THE EFFECT OF REGULATORY HARMONIZATION ON CROSS-BORDER LABOR MIGRATION: EVIDENCE FROM THE ACCOUNTING PROFESSION

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<u>ABSTRACT</u>

The paper examines the effect of international regulatory harmonization on cross-border labor migration. We analyze directives in the European Union (EU) that harmonized accounting and auditing standards. This regulatory harmonization should make it less costly for those who work in the accounting profession 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, on average, labor migration in the accounting profession increases relative to comparable professions by roughly 15% after harmonization. The findings illustrate that diversity in rules constitutes an important economic barrier to cross-border labor mobility and, more specifically, that accounting harmonization can have meaningful effect on cross-border migration.

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1.   Introduction

In recent years, we have witnessed a significant push towards a 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, 2008, 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 less costly 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). However, the role and potential benefits of regulatory harmonization for cross-border labor mobility have not been studied much.

In this paper, we analyze the effects of recent regulatory harmonization affecting the accounting profession in the European Union (EU) on cross-border labor migration. The setting has several desirable features from a research design perspective. First, the accounting profession generally has a much 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 the effect of regulatory harmonization on labor migration rather than immigration policies. 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) mandatory reporting under IFRS harmonized the accounting standards used by publicly traded firms and (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.

While regulatory harmonization has the potential to increase cross-border labor mobility, the effect is not a priori obvious. First, the benefits from regulatory harmonization could be too small relative to other costs involved in migrating to another country to have a meaningful effect on cross-border mobility. It is conceivable that factors like language and cultural differences swamp any harmonization effect. Moreover, there is evidence that local accounting practices often persist after formal harmonization of the standards (e.g., Kvaal and Nobes 2010, 2012). These local accounting practices and traditions could continue to act as an economic barrier, in essence mitigating or perhaps even thwarting the effects of regulatory harmonization. Thus, the magnitude of the effect is an empirical question.

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. The surveys are mandated by EU regulation and done in standardized fashion using (almost) the same questions and a standardized collection methodology. These features of our dataset substantially improve the comparability of mobility statistics across countries. Lack of comparability has often 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 mobility metric is based on the number of individuals that have a foreign nationality and were born abroad (Martí and Ródenas 2007).

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 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 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 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 similar across control groups and amounts to roughly 15% of the pre-treatment mobility rate in most specifications. To further gauge the economic magnitude of our estimates, we compute the number of additional migrants. That is, we multiply the number of migrants in the accounting profession prior to regulatory harmonization with the estimated percentage increase in migration due to harmonization and then invert the statistical weights provided for each individual in the LFS dataset to obtain a population estimate. A 15% treatment effect implies that regulatory harmonization increased the total number of migrants by approximately 11,000 accounting professionals. These effects appear economically significant.

Next, we perform a series of tests that evaluate key design choices of our main analysis. First, our main estimates are based on a stock measure of migration. For this measure, we cannot determine when migration occurred.[1] We therefore construct and examine two novel flow metrics that measure migration in a specific window around regulatory harmonization. The first flow measure uses the number of years for which an individual who was born abroad has been a resident in a country. The second flow measure uses information about the country of residence of the respondent one year before the survey year. Both flow measures allow us to determine when the migration occurred relative to regulatory harmonization. However, the incidence rates for both flow measures are low, creating power issues and what sometimes is called a "mini-

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[1] This data limitation should not affect estimates in the difference-in-differences analysis as long as the rate of migration that took place outside the analysis window does not systematically change over time.

domain problem" (e.g., Purcell and Kish, 1980). As a result, analyses based on flow measures can be less reliable than analyses based on stock measures (Martí and Ródenas, 2007). However, for our research design and question, the time dimension is important. Thus, we also present analyses using flow metrics. We find that the flow measures support our earlier inferences. However, as expected, power is low and hence the mobility effects are significantly positive only among those individuals that are expected to be most affected by regulatory harmonization (e.g., singles without children, especially when they are young or work for large employers).[2]

Second, we assess the validity of our key identification assumption 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 the accounting profession. We provide graphical evidence that the pre-period trends are similar. We also show that pre-treatment mobility rates are often comparable once we control for demographic characteristics. In addition, we explore several potential violations of the parallel-trend assumption that would provide alternative explanations for the documented increase in mobility in the accounting profession. These alternative explanations include differential changes in cross-border student mobility as well as licensing rules. None of these robustness tests alter our main inferences.

Third, we examine whether our results are affected by EU enlargement (e.g., migration from East to West Europe, remaining free movement restrictions) or by related changes in the country composition of the sample. We find that our results continue to hold when we restrict the sample to EU-15 destination countries. We still find significant (albeit smaller) increases in within-EU-15 mobility, i.e., when we restrict source and destination countries to the EU-15.

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[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).

Our paper makes several contributions to the literature. First, the literature on accounting harmonization focuses almost exclusively on informational effects in capital markets (see, e.g., Barth, 2006; Soderstrom and Sun, 2007, Hail et al., 2010; Brüggemann et al., 2013, for overviews).[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-border labor mobility, may appear inconsistent with prior capital-market studies. Recent evidence suggests 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 significant 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 effect of these forces in a labor market setting is less obvious. 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. Thus, 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 mobility. Much of the prior labor migration literature has focused on the effect of wage and unemployment

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[3] One notable exception is Wu and Zhang (2009) documenting that the sensitivity of CEO turnover and employee layoffs to accounting earnings increases after voluntary IFRS adoption. Labor market outcomes have also been examined in the accounting literature on CEO compensation and turnover (see Armstrong et al., 2010, for an overview) and for financial analysts (see Healy and Palepu, 2001, for an overview). The results in such studies are consistent with a stewardship role of accounting in labor markets and the notion that performance matters for labor market outcomes. Our research question is fundamentally different from these studies.

differentials and legal barriers in form of immigration laws (Skupnik 2013) or occupational licensing rules (Kleiner, Gay and Greene 1982). Immigration laws and occupational licensing rules are explicit government enforced rules restricting who can move into a particular country or who can offer services in a particular market. The general result in this literature is that explicit restrictions create mobility barriers (see Kleiner, 2000, for an overview). Differential rules governing 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 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). Prior research finds that labor mobility among the 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 common conjecture is that mobility rates within the EU-15 are low due to implicit barriers, such as language or culture, which are difficult to change. Our evidence suggests that disparate rules can be an economic barrier to cross-border mobility and that regulatory harmonization can reduce these barriers and have economically large effects on mobility. We acknowledge, however, that our findings are

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[4] In this regard, accounting and auditing rules are more akin to tax rules, which can also affect cross-border migration decisions. However, contrary to our evidence, the tax literature generally reaches the conclusion that disparate tax rates, rather than harmonization, encourage migration. See, e.g., Conway and Houtenville (1998, 2001), Bakija and Slemrod (2004), Coomes and Hoyt (2008).

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.

The remainder of the paper proceeds as follows. Section 2 develops our conceptual framework and provides more details on the institutional setting. Section 3 describes the data, how we measure migration, and provides descriptive evidence. Section 4 presents the results from our main analyses examining the effect of regulatory harmonization on migration. Section 5 provides various sensitivity analyses. Section 6 concludes.

2. Conceptual Underpinnings and Institutional Setting

In making a migration decision, individuals trade off the initial costs of migration against the expected increase in income (Roy, 1951; Sjaastad, 1962) and potential other benefits from moving. The expected increase in income is mainly determined by wage and unemployment differentials between country of origin and country of destination. The costs of migration (or mobility costs) include transportation costs, income losses during migration, and psychological costs (Stark and Bloom, 1985; Borjas, 1987).

Another cost of migration could come from differential rules and regulations of which knowledge is relevant or required when working in a particular profession. For instance, an auditor, lawyer, or building engineer that wants to move abroad needs to learn and know the accounting standards, laws, and building codes of the destination country, respectively, in order to perform the job. Thus, if learning costs are material, diversity in rules will act as an economic barrier to labor migration. National certification rules constitute a further barrier but they also rely on knowledge of the respective rules and regulations.

Harmonization of rules across countries should, ceteris paribus, reduce the mobility barrier and hence increase cross-border labor migration. Consistent with our arguments, 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 directives and legislative initiatives, explicitly cites the plan's potential to increase labor migration as one of the motivations for regulatory reform.[5]

It is, however, not obvious that regulatory harmonization can significantly increase labor mobility. The benefits from harmonizing the 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, evidence on the effects of regulatory harmonization on cross-border labor migration would in essence tell us that differential rules indeed constitute a substantial economic barrier and shed light on the effectiveness of (formal) regulatory harmonization as a policy instrument.

