	5,106	13%	62,679	10%	88,314	12%
3	1,850	2%	1,041	3%	13,235	2%	17,699	2%
4	230	0%	138	0%	1,976	0%	2,468	0%
5 or more	40	0%	25	0%	383	0%	456	0%
Missing	38,204	36%	6,655	17%	152,645	25%	159,474	21%
Total	105,940	100%	39,480	100%	600,982	100%	748,313	100%

The LFS dataset provides information on the number of children (aged less than 15 years) in the household of the survey respondents based on the items *QHHNUM* (serial number of household), *HHLINK* (relationship to reference person in household), and *AGE*.

Panel E: Level of education

Level of education	Accountants		Legal pros		All pros		Biz people	
	Obs.	Share	Obs.	Share	Obs.	Share	Obs.	Share
Pre-primary	10	0%	10	0%	140	0%	898	0%
Primary	171	0%	15	0%	879	0%	30,123	4%
Lower secondary	2,418	2%	125	0%	8,051	1%	89,679	12%
Upper secondary	23,324	22%	978	2%	70,621	12%	325,422	43%
Post secondary	4,548	4%	228	1%	16,498	3%	35,836	5%
Tertiary	72,818	69%	35,658	90%	473,722	79%	255,928	34%
Doctoral	1,910	2%	2,296	6%	28,325	5%	5,629	1%
Missing	741	1%	170	0%	2,746	0%	4,798	1%
Total	105,940	100%	39,480	100%	600,982	100%	748,313	100%

The LFS dataset provides information on the highest level of education that the survey respondent obtained (item *HATLEVEL*). This item is based on the International Standard Classification of Education (ISCED) from 1997.

Section IA4: Descriptive Migration Statistics by Country

This section provides country-specific migration statistics for accountants as well as changes in these rates over time. The statistics are based on the double-matched sample from Table 3, Panel C (see manuscript) with LFS weights. Table IA4 reports descriptive statistics for the pre-treatment period (2002-2004), the post-treatment period (2008-2010), changes from the pre- to the post-period, and changes from the pre- to the post-period of accountants relative to all professionals. We report these statistics for our migration metrics *NATBIRTH* and *NATBIRTH_CHG*. The descriptive statistics show that there is considerable cross-sectional variation in the fraction of migrants across host countries. Small host countries such as Luxembourg and Switzerland exhibit the highest migration base rates. The rates are the lowest in Eastern European countries such as Latvia, Lithuania, and Slovakia. This cross-country variation in mobility is largely in line with the statistics presented in Münz (2007). As one would expect, the fraction of migrants is not always higher in the post-treatment period for all countries. The average change from the pre- to the post-period across countries is 1.60 (1.28) percentage points for *NATBIRTH* (*NATBIRTH_CHG*). When we compare these changes for accountants relative to all professionals, the change in the country-level average drops to 0.78 (0.79) percentage points for *NATBIRTH* (*NATBIRTH_CHG*), which is (only a descriptive statistic but) close to the changes documented in Table 3, Panel C in the manuscript when using the double-matched approach. We further use a t-test to assess the statistical significance of these results and find that the average changes are positive and significant, at the 5%-level, for both *NATBIRTH* and *NATBIRTH_CHG*.

Table IA4 shows that certain, in particular smaller, countries exhibit strong increases in migrants (e.g., Austria, Cyprus, Luxembourg, and Switzerland), which seems plausible. These countries have a high fraction of firms that operate internationally and therefore are able to exploit regulatory harmonization. For instance, Nestle S.A., headquartered in Switzerland, argue in a comment letter to the European Commission from March 2014 that IFRS adoption has "enhanced internal mobility" of their workforce (see Table IA1). Iceland is the only country that exhibits a larger and consistently negative change, which likely reflects the fact that its financial sector was hit particularly hard by the financial crisis during the post-treatment period.

As reported in Footnotes 17 and 34, we also estimate treatment effects within six regions, for the primary purpose of gauging the robustness of our inferences for cross-sectional dependencies. However, this analysis also provides insights into regional heterogeneity in the treatment effects. Unlike the descriptive statistics in Table IA4, these regional analyses use the same difference-in-differences design as the regressions reported in Table 3, Panels A and B. We divide the EU countries into six groups based on geography and language: West (BE, FR, LU and NL), Middle (AT, CH and DE), East (CZ, EE, HU, LT, LV, RO and SK), South (CY, ES, GR, IT and PT), Scandinavia (DK, FI, IS, NO and SE), and English (UK, IE). Among these regions, we find the strongest treatment effects for the English-speaking region (UK, IE), consistent with the expectation that for these countries language poses a lower barrier to migration. However,

controlling for demand effects, there are also strong treatment effects in the West, the South and, in unweighted regressions, for the Middle. The latter illustrates that one has to exercise caution when interpreting Table IA4 because some countries that exhibit larger pre-post differences below, such as Austria and Switzerland, receive relatively small weights in the LFS-weighted regressions presented in Table 3 (Panels A and B). The East is the only region for which we do not find significantly positive treatment effects in any of the specifications. This finding comports with the fact that we essentially measure migration *into* a country as well as the broader migration pattern from Eastern countries to the rest of the EU shown in prior work, which likely reflects the economic situation in Eastern countries coupled with EU enlargement.

