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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, 2015, 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, the role and potential benefits of regulatory harmonization for cross-border labor mobility have not been studied.

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 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.

Our data are based on the EU's Labour Force Survey (LFS). The EU collects micro data on its labor force in annual surveys. The surveys are meant to generate a representative sample for each country and are conducted by the national statistical offices in each member state. They are mandated by EU regulation and conducted with a standardized methodology using (almost) the same questions. These features of our dataset substantially improve the comparability of mobility statistics across countries. Lack of comparability has hampered prior migration studies, as they have to rely on disparate data sources (e.g., population registers, border control data, permit data, census data), which often have different definitions and collection methods across countries (Rendall et al., 2003). In addition, the data are collected at the individual level, giving us a rich set of demographics to control for other factors that affect migration. The full LFS dataset covers about 32.7 million individuals from 29 countries over our sample period 2002 to 2010, with yearly totals varying between 1.9 and 5.6 million. We restrict the dataset to people between 20 and 59 years because this age group is likely to be active in the workforce. 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). To get closer to migration flows around regulatory changes, we also present analyses using a novel quasi-flow measure counting only foreigners that recently changed jobs and hence 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 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 university degree) for a year in the pre-period *and* a year in the post-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 also 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 17% and 24% of the pre-treatment mobility rate. This percentage

increase implies that regulatory harmonization increased the total number of migrants by 11,000 to 13,000 accounting professionals, which is economically significant.

An important challenge for estimating the 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 demand for accounting services and new matches between employers and accountants (e.g., with respect to internal control skills). Such demand effects could confound our analysis and overstate the mobility effects from 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, the cross-border harmonization effects that we are interested in are specific to foreigners. Thus, to control for demand effects, we introduce controls for changes in domestic job mobility into our analysis. 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

[1] We thank one of our reviewers for providing this example. We note that regulatory harmonization can also have demand effects, e.g., by lowering the wages for accountants. Such mobility effects are not a concern (see Section 2).

are close once we control for demographic characteristics. In addition, we consider several potential violations of the parallel-trend 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 job codes at the most granular level. As a result, we cannot perfectly identify accountants and auditors. For instance, the treatment group also contains personnel and career professionals. As long as the fraction of non-accountants in the treatment group does not systematically change around harmonization, the difference-in-differences analysis takes care of this data issue. We therefore made a special request to Eurostat to obtain statistics on more granular job codes. These statistics confirm that the fraction of accountants is about 50% and indeed remains stable.

The other LFS data constraint is that flow measures of cross-border migration are much less reliable than stock measures due to a "mini-domain" (or low-incidence) problem (Purcell and Kish, 1980) as well as sampling techniques that cause inconsistencies with population registers (Martí and Ródenas, 2007). Our main analysis is therefore based on a stock measure. However, given our research question, the timing of migration naturally matters, which is something that a simple stock measure does not allow us to determine. Again, this data limitation should not affect the difference-in-differences analysis as long as the rate of earlier migration outside the analysis window does not systematically change over time. Nevertheless, as discussed earlier, we refine the stock measure using information about recent job changes, and find very similar results. In addition, we provide evidence using two flow measures, for which we can determine the timing of migration. Consistent with prior research, both the incidence rates for these measures and power of the analyses are low. But still, we find that there are significantly positive mobility effects after harmonization among those individuals that are

expected to be more responsive to regulatory harmonization (e.g., singles without children, especially when they are young or work for large employers), supporting our main results.[2]

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 (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 unambiguously sign predictions for these factors as well as to isolate their effects, 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.[3] However, the efficiency of labor markets is economically relevant and potentially affected by accounting harmonization as well. Our study is the first to examine this effect. Our findings, which demonstrate relatively strong effects from accounting and auditing harmonization on cross-

[2] Singles without children are generally more mobile (Stark and Bloom, 1985) and therefore more likely to respond to regulatory harmonization. We also expect young people and people that work for large employers to be more responsive. Young people do most of the technical work in audit firms and their jobs therefore require the most detailed knowledge of rules. Similarly, large audit firms have most of the IFRS clients, and large preparers of financial statements are most likely to report under IFRS (both mandatorily and voluntarily).

[3] 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).

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 the 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. 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.

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 form of immigration laws (Skupnik, 2013) or occupational licensing rules (Kleiner et al, 1982). Immigration laws and occupational licensing rules are explicit government-enforced rules restricting who can move into a particular country and 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 in particular professions are different in that they constitute an implicit economic barrier, rather than an explicit government intervention aimed at restricting entry.[4]

Showing that differential professional rules indeed constitute a substantial barrier to cross-border labor mobility is important. It illustrates that the costs of learning and practicing

[4] In this regard, accounting and auditing rules are more akin to tax rules, which can also affect cross-border migration decisions. The tax literature finds that disparate tax rates encourage migration. See, e.g., Conway and Houtenville (1998, 2001), Bakija and Slemrod (2004), Coomes and Hoyt (2008).

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 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) 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, and psychological costs (Stark and Bloom, 1985; Borjas, 1987; Massey et al. 1993).

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.[5] 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 for examples).

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

[5] 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).

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 organization. Thus, there is almost complete formal harmonization of the rules and the remaining country-level variation in the rules after harmonization is relatively minor.[6] Prior to regulatory harmonization, however, there were substantial differences in countries' accounting and auditing standards.[7] 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.[8] Furthermore, the EU has been on the forefront of international regulatory harmonization in the accounting

[6] There is a significant debate whether formal harmonization of rules leads to harmonization of accounting practices (e.g., Hail et al., 2010). But as discussed, formal harmonization should be important in our setting. Incomplete harmonization of practices can constitute a (remaining) economic barrier, as accountants and auditors would have to be familiar with local customs and interpretations, which in turn should reduce any treatment effect.

[7] For evidence on prior accounting 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.

[8] 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.

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 are allowed to follow 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 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 than IFRS adoption, affecting all external auditors in the profession, but it does not directly affect accountants that in audited firms. 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.[9]

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

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

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. 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 they were similar in spirit or their application were to 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.[10] 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.[11] 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

[10] In addition, the EU LFS surveys are conducted annually. See Section 3.1 for details.
[11] Mandatory IFRS adoption applies to fiscal years ending in or after December 2005 and the adoption of Audit Directive 2006/43 followed a year later in 2006.