The accounting profession is suitable to study the impact of regulatory harmonization on labor migration for several reasons. First, there has been substantial regulatory harmonization in accounting and auditing in recent years, and it has affected virtually all aspects of the profession. Second, the accounting profession has a much higher level of standardization than comparable occupations (Madsen, 2011), which implies that accounting and auditing standards play an

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[5] The FSAP states, as part of the motivation for regulatory reform, that "the lack of a Community framework can also discourage labour mobility" but this motivation 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 is more likely to affect mobility, but the changes would occur after our sample period and hence cannot explain our results.

important role in the profession, and learning how to apply them is likely a significant human capital investment. Third, harmonization in accounting has generally taken the form of explicitly adopting a common set of standards issued by an international organization in English. 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] Indeed, many countries do not even translate international standards from English into local language. Fourth, the large auditing firms are set up as international networks, enabling them to take advantage of harmonization. In sum, these factors should make it easier to identify an effect of regulatory harmonization on labor migration, if one exists. At the same time, these factors imply that the magnitude of the treatment effect for the accounting profession may not generalize to other professions.

There are also several advantages to studying labor migration in the EU. First, labor can move freely among EU member states.[7] 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 to examine economic barriers and to measure the effect of regulatory harmonization on labor migration.[8]

Second, the EU has been on the forefront of international regulatory harmonization in the accounting profession and the changes over recent years have been substantial. In 2005, the

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[6] As noted earlier, there is a significant debate over the extent to which formal harmonization of rules implies or leads to harmonization of accounting practices (e.g., Hail et al., 2010). Conceptually, formal harmonization of rules should be an important construct in our setting. But as discussed, incomplete harmonization of practices can constitute a (remaining) economic barrier, as accountants and auditors would likely have to be familiar with local customs and interpretations, which in turn should reduce any treatment effect from formal harmonization.

[7] Citizens of the member states of the European Economic Area (EEA) and Switzerland also have the right of free movement within the EEA and Switzerland. The EEA includes the 28 EU members, Iceland, Liechtenstein, and Norway. Switzerland is not a member of the EEA but its citizens have the same free movement rights through bilateral agreements. For simplicity, we refer to these additional 4 countries as member states or EU countries.

[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.

application of IFRS became mandatory for almost all publically listed firms in the EU. In addition, private companies are allowed to follow IFRS in many member states. Accounting standards, such as IFRS, specify allowable accounting treatments and hence knowledge of accounting standards is required for any accountant involved in the production of general-purpose financial statements, regardless of whether they work as preparers or auditors.

While IFRS adoption is therefore an important element of accounting harmonization, it is not the only source of regulatory harmonization in the accounting profession in the EU. In Directive 2006/43 of 17 May 2006, the European Parliament and Council harmonized statutory auditing requirements for annual and consolidated accounts.[9] 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). The provisions of Directive 2006/43 were substantial and ranged from harmonization of educational requirements and ethical standards to granting the EU Commission the option to mandate the adoption of International Standards on Auditing (ISA) throughout the EU. Auditing standards specify generally accepted work practices for auditors such as independence requirements, documentation requirements, and sampling techniques, regardless of whether they audit private or public firms. As such the harmonization of auditing standards has potentially an even wider reach than IFRS adoption. The EU 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.

In sum, the accounting profession in the EU has witnessed a substantial harmonization of accounting and auditing standards, which in turn has the potential to eliminate economic barriers for auditors and accountants to move across borders. As such, the setting seems well suited to

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[9] In 2008, Directive 2006/43 was amended by Directive 2008/30.

estimate the effect of regulatory harmonization on labor migration. However, the setting 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, the measurement of the potential treatment effect in time 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 affects labor mobility, even though the event itself is sharply defined in time, i.e., mandatory IFRS adoption occurred for fiscal year 2005 in all EU member states and the adoption of Audit Directive 2006/43 followed the year after in 2006. For instance, the first mandatory financial statements reported under IFRS 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. However, it may also take time before people with the required knowledge are able to take advantage of the reduction in economic barriers.

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[10] In addition, the EU LFS surveys are conducted annually. See Section 3.1 for details.

Based on these institutional considerations, it seems reasonable to expect effects from regulatory harmonization to begin at some point in 2005 but to gradually increase in subsequent years. As we are unsure about the exact start date, we exclude year 2005 and compute the average treatment effects from 2006 and until the end of our sample period in 2010.

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.[11] The group of participating countries has grown steadily since the LFS was first organized in 1960 and currently comprises the 28 EU member states, three EFTA countries (Iceland, Norway and Switzerland) and two EU candidate countries (the former Yugoslav Republic of Macedonia and Turkey). The LFS dataset is based on quarterly or annual interviews that are conducted by the national statistical offices of the participating countries.[12] 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 collection methods, questions, concept, definitions, and classifications are (almost) identical across countries. The standardized methodology substantially improves the comparability of statistics across countries over 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 data, permit data, or census data (Rendall et al., 2003). In contrast, the LFS data allow us to estimate consistent

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[11] 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://epp.eurostat.ec.europa.eu/portal/ page/portal/microdata/lfs.

[12] In 2012, about 1.5 million interviews were conducted throughout the participating countries each quarter.

migration measures across all EU member states through time. We are not aware of other datasets that would allow that.

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. We further reduce the concern by performing double-matched sample analyses using the rich set of demographic characteristics in the LFS dataset to hold sample composition constant. Another potential issue with the dataset is that certain variables are provided to researchers only in an aggregated fashion. For instance, job codes but also information about the country of birth (if different from the country in which the person is surveyed) are not made available at the most granular level. We discuss these restrictions and their implications in later sections of this paper.

3.2   Treatment and Control Samples

LFS data are provided to researchers in quarterly and annual files. We conduct our analysis on the annual files because only these contain the information that we need to perform our analysis throughout the sample period. The 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. The sample period ends in 2010, which gives us four years of post-Directive 2006/43 data. We do not include years before 2002 or after 2010 because the coding of several key variables, including the job codes, were different before and after these years. We

restrict the sample to the subset of LFS respondents who are between 20 and 59 years old (based on LFS item AGE), because this group is likely to be active in the workforce.

The resulting dataset comprises 18.2 million respondents from 29 countries with yearly totals varying between 1.1 and 3.2 million.[13] Eurostat computes a weighting factor (LFS item COEFFY) for each respondent based on his/her representativeness in the country's population. The total weighted number of respondents is about 2,512 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. This item indicates the respondents' job based on the current version of the International Standard Classification of Occupations (ISCO-88) at the aggregate three-digit level.[14] 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 is not a problem given our research design provided their 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

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[13] 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 for our sample period.

[14] 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.

based on a sample of 16 countries over the period 2002 to 2010 and show that the fraction of accountants in ISCO3D = 241 is around 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. Instead of picking a particular profession based on ex-ante comparability, the second control group ("all pros") consists of all respondents who belong to the job code group "professionals" (first digit of ISCO-88 = 2) and hence have a similar job level. This group includes lawyers, physicists, engineers, computing professionals, teachers, etc. We exclude architects, veterinary surgeons, and healthcare professionals (ISCO3D = 214, 222, and 223) from the "all pros" control group because Directive 2005/36 entered into force in 2005 and granted these three groups automatic recognition of their licenses to practice is all EU countries. Thus, including them in the control group would likely violate the parallel trend assumption.[15] The third control group ("biz people") consists of respondents with business jobs regardless of whether they are at a professional level. We use the following ISCO3D job codes to define this group: 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).

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[15] 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 the applicant to take a test. We examine the potentially confounding effect of changes in licensing and recognition rules in Section 5.3.

In the Appendix, we present the distribution on demographic characteristics of accounting professionals and the three control groups. We focus on demographic and other characteristics that prior literature has shown to affect migration, i.e., gender (LFS item SEX), age (measured in 5-year bins based on LFS item AGE), marital status (LFS item MARSTAT), number of children (LFS items QHHNUM, HHLINK and AGE), and education level (LFS item HATLEVEL). The distributions are remarkably similar across accounting professionals and the three control groups, except for the educational level.[16] The similarities are particularly strong between the accounting professionals and the legal professionals as well as all professionals with respect to key characteristics such as gender, age, marital status, and number of children. But even comparing accounting professionals and business people, the distributions are quite similar.