TABLE IA4

Country-specific mobility statistics

Host country	NATBIRTH				NATBIRTH_CHG			
	2002-2004 (Pre)	2008-2010 (Post)	Diff (Post-Pre)	Diff relative all pros	2002-2004 (Pre)	2008-2010 (Post)	Diff (Post-Pre)	Diff relative all pros
Austria	6.69%	13.80%	7.12%	3.71%	1.76%	7.62%	5.86%	3.27%
Belgium	6.55%	6.21%	-0.33%	-1.05%	3.51%	3.15%	-0.36%	-0.31%
Bulgaria								
Cyprus	5.73%	9.83%	4.10%	4.75%	3.36%	5.92%	2.56%	3.07%
Czech Republic	1.11%	0.78%	-0.33%	-0.48%	0.58%	0.39%	-0.19%	-0.33%
Denmark	1.76%	2.28%	0.52%	0.70%	1.25%	1.38%	0.13%	0.28%
Estonia	4.29%	1.22%	-3.07%	-1.48%	0.00%	0.00%	0.00%	0.51%
Finland	0.28%	0.21%	-0.06%	-0.42%	0.09%	0.00%	-0.09%	-0.20%
France	3.29%	3.99%	0.70%	0.63%	1.14%	3.06%	1.92%	1.73%
Germany	3.31%	4.45%	1.14%	-0.28%	1.78%	1.92%	0.14%	-0.30%
Greece	1.70%	1.66%	-0.05%	0.02%	1.09%	0.24%	-0.85%	-0.97%
Hungary	0.46%	0.22%	-0.25%	-0.48%	0.26%	0.22%	-0.04%	-0.14%
Iceland	2.76%	0.96%	-1.81%	-1.41%	2.76%	0.27%	-2.49%	-2.06%
Ireland	5.35%	9.27%	4.40%	1.61%	4.09%	5.81%	1.71%	1.03%
Italy								
Latvia	0.00%	2.63%	2.63%	-0.60%	0.00%	0.00%	0.00%	-1.81%
Lithuania	0.18%	0.56%	0.38%	1.48%	0.00%	0.56%	0.56%	0.97%
Luxembourg	64.76%	75.26%	10.50%	4.63%	27.64%	38.30%	10.66%	7.12%
Netherlands	1.94%	2.60%	0.65%	0.70%	0.51%	1.38%	0.87%	0.92%
Norway	1.27%	4.18%	2.91%	2.37%	0.67%	2.54%	1.87%	0.45%
Poland								
Portugal	1.17%	1.68%	0.51%	0.95%	1.17%	1.59%	0.42%	0.73%
Romania								
Slovakia	0.00%	0.26%	0.26%	-0.04%	0.00%	0.00%	0.00%	-0.15%
Slovenia								
Spain	3.89%	5.53%	1.64%	0.61%	1.96%	4.51%	2.55%	1.94%
Sweden	3.00%	3.32%	0.32%	-0.24%	1.65%	1.67%	0.02%	-0.45%
Switzerland	17.34%	18.45%	1.11%	1.11%	9.22%	11.23%	2.01%	2.34%
United Kingdom	4.71%	10.02%	5.31%	1.84%	3.21%	6.67%	3.46%	1.31%
Total	5.90%	7.47%	1.60%	0.78%**	2.82%	4.10%	1.28%	0.79%**
				(2.22)				(2.04)

Table IA4 reports country-level statistics on mobility in the pre-treatment period (2002 to 2004), post-treatment period (2008 to 2010), the change in mobility from the pre- to the post-period, and the change in mobility from the pre- to the post-period for accountants relative to all professionals. All statistics are calculated based on the double-matched sample used in Table 3, Panel C of the manuscript. These statistics cannot be calculated for some countries because job codes are not available at the three digit level (Bulgaria, Poland and Slovenia) or because of missing data in the pre-treatment period (Italy and Romania). The t-statistics at the bottom of the table (in parentheses) are based on robust standard errors with one-way clustering by country-job group. ***, **, * indicate statistical significance at the 1%, 5% and 10% levels (two-sided), respectively.

Section IA5: Mobility Trends for Extended Period from 2002 to 2013

In our analyses in the manuscript, the sample period ends in 2010 because job codes change in 2011. As it is important to show that the documented cross-border mobility effects are persistent, we use data from the latest LFS release to extend the time series in Figure 1 until 2013. To adjust for changing job codes in 2011, we subtract the mobility change from 2010 to 2011 for accounting professionals and each of the control groups from the migration numbers of 2012 and 2013. We report graphs using the extended time-series in Figure IA5. The figure shows that the effect persists beyond 2010. However, given that the adjustment for the change in job codes is not perfect, we do not think that it is appropriate to extend the sample beyond 2010 in our primary analyses.