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 unreported sensitivity analyses, we use the years 2006 to 2008 as the post period and perform regression analyses using all years in the post period, and find similar results.

A third design challenge is that regulatory harmonization 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

Our analysis is based on the EU's Labour Force Survey (LFS). The LFS dataset is compiled by Eurostat, the statistical office of the EU.[12] The group of participating countries comprises the 28 EU member states, three EFTA countries (Iceland, Norway and Switzerland)

[12] 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.

and two EU candidate countries (Macedonia and Turkey).[13] The LFS dataset is based on quarterly or annual interviews that are conducted by the national statistical offices of the participating countries.[14] The national statistical offices follow strict guidelines laid out in EU Regulation when they survey their populations. The guidelines ensure that the sample is representative for 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. The construction of the dataset raises 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, including the use of an LFS weighing factor. In addition, the LFS

[13] 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 the 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 the 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.

[14] In 2010, on average about 1.8 million interviews were conducted throughout the participating countries each quarter (Quality report of the EU Labour Force Survey, 2010).

dataset offers a large set of demographic characteristics, which allow us to match individuals over time (see Section 4 for details on our double-matched sample analysis). Another potential issue with the dataset is that certain variables are provided to researchers only in an aggregated form. For instance, job codes but also details on country origins of migrants are not made available at the most granular level. We discuss these limitations and their implications later.

3.2 Treatment and Control Samples

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, including the job codes, was different before and after these years. As several variables require symmetric pre- and post-windows (see also Footnote 20), 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).

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.[15] Our treatment group ("accountants") consists of all respondents with ISCO3D equal to 241 which includes accountants (ISCO-88 = 2411) but also personnel and careers 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 the fraction remains roughly constant 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, because 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

[15] 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.

by the first digit of ISCO-88 = 2). This group includes lawyers, physicists, engineers, computing professionals, 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 groups automatic recognition of their licenses to practice in all EU countries. Including them would likely violate the parallel trend assumption.[16] 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 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 29 countries over the sample period.[17] 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 jobs may in some cases be 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 for all three control groups.

[16] 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.
[17] 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).

In the Internet Appendix (Table IA2), 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. The distributions are remarkably similar across accounting professionals and the three control groups, except for the educational level.[18] Based on these statistics, legal professionals and all professionals appear to be most comparable to the accounting professionals in terms of demographic characteristics that are known to determine cross-border migration decisions. Hence, they are our preferred control groups. But even comparing accountants and business people the distributions are still quite similar.[19]

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 can be reliably measured 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). 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 before and after 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

[18] Given the educational differences, we exclude respondents that do not have at least an upper secondary education to ensure that business people are more comparable to accounting professionals. See also Section 3.4.
[19] 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.

second refined measure that is designed to mitigate 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 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 not uncommon in large audit firms).

The LFS dataset allows the construction of metrics that are closer to flow measures. While these flow measures are conceptually desirable given the time dimension of our research question, they have severe drawbacks. First, flow measures generally tend to suffer from low incidence rates causing the so-called "mini-domain problem" (e.g., Purcell and Kish, 1980). Second, prior research finds that flow measures in the LFS database often produce very different migration estimates than population registers and concludes that they are much less reliable than stock measures (Martí and Ródenas, 2007). For these reasons, our main analysis focuses on the established stock measure, *NATBIRTH*, and our novel *NATBIRTH_CHG* measure, which has a time dimension to it. But we also present sensitivity analyses using two flow mobility metrics despite their fairly severe shortcomings (see Section 5.1 for details).

In the Internet Appendix, we provide by-country descriptive migration statistics for *NATBIRTH* and *NATBIRTH_CHG* for the pre- and the post-harmonization periods. Table IA3

shows considerable cross-sectional variation in the fraction of migrants across host countries as well as in the changes in this fraction over the sample period.

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.[20] 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 IA2).

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 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.[21] 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

[20] 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-harmonization 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).

[21] As the analysis is conducted 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 period, which we set to missing to allow matching with observations in the pre-period.

doctoral level is very small (see Internet Appendix, Table IA2) and the data are too sparse to create separate categories and the full set of interactions for these education levels.

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.

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 pre-period *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.[22]

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 group and the control group wash out in the first difference. Similarly, time-invariant measurement problems for one group wash out in the second difference.

We conduct the analysis at the individual level. The granularity of an individual-level analysis is a major advantage as it allows us to control for demographic and personal 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

[22] In the Internet Appendix, we graph the time trends in the aggregate mobility rates for the four groups from 1998 to 2013. Figure IA1 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 in our estimation due to the job code changes in 2011.

extrapolation and functional-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 is fairly conservative.

In Table 3 Panel A, we present OLS regression results for each of the three different control groups.[23] In the first six columns, we use *NATBIRTH* as migration measure and present regressions with and without weighing observations by the statistical weight provided for each individual in the LFS dataset. In the last six columns, we present the results using our refined, stock-based migration measure, *NATBIRTH_CHG*. The coefficient estimates on the interaction

[23] 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.

term *Accountant * Post* are positive and 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 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 weaker results against business people are primarily a matter of statistical significance. 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.[24] Towards this end, we determine the average number of survey 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

[24] 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.

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.[25] 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.

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: changes in cross-border student mobility as well as in the recognition of occupational qualifications. We find no evidence that these changes explain our results (see Section IA5).

4.2 Separating Regulatory Harmonization and Demand Effects

One 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

[25] 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).

harmonization per se. Regulatory changes can change 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.[26]

We first note that our migration measures are deliberately scaled 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 demand effect is already controlled for in the construction of the mobility measure. 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, our results could still be affected by demand effects from regulatory changes.

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 capturing *domestic* job mobility. If our results largely reflect demand effects, then controlling for domestic job mobility should attenuate the coefficient of interest.

[26] 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 Goerge et al. 2010) increase around IFRS adoption, although most of the effects are limited to the year of adoption. We therefore exclude the sample years 2005-2007—the period over which EU countries adopted IFRS and harmonized national audit standards with ISA.

