Three Essays on Firms in International Trade

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J.S.
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SUMMARY

As the nature of international trade changes, this thesis contributes to a deeper understanding of the role of modern international trade and policy for firms and employment. It consists of three chapters.

Chapter 1 investigates the effect of the Mutual Recognition Agreement (MRA) of 2002 between Switzerland and the European Union (EU) on the extensive margin of Swiss exports and thereby contributes to the literature on impact analysis of non-tariff measures. Embedding the theoretical setup in the heterogeneous firms in international trade literature, we model the MRA as a reduction in fixed exporting costs for firms in affected sectors exporting to the EU market, yielding predicted increases on the extensive margin of bilateral trade with the EU in those sectors. Using a double and triple difference approach, we find that the reduction of fixed costs induced by the MRA has entailed a relative diversification of Swiss exports of affected products into the EU.

Chapter 2 revisits the "self-selection vs. learning-by-exporting (LBE)" debate with new evidence on a large panel of German firms and sheds new light on the channels that foster export-induced productivity gains. In line with previous results, we find substantial pre-export differences in productivity between future exporters and domestic firms. Nevertheless, these pre-export differences remain constant over time and we find strong evidence against a conscious self-selection effect, in which firms would actively engage in increasing their productivity in temporal proximity to starting to export. In contrast, we find support for the LBE hypothesis in both the manufacturing and the services sector, notably among continuing exporters. However, the learning effect is not progressive and more short-lived in the
latter than in the former. We explain the different sectoral performances with significant differences in access to foreign markets, which is substantially lower and more concentrated within few firms in services. Furthermore, we show that across sectors, the size of the LBE effect depends on the level of within-sector competition. In line with basic microeconomic theory, productivity gains are higher for entrants into exporting, which operate in relatively uncompetitive domestic sectors, pointing to an important competitiveness channel for productivity gains.

Chapter 3 investigates the role of exchange rate fluctuations in labor market outcomes. It examines the effects of external exposure on the labor market in Switzerland. We exploit exogenous exchange rate movements to identify trade-induced shocks across all sectors of the Swiss economy and transpose industry-level exposure to the municipal level, using detailed employment data on the entirety of Swiss firms. We find strong evidence for three channels of antagonistic employment effects of currency appreciation: negative employment growth induced by increasing export uncompetitiveness and higher import competition, and positive employment growth induced by cheaper availability of foreign inputs. Importantly, the latter channel appears to trump the former two, and the Swiss economy appears to successfully have hedged itself through integration in global value chains.
ZUSAMMENFASSUNG

Diese Dissertation befasst sich mit dem veränderten Wesen internationalen Handels und internationaler Handelspolitik, im Hinblick auf die Implikationen für Firmen und den Arbeitsmarkt. Sie besteht aus drei Kapiteln.

Kapitel 1 erforscht die Auswirkungen des Mutual Recognition Agreements (MRA) von 2002 zwischen der Schweiz und der Europäischen Union in Bezug auf die Anzahl gehandelter Produkte, und reiht sich somit in die neuere Literatur zur Folgenabschätzung nichttarifärer Handelsliberalisierung ein. Der theoretische Teil lehnt sich an die heterogeneous firms Literatur des internationalen Handels an, und wir modellieren das MRA als Verringerung der fixen Exportkosten in denjenigen Sektoren, die von dem Abkommen betroffen sind, wodurch eine gesteigerte Anzahl an exportierten Produkten in die EU zu erwarten ist. Mithilfe von double und triple difference Methoden, weisen wir eine Diversifikation der betroffenen Schweizer Exporte in die EU nach.

Kapitel 2 untersucht, inwiefern Deutsche Firmen Selbstselektion in Exportmärkte betreiben und/oder inwiefern der Exporteintritt für anschließende Produktivitätssteigerungen verantwortlich ist. Unsere Resultate decken sich mit früheren Studien für andere Länder und wir weisen starke prä-Export Unterschiede in Produktivität von zukünftigen Exportern und Nichtexportern nach, die jedoch konstant bleiben und sich erst nach Exporteintritt verschärfen. Dies lässt auf exportspezifische Lernprozesse (LBE) schließen, die wir sowohl im verarbeitenden Gewerbe, als auch bei den Dienstleistungen nachweisen können, auch wenn sie im ersteren stärker ausgeprägt und über die Zeit monoton ansteigend sind. Diese Unterschieden lassen sich durch verschiedene Marktstrukturen erklären, die unterschied-
liche Wettbewerbsintensitäten im heimischen Markt zur Folge haben. Ganz im Sinne elementarer Wirtschaftstheorie sind die LBE Effekte am stärksten in den Unternehmen ausgeprägt, die in einem Sektor operieren, in dem es relativ wenig Wettbewerb gibt.

Kapitel 3 untersucht die Arbeitsmarkteffekte von Währungsfluktuationen und bezieht sich konkret auf die stürmischen letzten 15 Jahre des Schweizer Frankens. Wir identifizieren mehrere Kanäle, durch die der Wechselkurs lokale Arbeitsmärkte durch internationalen Handel beeinflusst und finden Hinweise auf negative Arbeitseffekte der Frankenaufwertung in Gegenenden, die besonders stark Importen ausgesetzt sind, sowie auch in Gegenenden, deren industrielle Struktur viel Umsatz im Ausland macht. Ein wichtiger, gegenläufiger Effekt ist jedoch dadurch gegeben, dass Schweizer Firmen oft Vorleistungen aus dem Ausland beziehen. Insgesamt hat diese Komponente die Schweizer Volkswirtschaft relativ gut vor dem Franken- schock geschützt und zumindest auf gesamtwirtschaftlichem Niveau die negativen Folgen für den Arbeitsmarkt abgefedert.
ABSTRACT

This paper analyzes the effect of the Mutual Recognition Agreement (MRA) of 2002 between Switzerland and the European Union (EU) on the extensive margin of Swiss exports and thereby contributes to the literature on impact analysis of non-tariff measures. Embedding the theoretical setup in the heterogeneous firms in international trade literature, we model the MRA as a reduction in fixed exporting costs for firms producing affected goods destined to the EU market and transpose the firm-level implications onto the product level, yielding predicted increases on the extensive margin of bilateral trade with the EU in affected sectors. Using a double and triple difference approach, we find that the reduction of fixed costs induced by the MRA has entailed a relative diversification of Swiss exports of affected products into the EU. Our results suggest that the MRA has been an effective tool for Swiss trade relations with the EU, as it has promoted a diversification of export opportunities through facilitated market access that Swiss firms have been able to seize.
1.1 INTRODUCTION

With the gradual dismantlement of traditional barriers to trade such as tariffs and quotas, the focus of modern trade policy reforms has shifted towards more targeted interventions aimed at reducing trade costs and addressing market failures that inhibit exports. However, surveying a large body of literature, Harrison and Rodríguez-Clare (2009) hardly find any microeconomic evidence to guide such trade policy interventions. Part of the problem is that non-tariff barriers (NTBs) are often much less tangible and harder to measure than tariffs. Conversely this property makes NTBs prone to abuse for "murky protectionism", as pointed out by Baldwin and Evenett (2011). Indeed, there seems to be increasing evidence that new product standards are being introduced as disguised protectionism (Grundke and Moser (2016)) and importers’ tariff cuts are frequently being followed by new concerns about the imposition of NTBs (Orefice (2016)).

NTBs are not new and have posed problems to economists for some time. Early reviews discuss several methods to measure NTBs include Baldwin (1989), Deardorff and Stern (1998), and Maskus et al. (2001). Among the various approaches to measure NTBs, one major such approach used in the subsequent literature is to translate NTBs into price-based ad-valorem tariff equivalents (e.g. Ganslandt and Markusen (2001), Kee et al. (2009), or more recently Cadot and Gourdon (2016)). Trade and welfare effects of variation in these estimates have subsequently often been used in Computable General Equilibrium models (see e.g. Winchester (2009), or the discussions in Dee and Ferrantino (2005) and Fugazza and Maur (2008)). Apart from general data concerns in CGE exercises, serious questions remain as to such price-based approaches. For instance, price wedges are calculated on the basis of a price level that already take the existence of the NTB into account and may therefore not be an appropriate benchmark (Deardorff and Stern (1998)). More generally, it is unclear whether some types of NTBs can reasonably be modeled as ad-valorem tariff equivalents, as they often
are often better modeled as fixed costs.

Other studies create indices based on simple count, frequency and other restrictiveness measures, such as in Hoekman and Nicita (2011) or Beghin et al. (2015), and to some extent in Kee et al. (2009). Similar techniques have been used most recently to construct new databases that quantify barriers to trade in services, described by Borchert et al. (2014) and Grosso et al. (2015). An important criticism of this approach is that the constructed indices are not bilateral and hence cannot be used to proxy for the restrictiveness of barriers to trade in gravity-based settings, which is a very common empirical approach (see Disdier et al. (2015)). One way to account for this is to consider products and country-pairs, which have been harmonized within a customs union (de Frahan and Vancauteren (2011), Vancauteren (2013)). Another technique used to explicitly consider policy heterogeneity is to consider the degree of adherence to international ISO standards or the existence of mutual recognition agreements, such as in Czubala et al. (2009) or Blind et al. (2013).