FIGURE IA5

Mobility trends at aggregate level from 2002 to 2013

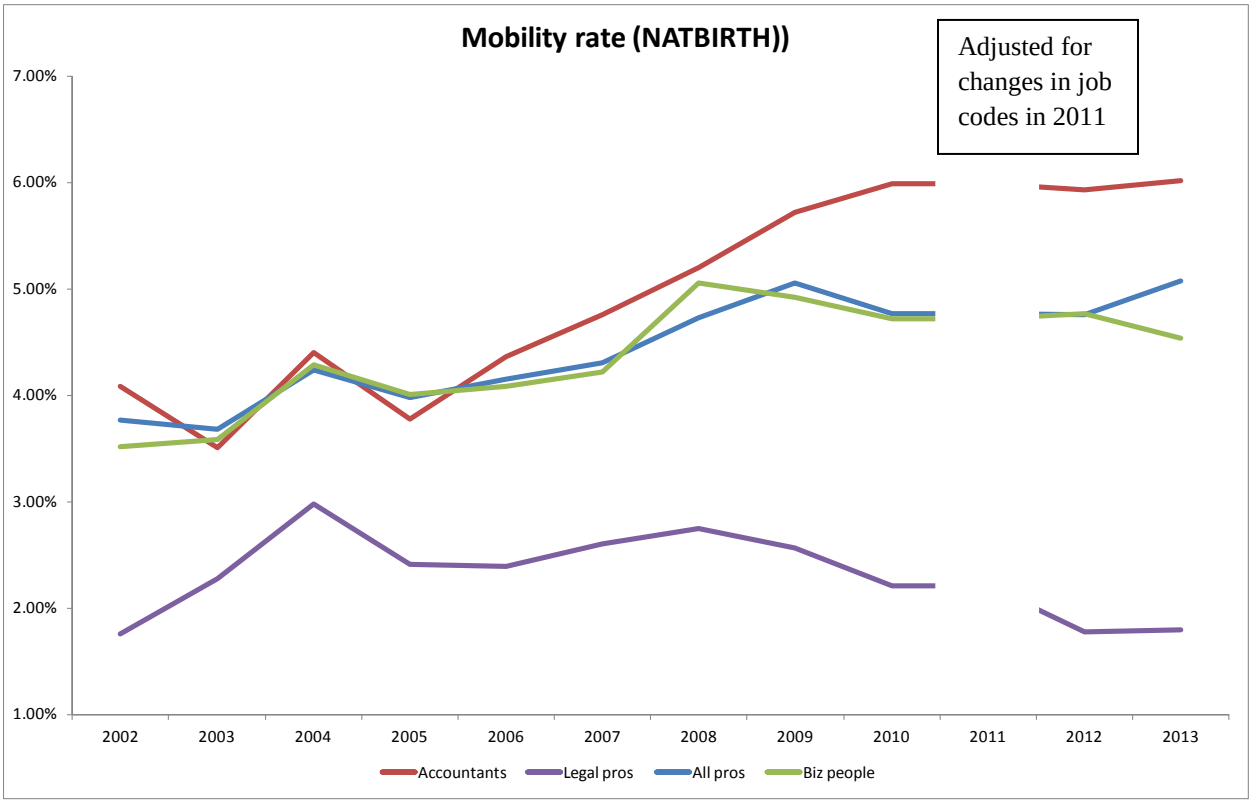

Figure IA5 compares mobility rates between the treatment group (accountants) and the control groups (legal professionals, all professionals, and business people) over time. To adjust for changing job codes in 2011, we subtract the mobility change from 2010 to 2011 for accounting professionals and each of the control groups from the migration numbers of 2012 and 2013. The analysis is based on the mobility metric *NATBIRTH*. This metric defines individuals as migrants (or mobile) if they were born outside the host country and have a foreign nationality. To make mobility rates representative of the population, the aggregate mobility rates in this graph are weighted averages using the statistical weight the LFS dataset provides for each individual.

Section IA6: Assessing Alternative Explanations

Our difference-in-differences estimation combined with matching of individuals of various professions by country (and year) implies a within-bin and within-country comparison across professions. This approach alleviates many concerns about concurrent events that are unrelated to accounting harmonization, provided these events apply to all professionals in a given country (and year). However, events and/or institutional changes that differentially affect the mobility rates of accounting professionals and of the control groups over time could affect our analysis. Such events or institutional changes essentially violate the parallel-trends assumption. In this section, we explore two potential violations and alternative explanations: (i) differential changes in cross-border student mobility and (ii) differential responses to changes in the recognition of professional qualifications.

Cross-Border Student Mobility

One concern arises from potentially differential trends in student mobility. Prior literature shows that studying abroad increases the likelihood that an individual works abroad later in life (e.g., Parey and Waldinger, 2011). Moreover, it is possible that cross-border student mobility differs across study subjects. For instance, if the fraction of business students that study abroad increases earlier than the fraction of law students, then such differential trends could be an alternative explanation for our findings. The launch of the Bologna Process in 1999 adds to this concern. The Bologna Process aims to facilitate mutual recognition of degrees and student mobility through exchange programs. If business schools, which educate the majority of accounting professionals, were more responsive to the Bologna Process than law schools, then the parallel-trends assumption would be invalid.

The concern about differential student mobility trends seems particularly pertinent for the control group consisting of legal professionals. Individuals in the business people control group and the accounting professionals likely have similar educational backgrounds. In fact, the use of business people as a control group could be seen as another way to mitigate the concern. As the control group consisting of all professionals spans a large set of educational backgrounds, it is unlikely that differential student mobility trends across treatment and control groups line up such that they induce our results. Thus, the first sensitivity test focuses on accounting and legal professionals. The second sensitivity test explores this alternative explanation for all three control groups.

First, we obtain data on the number of students participating in the European Community Action Scheme for the Mobility of University Students (ERASMUS) and hence visiting another European country during their studies.[35] The total number of participating students increases over time, as expected. To assess the relative trends, we compute the proportion of business and law students relative to the total number of ERASMUS students and inspect the trends over time.

[35] ERASMUS was created in 1987. It is the largest student exchange program in Europe. By 2013, over 3 million students have participated in the program.

Figure IA6 shows that between 2000 and 2010 the relative proportions of business and law students that participate in the ERASMUS program remain fairly constant over time.[36]

Second, in first three columns of Table IA6, we include the number of years since graduation (i.e., the difference between the sample year and the graduation year of the individual) as an additional control variable. Together with the country-year fixed effects, this variable controls for differences in mobility that are explained by differences in the year of graduation across treatment and control groups (which in turn could be related to differential student mobility). The estimated treatment effects after including this control variable are very similar to those reported in the main analyses in Table 3 of the manuscript.