Nevertheless, we recognize various trade-offs in choosing the control group. The legal professionals are conceptually appealing because their jobs also require knowledge of specific rules and they too face certain licensing requirements. But the relatively small sample of legal professionals limits the number of potential matches. There are also risks to choosing only one profession as a benchmark. All professionals and business people comprise larger sets of jobs and respondents, mitigating this concern. But their jobs may in some cases be quite different from the jobs of accounting professionals. For instance, while all professionals generally work in jobs that, similar to accounting and auditing, require substantial education, their jobs may be in sectors that face different environmental changes (e.g., some may work for the government or in government-regulated jobs). Business people, on the other hand, typically work in the private sector and generally should have more similar educational backgrounds but the group includes

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[16] Given the educational differences, we exclude respondents that do not have at least an upper secondary education to ensure that business people are comparable to accounting professionals.

respondents that arguably work at higher (e.g., managers) or lower (e.g., associates) job levels than the typical accounting professional, as indicated by the first digit of the ISCO3D.

Since there are pros and cons to each control group, we report treatment effects using each control group. We use the largest control group, all professionals, when we use the alternative (flow) migration metrics, for which the fraction of migrants is low. The group of all professionals is large enough to offer sufficient sample size for these metrics and at the time has enough common support with the accounting professionals to make matching feasible even in relatively small subsamples (see Appendix and Section 4).

Our treatment group comprises 183,102 respondents from 26 countries.[17] The number of respondents in the control group of legal professionals (all professionals) [business people] is 68,945 (1,183,696) [1,315,394], when adding over all countries and sample years (see middle columns in Table 1 for further details).

3.3   Measuring Migration

For our main migration metric (*NATBIRTH*), we code respondents that have a foreign nationality (LFS item NATIONAL) *and* were born outside the host country in which the survey was conducted (LFS item COUNTRYB) 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 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. Such a definition is favorable in our setting considering that we intend to study relatively recent mobility decisions before and after regulatory harmonization. Nevertheless, the measure has two drawbacks.

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[17] The sample is reduced to 26 countries, because Bulgaria, Poland and Slovenia do not provide ISCO3D information at the three-digit level (i.e., respondents from the treatment group and the control group of legal professionals cannot be identified).

Ideally, we would measure when migration occurred but this is not feasible for this metric. This data limitation should not affect our difference-in-differences analysis as long as the rate of migration that took place outside the analysis window does not systematically change over time. Another weakness of this measure is that demographics are measured at the time the person is surveyed, not at the time the individual moves. Ideally, we would match demographics in the year of the move.

The LFS dataset contains other data items that allow us to construct mobility metrics that are closer to flow measures. This is desirable given the time dimension of our research question. But the flow measures suffer from low incidence rates in the general population, which causes what sometimes is called a "mini-domain problem" (e.g., Purcell and Kish, 1980). For this reason, our main analysis focuses on the stock measure preferred in the literature. However, we also present results using two novel mobility metrics, which measure migration relative to regulatory harmonization. We describe these metrics in Section 4.3.

The last column in Table 1 provides country-level migration statistics for *NATBIRTH* over all professions before requiring non-missing control variables, restricting education level, or excluding year 2005. The metric is available for almost all countries and years. The column also shows that there is considerable cross-sectional variation in the fraction of migrants across host countries. The rates are highest in small host countries such as Luxembourg and Switzerland and lowest in Eastern European countries such as Bulgaria, Poland and Romania.

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 the year 2005 because the exact starting point of regulatory

harmonization is ambiguous. Second, we restrict the sample to respondents with at least an upper secondary educational (LFS item HATLEVEL>2). Respondents that have not obtained at least an upper secondary education are highly unusual among the accounting professionals but also most control groups, except for business people (see Appendix).

The analysis naturally restricts the sample to observations with non-missing information on all control variables as well as on the mobility metric *NATBIRTH*. As shown in Table 2, information indicating whether an individual has children is missing with some frequency in the LFS dataset. The reason is that 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.[18] We also combine upper-secondary and post-secondary education levels as well as tertiary and doctoral education levels. The fraction of individuals with education at the post-secondary level and at the doctoral level is very small (see Appendix). Since we create matched bins (or fixed effects) for the full set of interactions between all control variables in Table 2, the data are too sparse to create separate categories for these education levels.

Taken together, the analysis sample comprises individuals from 26 countries for the years 2002 to 2004 and 2006 to 2010, who are between 20 and 59 years old, and whose highest degree of education is at least at the upper secondary level. The first column of 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.

The remaining columns in the upper half of Table 2 report the mean mobility rates of accounting professionals, legal professionals, all professionals, and business people in the

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[18] Note that our analyses are conducted within bins of certain demographics and within country. Therefore, this design choice should be innocuous and primarily help the representativeness of the sample that enters the analysis as well as power.

respective samples using *NATBIRTH*. Accounting professionals exhibit the highest mobility rates. However, these statistics include years after harmonization and do not control for or match on 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 computed at the country and year level 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 of accounting professionals and the three control groups is affected similarly by shocks in the environment prior to regulatory harmonization. The pre-period *levels* of the mobility rates are also similar for accounting professionals, all professionals, and business people before regulatory harmonization. The mobility rates for the legal professionals are considerably lower in the unmatched sample throughout the sample period, but they still move remarkably in parallel with the accounting professionals over the pre-treatment period. Thus, mobility patterns over the pre-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. This pattern is broadly consistent with a treatment effect from regulatory harmonization.

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. A difference-in-differences 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, age, marital status, education, 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 group, i.e., for individuals with the same characteristics. The main advantage of the fully interacted fixed-effect structure is that it avoids extrapolation and functional-form assumptions for the control variables. As a result, the approach is less susceptible to non-linearities in the data (e.g., Cochran and Rubin, 1973; Rubin, 1973 and 1979). Non-linearities are 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 or trends in mobility common to all professions in a given country (e.g., shocks to economic growth or changes to the survey methodology). Thus, in this regression design, the treatment effect is identified by time-series variation in mobility rates between professions within countries, controlling for all combinations of the demographic characteristics. We draw statistical inferences based on standard errors clustered by country-job group. Since our sample comprises 26 countries, this approach yields 52 clusters.

In Table 3 Panel A, we present OLS regression results for each of the three different control groups.[19] In the first three columns, we use the raw number of survey respondents (LFS weighted (no)) and in the last three columns we weigh observations with the statistical weight provided for each individual in the LFS dataset (LFS weighted (yes)). The coefficient estimates on the interaction term *Accountant * Post* are positive and statistically significant for all three control groups, consistent with the hypothesis that regulatory harmonization increases mobility. The estimated treatment effect without the LFS weighing factor is similar across the control groups ranging from 59 basis points to 91 basis points, which translates into an increase in cross-border labor mobility between 11 and 17 percent of accountants' pre-treatment mobility rate. Using the LFS weighting factor reduces the precision of the estimates but produces treatment effects that are roughly 15 percent of the accountants' pre-treatment mobility rate when we use the two large control groups (and a larger percentage effect for the lawyers). Hence, the magnitude of the treatment effect is similar to the regressions without statistical weights.

As a way to further gauge the economic magnitude, we translate the percentage effect into the increase in the total number of accounting professionals that migrate. Specifically, we

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[19] We estimate OLS regressions rather than logit or probit models to avoid an incidental parameter problem given the heavy use of fixed effects.

multiply the raw number of mobile survey respondents in the accounting profession prior to regulatory harmonization with the estimated percentage increase in migration due to harmonization and then invert the LFS weights to obtain a population estimate. The range of treatment effects from 11 to 17 percent implies that regulatory harmonization increased the number of migrating accountants by approximately 9,000 to 13,000 individuals. These effects appear economically significant and do not suggest that the results are driven by a few accountants at the margin.

In sum, the regression results provide evidence that regulatory harmonization in the accounting profession is associated with a substantial increase in cross-border migration.

4.2   Double matched difference-in-differences estimates

The LFS dataset is a combination of separate annual cross-sections of respondents rather than a panel dataset that follows individuals through time. Thus, changes in sample composition over time can potentially obscure the treatment effect or 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 and Sweden is underrepresented (overrepresented) in the post-harmonization period relative to the pre-harmonization period, then such changes would downward (upward) bias the estimated treatment effects in the regression analysis in Section 4.1, regardless of the true effect of harmonization. To address this concern, we perform a "double-matched" difference-in-differences analysis. The idea is to form quadruplets of identical characteristics within countries and then to compute the difference-in-differences within each matched quadruplet, which circumvents any sample composition effects.