Our study relates to this strand of the literature in that we also employ a gravity-based approach and focus on a specific NTB, or rather its dismantlement. Rather than being interested in quantifying the importance of given NTBs, we examine the trade effect of a change in a given NTB on the extensive product margin, while being agnostic about its scale. In order to do so, we augment the standard gravity toolkit by combining it with double and triple differencing techniques to isolate causality, effectively borrowing impact evaluation tools from other fields of economics (health, development, etc.) to assess the impact of trade-related interventions, such as proposed by Cadot et al. (2011).

We exploit a natural experiment in Swiss trade policy to estimate its impact on bilateral trade flows between Switzerland and its trading partners. The policy measure in question is the Mutual Recognition Agreement
(MRA) between Switzerland and the EU, which is well defined in time as it has entered into force on 1 June 2002. The MRA provides for reciprocal recognition of conformity assessment procedures for a subset of industries, effectively lowering fixed trade costs for affected industries trading with the EU. In other words, a Swiss exporter in an affected sector may since June 2002 certify conformity of her product in Switzerland, and is automatically granted equivalent certification for the EU market, without need to repeat the burdensome procedure in front of EU authorities. We are not the first to study the impact of the MRA on the Swiss economy Bühler and Burghardt (2013) and Bühler et al. (2011) look at the impact of the MRA on vertical integration of Swiss firms and firm growth respectively. Focusing on domestic Swiss firms, they use a difference-in-difference (DiD) approach to isolate the effect of the MRA on affected vs. non-affected firms and plants and find a positive impact of the MRA on firm size, as proxied by total employment, as well as propensity to integrate vertically. Our study is also related to Frazer and Van Biesebroeck (2010), who employ a very similar triple difference technique to isolate causal trade response to the implementation of the "African Growth and Opportunity Act (AGOA)", exploiting the inherent discrimination of the act in terms of both affected products and countries.

More generally, our analysis is grounded in the literature taking fixed exporting costs into consideration and decomposing an intensive and and extensive margin of trade. Baldwin et al. (2000) explicitly models MRAs as reduction in fixed costs, which should entail an increase in the number of firms entering the implementing foreign market. Similar predictions can be obtained from the Melitz (2003) model, as shown in Baldwin and Forslid (2010) and Baller (2007). We embed our analytical framework in the multi-country extension of Chaney (2008), which allows for product- and country specific fixed export costs and yields testable predictions on the extensive margin in a multi-country setting, which suits our bilateral trade
policy setting best.

Given this background, our study hence relates to a relatively small, but growing empirical literature that corroborates the theoretical link between fixed exports costs and the extensive margin, which has generally relied on various proxies to estimate the effect of fixed cost reductions. These proxies include familiarity with foreign markets (Andersson et al. (2007), and to some extent Lawless (2010)), trade facilitation measures such as the efficiency of ports (Feenstra and Ma (2014)), or the number of days to import (Persson (2013)), immigrant networks (Peri and Requena-Silvente (2010) for Spain, Vézina et al. (2012) for Switzerland Kang (2017) for Korea). Other studies have focused more specifically on policies that may have a direct bearing on fixed costs. Combining the WTO TBT notifications database with product-level COMTRADE data, Bao and Qiu (2012) associate TBT notifications with a reduction in the propensity to observe a positive trade flow on the product level. Using the SPS\textsuperscript{1} Information Management System (SPS IMS) of the WTO, which contains information on specific SPS concerns that countries raise, Crivelli and Gröschl (2015) find that the imposition of such measures is associated with a reduction in the probability to observe a positive export in affected products and Fontagné et al. (2015), using firm-level data, find that the imposition of such measures discourage firm participation in that market. Interestingly - and in line with theory - this effect is less pronounced for larger, more productive firms. Similar results are obtained by Reyes (2011), who exploits destination-specific variation in products standards in a setting that is similar to ours. In particular, he examines the response of US manufacturers to harmonization of European product standards to international norms in the electronics sector and also finds support for the extensive margin channel, noting that new entrants are typically smaller and less productive than firms which already serve the European market.

\textsuperscript{1} Sanitary and Phytosanitary measures
To our knowledge, our study is the first to combine the impact evaluation approach reminiscent of Frazer and Van Biesebroeck (2010) with the gravity-based, product- and destination-specific fixed cost approach since Chaney (2008), with a unique focus on the extensive margin. Using product level trade data and given our theoretical framework, our definition of the extensive margin relates to the product-destination level and precludes the analysis of multi-product firms.\(^2\). The goal of this study is to contribute to a better understanding of the effect of isolated integration policies, in this case an MRA, in a comparative statics setting from the perspective of the exporting country. From a policy point of view, the extensive margin is an important unit of analysis, as it relates to market opportunities for SMEs and economic diversification more generally. Using a difference-in-difference approach, we find that the reduction of fixed costs induced by the MRA has increased the average number of products exported from Switzerland into the EU, compared with non-EU countries. Augmenting the DiD approach with standard gravity variables - hence controlling for other measures of trade costs and demand - we find that this effect disappears in the aggregate, but is maintained when refining the analysis to a triple differences approach that distinguishes between affected and non-affected products. Our results are robust to the omission of products whose classification has undergone changes in the period under observation.

The paper proceeds as follows: The next section 1.2 provides an in depth background account of the treaty framework governing Swiss-EU trade relations with a particular focus on the MRA and the accompanying Bilaterals I package that entered into force in 2002. In section 1.3, we will introduce the theoretical framework that we will use to inform our analysis. We will describe our dataset and empirical strategy, as well as the results in section 3.3. Section 3.5 contains robustness checks and we will conclude in section 3.6.

\(^2\) This is really due to the choice of analysis. Conceptually, an extension of the mechanism towards multi-product firms is possible and yields similar predictions (eg. Arkolakis and Muendler (2010))
1.2 SWITZERLAND, THE EU AND THE MUTUAL RECOGNITION AGREEMENT

Despite its geographical location in the heart of Europe, Switzerland is not part of the European Union (EU). With respect to economic integration, Switzerland follows a path that is unique in comparison with its neighbors. While Switzerland is part of the European Free Trade Association (EFTA), it does not take part in the European Economic Area (EEA), which unites the three other members of EFTA - namely Iceland, Liechtenstein and Norway - and the EU into an Internal Market governed by the same basic rules. In order to keep up with European integration and maintain access to its most important market, Switzerland has relied on bilateral treaties to regulate its economic relations with the EU. Major benchmark agreements prior to the MRA were:

- The Free Trade Agreement of 1972
  This agreement focused on abolishing tariffs on industrial goods and the prohibition of quotas. It also contained disciplines on tariffs and export subsidies in agricultural products.

- The Insurance Agreement of 1989
  This agreement guarantees certain insurers the right of establishment in the other Party’s territory and national treatment provisions for branches and subsidiaries of the other Party’s insurers.

- Agreement on the carriage of goods of 1990
  This agreement consisted of customs facilitation measures and set rules on the controls and formalities applying to goods traffic between Switzerland and the European Union (EU), as well as cooperation at border posts.

Following the rejection of the Swiss voters to join the EEA in 1992, Switzerland sought to negotiate bilateral agreements with the EU in order to guarantee its firms non-discriminatory access to the EU market, even as an out-
sider of the deepening economic integration within the EU and the EEA. At the end of 1993, the EU agreed to start negotiations, but required the simultaneous negotiation, signing and entry into force of agreements in seven distinct areas, subject to the so-called "guillotine clause", which would also treat the exit of any one subagreement as the termination of the entire package. This package of seven distinct agreements was signed on 21.6.1999, entered into force on 1.6.2002 and became known as the Bilaterals I.

1.2.1 Bilateral Agreements I

We will briefly discuss the content of each of the six areas of the Bilaterals I package, evaluating their potential interactions with our study. We provide an in-depth discussion of our focus area, the Mutual Recognition Agreement (MRA), in the next subsection.

1. Freedom of movement of persons

The agreement confers upon citizens of both Parties the mutual right to freely choose their place of employment and residence within the territories of the contracting parties, conditional on certain criteria like a valid employment contract or proof of financial independence and health insurance. Traditional trade theories suggest that movement of labor can substitute for international trade and even modern trade theories can be used to explain interaction patterns (e.g. Ottaviano et al. (2013)). Migration can also have effects on trade through other channels, such as increasing total factor productivity (e.g. Ortega and Peri (2014)) or reducing information barriers with respect to the source country market (e.g. Felbermayr and Toubal (2012)). While we acknowledge the potentially confounding effect of this agreement in isolating the effects of the MRA, we emphasize that a number of so-called ‘accompanying measures’ were put in place in order to allow for a smooth transition phase. Important elements of these measures were necessity tests when hiring foreigners, restrictions on wages,
as well as quotas, which significantly limited the inflow of workers from EU countries until May 31, 2007, i.e. beyond the time frame under analysis in our study. In fact, Muller-Jentsch and Zurcher (2008) show that immigration from EU countries has displayed a sharp increase only in 2007, i.e. once these measures were fully removed. Moreover, the agreement is cross-sectoral, it does not restrict migration in terms of employer’s industrial activity or specific skill level and should therefore be immaterial to our triple differences strategy.