Finally, in the last column of Table IA6, we include the share of business and law students among all ERASMUS students at host country-year level for accountants and legal professionals, respectively, as an additional control variable (with a two year lag to account for the likely time lag between studying abroad and seeking employment). Again, the estimated treatment effect after including this control variable is very similar to the treatment effect we report in Table 3 of the manuscript.

In sum, there is little evidence that differential trends in student mobility over time explain our results.

Recognition of Professional Qualifications

Another concern arises from differential trends in the recognition of professional qualifications obtained in other EU member states. The EU has taken several regulatory initiatives aimed at promoting the free movement of professionals, including those in the accounting profession, since 1988 (Directive 89/48). During our sample period, the EU enacted Directive 2005/36, which confers individuals that obtained their professional qualifications in one EU member state access to the same profession in another member state. This directive came into force in 2007, although individual member states implemented some of the provisions after this date. Thus, Directive 2005/36 and related prior EU directives offer a path to temporary or permanent recognition of a professional qualification from another EU member state.

To the extent that the accounting profession is more responsive to such regulatory changes than our control professions, the parallel-trends assumption could be violated. That is, the increased mobility of accounting professionals relative to other professions could be due to changes in the explicit recognition of foreign professional qualifications rather than the harmonization of accounting and auditing standards. To address this concern, we obtain data on the number of individuals applying for the recognition of a professional qualification. We compare the number

[36] The ERASMUS program data have two drawbacks. They cover only a specific exchange program (albeit the largest) and they do not allow us to scale the number of participating students with the total number of (foreign and domestic) students in an area. Therefore, Figure IA6 also presents data on the proportion of *all* foreign students at German universities scaled by the *total* number of students in the respective field of study, i.e., business, law and economics over the period from 1990 to 2013. While these proportions change substantially over time, the trends are again similar across fields of study. The drawback of this dataset is that it covers only one country.

of these applications from accounting professionals (in this dataset narrowly defined as accountants and auditors) and other professionals.

Each year an average of 94 accounting professionals across the EU applied for the recognition of a professional qualification from another EU member state over our sample period (only 6% percent of these received a negative answer). This number is relatively small and much too small to influence our estimated treatment effects (which are at least two orders of magnitude larger).[37] Moreover, the number of applications by accounting professionals is much smaller than the number of applications by legal professionals (also narrowly defined as lawyers, barristers and solicitors). Importantly, we do not see a change in the frequency of applications for recognition in our sample countries around years 2005 to 2007. Thus, although there were changes in the regulation of professional qualifications around the time of IFRS and ISAs adoption, these changes cannot explain the results in our main analyses.

[37] On top of that, two-thirds of the 94 applications are submitted in Cyprus and Romania for qualifications obtained in the UK. In Cyprus, the accounting qualification is identical to the UK ACCA qualification. Romania also automatically accepts the UK qualification in place of its national qualification. Hence, the likely explanation for the high number of applications for the recognition of UK qualifications in Cyprus and Romania is that their citizens take the exam in the UK and then apply for recognition in their home country. Cyprus and Romania do not enter our main analyses (due to lack of LFS data) and hence these applications are not relevant for our analysis.