We implement this approach as follows. Within each country and year, we first match all accounting professionals and all control group observations with the exact same characteristics for gender, age, marital status, education, and the presence of one or more children. 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 controls) 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. This design is further 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).[20] 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. This example illustrates why the double-matched approach eliminates sample composition changes.

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[20] In so doing, we exclude years 2005-2007 from our double-matched analysis. By focusing on later post-years, our results should be less susceptible to temporary changes in mobility (e.g., if audit firms import workers to help with IFRS adoption 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). With the alternative post-year definition, the treatment effect is attenuated but remains statistically significant (the attenuated magnitude is consistent with the gradual increase in accountant mobility after 2005 documented in Figure 1).

Within-quadruplet estimation of the treatment effect requires that the four components of a quadruplet[21] 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 concern, we give each quadruplet a weight equal to the minimum sample 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 average treatment effect is hence a weighted average across the effects for the quadruplets. As in the unmatched sample, we draw statistical inferences based on standard errors clustered by country-job group.

The double-matched approach is very demanding. As shown in Table 2, we lose a substantial fraction of our sample due to double-matching, especially when using the legal professionals as a control group. At the same time, the design is very tight and should provide a clean identification of the treatment effect. The double match approach also has the benefit of making the pre-harmonization mobility levels more comparable. As reported in Table 3 Panel B, the pre-harmonization mobility rates across accountants, all professionals and business people are similar and the differences are statistically insignificant. For legal professionals the pre-harmonization mobility rate remains lower and the difference to the accountants is statistically significant (roughly 106 basis points, t=2.35). Strictly speaking, such a difference is not a concern in a difference-in-differences design as long as the parallel-trend assumption holds. To

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[21] The four components of a quadruplet are: Accountant-pre, accountant-post, control-pre and control-post.

assess whether this assumption is valid even for the lawyers, we also graphically inspect the mobility rates for the double-matched sample by profession and year during the pre-harmonization period. The mobility rates do indeed move closely together during the three pre-harmonization years (2002-2004) and this is especially true for the legal professionals. Thus, the pre-harmonization difference in the mobility rates between accounting and legal professionals is not a major concern.

The estimated treatment effects from these double-matched difference-in-differences analyses are also presented in Table 3 Panel B. Again, we find a positive and statistically significant treatment effect for all three control groups. Our estimates based on the raw number of survey respondents (LFS weight (no)) suggest that the cross-border mobility of double-matched accountants increases by roughly 122 (76) [69] basis points relative to legal professionals (all professionals) [business people]. These estimates increase slightly when we augment the analysis with the statistical weights provided for each individual in the LFS dataset (LFS weighted (yes)): the estimates increase to 153 (77) [77] basis points relative to legal professionals (all professionals) [business people]. Compared to the pre-harmonization mobility rates, however, the magnitudes of the estimated treatment effects are similar with and without statistical weights and also comparable to those estimated in the regression analysis in Section 4.1. The effect is approximately 15 percent of pre-treatment mobility relative to all professionals and the business people, respectively. Relative to the legal professionals, we obtain a larger percentage increase (28%) similar to the weighted regressions in Section 4.1.[22]

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[22] The magnitudes are also similar when we compute the difference-in-differences in percentage terms to account for different base mobility rates. For example, in Panel B1 of Table 3, the mobility rate increases by 59% (from 4.361 to 6.937) for accountants but only by 41% (from 3.301 to 4.660) for legal professionals.

Overall, our analyses suggest that regulatory harmonization in the accounting profession reduced the net costs of migration leading to greater cross-border labor mobility for accounting professionals.

4.3   Alternative Flow Measures of Labor Migration and Return Migration

Prior work using the LFS dataset generally prefers to use stock measures of migration due to concerns about the small fraction of people moving in a given year (Martí and Ródenas, 2007, Skupnik 2013). In our main analyses, we follow this approach and use a commonly used stock measure based on nationality and country of birth (*NATBIRTH*). However, there are two related concerns about this migration measure.

First, *NATBIRTH* also captures mobility that occurs early in life and hence is unrelated to regulatory harmonization and perhaps even unrelated to employment. The difference-in-differences estimation addresses this problem as long as the fraction of early-in-life mobility does not change across periods or if such changes are similar across treatment and control groups. While we have no reason to suspect changes or even differential changes, it is preferable to measure mobility changes that occur in a window around regulatory harmonization.

Second, demographics variables are measured at the time the person is surveyed, not at the time the individual moves. Thus, for *NATBIRTH* we cannot be sure that a foreign resident that moved 5 years ago already had children or was married at the time of the move. Ideally, we would match demographics in the year of the move.[23]

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[23] The issue that demographic variables are measured at the time of the survey rather than at the time of migration is also a challenge in tests that examine cross-sectional variation in the treatment effect for *NATBIRTH* based on demographic variables. For instance, if migrants are more likely to have children and become married after they moved to the foreign country relative to the average respondent in the survey, then cross-sectional splits for *NATBIRTH* will not yield meaningful results. For this reason, we perform splits only for the two flow measures for which the demographic characteristics are measured close in time to when migration occurs.

To address these two concerns, we develop two alternative migration metrics that are closer to flow measures. The first alternative measure (*YEARESID*) is a quasi-flow measure. This measure 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 has the weakness that the distribution is truncated. For instance, a foreigner who is surveyed in 2007 counts as mobile in the post-harmonization period only if the person has moved in the last two years, i.e., the number of years of residence is smaller or equal to 2. In 2008, however, a person counts as mobile if she has moved in the last three years. Thus, by construction, mobility rates increase over time (relative to a fixed starting point). The double-matched difference-in-differences analysis strips out such effects because we pair specific pre- and post-years in a symmetric window around regulatory harmonization.

The second alternative measure is *COUNTR1Y*. For this measure we use the place of residence one year before the survey (LFS item COUNTR1Y). This measure is conceptually ideal because it is truly a flow measure and because respondent characteristics are measured close to the time of the move. However, the measure counts only recent migration. For example, a foreign resident that moved two or three years prior to the survey does not count as mobile. As a result, *COUNTR1Y* by construction yields much smaller numbers of migrants. To ensure that the analysis is not influenced by returning workers (repatriates), we augment the *COUNTR1Y*

measure by coding all individuals as zero if *NATBIRTH* indicates that the person is national or born in this country.[24]

We perform double-matched difference-in-differences analyses for the alternative flow measures and report them in Table 4. As noted earlier, the low incidence rates for the flow measures have an adverse effect on the power of our analyses. To increase the power of our tests for these measures, we also split the double-matched sample into various subsamples for which mobility is expected to be larger and hence the problem of low incidence rates should be (relatively) smaller. 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).

Table 4 also presents difference-in-differences estimates for various combinations of these splits. Each estimate is attained using the same double-matched approach described in Section 4.2, except that we use the alternative flow mobility measures. For this analysis, we use all professionals as the control group, as it is the largest group and sample size is crucial in dealing with the mini domain problem.

As expected, the mobility rates for the flow measures are much lower than for *NATBIRTH*. Consistent with our prediction that cross-border mobility is higher for: (1) singles

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[24] We note that accounting 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.

without young children; (2) younger workers and (3) workers at larger firms, we find that pre-harmonization accountant mobility rates increase monotonically, as these three restrictions are introduced cumulatively. Using pre-treatment averages for *YEARESID* (*COUNTR1Y*) without LFS weights, the mobility rate climbs from 175 (5) basis points for the full sample to as high as 389 (13) basis points for the most restricted sample. Thus, we expect these restrictions to be effective in mitigating the mini domain problem and improving power. However, these results from the restricted samples need to be interpreted carefully because we expect larger treatment effects for these subgroups and hence caution that the evidence cannot be extrapolated to the entire population.