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. 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 around regulatory changes. More importantly, introducing this control variable into the model 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 an additional test (in the Internet Appendix) using the correlation of labor migration inflows and outflows to gauge the presence of asymmetric demand shocks *across countries*. We find that country-level in- and outflows are positively associated, which is more consistent with a harmonization effect than an asymmetric

demand effect that causes some countries to act as "migration sources" and other countries to act as "migration sinks" (see Table IA6 for details).[27]

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. However, it would be appropriate to include such knock-on demand effects in the estimation (as their source is harmonization). For this reason, including domestic job mobility in the model could over-control and is not without costs.

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-harmonization period relative to the pre-harmonization period, then such changes would upward bias the estimated treatment effects in the regression analysis presented in Section 4.1. To address this concern, we perform a "double-matched" difference-in-differences analysis. 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.

[27] 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 persist after 2010 and does not look anything like the demand effects in audit fees after SOX adoption.

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-harmonization accounting professionals to post-harmonization 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-harmonization and some are post-harmonization). Thus, we have precisely the variation required for a difference-in-differences analysis, but hold everything else constant. This design is illustrated in Figure 2.

In order to implement the double-matched approach, we must specify a "year-pair" linking a pre-harmonization year with a particular post-harmonization year. For each year *t* of the pre-harmonization period (2002-2004) we define year *t+6* as the corresponding year (e.g., observations in 2002 are matched to observations in 2008).[28] 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

[28] In so doing, we exclude years 2005-2007. By focusing on later post-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 post-year definition yields only slightly smaller treatment effects that are statistically significant in most specifications. This attenuation is consistent with the gradual increase in the mobility rates of accounting professionals after 2005 documented in Figure 1.

for Italy in the pre-harmonization period, all observations for Italy drop out. This example illustrates why the double-matched approach eliminates sample composition changes.

Within-quadruplet estimation of the treatment effect requires that the four components of a quadruplet be weighted equally. 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 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.

The double-matched approach is 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 (Internet Appendix, Table IA2).[29] At the same time, double-matching provides tighter identification and makes the pre-harmonization mobility rates quite similar across treatment and control groups.[30]

[29] We report findings for the other control groups in the Internet Appendix, Table IA7. The effects are generally larger in magnitude when using legal professionals, and smaller and less significant using business people.

[30] Strictly speaking, a difference in pre-period mobility rates is not a concern in our design as long as the parallel-trends assumption holds. As discussed in Section 3.4, the mobility rates of treatment and control groups do indeed move closely in sync with each other during the pre-harmonization years.

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 46 to 55 basis points relative to all professionals. The estimates without LFS weights are slightly larger (71 to 78 basis points). Weighted and unweighted estimates imply very similar percentage increases in cross-border migration of accounting professionals relative to the pre-treatment migration rates (17% and 15% for NATBIRTH; 24% and 25% 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. As before, 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 by roughly 11,000 to 13,000 accountants.[31] 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 are driven by 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 the latter is closely related to domestic

[31] 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 estimated treatment effect yields an estimate for the increase in migration due to regulatory harmonization over the post-treatment period. For example, the first specification in Table 3, Panel C (NATBIRTH, with LFS weights and all pros as control group), shows an estimated treatment effect of 0.547 percentage points. This translates into an increase of about 12,732 migrating accountants (0.547 * 2.3 million / 100).

job mobility, we essentially estimate the cross-border migration effects relative to domestic job mobility. We present the results in the last column of Table 3, Panel C.

The estimated treatment effects are still statistically significant and even larger in magnitude within the subsample of workers who recently changed jobs. Specifically, we find that cross-border migration of accountants increases by 85 basis points after regulatory harmonization, relative to all other professionals. 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.

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

In this section, we examine alternative flow migration measures 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 using demographic characteristics of the respondents. For the stock measures we examine cross-country variation in the treatment effects.

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

Prior work using the LFS dataset generally prefers to use stock measures of migration due to concerns about the flow measures that can be constructed based on the LFS dataset (Martí and Ródenas, 2007, Skupnik 2013). In our primary analyses, we therefore follow the prior literature and use a stock measure based on nationality and country of birth (*NATBIRTH*) and a novel quasi-flow measure which counts only foreigners that recently changed jobs (*NATBIRTH_CHG)*. The pros and cons of these two measures are discussed in Section 3.3. Our difference-in-differences design addresses their shortcomings, provided they are constant through time (or changes are unsystematic). While we have no indication for systematic changes

in the direction of our results, this section presents sensitivity analyses using two flow migration measures that ensure migration occur in a window around regulatory harmonization.

The first flow measure (*YEARESID*) is based 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). 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, it mechanically increases over time and hence requires a symmetric pre- and post-estimation window, just as *NATBIRTH_CHG* (see Footnote 20). By construction, it also has relatively low incidence rates.

For the second flow measure (*COUNTR1Y*), we use the question regarding the place of residence one year before the survey (LFS item COUNTR1Y).[32] This measure also allows us to determine the exact time of the migration and it has the desirable feature that respondent 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 it difficult to sign cross-sectional predictions for demographic characteristics.[33]

[32] To account for returning workers (repatriates), we augment *COUNTR1Y* by coding individuals as zero if the data indicate that the person is a national of or born in the country where the survey is performed. Note, however, that harmonization could increase the number of accountants returning to their home countries (e.g., after IFRS adoption expatriates of German companies are no longer needed to help with German GAAP reporting in foreign subsidiaries). Thus, adjusting for returning workers should be conservative.

[33] This is particularly true for people that moved a long time ago. The latter are differenced-out by the design when computing average treatment effects. But they could still skew cross-sectional tests. For instance, if migrants are more likely to have children and become married after they move to a new foreign country relative to the average respondent, then cross-sectional splits for *NATBIRTH* do not yield meaningful results. For this reason, we perform cross-sectional splits with respect to demographic characteristics for the two flow measures only.

While *COUNTR1Y* at first may seem ideally suited, it actually has several severe drawbacks as migration measure. First, it counts only recent migration. For example, a foreign resident that moved two or three years prior to the survey is not counted in the migration rate. As a result, *COUNTR1Y* by construction yields much lower incidence rates that then can cause the mini-domain problem (e.g., Purcell and Kish, 1980). 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., Austria, Germany and Sweden). Third, in some countries and sample years, the data item to code *COUNTR1Y* has simply not been collected (see Table 1), creating gaps in the time series, which is particularly problematic for our study. Given the severity of these issues, we view *COUNTR1Y* as a sensitivity test and use the stock-based measures primarily.