2. **Public Procurement**

The agreement on public procurement expands the area of application of the WTO Agreement on Government Procurement (GPA). While the expansion towards subnational procurement may indeed trigger additional demand for Swiss goods, the sectoral expansion concerns mainly services sectors such as construction, architecture and engineering, which are not reflected in our analysis. While reliable data on government procurement is scarce, an early survey conducted by the Swiss State Secretariat for Economic Affairs indicates that the benefits of a liberalized EU procurement market for Swiss companies are unclear: While acknowledging increased transparency, almost 80% found the process to have become more cumbersome and 49% found the agreement to be irrelevant (Balaster and Schupbach (2008)). With respect to the focus of this study, it is important to keep in mind that the thresholds for publicly advertising calls for tenders are relatively high (roughly 700,000 CHF for non-construction). As such, EU procurement markets are typically a target for large, productive firms. As we are interested in changes on the extensive margin, the potential confounding effect of this agreement should therefore not be of too much concern.

3. **Agriculture**

The main part of this agreement consists of the complete elimination of tariffs on cheese products, which has been undertaken in 2007, i.e.
after the period under consideration in our study. The second part
deals with the reduction of non-tariff measures and overlaps with the
provisions in the mutual recognition agreement, which we fully ac-
count for in defining the relevant agricultural sectors. In particular,
the agreement establishes the equivalence of product standards no-
tably with respect to phytosanitary quality certificates for fruit and
vegetables, as well as for all products of animal origin such as eggs
or honey.

4. **Research**
The agreement provides for the gradual integration of Switzerland
for full participation in the research framework programmes of the
European Union (EU). While potentially of exceptional relevance in
terms of strengthening Swiss innovative capacity and productivity,
these effects are of a rather long term nature and highly unlikely to
affect trade in the period under consideration.

5. **Civil Aviation**
The agreement regulates the access of Swiss airline companies to
Europe’s liberalised civil aviation market. A potential result of de-
creased transport costs could be picked up in our gravity estimates
and therefore bias our coefficients. How likely is this scenario? The
immediate impact of the agreement is unclear, as its entry into force
coincided with the grounding of the former national carrier Swis-
sair, by far the main international Swiss carrier. Three interim years
passed until former Swissair was taken over by Lufthansa in 2005,
during which time substantial efficiency gains seem unlikely. More-
ever, the liberalization of civil aviation was progressive and is still not
completed. Considering also that Switzerland already had very simi-
lar agreements on civil aviation in place with the single EU Member
States prior to the 2002, this indicates that substantial cost reductions
would not be likely to be picked up in the immediate aftermath of
entry into force. Even then, the airline industry being global by its
nature, it is unlikely that potential efficiency gains resulting from factors such as improved fleet capacity utilization should be confined to the markets from which they immediately result. Given geographical proximity, Switzerland exports only a small fraction of its exports into the EU by air. In 2014, the number for such exports into the EU was 10.6%, whereas it was 69.9% for exports outside the EU.\(^3\) Fourth, the potential price reductions are not industry-specific and could be taken advantage of - in principle - by all exporters.

6. **Land Transport**

This agreement’s primary purpose is to regulate transit through Switzerland, which is a crucial axis for European traffic, notably between the North and the South crossing the Alps. As such, the Agreement specifies levies the Swiss government collects to invest from foreign transport companies which it in turn invests in transport infrastructure. The agreement also stipulates steps that would progressively facilitate transport of goods and persons between the two jurisdictions after an initial transition and adaptation phase. The first novelty that could have a bearing on trade costs is the granting of "great cabotage" rights to Swiss transport providers across distinct EU Member States, which came into force in 2005, towards the end of our observation period. Again, a potential bias would be cross-sectoral as it does not \textit{a priori} favor certain industries or products over others.

### 1.2.2 The Mutual Recognition Agreement

**Genesis and content**

Following the rejection of joining the EEA by Swiss voters and faced with the prospect of losing market shares as the EU Internal Market was gradually expanded towards the other EFTA States, the Swiss Federal Council

has in 1993 initiated a review of existing national product standards and regulations, with the aim of determining and possibly adjusting their compatibility with EU law. At the same time, negotiations with the EU began. Entering into force in July 1996, the Federal Law on Technical Barriers to Trade\(^4\) mandated a systematic alignment of all Swiss technical regulations with those of the EU wherever possible and across all sectors. Its article 18.2 specifically provides for recognition of EU certificates of conformity undertaken by EU conformity assessment bodies (CAB), if i) the assessment procedure satisfies Swiss standards or ii) the foreign CAB possesses a qualification that is equivalent to those required in Switzerland. Unlike the EU, Switzerland does not require such provisions to be enshrined in an international treaty, which implies that Swiss import regulations on recognition of EU certificates have predated the recognition of its exports into the EU (Meier and Hertig (2008)). The broader set of bilateral agreements was signed by the Parties on 21. Juni 1999 and submitted to approval by popular vote on 21.5.2000, entering into force on 1.6.2002. The MRA provides for reciprocal recognition of conformity assessment procedures for only a subset of industries. In other words, a Swiss exporter in an affected sector may since June 2002 certify conformity of her product in Switzerland, and is automatically granted equivalent certification (the CE label) for the EU market, without need to repeat the burdensome procedure in front of EU authorities. As per the wording of the legal texts, the sectoral breadth of the MRA is listed in table (1.1).

**Properties of the MRA for empirical analysis**

- *Two dimensions of heterogeneity*
  - i) The MRA has entered into force between the EU and Switzerland. Its provisions are hence applicable to bilateral trade, whereas third countries are not directly affected. ii) The MRA specifies the sectors

\(^4\) SR 946.51.
Table 1.1: MRA Sectors

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<tr>
<td>1</td>
<td>Machinery</td>
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<td>2</td>
<td>Personal protective equipment</td>
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<td>3</td>
<td>Toys</td>
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<td>4</td>
<td>Medical devices</td>
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<td>5</td>
<td>Gas appliances and boilers</td>
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<tr>
<td>6</td>
<td>Pressure vessels</td>
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<td>7</td>
<td>Telecommunications terminal equipment</td>
</tr>
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<td>8</td>
<td>Equipment and protective systems</td>
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<td>9</td>
<td>Electrical equipment and electromagnetic compatibility</td>
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<tr>
<td>10</td>
<td>Construction plant and equipment</td>
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<tr>
<td>11</td>
<td>Measuring instruments and prepackages</td>
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<tr>
<td>12</td>
<td>Motor vehicles</td>
</tr>
<tr>
<td>13</td>
<td>Agricultural and forestry tractors</td>
</tr>
<tr>
<td>14</td>
<td>Good laboratory practice (GLP)</td>
</tr>
<tr>
<td>15</td>
<td>medicinal products GMP Inspection and Batch Certification</td>
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... to which it is applicable, which opens the possibility to test its effects against non-affected sectors.

- **The MRA is well-defined in time**
  Unlike large parts of the Bilaterals I described in 1.2.1, the provisions of the MRA entered into force on 1.6.2002, without phase-in period. Note that this is the case only for exports, as mentioned in 1.2.2, which warrants our focus on Swiss exports into the EU and not its imports from the EU.

- **The MRA as an isolated event with respect to trade costs between the EU and CH**
  During the period of observation (1998-2006), no other treaty or other punctual factor that could have a significant bearing on CH-EU trade relations has been introduced. The Bilateral II package came only in 2008, including the adhesion of Switzerland to the Schengen zone...
through the abolition of border controls. Recent studies demonstrate potential trade effects through costs channels of the abolition of border controls (e.g., Chen and Novy (2011), or Davis and Gift (2014) Felbermayr et al. (2016) for Schengen specifically).

- The MRA’s terms are largely exogenous with respect to pre-liberalization expectations

While any negotiation in international trade is likely to be influenced by lobbying from all sides, several indications point to a very limited extent of potential endogeneity.

i) The text of the agreements follows closely the EU template for similar agreements previously concluded between the EU and Australia (1998), Canada (1998), Israel (1999), Japan (2001), New Zealand (1998), and the US (1999).

ii) There are strong indications for Switzerland having been the junior partner in the negotiation of the agreement. First, the scope of covered sectors in the agreement is much narrower than Swiss initial ambitions, as set out in the Federal Law on Technical Barriers to Trade mentioned in 1.2.2, which aimed at interoperability of all regulations and standards. Second, given the drive towards European integration in the 1990s and the expansion of the EU Internal Market towards all other EFTA Members, Switzerland faced isolation and had no choice but to accept the EU’s "guillotine clause", stipulating that non-acceptance or termination of one treaty will result in non-acceptance or termination of all other treaties.

iii) Even if Swiss industry were able to significantly influence the sectoral breadth of the MRA, it was far from obvious that acceptance of the entire Bilateral I package would pass the popular vote. A very similar argument in favor of exogeneity of the Canada-US Free Trade

---

5 Note that border controls between Switzerland and the EU are prohibited only for persons. The Parties retain the right to inspect goods that cross the borders, because Switzerland has no customs union with the EU.
Agreement - which has been used extensively - has been put forward by Breinlich (2008) to explain its popularity in research on international trade. Recall also that in 1992 the Swiss constituency rejected joining the EEA, which this very series of treaties sought to redress. Notably the provisions on the free movement of persons has been a key bone of contention in the Swiss public debate.