FIGURE IA6

Mobility trends for business and law students

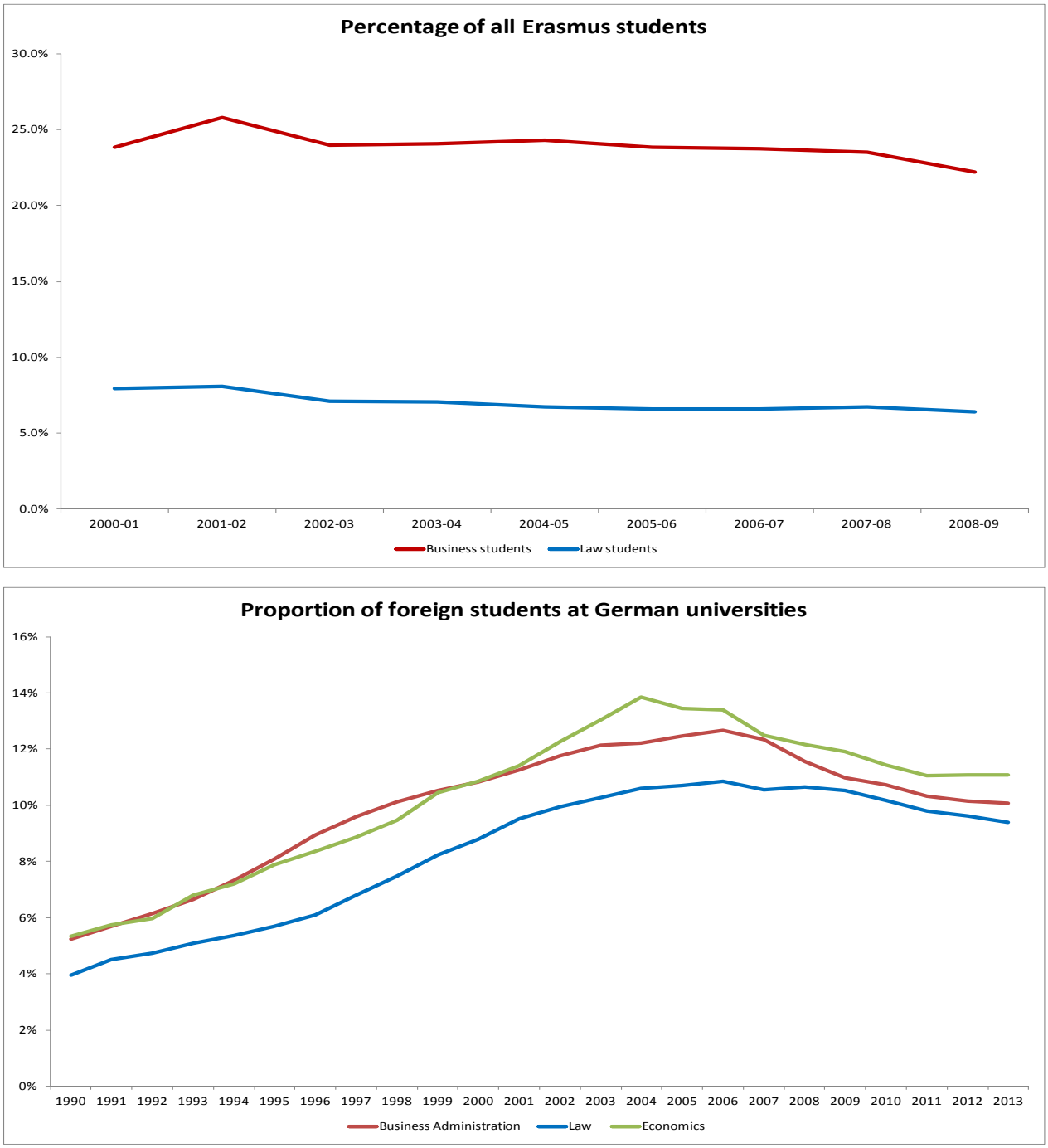

Figure IA6 presents statistics on student mobility over time. The upper graph shows the proportion of business and law students in the group of students who went abroad with the ERASMUS program (i.e., European Community Action Scheme for the Mobility of University Students) during academic years 2000-01 to 2008-09. This dataset was provided by the German Academic Exchange Service (DAAD). The lower graph shows the proportion of all foreign students at German universities relative to number of total students (foreign and domestic) in the respective fields: business administration, law and economics over the period 1990 to 2013. These data were obtained from the website of the Federal Statistical Office of Germany (www.destatis.de).

TABLE IA6

Controlling for years since graduation

Independent variables	Dependent variable: Mobility yes/no (NATBIRTH_CHG)			
Accountant * Post	1.113*	0.827***	0.805**	1.038*
	(1.89)	(2.70)	(2.29)	(1.97)
Years since graduation	-0.087*	-0.091***	-0.075***	
	(-1.84)	(-2.78)	(-4.14)	
Student mobility				-0.043
				(-1.00)
Domestic job mobility	0.033	0.031	0.103***	0.029
	(0.98)	(0.88)	(3.16)	(0.85)
Control group	Legal pros	All pros	Biz people	Legal pros
Fixed effects	Bin			
	Country * Accountant			
	Country * Year			
Observations	123,780	579,102	601,207	127,639
R-squared	0.07	0.04	0.03	0.06

Table IA6 presents results from sensitivity analyses related to differential student mobility trends. The sample comprises individuals from 26 countries for the years 2002 to 2004 and 2008 to 2010, respectively. The research design is the same as in the regression analysis presented in Panel B of Table 3 (NATBIRTH_CHG). Results are reported using LFS-weighted regressions and controlling for domestic job mobility as well as two additional control variables: *Years since graduation* is defined as the difference between the sample year and the graduation year of the respondent. *Student mobility* is the share of students in the respective field among all ERASMUS students at host country-year level. In constructing this variable, we use business students for accountants and law students for legal professionals.

Section IA7: Correlation in Country-level Migration In- and Outflows

Our analysis deliberately scales by the number of professionals in the country and year. Such scaling captures some demand effects. For instance, if a (non-harmonizing) regulatory shock increases the demand for accountants and does so symmetrically for domestic and foreign accountants, then the demand effect is already controlled for in the construction of the mobility measure. However, a regulatory change could also lead to asymmetric demand shocks across the EU countries. For instance, it is conceivable that IFRS adoption leads to more demand in some countries than others irrespective of harmonization. In this scenario, countries with relatively small demand shocks should act as "sources," having increases in outflows and declines in inflows, while countries with relatively large demand shocks act as "sinks," having increases in inflows and declines in outflows. Thus, under the asymmetric demand story, changes in inflow and outflow rates for accountants relative to control groups will be negatively correlated because sources (sinks) will have decreased (increased) inflows and increased (decreased) outflows. In contrast, under the harmonization story, relative changes in inflow and outflow rates will be positively correlated because harmonization allows mobility in both directions.

Testing this hypothesis is difficult with LFS data as it requires inflow and outflow data by country. As explained in Section 3, the LFS dataset comes with a host country perspective, surveying people that are located in a given country. It does not allow tracking of individuals, and origin information for migrants is limited to the LFS variable COUNTR1Y (country of residence last year). Despite the limitations of COUNTR1Y (see Sections 3.3 and 5.2), we can construct the following flow measures: *INFLOW* is the proportion of people living in country X who did not live in country X in the prior year (but lived elsewhere in the EU); and *OUTFLOW* is the proportion of people not living in country X (but elsewhere in the EU) who lived in country X in the prior year. Since the COUNTR1Y variable is sparsely populated, we can construct the measures for only 18 countries.

We use a difference-in-differences design by benchmarking pre- versus post-treatment changes (2002 to 2004 versus 2008 to 2010) in accountants' inflow and outflow rates against changes in the inflow and outflow rates of all professionals, both at the country-level. Table IA7a reports results from rank regressions. We find that relative changes in accountants' inflows are positively correlated with relative changes in accountants' outflows. This result is consistent with the harmonization explanation. In Table IA7b, we cross-tabulate all 18 countries based on the sign of their relative inflow and outflow changes. We find that 15 out of 18 countries have either both positive inflow and outflow changes or both negative inflow and outflow changes relative to all professionals, both with LFS weights (Panel A) and without LFS weights (Panel B).