For the full double-matched sample, the estimated treatment effects are positive and quite sizeable (relative to pre-treatment mobility rates) but not statistically significant, consistent with power being low. However, when we start restricting the sample, treatment effects and statistical significance begin to increase, as expected. For the *YEARESID* measure, the estimated coefficients indicate that recent mobility of single accountants without children increases by roughly 21 (38) basis points without LFS weighting (with LFS weighting) relative to matched professionals. However, the coefficient is significant with LFS weighting only. For the *COUNTR1Y* measure, we estimate that recent mobility of single accountants without children increases by roughly 14 (14) basis points without LFS weighting (with LFS weighting) relative to matched professionals. Again, statistical significance is modest. Further restricting to younger, single workers without children, we find that accountants experience a relative mobility increase of 33 (40) basis points using *YEARESID* and 16 (14) basis points using *COUNTR1Y* without LFS weighting (with LFS weighting). Lastly, we add the firm-size restriction. For this highly selected group of younger, single workers with no children working at large firms, the estimated

treatment effects are even larger. Using *YEARESID* the effect is 57 (161) basis points without LFS weighting (with LFS weighting). Using *COUNTR1Y* the effect is 31 (36) basis points without LFS weighting (with LFS weighting). Relative to the pre-treatment mobility rates, these effects are economically large. But again, we caution that these magnitudes be interpreted in the context of the highly restrictive subsample for which they are estimated.

Overall, the results in Table 4 using two flow measures corroborate our findings in Section 4.1 using a stock measure. As expected, the statistical power of the analyses using flow measures is low but the evidence is consistent with the interpretation that accounting and auditing harmonization have a sizeable effect on cross-border migration of accountants.

5. Assessing Alternative Explanations

Our difference-in-differences estimation combined with matching of individuals of various professions by country and year implies a 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. However, our analysis is susceptible to other 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 several alternative explanations for the documented increase in accountant mobility.

5.1 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 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 could be seen as a test mitigating the concern. The control group consisting of all professionals spans a large set of different educational backgrounds and hence it is unlikely that differential student mobility trends across treatment and control groups line up such that they induce our results. Thus, the first set of tests focuses on accounting and legal professionals. The two other sensitivity tests explore 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.[25] 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. Figure 3 shows that between 2000 and 2010 the relative proportions of business and law students

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[25] ERASMUS was created in 1987. It is the largest student exchange program in Europe. By 2007, over 5 million students have participated in the program.

that participate in the ERASMUS program remain fairly constant over time.[26] To more formally gauge the effect of cross-border student mobility on our results, we introduce the lagged fraction of students in the ERASMUS program in the respective field (i.e., business for the accountants and law for the legal professionals) as a control variable in the regression analysis in Table 3, Panel A. We choose a two-year lag considering the normal length of study as well as data availability, i.e., the fact that we have data for program from the academic year 2000/2001 onwards. The results (not tabulated) show that the introduction of this control variable does not materially affect the coefficient estimates of interest and that the inferences remain unchanged.

Second, in the three left columns of Panel A in Table 5, we restrict the analyses to individuals that have graduated or finished their highest degree of education prior to 1999, the year the Bologna Process was launched. These individuals should not be affected by recent increases in student mobility as well as the Bologna Process. The estimated treatment effects for the restricted sample are very similar to those reported in the main analyses in Table 3, Panel A.

Third, in the three right columns of Panel A in Table 5, 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). Again, we find similar results and that the inferences are unchanged from the main analysis in Table 3, Panel A.

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[26] 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 3 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.

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

5.2 Enlargement of the European Union

In 2004 and 2007, the EU was enlarged by in total twelve new, mainly Eastern European member states (EU-10 and EU-2). [27] 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 citizens of EU-10 and EU-2 have significantly higher labor mobility than the citizens of the EU member states prior to 2004 (EU-15). Thus, a potential concern is that changes in the composition of the EU and the removal of labor migration restrictions play into the documented increase in cross-border labor mobility for accounting professionals relative to other professions. The changes could affect the estimates if the accounting profession is better able to take advantage of EU enlargement than other professions, for example, because of the concentration of international audit firms or because the accounting profession has a higher level of standardization than comparable occupations.

We examine this alternative explanation by examining migration solely within EU-15 countries. We have two specifications. In the first specification, we restrict the sample to individuals surveyed in EU-15 (i.e., EU-15 host countries only). In the second specification, we restrict the sample 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 (EU-15 host and EU-15 source country only) but moved to another EU-15 country. Table 5 Panel B, shows that the treatment effects are very

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[27] In 2004, the following countries became members of the EU: Cyprus, the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia, and Slovenia (EU10). In 2007, Bulgaria and Romania became members (EU2).

similar when we restrict the sample to EU-15 host countries. We also find significant, albeit smaller, increases in within-EU-15 mobility. The smaller effect within EU-15 is expected because labor migration, in general, is lower between these countries (OECD 2014).

These results are inconsistent with the alternative explanation that EU enlargement and/or the removal of labor migration restrictions for citizens from new EU members drive the findings.

5.3   Recognition of Qualifications

A final 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 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).[28] 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 2005. 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.

6. Conclusion

This paper examines the effect of international regulatory harmonization on cross-border labor migration. We exploit a substantial increase in regulatory 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' annual accounts and consolidated financial statements. We analyze changes in cross-border labor mobility around these regulatory changes.

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

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[28] 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.

professionals, and a set of business people. We control for demographic characteristics that prior studies show as determining migration (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 in a year before harmonization with a year after harmonization.

We find that labor migration in the accounting profession increases relative to matched other professionals by roughly 15% after regulatory harmonization. We document this increase using several mobility measures. To further corroborate our findings, we consider a number of alternative explanations and show that the mobility effects are not explained by differential changes in student mobility across professions or by licensing rules. We also show that the increase in cross-border mobility is not explained by EU enlargement and hence does not simply reflect migration from Eastern to Western Europe.

Overall, we conclude that our findings illustrate that diversity in rules and regulations constitutes an important economic barrier to cross-border labor mobility. More specifically, our results imply that, due to these barriers, accounting and auditing harmonization can have a meaningful effect on cross-border migration. Thus, regulatory harmonization could 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. Thus, the magnitude of the estimated treatment effects needs to be interpreted carefully.

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APPENDIX

Distribution of key characteristics for treatment and control groups

This appendix provides 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, respectively). The statistics are based on the raw number of survey respondents in the LFS dataset who are between 20 and 59 years old. The LFS dataset provides information from 29 European countries over the years 2002 to 2010 (see Table 1 for further details).

Table A1: Gender

Gender	Accountants		Legal pros		All pros		Biz people	
	Obs.	Share	Obs.	Share	Obs.	Share	Obs.	Share
Male	92,551	51%	35,134	51%	502,745	42%	846,580	64%
Female	90,551	49%	33,811	49%	680,951	58%	468,814	36%
Total	183,102	100%	68,945	100%	1,183,696	100%	1,315,394	100%

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

Table A2: Age

Age	Accountants		Legal pros		All pros		Biz people	
	Obs.	Share	Obs.	Share	Obs.	Share	Obs.	Share
20-24	7,512	4%	1,396	2%	38,716	3%	47,317	4%
25-29	23,961	13%	8,692	13%	143,283	12%	111,664	8%
30-34	30,062	16%	12,869	19%	172,391	15%	164,333	12%
35-39	31,068	17%	12,783	19%	177,982	15%	209,761	16%
40-44	28,540	16%	10,957	16%	178,950	15%	226,000	17%
45-49	24,206	13%	8,957	13%	177,575	15%	214,433	16%
50-54	20,562	11%	7,619	11%	167,086	14%	190,029	14%
55-59	17,191	9%	5,672	8%	127,713	11%	151,857	12%
Total	183,102	100%	68,945	100%	1,183,696	100%	1,315,394	100%

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

Table A3: Marital status

Marital status	Accountants		Legal pros		All pros		Biz people	
	Obs.	Share	Obs.	Share	Obs.	Share	Obs.	Share
Divorced	14,388	8%	4,306	6%	92,684	8%	112,659	9%
Single	61,351	34%	24,399	35%	376,254	32%	331,573	25%
Married	107,297	59%	40,186	58%	713,976	60%	870,596	66%
Missing	66	0%	54	0%	782	0%	566	0%
Total	183,102	100%	68,945	100%	1,183,696	100%	1,315,394	100%

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

Table A4: Number of kids

Number of kids	Accountants		Legal pros		All pros		Biz people	
	Obs.	Share	Obs.	Share	Obs.	Share	Obs.	Share
0	83,132	45%	37,721	55%	627,759	53%	681,665	52%
1	23,697	13%	10,731	16%	178,595	15%	218,174	17%
2	19,096	10%	9,399	14%	133,996	11%	168,855	13%
3	4,070	2%	2,122	3%	28,256	2%	36,180	3%
4	561	0%	298	0%	4,398	0%	5,379	0%
5 or more	80	0%	62	0%	887	0%	1,001	0%
Missing	52,466	29%	8,612	12%	209,805	18%	204,140	16%
Total	183,102	100%	68,945	100%	1,183,696	100%	1,315,394	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*.