- **The mechanisms of the MRA can be modelled in a neat way and yield testable predictions**

The mutual recognition of conformity assessment procedures and bodies implies that a product certified to be sold on the Swiss market may automatically be sold in the EU, without the need to undergo another conformity assessment procedure by a CAB in the EU, as used to be the case before the MRA. The MRA hence implies a reduction in the fixed cost of introducing a new product into the EU market, since the cost incurred does not depend on the quantity of the same product subsequently sold in the EU. Once the product has been tested and upon certification of conformity been awarded the right to bear the CE label, the quantity of product shipped into the EU market can be freely chosen and is not subject to further certification costs. The following section will formalize the way in which this reduction of fixed costs is expected to affect Swiss exports.

In general, the two-fold heterogeneity allows us to employ a triple difference estimator, which reduced the likelihood of potential endogeneity problems (see Besley and Case (2000)). To see this, suppose that we would perform the analysis only on the country level. Suppose further that we could not entirely rule out a confounding effect of other simultaneous developments, which we discussed above and which might have a bearing on a reduction in bilateral trade costs. A country-by-country difference-in-difference estimator may pick up this effects and erroneously ascribe it to the MRA. Likewise, focusing a DiD analysis solely on the product level may pick up effects that are specific to these products, but unrelated to
the MRA, such as productivity gains in these sectors or increased external demand. In either case, a triple difference approach helps us reduce the scope for picking up an omitted variable bias, as the variation across both dimensions nets out non-discriminatory confounding factors that may be present in each.

1.3 THE MODEL

The effect of the MRA can be placed into the general set-up of the Melitz model of intra-industry trade with heterogeneous firms (see Melitz (2003), which has been extended into a multi-country setting including the gravity structure by Chaney (2008). This section will describe the main elements of relevance and close with testable implications on the extensive margin of trade, defined on the country-product level as in Besedes and Prusa (2011).

1.3.1 Deriving the Equilibrium

The point of departure is a world of \(N\) countries and \(H + 1\) sectors, where we consider exports from the home country into export partner country \(j\) and hence drop the subscript for the exporting country. Sector 0 produces a homogeneous good, which is produced under constant returns to scale with one unit of labor, its price being set to one, such that it serves as the numeraire and wage \(w\) indicates productivity. The other \(H\) sectors are made up of a continuum of differentiated goods and are characterized by monopolistic competition and increasing returns. Within these sectors, each firm produced one distinct variety \(\omega\), which collapses the discussion between the number of firms and the number of varieties in each sectors
into the same unit.\footnote{Alternative modelling of the extensive margin takes the existence of multi-product firms explicitly into account (see eg. Arkolakis and Muendler (2010), Bernard et al. (2010), or Bernard et al. (2011)).} For each sector $h$ consumers in country $j$ have utility functions across product varieties $\omega$ and all countries of a Dixit-Stiglitz form:

$$U_j = \left( \int_\Omega q_j(\omega) \frac{\epsilon-1}{\epsilon} d\omega \right)^{\frac{\epsilon-1}{\epsilon}} \mu$$

(1.1)

where $\epsilon$ is the elasticity of substitution between varieties, $\mu$ is the sectoral spending share and $\Omega$ indicates the set of available varieties in the contemplated sector. Each firm in the differentiated goods sector draws a random unit productivity $a$. Exports from sector $h$ face variable, iceberg-type trade costs $\tau_j \geq 1$, implying that only a fraction $\frac{1}{\tau_j}$ of any differentiated good arrives in country $j$. Moreover, entry into foreign markets for differentiated goods $h$ is associated with fixed destination-specific costs $f_j$, which are also sector-specific.\footnote{For notational simplicity, and without loss of generality, we currently focus on a single sector $h$.} A firm exporting a differentiated good into country $j$ hence faces costs:

$$c_j(q) = \frac{w\tau_j}{a} q + f_j$$

(1.2)

The corresponding price an exporter with productivity $a$ will charge in country $j$ is:

$$p_j(a) = \frac{\epsilon}{\epsilon - 1} \frac{w\tau_j}{a}$$

(1.3)

Given income $Y_j$ and spending behavior of workers in destination country $j$, exports by our firm with productivity $a$ are given by:

$$x_j(a) = \mu Y_j \left( \frac{p_j(a)}{p_j} \right)^{1-\epsilon}$$

(1.4)
where \( P_j \) is price index over all varieties of the sector in country \( j \).

A firm only opts for exporting if it can incur the associated costs. If it decides to export, its profits are given by:

\[
\pi_j(a) = \left( p_j(a) - c_j(a) \right) q_j(a) - f_j
\]

\[
= \frac{\mu}{\epsilon} Y_j \left[ \left( \frac{\epsilon}{\epsilon - 1} \right) \left( \frac{w \tau_j}{a P_j} \right) \right]^{1-\epsilon} - f_j
\]

The zero-profit condition then implies a productivity threshold \( a_j \) from \( \pi_j(a_j) = 0 \), which denotes the productivity level of the least productive firm that is still able to export to country \( j \):

\[
\bar{a}_j = \left( \frac{\epsilon}{\mu} \right)^{\frac{1}{\epsilon - 1}} \left( \frac{\epsilon}{\epsilon - 1} \right) \left( \frac{f_j}{Y_j} \right)^{\frac{1}{\epsilon - 1}} \left( \frac{w \tau_j}{P_j} \right)
\]

So far, we have focused merely on one exporting country and treating aggregate prices as given. Pinning down wages through the homogeneous good sector and assuming the number of potential entrants as exogenously given, Chaney (2008) derives an equilibrium expression for the cutoff productivity levels for exports from country \( i \) to country \( j \) of the form:

\[
\bar{a}_{ij} = \lambda \left( \frac{\gamma}{\Phi_j} \right)^{\frac{1}{\gamma}} \left( \frac{w \tau_{ij}}{\Phi_j} \right)^{\frac{1}{\epsilon - 1}} f_{ij}^{-\gamma} f_{kj}^{-\left[\gamma / (\epsilon - 1) - 1\right]}
\]

where \( \lambda \) is a constant and \( Y \) denotes world output, and

\[
\Phi_j = \sum_{k=1}^{N} \left( \frac{Y_k}{Y} \right) \left( \frac{w_k \tau_{kj}}{\Phi_j} \right)^{-\gamma} f_{kj}^{-\left[\gamma / (\epsilon - 1) - 1\right]}
\]

The equilibrium expression of the cutoff value \( \bar{a}_{ij} \) is a function of fundamentals only: World output \( Y \), output of partner country \( Y_j \), variable
and fixed country- and industry-specific trade costs $\tau_{ij}$ and $f_{ij}$, as well as a measure of country $j$’s remoteness from the rest of the world $\Phi_j$, which is reminiscent of the "multilateral resistance term" introduced by Anderson and Van Wincoop (2003).

1.3.2 Comparative Statics and the Extensive Margin

Recall from equation (1.7) that the equilibrium productivity threshold is a function of fixed costs. More specifically Chaney (2008) model predicts a change in bilateral fixed costs in sector $h$ between countries $i$ and $j$ to be related to $\bar{\alpha}_{ij}$ in the following way:

$$\frac{\partial \bar{\alpha}_{ij}}{\partial f_{ij}} = \frac{1}{\varepsilon - 1} \times \frac{\bar{\alpha}_{ij}}{f_{ij}} > 0 \quad (1.9)$$

A reduction in industry-specific bilateral fixed costs will - ceteris paribus - result in a reduction of the productivity level of the least productive firm that is still able to export to country $j$, increasing the total amount of firms/-varieties exporting to country $j$ in sector $h$. All else equal, the implications for the MRA-induced reduction of fixed costs for Swiss exporters are hence twofold:

1. The total number of firms/varieties with destination EU should increase vis-a-vis other countries

2. The relative increase of firms/varieties in sectors affected by the MRA into the EU should increase more than this ratio into non-EU countries.

1.4 EMPIRICS

A neat implication for empirical analysis is the fact the predictions of section (1.3.2) do not necessarily require trade-related micro-data, which
is unavailable for Switzerland. Sufficiently disaggregated customs data on
the product-destination level should allow for enough detail for our pur-
poses.

This being said, analyzing bilateral trade with the EU bears significant
complications, as it relates to an economic entity whose trade rules apply
throughout all member states, but bilateral flows are recorded on the level
of Member States. Given the level of required disaggregation and comput-
ing limitations, we hence cannot use each member’s bilateral import data
with the rest of the world to account for potential third country effects. Con-
versely, focusing on the EU as a whole would not yield sufficient variation
to detect changes on the extensive margin, which is crucial to our identi-
fication strategy. We hence opt to analyze the issue from the perspective
of Swiss exports. Therefore, we explicitly acknowledge the investigation of
ceteris paribus effects only, given the comparative statics predictions of the
model, and noting that while the main derivations in (1.3.2) remain valid
and we will control for foreign demand \(Y_j\), we cannot account for changes
in trade relations between the EU and third countries.