Taken together, the results in Tables IA7a and IA7b do not support the asymmetric demand shock explanation.

TABLE IA7a

Correlation in country-level migration in- and outflows of accounting professionals relative to all professionals

Independent variables	Dependent variable: Inflow Diff-in-Diff Rank	
	LFS weighted (yes)	LFS weighted (no)
Outflow Diff-in-Diff Rank	0.822***	0.575**
	(5.78)	(2.81)
Constant	1.686	4.039*
	(1.10)	(1.82)
Observations	18	18
R-squared	0.66	0.33

Table IA7a presents the association between country-level changes in accountants' inflow rates and changes in accountants' outflow rates. Using a difference-in-differences design, we benchmark pre- versus post-treatment changes (2002 to 2004 versus 2008 to 2010) in accountants' inflow and outflow rates against changes in the inflow and outflow rates of all professionals. The unit of observation is the host country. Due to the kurtosis of the difference-in-difference estimates, we perform a ranked regression instead of OLS. We present results both with and without LFS weights. Inflow and outflow rates are estimated using the COUNTR1Y variable.

TABLE IA7b

Cross-tabulation of countries with positive and negative in- and outflows of accounting professionals relative to all professionals

Panel A: LFS weighted (yes)

	Positive Outflow Dif-in-dif	Negative Outflow Dif-in-dif	Total
Positive Inflow Dif-in-dif	8	1	9
Negative Inflow Dif-in-dif	2	7	9
Total	10	8	18

Panel B: LFS weighted (no)

	Positive Outflow Dif-in-dif	Negative Outflow Dif-in-dif	Total
Positive Inflow Dif-in-dif	7	1	8
Negative Inflow Dif-in-dif	2	8	10
Total	9	9	18

Table IA7b presents cross-tabulations comparing the sign of changes in accountants' inflow rates and with the sign of changes in accountants' outflow rates. Using a difference-in-differences design, we benchmark post-treatment changes (2002 to 2004 versus 2008 to 2010) in accountants' inflow and outflow rates against changes in the inflow and outflow rates of all professionals. The unit of observation is the host country. We present results both with (Panel A) and without LFS weights (Panel B). Inflow and outflow rates are estimated using the COUNTR1Y variable.

Section IA8: Double-Matched Analysis using Legal Professionals and Business People

In Table IA8, we report the double-matched difference-in-difference analyses for accountants against two alternative control groups: legal professionals and business people. The sample selection criteria, double-matching procedure and the specifications shown are identical to those reported in Table 3C of the manuscript.

Panel A presents the results relative to legal professionals, while Panel B presents results relative to business people. Results relative to business people are weaker than those relative to all pros (see Table 3, Panel C) or legal pros, especially for the analyses that do not use the LFS weights. The reason for this finding is the inclusion of Ireland, which comprises roughly 8% of the sample of business people, but only represents approximately 1.5% of the population. Compounding this issue is the fact that the distribution of job codes within business people (in particular, their job levels) differs substantially for Ireland relative to the distribution of job codes within business people for the EU as a whole. In untabulated analyses, we drop Ireland and find that our difference-in-differences results using business people as a control group become substantially stronger (and more in line with those reported for the other two control groups), both in terms of magnitude and statistical significance.

TABLE IA8

Double matched difference-in-differences analysis of cross-border mobility effects from regulatory harmonization using legal and business professionals as control groups

Panel A: Legal pros as control group

Double matched diff-in-diff analysis	Full sample						Only recent job changers		
	Mobility rate: NATBIRTH			Mobility rate: NATBIRTH_CHG			Mobility rate: NATBIRTH		
	Pre-period (1)	Post-period (2)	Difference (2) - (1)	Pre-period (1)	Post-period (2)	Difference (2) - (1)	Pre-period (1)	Post-period (2)	Difference (2) - (1)
LFS weighted (yes)									
(a) Treatment (Accountants)	3.216	4.969	1.753	1.773	2.834	1.061	3.922	7.231	3.309
(b) Control (Legal pros)	1.882	2.551	0.669	0.898	1.425	0.528	1.959	3.170	1.211
Difference (a) - (b)	1.333	2.418	**1.084*** (3.29)**	0.876	1.409	**0.533* (1.80)**	1.963	4.061	**2.098*** (4.17)**
Observations	78,624			78,624			23,449		
LFS weighted (no)									
(a) Treatment (Accountants)	5.318	7.293	1.975	2.769	3.968	1.199	6.833	10.531	3.698
(b) Control (Legal pros)	3.332	4.295	0.963	1.798	2.422	0.624	5.051	5.900	0.849
Difference (a) - (b)	1.986	2.998	**1.014** (2.37)**	0.971	1.546	**0.575** (2.01)**	1.782	4.631	**2.849*** (2.70)**
Observations	80,432			80,432			23,794		

TABLE IA8 (continued)