Table A5: Level of education

Level of education	Accountants		Legal pros		All pros		Biz people	
	Obs.	Share	Obs.	Share	Obs.	Share	Obs.	Share
Pre-primary	24	0%	10	0%	204	0%	1,645	0%
Primary	297	0%	20	0%	1,547	0%	57,546	4%
Lower secondary	4,195	2%	184	0%	13,807	1%	163,330	12%
Upper secondary	41,194	22%	1,769	3%	136,363	12%	580,468	44%
Post secondary	7,833	4%	333	0%	31,739	3%	60,800	5%
Tertiary	125,794	69%	63,030	91%	947,791	80%	436,829	33%
Doctoral	2,845	2%	3,405	5%	48,680	4%	8,854	1%
Missing	920	1%	194	0%	3,565	0%	5,922	0%
Total	183,102	100%	68,945	100%	1,183,696	100%	1,315,394	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.

TABLE 1

Description of LFS dataset

Host country	LFS Dataset (2002 to 2010)		Treatment (Accountants)	Control (Legal pros)	Control (All pros)	Control (Biz people)	Mobility for LFS dataset (NATBIRTH)	
	Number of respondents	LFS weighted (in '000)	Number of respondents	Number of respondents	Number of respondents	Number of respondents	Rate	Years available
Austria	727,601	41,306	4,751	2,970	40,828	77,343	8.50%	all
Belgium	413,938	51,931	7,534	2,418	37,629	37,213	8.43%	all
Bulgaria	342,196	38,096	0	0	27,371	0	0.14%	2006-2010
Cyprus	149,675	3,768	2,467	800	10,234	9,897	16.71%	all
Czech Republic	904,756	54,371	9,907	2,651	42,657	66,878	0.99%	all
Denmark	312,327	26,385	6,824	981	28,315	28,638	3.74%	all
Estonia	77,250	6,669	1,176	195	5,110	8,257	4.62%	all
Finland	249,488	25,701	1,766	439	15,805	17,762	1.78%	all
France	1,378,780	291,151	1,447	3,828	94,819	116,847	6.20%	all
Germany	916,900	411,538	9,832	4,048	59,105	53,309	6.13%	all
Greece	1,067,303	53,883	7,132	6,365	67,971	79,847	6.83%	all
Hungary	1,106,627	50,825	9,097	2,769	53,881	69,647	0.57%	all
Iceland	63,653	1,455	1,265	387	6,586	6,687	3.77%	all
Ireland	623,048	21,596	10,112	2,328	44,431	72,576	9.95%	all
Italy	2,400,271	293,207	14,602	11,700	97,420	184,641	5.21%	2005-2010
Latvia	113,100	11,474	1,847	462	7,147	10,081	4.50%	2004-2010
Lithuania	211,151	16,904	6,983	744	13,408	16,875	0.40%	all
Luxembourg	165,414	2,377	5,758	1,481	14,951	11,770	34.00%	all
Netherlands	766,527	81,637	18,521	4,899	78,668	90,697	2.85%	all
Norway	185,623	22,837	2,136	612	10,941	17,654	4.27%	all
Poland	924,071	196,248	0	0	81,537	0	0.09%	2004-2010
Portugal	617,708	53,475	3,268	1,925	24,295	39,471	3.04%	all
Romania	909,150	112,001	3,071	3,078	39,216	20,305	0.06%	2004-2010
Slovakia	416,165	28,696	3,620	1,044	21,286	22,724	0.16%	2003-2010
Slovenia	278,735	10,664	0	0	31,135	0	0.69%	all
Spain	930,113	229,978	3,678	4,767	53,193	64,505	4.34%	all
Sweden	1,059,614	43,040	29,873	3,894	105,242	92,158	4.29%	all
Switzerland	297,757	38,069	7,521	1,576	28,276	13,463	38.92%	all
United Kingdom	586,491	292,933	8,914	2,584	42,239	86,149	6.87%	all
Total	18,195,432	2,512,215	183,102	68,945	1,183,696	1,315,394	5.07%	

Table 1 presents details on the composition our dataset and descriptive statistics on mobility rates 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. In the second column, we report for each country 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 column of the table, we report country-level statistics for the LFS dataset on the mobility metric *NATBIRTH* which is defined as the share of individuals who were born outside the host country and have a foreign nationality. We compute *NATBIRTH* with the LFS weighted number of relevant respondents in the numerator and denominator to ensure that the rates are representative of the population. The mobility rates are not available for some country-years due to missing information on the relevant LFS items.

TABLE 2

Descriptive statistics for difference-in-differences analysis

Variables	Regression analysis		Double matched diff-in-diff analysis					
			Control (Legal pros)		Control (All pros)		Control (Biz people)	
	Obs.	Mean	Obs.	Mean	Obs.	Mean	Obs.	Mean
Mobility yes/no (NATBIRTH)								
Treatment (Accountants)	145,339	5.64%	55,157	5.81%	82,095	5.08%	81,839	4.88%
Control (Legal pros)	54,361	2.89%	28,067	2.52%				
Control (All pros)	931,131	3.49%			428,343	3.73%		
Control (Biz people)	886,224	3.44%					400,991	3.30%
Control variables (Accountants)								
Female yes/no	145,339	0.501	55,157	0.470	82,095	0.497	81,839	0.494
Age	145,339	39.842	55,157	39.496	82,095	40.134	81,839	40.145
Has kids yes/no	101,899	0.358	39,539	0.382	55,754	0.366	55,512	0.367
Single yes/no	145,339	0.340	55,157	0.337	82,095	0.327	81,839	0.325
Higher education yes/no	145,339	0.724	55,157	0.963	82,095	0.740	81,839	0.734

Table 2 presents descriptive statistics for the difference-in-differences analysis in Table 3. The statistics are based on the raw number of survey respondents in the LFS dataset. Our sample for the difference-in-differences analysis comprises individuals from 26 countries for the years 2002 to 2004 and 2006 to 2010. 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 metric *NATBIRTH*. This metric defines individuals as migrants (or mobile) if they were born outside the host country and have a foreign nationality. The sample for the double matched difference-in-differences analysis is further restricted to pairs of treatment and control group individuals who share the same characteristics (i.e., the same values for the control variables listed in this table) 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). 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 part of Table 2 shows statistics on the mobility metric *NATBIRTH*. The lower part presents statistics on the following control variables for the treatment group of accountants: *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: Regression analysis

Independent variables	Dependent variable: Mobility yes/no (NATBIRTH)					
	LFS weighted (no)			LFS weighted (yes)		
Accountant * Post	0.763***	0.907***	0.594**	0.927*	0.583**	0.610*
	(3.14)	(3.71)	(2.59)	(2.00)	(2.06)	(1.73)
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	199,700	1,076,470	1,031,563	195,841	1,024,571	972,911
R-squared	0.28	0.15	0.12	0.10	0.05	0.04

Panel A of Table 3 presents 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). The sample comprises individuals from 26 countries for the years 2002 to 2004 and 2006 to 2010. The dependent variable is a binary variable and equals one if the individual is mobile according to mobility metric *NATBIRTH*, and zero otherwise. *NATBIRTH* defines individuals as mobile if they were born outside the host country and have a foreign nationality. 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 2006 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. In the left column, we report results of regressions using the raw number of survey respondents (i.e., not LFS weighted). In the right column, we present results of LFS-weighted regressions in which each observation is weighted with the statistical weight for the individual provided in the LFS dataset. 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 B1: Double matched diff-in-diff analysis (Legal pros)

Diff-in-diff analysis (Quadruplets = 1,437; Observations = 83,224)	Mobility rate: NATBIRTH		
	Pre-period (1)	Post-period (2)	Difference (2) - (1)
LFS weighted (no)			
(a) Treatment (Accountants)	4.361	6.937	2.576
(b) Control (Legal pros)	3.301	4.660	1.359
Difference (a) - (b)	1.060**	2.277	1.217**
T-stat (a) - (b)	(2.35)		(2.31)
LFS weighted (yes)			
Difference (a) - (b)			1.534**
T-stat (a) - (b)			(2.48)