1.4.1 Data

We obtain a very detailed dataset on yearly trade flows from the Swiss
Federal Customs Administration Swiss Federal Customs Administration
EZV (2006). This dataset is disaggregated at the 8-digit level, which, for
our purposes, makes it superior to other, more aggregated available trade
data, such as Comtrade (2007) or Gaulier and Zignago (2012) data, which
are available only on the 6-digit level. We restrict our sample to all current
OECD countries Switzerland exports to, as well as the BRICS countries and
four non-OECD EU countries (Malta, Cyprus, Latvia and Lithuania). We
exclude Belgium and Luxembourg from the analysis, as the Swiss Federal
Customs administration does not consistently consider them as one sin-
gle export destination throughout the period of observation. Our choice
of export destinations hence yields a total of 40 countries. The choice of countries is motivated by the need to have a relatively comparable set of countries, as the Swiss export portfolio decreases drastically with the number of trading partners and level of country development. Restricting the set of countries to the current sample hence proxies to some degree for similar country characteristics, as defined by OECD membership criteria and emerging market status. As such, this set of 17 non-EU countries will form our control group.

Table 1.2: Summary statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min.</th>
<th>Max.</th>
<th>N</th>
</tr>
</thead>
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<td>Year</td>
<td>2002</td>
<td>2.582</td>
<td>1998</td>
<td>2006</td>
<td>2918160</td>
</tr>
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<td>Country code</td>
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<td>128.878</td>
<td>111</td>
<td>509</td>
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<td>EU</td>
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<td>0</td>
<td>1</td>
<td>2918160</td>
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<td>1</td>
<td>2918160</td>
</tr>
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<td>MRA</td>
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<td>1</td>
<td>2918160</td>
</tr>
<tr>
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<td>0.3</td>
<td>0</td>
<td>1</td>
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</tr>
<tr>
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<td>0.357</td>
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<td>Pop</td>
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<td>252.9</td>
<td>0.274</td>
<td>1311.798</td>
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<td>8.163</td>
<td>16.396</td>
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<tr>
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<td>0</td>
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<td>0.307</td>
<td>0</td>
<td>1</td>
<td>2918160</td>
</tr>
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</table>

We hence construct a balanced panel of 40 countries $c$, spanning 9 years of observation $t$, from 1998 to 2006. This period comprises the EU enlargement of 2004, at which time a number of Eastern European States have become part of the MRA as well. Otherwise, this period has witnessed the

---

8 The countries are: Australia, Austria, Brazil, Canada, Chile, China, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, India, Ireland, Israel, Italy, Japan, Latvia, Lithuania, South Korea, Malta, Mexico, Netherlands, New Zealand, Norway, Poland, Portugal, Russian Federation, Slovakia, Slovenia, South Africa, Spain, Sweden, Turkey, U.S.A, United Kingdom
entry into force of only one additional trade agreement, namely the FTA with Chile that has entered into force end of 2004. The coverage of the MRA in terms of range of products has remained constant over this time. Our dataset contains all product-categories $j$ that have been exported at least once during the entire period of observation. This yields a total of 8106 tariff lines, which we define as different products in what follows. As such, we have a total of $8106 \times 40 \times 9 = 2,918,160$ observations. The summary characteristics of the main variables used in this paper are displayed in table (1.2).

1.4.2 Difference-In-Differences

The aggregate

We start by plotting the average number of products exported from Switzerland to each group (EU vs non EU) over time in figure 1.1. Inspection of this figure reveals a number of interesting facts. First, we can observe a parallel trend in the evolution of the number of exported products in both groups, which is crucial for our difference-in-differences estimation strategy. Second, if anything, the average effect of the MRA on the total amount of products exported seems to be limited. Third, Switzerland exports a much broader basket of products to EU countries, as opposed to our control group consisting of OECD and BRICS countries. This fact may actually bias against a potentially positive effect of the MRA, as the number of different product Switzerland is able to export is - given the limits imposed by the classification - not open ended.

We investigate the potential effect of the MRA by estimating a baseline model of the following form:

$$Y_{jct} = \gamma_0 + \gamma_1 EU_c + \gamma_2 Post_t + \gamma_3 (EU_c \times Post_t) + \gamma_4 X + \tau_i + \kappa_i + \varepsilon_{jct}$$

(1.10)

9 We control for this in our empirical strategy through the inclusion of an FTA dummy
Figure 1.1: Average number of products exported by destination

\( Y_{jct} \) is an indicator variable that takes on the value of 1 if product \( j \) is exported to country \( c \) at time \( t \). \( EU_c \) is a dummy variable that takes on the value 1 if country \( c \) is an EU member. \( Post_t \) is a dummy variable that takes on the value 1 for observations in years from the year of entry into force of the MRA. We want to identify the mean change in probability of observing a positive Swiss export to the EU before and after entry into force of the MRA, net of the mean change in probability of observing a positive export before and after the event to non-affected, i.e. non EU countries. The causal effect of interest is thus given by:

\[
\text{DiD} = E[Y_{jct} \mid EU = 1, Post = 1] - E[Y_{jct} \mid EU = 1, Post = 0] - (E[Y_{jct} \mid EU = 0, Post = 1] - E[Y_{jct} \mid EU = 0, Post = 0])
\]

(1.11)

\[
= ((\gamma_0 + \gamma_1 + \gamma_2 + \gamma_3) - (\gamma_0 + \gamma_1)) - ((\gamma_0 + \gamma_2) - (\gamma_0)) = \gamma_3
\]

We are essentially comparing four means, that is the mean difference in probability of observing a positive export to an EU partner country before
and after entry into force of the MRA, minus that same mean difference for countries that are not in the EU.

We pool the data and estimate (1.10) as a pooled cross section in a Linear Probability Model\textsuperscript{10}, using time dummies $\tau_t$. We further use sector-specific fixed effects $\kappa_i$ to account for sector-specific export tendencies that do not change over time, which proxies for heterogeneous spending shares $\mu_h$ in equation (1.1) that vary across sectors $h$. We define a sector at the 2-digit level of the 8-digit tariff line, yielding 96 different sectors.

Column (1a) of table (1.3) displays the results of an exploratory regression without any country-level covariates except for the DiD dummies. The statistically significant positive coefficient quantifies the graphical finding in figure (1.1). Accordingly, Switzerland has exported slightly more than 4% more products into EU States post MRA and relative to non-EU countries. More concretely, this number amounts to an average relative increase of $0.0424 \times 8106 \approx 347$ products.

Of course it would be premature to attribute this effect to the MRA without further investigation. Indeed, we need to get a better grasp of cross-country differences in order to isolate the MRA’s impact. The first set of controls flows directly from equation (??), which maintains that productivity cut-off is influenced by variable and fixed trade costs. We proxy for these in equation (1.10) by including a variable $\text{Contig}_c$, indicating whether Switzerland shares a common border with the export destination, a variable $\text{Comleg}_c$, indicating a similar legal system, a variable $\text{Distance}_c$ measuring the log of the geographic distance, $tdiff_c$ measuring time differences, whether a significant ethnic group shares a common language ($\text{Comlang}_c$), and whether the export destination is part of $GATT_{ct}$ or in a Free Trade Agreement with Switzerland ($RTA_{ct}$). All data except the geographic distance comes from Gaulier and Zignago (2012). We also include controls in order to account for country size, proxying for foreign demand, and add variables for a country’s area in log square kilometres ($\text{Area}_c$), the log of

\textsuperscript{10} using a Probit model yields very similar results, which, however, we do not report
Table 1.3: Difference-In-Differences

<table>
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<th></th>
<th>(1a)</th>
<th>(1b)</th>
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<th>(2b)</th>
<th>(3a)</th>
<th>(3b)</th>
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</thead>
<tbody>
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<td>0.0418***&lt;br&gt;(44.11)</td>
<td>0.00170&lt;br&gt;(1.67)</td>
<td>-0.00119&lt;br&gt;(-1.26)</td>
<td>0.00197***&lt;br&gt;(6.99)</td>
<td>0.00103***&lt;br&gt;(7.05)</td>
</tr>
<tr>
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<td>0.174***&lt;br&gt;(41.82)</td>
<td>0.133***&lt;br&gt;(22.99)</td>
<td>0.139***&lt;br&gt;(21.86)</td>
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<tr>
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<td>0.0167***&lt;br&gt;(7.12)</td>
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<td>0.0539***&lt;br&gt;(21.66)</td>
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<td>-0.0000215***&lt;br&gt;(-7.65)</td>
<td>-0.0000263&lt;br&gt;(-0.63)</td>
<td>-0.0000124&lt;br&gt;(-0.27)</td>
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<td>0.0684***&lt;br&gt;(111.83)</td>
<td>0.0698***&lt;br&gt;(79.60)</td>
<td>0.0726***&lt;br&gt;(77.61)</td>
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<tr>
<td><strong>Constant</strong></td>
<td>0.257***&lt;br&gt;(253.87)</td>
<td>0.273***&lt;br&gt;(251.15)</td>
<td>-0.265***&lt;br&gt;(-19.97)</td>
<td>-0.277***&lt;br&gt;(-18.21)</td>
<td></td>
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</tr>
<tr>
<td><strong>Time FE</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<td>Yes</td>
</tr>
<tr>
<td><strong>Sector FE</strong></td>
<td>Yes</td>
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<td>Yes</td>
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</tr>
<tr>
<td><strong>R²</strong></td>
<td>0.157</td>
<td>0.160</td>
<td>0.251</td>
<td>0.260</td>
<td>0.242</td>
<td>0.254</td>
</tr>
</tbody>
</table>

1 statistics in parentheses, S.E. clustered on country-product levels
2 p < 0.05, ** p < 0.01, *** p < 0.001
population \((P_{oc})\), and the log of GDP \((GDP_{oc})\), also from Gaulier and Zignago (2012) and completed with World Development Indicators data where needed.\(^{11}\)

Once corrected for these country characteristics, our coefficient \(\gamma_3\) loses significance in column (2a) of Table (1.3). In other words, it appears that conventional gravity variables are much better in explaining the evolution of the aggregate Swiss export portfolio than the MRA. A quick look at the control variables confirms that almost all of them bear the expected signs that have been established in the literature on gravity models.