To mitigate the mini-domain problem, we use the all professionals control group, which provides a large sample of individuals at the same professional level as accountants and with similar demographics (Internet Appendix, Table IA2). 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, splitting into subsamples for

which mobility rates are expected to be larger should 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 (Stark and Bloom, 1985) 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 most of the IFRS clients and large preparers of financial statements 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. Each estimate is obtained 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-harmonization mobility rates increase monotonically, as the three restrictions are introduced cumulatively. Using pre-treatment averages for *YEARESID* (*COUNTR1Y*) with LFS weights, the mobility rate climbs from 144 (9) basis points for the full sample to as high as 311 (26) 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 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. However, when we restrict the analysis to subsamples, treatment effects and statistical significance begin to increase, as expected. 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 38 (18) 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 161 and 62 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 2,000 and 4,000 individuals using *YEARESID* and 3,000 and 5,000 for *COUNTR1Y*.[34] 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.

Thus, 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.

[34] 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 4,400 migrating accountants due to regulatory harmonization over the post-treatment period (1.605 * 275,000 / 100). Multiplying the population estimate with the relevant treatment effect for COUNTR1Y (with LFS weights) yields an increase of about 1,700 migrating accountants per year (0.622 * 275,000 / 100). This yearly effect translates into an increase of about 5,100 migrating accountants over the post-treatment period (1,700 people * 3 years).

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). Similarly, cross-country differences may 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).[35] EU membership gave the citizens of the twelve new member states access to the labor markets of the EU-15 countries with certain restrictions for a transition period (see Skupnik, 2013, for details). 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 (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.

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

[35] 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).

(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 studies indicating lower within-EU-15 mobility and migration responses (OECD, 2014; Dao et al., 2014). 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 concerns that EU enlargement or the removal of labor migration restrictions on workers from EU-10 and EU-2 countries play into the documented migration effects for accounting professionals.

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

In this section, we explore cross-country 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 sign cross-country predictions for the magnitude of treatment effects unambiguously. One reason is that cross-country 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 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,

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.

Despite these limitations, we explore cross-country variation along two dimensions, but we view these analyses 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 the extent of IFRS adoption 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.

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 IA8b, 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 IA8b, 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, despite being large in magnitude. This evidence is nevertheless 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 to be a factor in 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 the 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 17% and 24% 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 11,000 to 13,000 accounting professionals. 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 that occur after any regulatory change, as such demand effects 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. Furthermore, the LFS dataset also has several drawbacks in measuring cross-border mobility around regulatory harmonization and in identifying accounting professionals. Thus, the magnitude of the estimated treatment effects needs to be interpreted 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 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

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 baseline regressions without additional controls (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, country, and profession (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).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)	3.175	5.045	1.870	1.918	3.104	1.186	4.432	6.994	2.562
(b) Control (All pros)	3.945	5.268	1.323	2.250	2.976	0.726	5.468	7.179	1.711
Difference (a) - (b)	-0.770	-0.223	**0.547*** (3.84)**	-0.332	0.128	**0.460** (2.61)**	-1.036	-0.185	**0.851*** (2.82)**
LFS weighted (no)									
(a) Treatment (Accountants)	5.022	7.711	2.689	2.850	4.668	1.818	6.285	9.763	3.478
(b) Control (All pros)	5.171	7.085	1.914	2.880	3.991	1.111	7.300	9.928	2.628
Difference (a) - (b)	-0.149	0.626	**0.775*** (3.64)**	-0.030	0.677	**0.708** (2.37)**	-1.015	-0.165	**0.851** (2.15)**
Quadruplets	2,242			2,242			1,574		
Observations	537,382			537,382			183,372		

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 and Romania drop out due missing observations in the pre-period). The sample is restricted to pairs of treatment and control group individuals with the same characteristics for a year in the pre-period and a year in the post-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). At the bottom of the table, we report the number of quadruplets that we can form for the treatment and the control group as well as the number of individuals in these quadruplets. 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 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 using flow-based mobility 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	1.435	1.931	2.065	2.187	2.132	2.407	3.106	3.890
Treatment effect	**0.298**	**0.224**	**0.376***	**0.205**	**0.399***	**0.333^**	**1.605*****	**0.569^**
	(1.08)	**(0.93)**	**(1.93)**	**(0.98)**	**(2.01)**	**(1.44)**	**(7.28)**	**(1.62)**
Quadruplets / Observations	2,352 / 585,289		549 / 110,313		348 / 89,615		240 / 27,688	
Mobility rate: COUNTR1Y								
Pre-treatment mobility	0.068	0.073	0.150	0.129	0.156	0.148	0.259	0.189
Treatment effect	**-0.039**	**0.087**	**0.184****	**0.180^**	**0.191****	**0.213^**	**0.622*****	**0.477^**
	(-0.72)	**(0.85)**	**(2.33)**	**(1.63)**	**(2.38)**	**(1.57)**	**(3.10)**	**(1.44)**
Quadruplets / Observations	1,703 / 359,864		485 / 93,643		314 / 76,204		216 / 22,794	

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 were born abroad and 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 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

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).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