Panel B: Biz people as control group

Double matched diff-in-diff analysis	Full sample						Only recent job changers		
	Mobility rate: NATBIRTH			Mobility rate: NATBIRTH_CHG			Mobility rate: NATBIRTH		
	Pre-period (1)	Post-period (2)	Difference (2) - (1)	Pre-period (1)	Post-period (2)	Difference (2) - (1)	Pre-period (1)	Post-period (2)	Difference (2) - (1)
LFS weighted (yes)									
(a) Treatment (Accountants)	2.923	4.833	1.910	1.594	2.774	1.181	4.386	7.439	3.053
(b) Control (Biz people)	3.431	4.784	1.352	1.658	2.496	0.838	5.096	7.561	2.465
Difference (a) - (b)	-0.508	0.049	**0.557** **(1.72)**	-0.064	0.278	**0.342** **(1.27)**	-0.711	-0.122	**0.589** **(2.62)**
Observations	453,481			453,481			118,548		
LFS weighted (no)									
(a) Treatment (Accountants)	5.264	6.799	1.535	2.743	3.765	1.022	6.754	9.183	2.429
(b) Control (Biz people)	5.655	6.946	1.291	2.705	3.639	0.934	7.745	9.609	1.864
Difference (a) - (b)	-0.391	-0.147	**0.244** **(0.84)**	0.038	0.126	**0.089** **(0.47)**	-0.991	-0.426	**0.566** **(1.18)**
Observations	470,994			470,994			121,466		

Table IA8 presents results from a double matched difference-in-differences analysis that tests for differences in mobility trends between the treatment group (accountants) and the control groups of legal professionals (Panel A) and business people (Panel B). The research design is the same as in Table 3, Panel C of the manuscript.

Section IA9: Cross-Sectional Variation in the Treatment Effect

In this section, we report the results of a descriptive analysis of the cross-sectional variation in the treatment effects with respect to certain country characteristics. We describe but do not tabulate the analysis in Section 5.2.2 of the manuscript. There are two important caveats to this analysis.

First, it is difficult to make signed predictions for the cross-sectional effects. The reason is that cross-country variation also affects the size of the profession in the first place. In addition, migration is not the only margin along which the system can adjust after the regulatory change. Take the prediction that countries with a larger fraction of publicly-listed firms that need to report under IFRS should see a larger inflow of migrants. While sensible, it is also conceivable that these countries already had a much larger audit profession to begin with and hence even if they receive a larger number of migrants, the relative effect of migration could be larger in countries with fewer publicly-traded firms reporting under IFRS (and a smaller audit profession). Furthermore, in countries with a larger fraction of publicly-traded firms, IFRS adoption could also lead to a larger inflow of domestic people into the accounting and auditing profession. As a result, it is not clear that the effect (as measured by us) is necessarily larger in these countries.

Second, identification in these tests is based on cross-sectional variation in country characteristics. However, at the country level, many variables are highly correlated (for endogenous reasons) and hence it is difficult to isolate the effect of any particular factor.

For these two reasons, we view the tests below as primarily descriptive in nature. We have identified and collected data for several constructs for which we can make reasonable predictions (still acknowledging the concern discussed above). Our split variables capture (i) the extent to which standards are harmonized and (ii) to which regulatory harmonization is likely to affect migration. We proxy for the extent of harmonization with the degree of harmonization of audit standards and the share of the economy that is subject to mandatory IFRS (two first proxies discussed below). We proxy for the extent to which regulatory harmonization is likely to affect migration with the strength of licensing rules and the market share of Big-4 auditors (two last proxies discussed below).

Degree of Regulatory Harmonization of Auditing Standards

To capture the harmonization of auditing standards, we focus on changes in standards that have occurred over the sample period. Specifically, we classify countries as “high harmonization” countries if they are full ISA adopters by 2012. We expect migration inflows to be greater in countries with high harmonization.

Share of Market Subject to Mandatory IFRS

Unlike auditing standards that apply to all firms, IFRS adoption affected primarily publicly listed firms in most countries. Thus, we can use the market capitalization of listed firms to GDP as a

size-weighted measure for the degree to which the accounting standard harmonization affected a country. We classify those countries with an above median market capitalization of listed firms to GDP as "high harmonization" countries. We expect migration inflows to be greater in countries with high harmonization.

Licensing Rules

Licensing rules constitute a barrier to migration that still exists after IFRS and ISA adoption. We measure the extent of this barrier by coding variation in three national licensing-requirements: 1) whether a candidate needs at least three years of practical experience, 2) whether there is a final qualifying exam, and 3) whether the practical experience must be completed before the final qualifying exam. If the answer is yes to at least two of these three questions, we classify the country as a "high licensing requirements" country. We expect migration inflows to be lower in countries with high licensing requirements.

Market Share of Big-4 Auditors

The audit market is characterized by a few large firms. For these Big-4 auditors, IFRS and ISA are likely to be much more important because they are set up as international networks, which can facilitate cross-border labor migration (see discussion in Section 2 of the manuscript). Put differently, regulatory harmonization is likely more important to the Big-4 and their networks are likely better able to take advantage of regulatory harmonization.[38] Based on this logic, we split countries by the median market share of Big-4 auditors and classify those above the median as high Big-4 market share countries. We expect migration inflows to be greater in countries with a high Big-4 market share.

Table IA9a reports descriptive statistics on the institutional variables. In Table IA9b, we report results for regressions in which we include the interaction between the coefficient of interest (Accountant*Post) and the above institutional variables to test for heterogeneity in the treatment effects. For these tests, we focus on the weighted regressions because the weights assure comparability across countries and across time, which is important when examining country-level variation in the treatment effect. We find that the interaction of interest is positive in all cases and often quite large in magnitude. However, the interaction term is often not statistically significant. Nevertheless, the results indicate directionally that the treatment effects tend to be larger when the harmonization of standards is greater (Table IA9b, Panels A and B) and when regulatory harmonization is expected to affect migration the most (Table IA9b, Panels C and D). However, considering the lack of statistical significance and the conceptual concerns about the cross-sectional tests using country-level variables, we caution the reader to interpret the results carefully. The evidence is nevertheless suggestive and descriptively useful.