**Affected sectors**

The legal text of the MRA does not precisely define the range of sectors under its coverage, neither in terms of classifications of economic activity, nor tariff lines. Moreover no official document describes how the MRA text translates into affected and non affected sectors. Nevertheless, inspection of the MRA text makes obvious that not all Swiss exports are affected by its conformity assessment facilitation.

Based on an unofficial attempt by SECO, the Swiss State Secretariat for Economic Affairs and earlier classification of sector correspondence undertaken by Loridan (2008) and Bühler et al (2011), as well as textual analysis of the legal documents, we manually define a list of potentially affected Swiss export industries in order to further exploit the variation stemming from the differential impact the MRA may have across sectors. Accordingly, our list of affected industries defines 3584 8-digit tariff lines as potentially affected by the MRA, which is roughly 44% of all products. For comparison, Bühler et al (2011) find that roughly 28% of all Swiss firms have been affected by the MRA. The higher share of affected products we find can be explained by the fact that we limit our analysis to only those products that are tradable, in that each product we include in our database has been exported by a Swiss exporter to at least one country in at least one of

\(^{11}\) GDP for Cyprus (years 2005 and 2006) as well as Malta (2006)
the years we observe. In other words, we do not include products that Switzerland has never been exporting and are hence beyond the realm of Swiss export capacities. Furthermore, we are restricted to trade in goods, whereas Bühler et al. (2011) classify firms across all economic sectors that may be affected.

We hence create a dummy variable $MRA_j$ that takes on the value of 1 if the product in question falls under the coverage of the MRA and 0 if not.

In order to get a first impression of the evolution of the Swiss export portfolio with respect to these goods, we proceed by calculating the difference $D_t$ in the average number of exports of these "MRA" goods across EU and non-EU destination countries:

$$D_t = \frac{1}{23} \sum_{c=EU} \sum_{j=MRA} Y_{cjt} - \frac{1}{17} \sum_{c \neq EU} \sum_{j=MRA} Y_{cjt}$$

Fixed market entry costs that Swiss exporters face prior to the MRA may vary across destinations for a variety of reasons that are beyond the scope of this paper. However, as we have noted earlier, Switzerland has not engaged in any significant trade agreement during the time of observation, except for a bilateral FTA with Chile. Our theoretical modeling of the likelihood to observe a positive export in MRA affected sectors hence implies that while average differences in propensities to export in affected products may exist, these should be constant prior to the entry into force of the agreement. It is only after the entry into force that we expect the propensity to observe positive exports in 'MRA" goods to rise in favor of EU destinations. Plotting the yearly average difference between EU and non-EU destination countries in figure (1.2), this is the exact pattern we find.

The positive difference confirms the conjecture informed by figure (1.1) that the number of affected products is higher for exports to the EU than elsewhere. Being located in the center of Europe, this finding is not surprising and the logic directly flows from 1.3 and the gravity literature. More interestingly, we observe that while the pre-2002 difference hovered around
120 products, this number shot up to 200 by 2006, which is - qualitatively - exactly what our theory based prediction would have implied. But is this difference again a simple product of gravity-informed country characteristics, as we witnessed over the whole range of the Swiss export mix? We investigate this issue by estimating equation (1.10) over the restricted sample $MRA_j = 1$, but using the full set of covariates. The results are displayed in column (3a) of table (1.3). In fact, the coefficient $\gamma_3$ is back up to a highly significant roughly 1% relative increase in exports of those products that are affected amounting to approximately 38 products. Controlling for country-level characteristics that do in part explain the observed differences in propensity to export, MRA partner countries have seen a relative increase of MRA-affected products compared to non-MRA countries. This result is very much in line with the theoretical predictions, reinforcing the notion of a reduction in fixed export costs having a positive impact on the extensive margin of trade in affected sectors.
1.4.3 \textit{Difference-In-Difference-In-Differences}

While figure (1.2) plots differences in affected exports between partner and non-partner countries, it cannot show whether the actual shares of affected products in the total export mix has also increased. This should be another \textit{ceteris paribus} consequence of the MRA that we would be able to pick up. In order to get a graphical impression of that possibility, we hence proceed by calculating the difference in average shares of MRA-products in total destination exports by group of partner countries as follows:

\[
DS_t = \frac{1}{23} \sum_{c=EU} \frac{\sum_{j=MRA} Y_{cjt}}{\sum_{j\neq MRA} Y_{cjt}} - \frac{1}{17} \sum_{c\neq EU} \frac{\sum_{j=MRA} Y_{cjt}}{\sum_{j\neq MRA} Y_{cjt}}
\]

We plot the results over time in figure (1.3). While the difference in absolute numbers is positive, the difference in shares is negative, indicating that the numerical importance of MRA goods in the export mix to non-EU countries is higher. However, we are interested in the evolution of these shares and inspection of figure (1.3) again reveals a very similar pattern: The difference in shares remained relatively constant before 2002, but consistently and substantially decreased thereafter to almost half of the pre-MRA difference (an increase on the negative y-axis).

The graph hence supports the conjecture that there has been a relative increase on yet another dimension of our comparison, namely the number of exported affected goods relative to the non-affected goods. With this additional variation stemming from the classification into affected and non-affected products, we can now refine our econometric analysis further. Our Triple-Differences equation now becomes:

\[
Y_{jct} = \gamma_0 + \beta_1 EU_c + \beta_2 MRA_j + \beta_3 Post_t + \delta_1 EU_c \times MRA_j + \delta_2 EU_c \times Post_t + \delta_3 Post_t \times MRA_j + \gamma_3 EU_c \times Post_t \times MRA_j + \gamma_4 X + \tau_t + \kappa_i + \varepsilon_{jct}
\]

\hfill (1.12)
We are now comparing six means instead of previously four, adding the dimension of mean growth in products that are covered by the MRA against those that are not covered as our additional "control group". The causal effect of interest is given by:

$$\hat{\gamma}_{DDD} = (E[Y|EU = 1, MRA = 1, Post = 1] - E[Y|EU = 1, MRA = 0, Post = 0])
- (E[Y|EU = 1, MRA = 0, Post = 1] - E[Y|EU = 1, MRA = 0, Post = 0])
- (E[Y|EU = 0, MRA = 1, Post = 1] - E[Y|EU = 0, MRA = 1, Post = 0])
- (E[Y|EU = 0, MRA = 0, Post = 1] - E[Y|EU = 0, MRA = 0, Post = 0])
= ((\beta_1 + \beta_2 + \beta_3 + \delta_1 + \delta_2 + \delta_3 + \gamma_3) - (\beta_1 + \beta_2 + \delta_1))
- ((\beta_1 + \beta_3 + \delta_2) - (\beta_1))
- ((\beta_2 + \beta_3 + \delta_3) - (\beta_2))
- (\beta_3)
= (\beta_3 + \delta_2 + \delta_3 + \gamma_3) - (\beta_3 + \delta_2) - (\beta_3 + \delta_3) - (\beta_3)
= \gamma_3
where \( MRA \) denotes the subset of affected industries. The DDD estimate starts with the time change in averages for affected industries in the EU and then nets out the change in means in non-affected industries, as well as the difference-in-differences for affected industries in the the rest of the world and the change in means for non-affected industries there. The idea is that this controls for two kinds of potentially confounding trends: 1) changes in exports in affected industries across EU and non-EU states that would have nothing to do with the MRA and 2) general changes affecting exports to the EU, possibly due to other policies or trends that affect all exports.

Column (1a) of Table (1.4) displays the results of estimating (1.12) first without controls. The coefficient \( \gamma_3 \) on \( EU_c \times Post_t \times MRA_j \) is positive and significant, largely confirming the intuition developed previously in the graphical analysis and DiD results. The results support the notion that Swiss exports at the extensive margin have experienced disproportionate relative growth in sectors and to countries affected by the MRA.

In column (2a), we display the results of a regression over the full set of covariates. We note that this time the level of significance and magnitude of the coefficient virtually remain unchanged, despite the inclusion of a number of country-level characteristics.