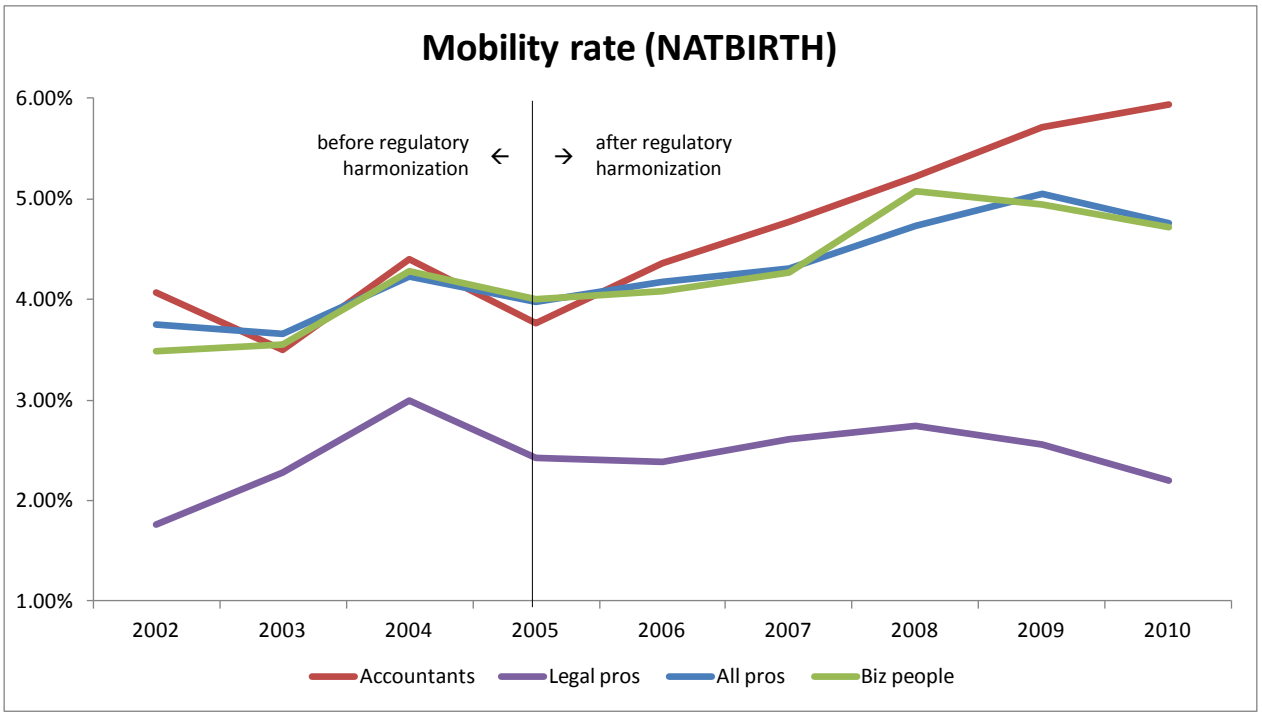

Figure 1 compares mobility rates between the treatment group (accountants) and the control groups (legal professionals, all professionals and business people) over time. The analysis is based on the mobility metric *NATBIRTH*. This metric defines individuals as migrants (or mobile) if they were born 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

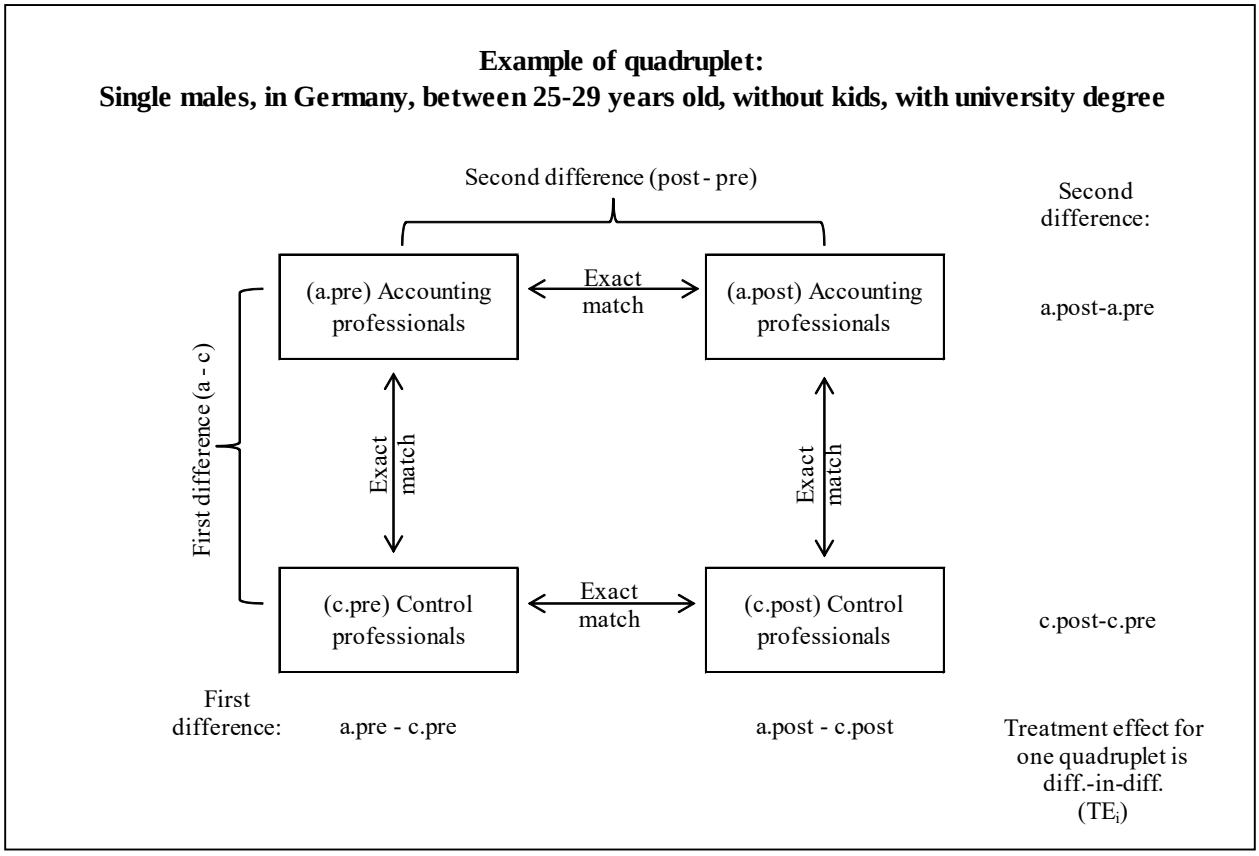

Figure 2 illustrates the double-matched difference-in-differences approach. Within each country and year, we first match all accounting professionals and all control observations. We further match pre-harmonization observations in year t to post-harmonization observations in year t+6 (e.g., observations 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-harmonization), 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

Section IA2: Distribution of Key Characteristics for Treatment and Control Groups

Section IA3: Descriptive Statistics on Mobility by Country

Section IA4: Mobility Trends for Extended Period from 1998 to 2013

Section IA5: Assessing Alternative Explanations

Section IA6: Correlation in Country-level Migration In- and Outflows

Section IA7: Double-Matched Legal Professionals and Business People

Section IA8: Cross-Country Variation in the Treatment Effect

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 increase labor migration, we collect 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) have helped the mobility of accountants in the EU. While this evidence is clearly more anecdotal, 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: Distribution of Key Characteristics for Treatment and Control Groups

In Table IA2, 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 (see Table 1 for further details).