Panel B2: Double matched diff-in-diff analysis (All pros)

Diff-in-diff analysis (Quadruplets = 2,155; Observations = 510,438)	Mobility rate: NATBIRTH		
	Pre-period (1)	Post-period (2)	Difference (2) - (1)
LFS weighted (no)			
(a) Treatment (Accountants)	5.030	7.761	2.731
(b) Control (All pros)	4.966	6.936	1.970
Difference (a) - (b)	0.064	0.825	0.761***
T-stat (a) - (b)	(0.07)		(3.23)
LFS weighted (yes)			
Difference (a) - (b)			0.774***
T-stat (a) - (b)			(4.26)

Panel B3: Double matched diff-in-diff analysis (Biz people)

Diff-in-diff analysis (Quadruplets = 2,135; Observations = 482,830)	Mobility rate: NATBIRTH		
	Pre-period (1)	Post-period (2)	Difference (2) - (1)
LFS weighted (no)			
(a) Treatment (Accountants)	4.975	7.686	2.711
(b) Control (Biz people)	4.458	6.475	2.017
Difference (a) - (b)	0.517	1.211	0.694*
T-stat (a) - (b)	(0.91)		(1.74)
LFS weighted (yes)			
Difference (a) - (b)			0.770**
T-stat (a) - (b)			(2.60)

TABLE 3 (continued)

Panel B 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 groups (legal professionals, all professionals or business people). The sample comprises individuals from 26 countries for the years 2002 to 2004 and 2008 to 2010. We drop sample years 2006 and 2007 in this analysis to have equal-size measurement windows. 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). In the top left corner of each table, we report the number of quadruplets that we can form for the treatment and the respective control group as well as the number of individuals in these quadruplets. *NATBIRTH* is a binary variable and equals one if the individual is mobile, and zero otherwise. *NATBIRTH* defines individuals as mobile if they were born outside the host country and have a foreign nationality. The upper part of each table reports the mobility rate for each cell in the difference-in-differences analysis (treatment and control; pre and post) based on the raw number of survey respondents (i.e., not LFS weighted). 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 bottom row presents the LFS-weighted treatment effect using in addition the statistical weight provided for each individual in the LFS dataset. 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 (no)	LFS weighted (yes)	LFS weighted (no)	LFS weighted (yes)	LFS weighted (no)	LFS weighted (yes)	LFS weighted (no)	LFS weighted (yes)
Mobility rate: YEARESID								
Pre-treatment mobility	1.746	1.348	2.187	2.065	2.407	2.132	3.890	3.106
Treatment effect	0.180	0.276	0.205	0.376*	0.333^	0.399*	0.569^	1.605***
T-stat	(0.69)	(0.97)	(0.98)	(1.93)	(1.442)	(2.01)	(1.62)	(7.28)
Quadruplets / Observations	2,265 / 514,759		549 / 101,512		348 / 84,410		240 / 25,518	
Mobility rate: COUNTR1Y								
Pre-treatment mobility	0.054	0.046	0.094	0.094	0.109	0.097	0.125	0.147
Treatment effect	0.045	-0.016	0.136^	0.138**	0.163^	0.143**	0.311	0.361***
T-stat	(0.81)	(-0.63)	(1.61)	(2.21)	(1.57)	(2.24)	(1.30)	(2.73)
Quadruplets / Observations	2,317 / 516,360		560 / 102,120		354 / 84,841		244 / 25,712	

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. The research design is the same as in the analysis presented in Panel B of Table 3. All results in this table are based on tests that use all professionals as the control group (which is the largest 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 sub-samples: *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/n*o = 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 the quadruplets. We report these statistics based on the raw number of survey respondents (i.e., not LFS weighted) and LFS weighted. 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

Sensitivity analyses

Panel A: Sensitivity to student trends

Independent variables	Dependent variable: Mobility yes/no (NATBIRTH)					
	Early graduates only (HATYEAR <= 1999)			Controlling for time since graduation		
Accountant * Post	0.754*	0.801**	0.590*	0.664**	0.847***	0.543*
	(1.88)	(2.59)	(1.83)	(2.34)	(2.98)	(1.91)
Years since graduation				-0.027	-0.071**	-0.040
				(-0.40)	(-2.15)	(-1.27)
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	131,428	720,291	782,335	186,831	1,015,351	955,920
R-squared	0.33	0.17	0.13	0.30	0.16	0.13

Panel B: Migrations to and within EU-15

Independent variables	Dependent variable: Mobility yes/no (NATBIRTH)					
	EU-15 host countries only			EU-15 host and EU-15 source countries only		
Accountant * Post	0.708**	0.913***	0.547*	0.416**	0.484**	0.292*
	(2.49)	(3.27)	(1.90)	(2.32)	(2.12)	(1.81)
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	146,786	731,128	778,372	146,786	731,128	778,372
R-squared	0.30	0.10	0.11	0.32	0.12	0.14

Table 5 presents results from sensitivity analyses. The sample comprises individuals from 26 countries for the years 2002 to 2004 and 2006 to 2010. The research design is the same as in the regression analysis presented in Panel A of Table 3. All regression results in this table are based on the raw number of survey respondents (i.e., not LFS weighted). Panel A shows sensitivity analyses related to differential student mobility trends. In the first set of regressions the sample is restricted to the sub-sample of individuals who completed their highest level of education in 1999 or earlier (LFS item *HATYEAR*). The other set of regressions includes *Years since graduation* (i.e., the difference between the sample year and the graduation year of the individual) as an additional control variable. Panel B presents results for two sets of specifications on migrations to and within EU-15 countries: The first set of regressions is restricted to the sub-sample 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 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.
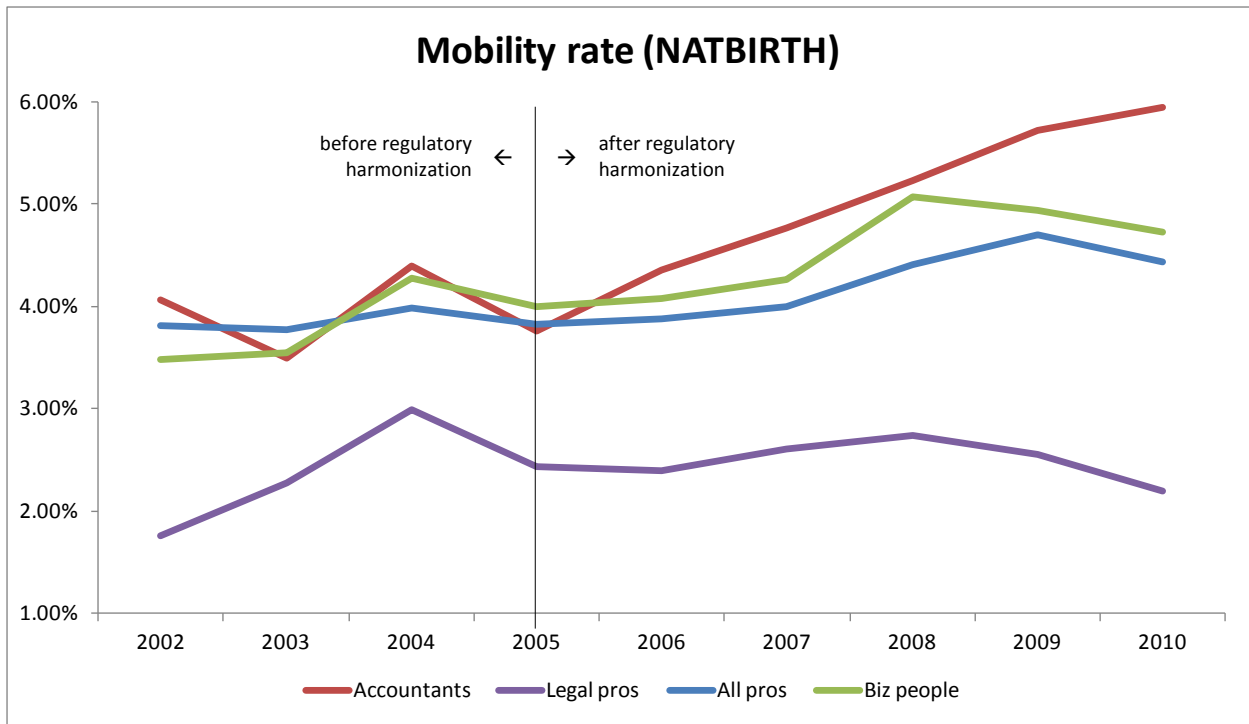
FIGURE 1