1.4.4 Economic Significance

The results in 1.4.3 are best conceptualized as being the result the difference between two DiD approaches: 1) Among EU destination, MRA affected goods are the treated group and non-affected goods are the control group and 2) the same among non-EU destinations. That is the MRA has induced a 2% increase in probability of exporting an MRA product vis-a-vis non-MRA products, comparing their differential growth into EU and non-EU countries. These results are highly similar to Frazer and Van Biesebroeck (2010), who find the same triple difference impact of AGOA
Table 1.4: Triple Differences

<table>
<thead>
<tr>
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<th>(1a)</th>
<th>(1b)</th>
<th>(2a)</th>
<th>(2b)</th>
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<tr>
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<td>0.0222***</td>
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<td></td>
<td>(9.61)</td>
<td>(11.67)</td>
<td>(9.73)</td>
<td>(11.87)</td>
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<td>(7.14)</td>
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<td>0.0197***</td>
<td>(-0.0356***)</td>
<td>(-0.0376***)</td>
</tr>
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<td>(7.00)</td>
<td>(7.14)</td>
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<td>-0.0376**</td>
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<tr>
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<td>(21.71)</td>
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<td>(2.16)</td>
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<td>Sector FE</td>
<td>Yes</td>
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<td>Yes</td>
<td>Yes</td>
</tr>
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<td>2610360</td>
<td>2918160</td>
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<tr>
<td>$R^2$</td>
<td>0.159</td>
<td>0.162</td>
<td>0.252</td>
<td>0.262</td>
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</table>

* statistics in parentheses, S.E. clustered on country-product levels

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
on the extensive margin of eligible products from eligible African countries into the US to be 1% for goods entering through the General System of Preferences and 3% for goods entering through the apparel provisions. The similar impact of both AGOA and the MRA on the extensive margin is striking, given their different natures. While the MRA has an exclusive effect via the reduction of fixed costs of double certification (recall that product standards were already in the process of harmonization since the law of 1996 described in 1.2.2), AGOA captures a mix of both significant variable and fixed trade cost reductions (tariff and quota free market access for certain African exporters). This result is rationalizable by the results of studies that find the reduction of variable costs to be of little significance to the extensive margin, such as Debaere and Mostashari (2010) or Reyes (2011). Indeed, Frazer and Van Biesebroeck (2010) find much larger impacts of AGOA on the intensive margin of trade. Another way of looking at this question is to compare the coefficient on the interaction term with the other gravity coefficients in table (1.4). The importance of the MRA on the extensive margin is superior as compared to the average RTA, which typically deals with variable trade costs. The comparison with the distance term is less straightforward. While it is usually included primarily as a proxy for variable trade costs, it often has an important effect on the extensive margin and may pick up other types of trade costs (Eaton et al. (2004), Bernard et al. (2007), Hillberry and Hummels (2008), Lawless (2010), Crozet and Koenig (2010)).

12 Estimating equation (1.12) only over its gravity variables, we find a negative distance coefficient of -0.045 (unreported), which is highly comparable to the findings of above-mentioned studies. This coefficient is reduced to -0.036 in table (1.4). If we consider the coefficient on our triple interaction term (0.019), our analysis suggests that the MRA has reduced the negative effect of distance on the probability to export.

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12 A recent paper attempts a cleaner dissection of the different costs associated with distance in gravity models, see Besedeš and Cole (2017)
The most important implication in terms of economic significance is perhaps the scope for strategic trade policy, through the effect of changing the composition of trade and hence creating export markets for products whose production has desirable properties for the exporting country in the sense of Hausmann et al. (2007). However, growth in the extensive margin is not necessarily related to subsequent export growth, as documented in Besedeš and Prusa (2011). In order to better assess this possibility, we would need to augment the analysis jointly with more information on the characteristics of those products and the volumes traded, the underlying production structure, firm survival and their contribution to employment, just to name a few. Within the scope of this paper, we cannot reasonably make such assessments, but they could be promising avenues for further research, notably in countries with more narrow export portfolios than Switzerland.

1.5 Robustness

Not all tariff lines have remained valid throughout the period under observation. In fact, revisions have been made almost on a yearly basis, with the most substantial recoding having taken place precisely in 2002. Unfortunately, there is not always a one-to-one correspondence between old and new tariff lines, which makes it impossible to track single products at this level of disaggregation throughout the observed time period. For example, some discontinued tariff lines have been split into several new ones and/or merged with existing ones. UN COMTRADE provides time consistent HS tariff codes, which, however are available only on the 6-digit level. Since the identification strategy of this paper relies heavily on the degree of disaggregation, we cannot make use of this dataset. While the problem of changes in the tariff nomenclature is certainly a shortcoming of our dataset, we have reason to believe that it does not affect our results substantially. The revisions are undertaken in concerted international ef-
fort, which lends credence to the assumption that it may be regarded as an exogenous event with only random impact on particular product mixes exported to different countries. However, in order to investigate a potential confounding issue, we identify all tariff lines that have been modified at some point throughout the nine years of observation and calculate their actual shares in the total number of Swiss exports by country and per year. Any systematic differences in these shares across both groups may have potential bearing on the validity of our results. Inspecting the plot in figure (1.4), we fail to visually detect any systematic differences in shares of those modified tariff lines in total export between EU and non-EU countries. For both set of destination regions, the share is relatively low and the evolution over time follows a similar pattern. In fact, performing a two-sample t-test over the means of each group, we fail to reject the null hypothesis of equality of means, with a corresponding p-value of 0.169 on the alternative hypothesis of inequality of means.
Nevertheless, we perform robustness checks on all our econometric results. Since there is no way to track products across reclassification periods, we simply delete all tariff lines that have undergone at least one change between 1998 and 2006 from our set of products, effectively reducing the total number of contemplated tariff lines from 8106 to 7251. Columns (b) of tables (1.3) and (1.4) replicate the regressions of the respective (a) columns in these tables, but over the reduced sample of time-consistent tariff lines. The coefficients in our difference-in-differences regressions remain remarkably similar and tend to reinforce our findings on obtained over the entire set of products. Only in column (2) of table (1.3) does the sign of the coefficient change. However, it is very small and statistically insignificant in both regressions, with and without modified tariff lines. Omitting the modified tariff lines in the triple differences regressions displayed in table (1.4) yields more precision and a slightly higher coefficient in both specifications (1) and (2).
1.6 CONCLUSION

This paper has analyzed the effect of the Mutual Recognition Agreement (MRA) of 2002 between Switzerland and the EU on the extensive margin of Swiss exports and thereby contributes to the literature on impact analysis of non-tariff measures. Embedding the theoretical setup in the heterogeneous firms in international trade literature, we model the MRA as a reduction in fixed exporting costs for firms producing affected goods destined to the EU market and transpose the firm-level implications on the product level, yielding predicted increases on the extensive margin of bilateral trade with the EU in affected sectors. Using a difference-in-difference approach, we find that the reduction of fixed costs induced by the MRA has increased the average number of products exported from Switzerland into the EU, compared with non-EU countries. Controlling for standard gravity variables that have been found to be strong determinants of bilateral trade and proxy in part for other types of fixed exporting costs, we find that this effect disappears in the aggregate, but is maintained when focusing only on affected products. Refining the analysis to a triple differences approach, we find that the data fit the theoretical predictions well in that the MRA has induced a disproportionate increase in exports of affected products into EU countries relative to non-affected products and non-EU countries. Our results are robust to the omission of products whose classification has undergone changes in the period under observation. Our results suggest that the MRA has been an effective tool for Swiss trade relations with the EU, as it has promoted a diversification of export opportunities through facilitated market access that Swiss firms have been able to seize.
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Blind, K., Mangelsdorf, A., and Wilson, J. S. (2013). Mutual recognition of accreditation: Does it matter to trade? evidence from the food, beverage,
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THE EFFECTS OF EXPORTING ON LABOR PRODUCTIVITY: EVIDENCE FROM GERMAN FIRMS

ABSTRACT

We revisit the "self-selection vs. learning-by-exporting (LBE)" debate with new evidence on a large panel of German firms of all economic sectors up to the 3-digit NACE level, between 1993-2014, and shed new light on the channels that foster export-induced productivity gains. In line with previous results, we find substantial pre-export differences in productivity between future exporters and domestic firms. Nevertheless, these pre-export differences remain constant over time and we find strong evidence against a conscious self-selection effect, in which firms would actively engage in increasing their productivity in temporal proximity to starting to export. In contrast, we find support for the learning-by-exporting hypothesis in manufacturing but less for the services sector, but strong evidence in favor of the hypothesis when considering continuing exporters across both sectors. We explain the different sectoral performances with significant differences in access to foreign markets, which is substantially lower and more concentrated within few firms in services. Furthermore, we show that across sectors, the size of the LBE effect depends on the level of within-sector competition. In line with basic microeconomic theory, productivity gains are higher for entrants into exporting, which operate in relatively uncompetitive domestic sectors, pointing to an important competitiveness channel for productivity gains. Our results suggest that the services sector offers the largest scope for productivity gains through trade policies aiming at facilitating market access.
2.1 **INTRODUCTION**

In the workhorse theory of international trade as developed by Melitz (2003), firms are endowed with different productivity draws, which are predetermined and unchanging over time. Only those firms obtaining a productivity draw above the threshold for exporting will enter foreign markets. Indeed, there is widespread empirical backing for this prediction that firms engaging in exports are on average more productive than their purely domestically operating counterparts (see e.g. Bernard et al. (2012) for a recent literature review).

In reality, of course, firm productivity levels may be endogenous to firm decisions and may hence also change over time. Similarly, entry into and exit from exporting are recurring features of individual firms. Over recent years, there has been increased interest in better grasping the direction of causality in the strong correlation between productivity and exporting.

Two hypotheses are generally put forward to explain the mechanism underlying the "black box" of higher observed productivity in exporting firms: Self-Selection and Learning-By-Exporting (LBE).