TABLE IA2

Distribution of key 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 IA2 (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 IA3: Descriptive Statistics on Mobility by Country

This section provides country-specific mobility statistics. The statistics are based on the double-matched sample from Table 3, Panel C in the manuscript.

Table IA3 reports descriptive statistics on mobility in the pre-harmonization-period (2002-2004), post-harmonization-period (2008-2010), and changes from the pre- to post-period. We report these statistics for our mobility 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 rates. The rates are the lowest in Eastern European countries such as Latvia, Lithuania, and Slovakia. As one would expect, the change in the migration rate from the pre-to the post-period is not always positive in all countries and, mainly in small countries, varies somewhat depending on whether we use NATBIRTH or NATBIRTH_CHG. The average change in mobility across countries is 1.66 (1.58) percentage points for *NATBIRTH* (*NATBIRTH_CHG*). The country-level change in migration from the pre- to the post-period is 88 (167) percent more likely to be positive than negative for *NATBIRTH* (*NATBIRTH_CHG*).

We find that several countries exhibit a strong increase in migrants (e.g., Austria, Cyprus, Luxembourg, and Switzerland), which seems plausible. These countries have a high fraction of firms that operates 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). Only Iceland has a consistently strong negative change from the pre-to post-period, which may be explained by the financial sector in Iceland being particularly hard hit by the financial crisis in the post-period.

TABLE IA3

Country-specific mobility statistics

Host country	NATBIRTH			NATBIRTH_CHG		
	2002-2004 (Pre)	2008-2010 (Post)	Diff (Post-Pre)	2002-2004 (Pre)	2008-2010 (Post)	Diff (Post-Pre)
Austria	5.02%	13.31%	8.29%	1.43%	8.09%	6.66%
Belgium	6.47%	6.11%	-0.36%	3.76%	3.72%	-0.04%
Bulgaria						
Cyprus	5.54%	9.95%	4.41%	3.25%	5.89%	2.64%
Czech Republic	1.28%	0.75%	-0.53%	0.86%	0.56%	-0.30%
Denmark	1.72%	2.58%	0.86%	1.22%	2.04%	0.82%
Estonia	2.93%	1.20%	-1.73%	0.00%	0.97%	0.97%
Finland	0.27%	0.20%	-0.07%	0.08%	0.00%	-0.08%
France	4.43%	3.41%	-1.02%	1.01%	2.61%	1.60%
Germany	3.63%	4.57%	0.94%	2.26%	2.67%	0.41%
Greece	1.70%	1.66%	-0.04%	1.09%	0.47%	-0.62%
Hungary	0.44%	0.38%	-0.06%	0.25%	0.38%	0.13%
Iceland	2.76%	1.01%	-1.75%	1.86%	0.49%	-1.37%
Ireland	5.02%	7.19%	2.17%	3.91%	5.37%	1.46%
Italy						
Latvia	0.00%	3.18%	3.18%	0.00%	0.94%	0.94%
Lithuania	0.16%	0.53%	0.37%	0.16%	0.53%	0.37%
Luxembourg	64.19%	74.72%	10.53%	33.49%	44.73%	11.24%
Netherlands	1.83%	2.61%	0.78%	0.60%	1.52%	0.92%
Norway	1.46%	3.91%	2.45%	0.78%	2.94%	2.16%
Poland						
Portugal	1.40%	1.57%	0.17%	1.40%	1.48%	0.08%
Romania						
Slovakia	0.00%	0.32%	0.32%	0.00%	0.00%	0.00%
Slovenia						
Spain	3.77%	5.53%	1.76%	1.90%	4.51%	2.61%
Sweden	3.26%	3.43%	0.17%	2.26%	1.96%	-0.30%
Switzerland	12.26%	17.89%	5.63%	6.95%	11.32%	4.37%
United Kingdom	6.20%	10.15%	3.95%	4.61%	7.76%	3.15%
Total	5.66%	7.34%	1.68%	3.05%	4.62%	1.58%

Table IA3 reports descriptive statistics on mobility in the pre-harmonization-period (2002 to 2004), post-harmonization-period (2008 to 2010), and the change in mobility from the pre- to the post-period. Statistics are calculated based on the double-matched sample used in Table 3, Panel C of the manuscript.

Section IA4: 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 have, in this section, used 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 IA4. 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 IA4