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

Example of quadruplet:
Single males, in Germany, between 25-29 years old, without kids, with university degree

Second difference (post - pre)

Second difference:

(a.pre) Accounting professionals ↔ Exact match ↔ (a.post) Accounting professionals

a.post-a.pre

First difference (a - c)

Exact match

Exact match

(c.pre) Control professionals ↔ Exact match ↔ (c.post) Control professionals

c.post-c.pre

First difference: a.pre - c.pre   a.post - c.post

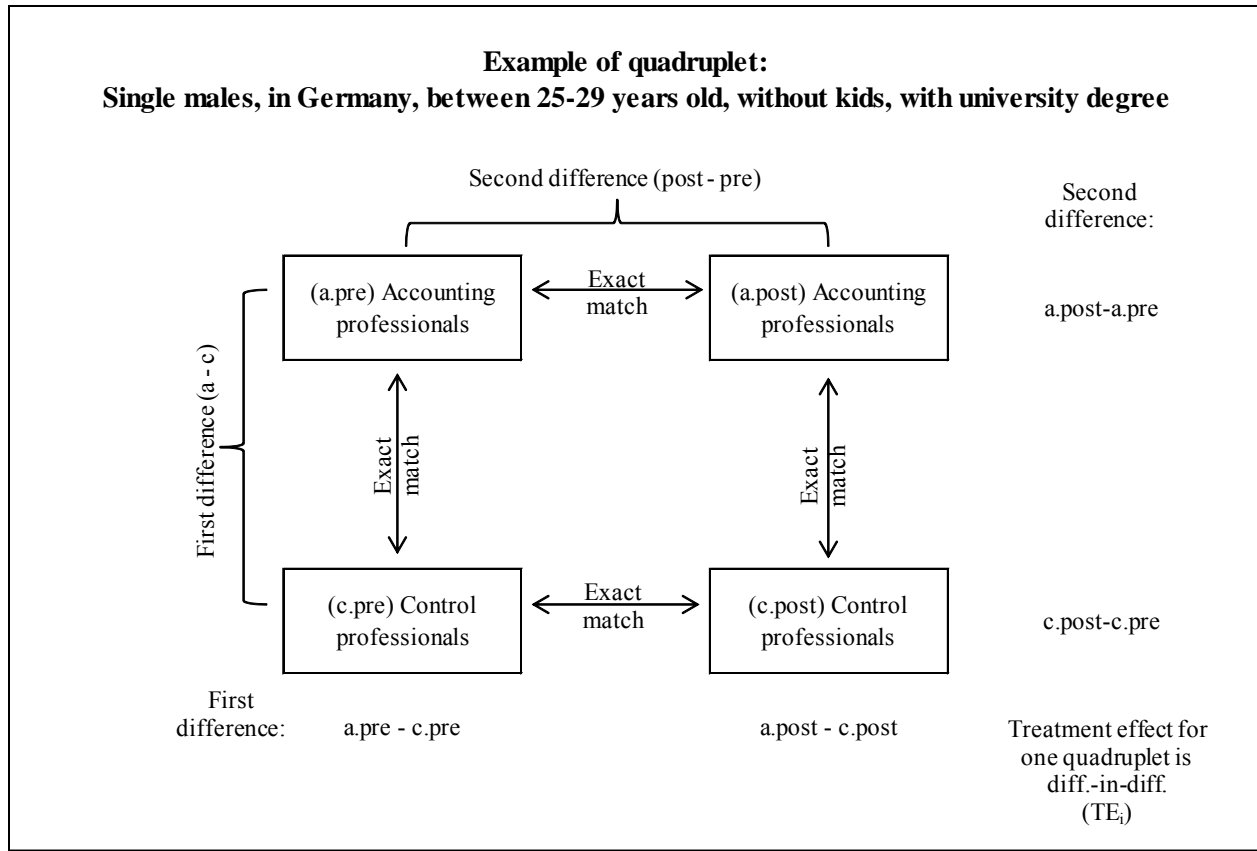Treatment effect for one quadruplet is diff.-in-diff. (TE_i)

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, age, presence of children, marital status, and education level (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.

FIGURE 3

Mobility trends for business and law students

Percentage of all Erasmus students

Business students — Law students

Proportion of foreign students at German universities

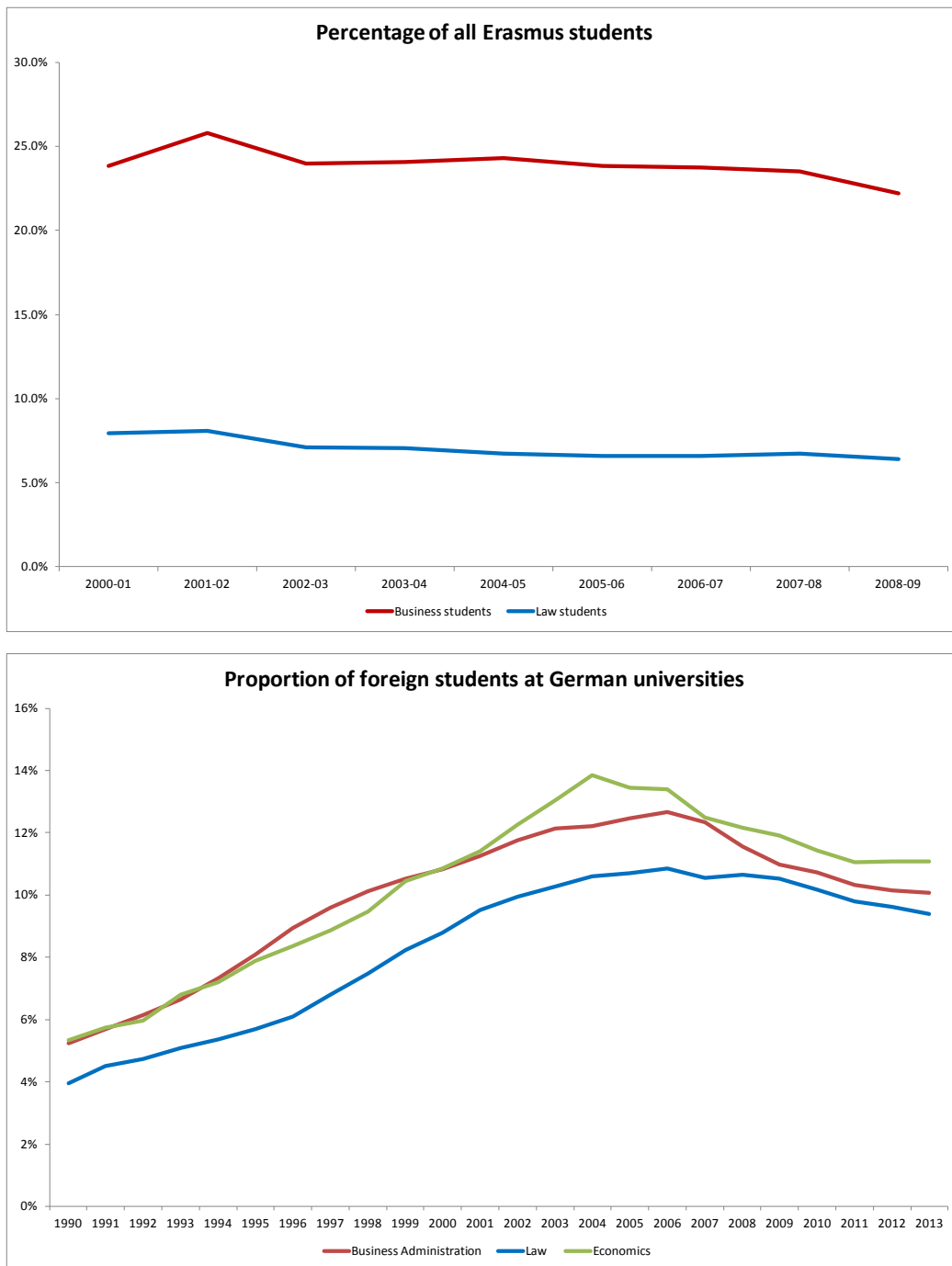Business Administration — Law — Economics

Figure 3 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).