[38] As an illustration of our point that cross-sectional predictions are tricky, we note that one could also argue that Big-4 networks provided ways to overcome the barrier that different standards created prior to regulatory harmonization.

TABLE IA9a

Institutional variables used to examine cross-sectional variation in treatment effects

Country	Audit Standards = ISA in 2012	MCAP/GDP		Licensing requirements				Big4 market share	
		Ratio	High	Experience > 3 yrs.	Final Qualifying	Experience before exam	Aggregate	Percent	High
Austria	No	39.53	0	0	1	1	2	14%	0
Belgium	Yes	74.56	1	0	1	0	1	22%	1
Bulgaria	Yes	17.36	0	0	1	0	1	14%	0
Cyprus	Yes	36.03	0	0	1	0	1	20%	1
Czech Republic	Yes	28.20	0	0	0	0	0	5%	0
Denmark	Yes	67.30	1	0	1	0	1	40%	1
Estonia	Yes	24.96	0	0	1	0	1	6%	0
Finland	Yes	102.48	1	0	1	0	1	26%	1
France	No	79.81	1	0	1	0	1	21%	1
Germany	No	42.74	0	1	1	1	3	19%	0
Greece	Yes	58.55	1	1	1	1	3	na	na
Hungary	Yes	29.11	0	0	0	0	0	9%	0
Iceland	Yes	165.48	1	0	1	1	2	na	na
Ireland	Yes	54.26	1	0	0	0	0	25%	1
Italy	No	43.06	0	0	1	0	1	12%	0
Latvia	Yes	14.79	0	0	1	0	1	15%	0
Lithuania	Yes	31.32	0	0	1	1	2	25%	1
Luxembourg	Yes	138.43	1	0	1	0	1	44%	1
Netherlands	Yes	88.18	1	0	1	1	2	29%	1
Norway	Yes	61.85	1	0	1	0	1	na	na
Poland	No	30.84	0	0	1	0	1	7%	0
Portugal	No	33.95	0	0	1	0	1	11%	0
Romania	Yes	20.76	0	0	1	0	1	14%	0
Slovakia	Yes	7.01	0	1	1	0	2	24%	1
Slovenia	Yes	21.73	0	1	1	1	3	2%	0
Spain	No	82.96	1	0	0	0	0	13%	0
Sweden	Yes	103.83	1	0	1	0	1	35%	1
Switzerland	Yes	230.32	1	0	1	1	2	na	na
United Kingdom	Yes	126.78	1	0	1	0	1	40%	1

Table IA9a presents country-level proxies for 1) the extent of ISA adoption; 2) the size of public equity markets; 3) strength of licensing requirements; and the market share of Big4 auditors. *ISA Adoption* is assumed to be harmonized more if national standards were fully harmonized with ISA in 2012 (We cannot publicly reveal the source of the data for contractual reasons but we have identified the source to the Editor in compliance with JAR's data policy). *Size of public equity market* is defined to be high if the market capitalization of listed companies (which are generally subject to mandatory IFRS) as a percent of GDP (*MCAP/GDP*) exceeds the median of all EU countries in 2005 (data obtained from the Worldbank). *Licensing requirements* are defined as high if certified auditors must meet at least two of the following three requirements: i) have at least three years of practical experience; ii) pass a qualifying exam, iii) have practical experience before taking the qualifying exam (data obtained from the FEE website). *Big4 market share* is defined as high if the market share of Big-4 audit firms in a country exceeds the median of all countries in 2009 (data obtained from Le Vourc'h and Morand, P., 2011).

TABLE IA9b

Cross-sectional variation in treatment effects

Panel A: ISA adoption

Treatment effect by control group	Audit standards = ISA in 2012		Diff.
	Yes	No	
Legal pros	1.646**	0.345	1.301
	(2.43)	(0.73)	(1.54)
All pros	0.829***	0.606	0.223
	(2.92)	(1.05)	(0.35)
Biz people	0.845**	0.557	0.288
	(2.15)	(1.35)	(0.51)

Panel B: Size of public equity market

Treatment effect by control group	MCAP/GDP		Diff.
	High	Low	
Legal pros	1.903***	-0.286	2.189***
	(3.57)	(-1.08)	(3.78)
All pros	1.156***	0.090	1.066***
	(4.44)	(0.40)	(3.19)
Biz people	1.083***	0.164	0.919**
	(3.20)	(0.81)	(2.39)

Panel C: Licensing requirements

Treatment effect by control group	Licensing requirements		Diff.
	Low	High	
Legal pros	1.938***	-0.030	1.968***
	(3.47)	(-0.09)	(3.09)
All pros	0.916***	0.549	0.367
	(2.80)	(1.37)	(0.72)
Biz people	0.938**	0.476*	0.462
	(2.24)	(1.74)	(0.95)

Panel D: Big 4 market share

Treatment effect by control group	Big 4 market share		Diff.
	High	Low	
Legal pros	2.015***	0.201	1.814**
	(2.80)	(0.51)	(2.18)
All pros	0.946***	0.434	0.512
	(3.27)	(0.95)	(0.95)
Biz people	1.014**	0.372	0.642
	(2.48)	(1.15)	(1.25)

Table IA9b presents results cross-sectional variation in the treatment effect. The research design is the same as in the regression analysis presented in Panel B of Table 3 (NATBIRTH_CHG). Results are reported using LFS-weighted regressions and controlling for domestic job mobility. Partitioning variables are defined in the notes to Table IA9a.

References in Internet Appendix

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