Mobility trends at aggregate level from 2002 to 2013

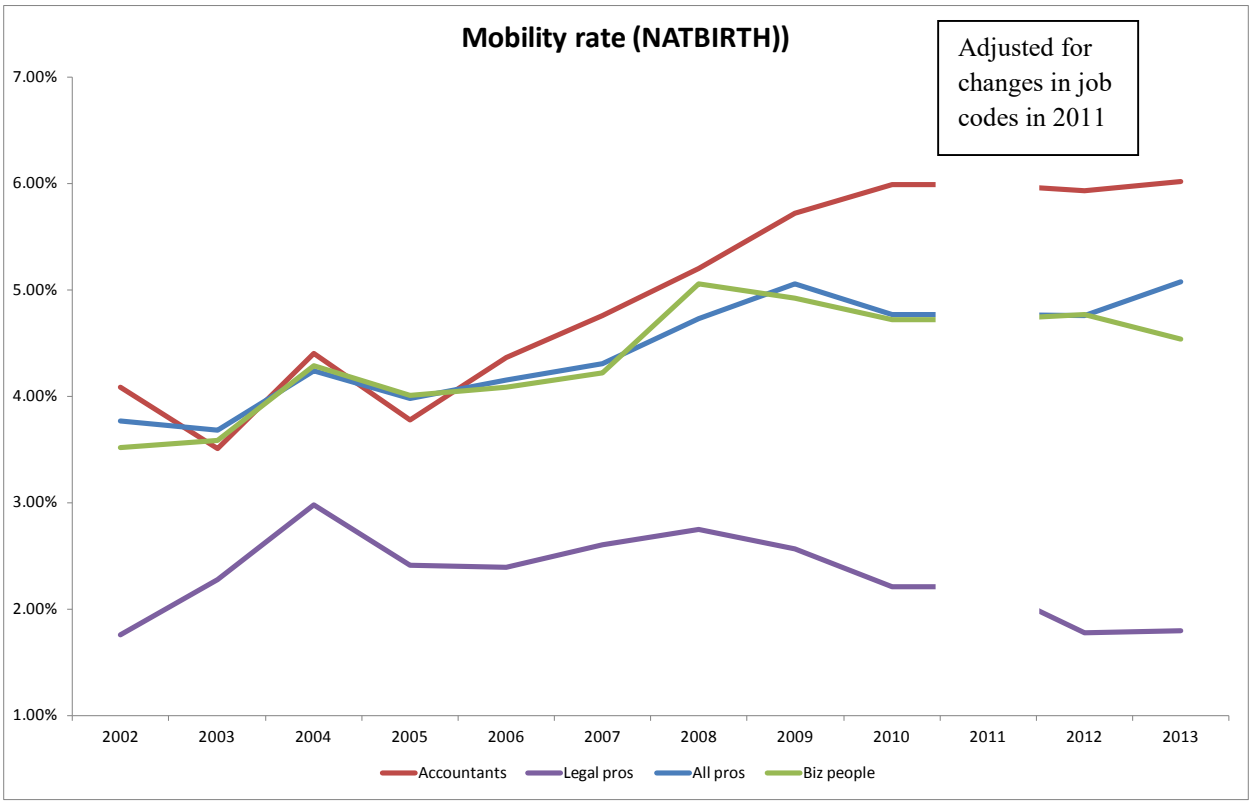

Figure IA4 compares mobility rates between the treatment group (accountants) and the control groups (legal professionals, all professionals, and business people) over time. To adjust for changing job codes in 2011, we subtract the mobility change from 2010 to 2011 for accounting professionals and each 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 IA5: 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, our analysis could be affected by events or institutional changes that differentially affect the mobility rates of accounting professionals and of the control groups over time. 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.[36] 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.

[36] 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 IA5 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.[37]

Second, in first three columns of Table IA5, 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 IA5, 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 our results are explained by differential trends in student mobility over time.

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

[37] 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 IA5 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 low and much too small to influence our estimated treatment effects (which are at least two orders of magnitude larger).[38] 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.

[38] 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 IA5

Mobility trends for business and law students

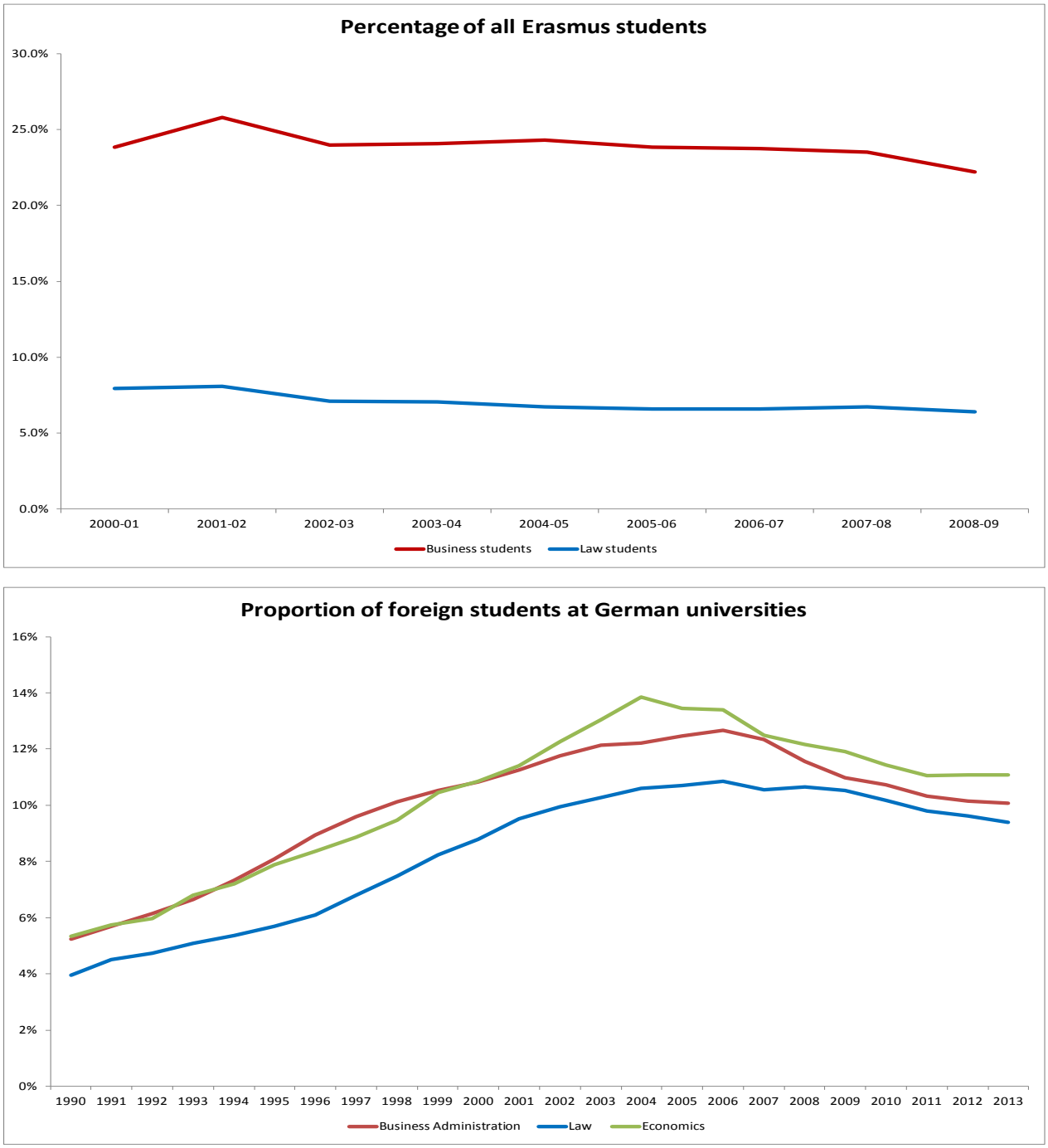

Figure IA5 presents statistics on student mobility over time. The upper graph shows the proportion of business and law students in the group of students who went abroad with the ERASMUS program (i.e., European Community Action Scheme for the Mobility of University Students) during academic years 2000-01 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 IA5

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 IA5 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 IA6: 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, certain countries should act as "sources" having primarily outflows (the countries with relatively small demand shocks), while others will act as "sinks" having primarily inflows (the countries with relatively large demand shocks). Thus, under the asymmetric demand story, changes in inflow and outflow rates for accountants (relative to controls) will be negatively correlated because sources (sinks) will have decreased (increased) inflows and increased (decreased) outflows. In contrast, under the harmonization story, changes in inflow and outflow rates for accountants (relative to controls) will be positively correlated because harmonization allows mobility in both directions.

Testing these hypotheses empirically is difficult with our data as it requires inflow and outflow data by country. As explained in Section 3 of the manuscript, 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 information from which countries these come from is limited to COUNTRY and COUNTR1Y (country of residence this year and last year). Acknowledging the issues with COUNTR1Y and its low incidence rates (see Sections 3.3 and 5.2 of the manuscript), we can use the two variables to construct two flow measures: *INFLOW* is the proportion of people living in country X who did not live in country X in the prior year; and *OUTFLOW* is the proportion of people not living in country X (but elsewhere in the EU) who did live in country X in the prior year.

Using a differences-in-differences design, we find that post-harmonization changes in accountants' inflow and outflow rates, relative to all professionals, are positively correlated, which is consistent with the harmonization explanation. Thus, the test does not provide evidence for the asymmetric demand shock explanation. We report the results in Table IA6.

TABLE IA6

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	0.792***	0.447*
	(5.181)	(1.998)
Constant	1.980	5.255**
	(1.198)	(2.171)
Observations	18	18
R-squared	0.63	0.20

Table IA6 presents the association between changes in accountants' inflow rates and changes in accountants' outflow rates. Using a difference-in-differences design, we benchmark post-harmonization changes (2002 to 2004 versus 2008 to 2010) in accountants' inflow and outflow rates against changes in all pros' inflow and outflow rates, in order to control for any broad cross-border mobility trends. 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.

Section IA7: Double-Matched Legal Professionals and Business People

In Table IA7, we report the double-matched difference-in-difference results for accountants relative to two alternative control groups: legal professionals and business people. The sample selection criteria, double-match procedure and specifications shown are identical to those of Table 3C in 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 Ireland differs substantially from 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 IA7

Double matched difference-in-differences analysis 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)	3.122	4.961	1.839	1.878	3.159	1.281	4.214	6.697	2.483
(b) Control (All pros)	2.203	2.907	0.704	1.346	1.873	0.527	2.868	3.865	0.997
Difference (a) - (b)	0.919	2.054	**1.136** (2.43)	0.532	1.287	**0.745*** (1.81)	1.346	2.832	**1.486*** (1.89)
LFS weighted (no)									
(a) Treatment (Accountants)	4.347	6.971	2.623	2.445	4.226	1.781	5.732	9.632	3.900
(b) Control (All pros)	3.242	4.453	1.211	1.922	2.761	0.838	4.896	6.033	1.137
Difference (a) - (b)	1.105	2.518	**1.413*** (2.72)**	0.523	1.465	**0.943*** (2.72)**	0.835	3.599	**2.763*** (2.95)**
Quadruplets	1,513			1,513			736		
Observations	102,198			102,198			35,710		

TABLE IA7 (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)	3.145	4.875	1.730	1.886	2.988	1.102	4.740	7.100	2.360
(b) Control (All pros)	3.408	4.561	1.153	1.811	2.547	0.736	5.077	6.979	1.903
Difference (a) - (b)	-0.263	0.314	**0.577** (2.18)	0.075	0.441	**0.366**^ (1.33)	-0.336	0.121	**0.457** (1.08)
LFS weighted (no)									
(a) Treatment (Accountants)	4.973	7.644	2.671	2.819	4.614	1.794	6.051	9.495	3.444
(b) Control (All pros)	4.517	6.782	2.265	2.389	3.970	1.581	6.223	9.093	2.870
Difference (a) - (b)	0.456	0.862	**0.405** (0.90)	0.430	0.644	**0.213** (0.63)	-0.172	0.402	**0.573** (0.75)
Quadruplets	2,222			2,222			1,575		
Observations	579,611			579,611			190,544		

Table IA7 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 IA8: Cross-Country Variation in the Treatment Effect

In this Section, we report the results from an analysis of the cross-country variation in the treatment effects, described but not tabulated 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. 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 country was affected by accounting standards

harmonization. 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.[39] 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 IA8a reports descriptive statistics on the institutional variables. In Table IA8b, 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 time, which is important when examining country-level variation in the treatment effect. We find that this interaction 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 IA8b, Panels A and B) and when regulatory harmonization is expected to affect migration the most (Table IA8b, 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 are hesitant to make too much of these findings and caution the reader to interpret the results carefully. The evidence is nevertheless descriptively useful.

[39] 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 IA8a

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

Country	Extent of Harmonization of Standards			Extent to which regulatory harmonization is likely to affect migration					
	ISA Adoption	IFRS Adoption		Licensing requirements				Big4 market share	
	National Standards = ISA in 2012	MCAP/ GDP	High	Experience > 3 yrs.	Final Qualifying	Experience before exam	High	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 IA8a presents country-level proxies for 1) the extent of ISA adoption; 2) the extent of IFRS adoption; 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 (data obtained through internet search). *IFRS adoption* 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 IA8b

Cross-country 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 IA8b presents results cross-country 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 IA8a.

References in Internet Appendix

Le Vourc'h, J. Morand, P., 2011. Study on the effects of the implementation of the acquis on statutory audits of annual and consolidated accounts including the consequences on the audit market. ESCP Europe.

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