*Self-selection* into exporting implies that firms with higher productivity "self-select" into exporting, as their productivity edge allows them to amortize the higher costs of serving foreign markets. The self-selection hypothesis hence implies that firms which become exporters are simply more productive to start with. There is a broad consensus in the empirical literature as reviewed in Wagner (2007), Greenaway and Kneller (2007a), and Bernard et al. (2012) to this effect, confirming substantial differences in firm-level productivity between domestically operating firms and future exporters prior to their entry into exporting.
The Learning-By-Exporting (LBE) hypothesis stipulates that firms increase their productivity as a consequence of exporting. Its early formulations can be traced back to endogenous growth models, such as Grossman and Helpman (1993), who point to technology diffusion through participation in international markets, which may enhance within-firm productivity. Demand side driven exploitation of economies of scale was stressed as an important productivity-enhancing factor in traditional export-led growth hypotheses (eg Kaldor (1970)). Much of the early arguments as made by Pack (1992) or Westphal (2002), however, were based on case studies of the rapidly industrializing Asian Tiger countries. As such, LBE hypotheses often emphasize a variety of mechanisms such as learning from foreign markets in terms of buyer-seller relationships, and increased competition with foreign suppliers, or adapting and improving product quality to suit foreign preferences. While empirical studies generally remain agnostic about the exact mechanism underlying LBE, identification of the effect rests on the assumption that the productivity effect of a firm’s international activity must - by definition - be specific to entering foreign markets, entailing activities and knowledge that non-exporters do not possess. The evidence for this effect so far is rather sparse, some examples include Hosono et al. (2015), Fernandes and Isgut (2015), Manjóon et al. (2013), Lileeva and Trefler (2010), Bigsten and Gebreeyesus (2009), De Loecker (2007), and Van Biesebroeck (2005), Girma et al. (2004).

A number of recent theoretical developments are consistent with certain aspects of alleged mechanics of the latter effect. Emerging literature on multi-product firms points to adjustments in product mixes as a result of increased competition in export markets, which induces firms to focus on their core competencies and adjust their product offer accordingly, resulting in firm-level productivity gains (eg. Mayer et al. (2014), or Bernard et al. (2010)). Bustos (2011) models technology adoption jointly with entry in to exporting and finds empirical evidence for trade liberalization induced innovation in both new and existing exporters from Argentina. Importantly,
this mechanism seems to hold also in advanced economies: Lileeva and Trefler (2010) find evidence for similar predictions for Canadian exporters following tariff reductions in the US. Verhoogen (2008) associates the decision to export with a joint decision of product quality upgrading to serve consumer preferences in the foreign market. All these papers underline a specific aspect that may help explaining the observed correlation between exporting and productivity. However, the theoretical basis for motivating either effect is often problematic, as the models of heterogeneous firms are generally static. As such, the specific predictions they make are immediately associated with entry into exporting and complicate empirical work that seeks to disentangle pre- and post exporting firm performance. As noted by Tybout (2003), identification of the immediate link between productivity at the time of entry into exporting is often problematic, as the econometrician usually does not have all the necessary information, especially around the time of entry into exporting. In other words, the decision to enter exporting may matter more than the actual entry into exporting. There are two main strategies that have been adopted to remedy this issue. First, depending on the availability of reasonable instruments, the treatment variable (entry into exporting) can be instrumented by a third variable that correlates with export, but not with productivity. Such an approach has been used with a broad variety of context-specific instruments, for example in Van Biesebroeck (2005) (Ethnicity of firm owner and state ownership, Lileeva and Trefler (2010) (tariff cuts), Verhoogen (2008) (Peso devaluation), and Bustos (2011) (lagged tariffs). A second approach that has been used to identify the effect of exporting on productivity is motivated by the fact that the counterfactual productivity trajectory of exporters - had they not started exporting - cannot be observed, even though that counterfactual should ideally be the benchmark against which one wants to test potential productivity effects. During the latter 2000s, the trade literature started borrowing from techniques developed in the field of labour economics to construct a proxy for this counterfactual, essentially following the approach developed by Heckman et al. (1997). The approach consists
of creating control groups using matching techniques based on observable characteristics. This is the approach followed by Girma et al. (2004) Greenaway and Kneller (2007b), De Loecker (2007) and Bigsten and Gebreeyesus (2009), for example. Even though matching on observables does not solve the potential bias introduced by omitting variables that are unobserved to the econometrician, the approach may help reduce the bias under the assumption that firms that tend to be similar in observables should also be similar in unobservables.

Other recent contributions allow for more explicit post-entry "learning" effects in dynamic setting. Eaton et al. (2009) develop a model of firm-level exporting behavior that takes account of search and learning processes in foreign markets by allowing learning from those markets and its competitors there to identify potential buyers. Freund and Pierola (2010) introduce uncertainty and sunk cost associated with the development of export-market specific products to explain survival patterns of new entrants. Aw et al. (2011) model endogenous R&D decisions jointly with exporting, which can explain post-entry productivity growth of exporters. Albornoz et al. (2012) develop a model which assumes uncertainty about firms’ general ability to earn profits abroad, which can be resolved only through trial-and-error experience in foreign markets. Similarly, Timoshenko (2015) develops a model in which learning about foreign demand accounts for product switching in foreign markets. Chaney (2014) develops a model that stresses the importance of buyer-seller networks and derives predictions about history-dependent export expansion induced by the reduction of search frictions through participation in foreign markets.

Conceding important data limitations as contextualized in section 2.2, our study uses a robust set of specifications to investigate the LBE effect in Germany, using data on firms of all economic sectors. As noted by (Wagner (2012), p.23), while "we have evidence on the links between international trade and productivity in manufacturing firms from a large number of em-
Empirical studies published during the past 15 years, comparable information for firms from services industries is scarce and of a recent vintage. General comparability of firm characteristics in the context of international trade in goods and services was first confirmed by Breinlich and Criscuolo (2011) on a large sample of UK firms. Vogel and Wagner (2011) find a statistically significant exporter premium for firms in German business services sectors (NACE 72, 73, and 74) between 2003 and 2007. However, this premium appears to be driven by outliers and becomes insignificant once they control for those in their regression. For the same time period, sectors and comparing German data with available data from France and the UK, Temouri et al. (2013) find no evidence for LBE for various measures of firm performance. Using a very comprehensive dataset on Danish firms in services and manufacturing, Malchow-Møller et al. (2015) are able to disentangle services and goods traders and investigate the respective links with long term (2002-2008) productivity growth. Their findings suggest that firms that have started exporting goods in this period have experienced higher average productivity growth than firms that have never exported in this period. Having started to export services is also associated with increases in productivity growth, but less so and only for firms in the services sector.

Our study contributes to the literature on the productivity effects of exporting by proposing an unprecedented look at productivity developments in temporal proximity of each firm's first entry into exporting, across all economic sectors. To this end, we are drawing on the methodology developed by Autor (2003), employing it for the first time to analyze the link between exporting and productivity. Our analysis also relates to studies that focus on the interplay between sectoral competition and productivity, such as Greenaway and Kneller (2007b), Mayer et al. (2014), and more generally Aghion et al. (2015). We are using a large panel of German firms spanning the period from 1993-2014, exploiting the panel structure to identify a causal effect and disaggregating our analysis up to the 3-digit NACE level, hence comprising both exports in the manufacturing and services sector. In
line with previous results, we find substantial pre-export differences in productivity between future exporters and domestic firms, across all sectors, but indications for less important differences in the services sector. Nevertheless, these differences remain constant over time and we find strong evidence against a conscious self-selection effect, in which firms would actively engage in increasing their productivity in temporal proximity to starting to export. In contrast, we find strong support for the LBE hypothesis in both the manufacturing and the services sector, as average productivity rises after initial entry into exporting, regardless of whether the export status is maintained in subsequent years or not. However, the effect is stronger in manufacturing firms than in services firms. The former exhibit increasing yearly productivity growth rates even more than two years after exporting, while the productivity growth rates of the latter group decrease (albeit remaining above pre-export averages). We also find increasing productivity rates for continuing exporters. We explain the different performances of the manufacturing and services sector with significant differences in foreign market access and propensities to export and are able to show that across sectors, the size of the LBE effect depends on the level of domestic within-sector competition.

Section 2.2 describes the dataset we use in detail and discusses advantages and shortcomings in the context of recent advances in the literature. It also contains discussions of methodological issues and the construction of variables. Section 2.3 contains a step-by-step approach towards our main analysis in 2.3.5. We then turn to three different robustness checks in section 3.5, that address several shortcomings of our analysis from a different methodological angle. We conclude in section 3.6.
2.2 DATA

We use confidential, representative German establishment-level survey data\textsuperscript{13}, which is managed by and kindly provided through the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) (Ellguth et al. (2014) and Fischer et al. (2009)).\textsuperscript{14} A firm-level unique identifier allows us to observe firms over time and we hence link each wave of the survey (roughly 14,000 yearly responses) over time to obtain a panel on key firm characteristics for the period 1993-2014. As responses are not always complete, firms enter and exit the survey, we obtain a very large unbalanced panel with key observations on total turnover, the share of foreign sales in total sales, input volumes, average wages, employment and investment.

The dataset has been extensively used for German labor market research, but surprisingly little in trade. As any researcher who is entitled to use the data is contractually obliged to register his publications in a database managed by the Institute for Employment Research, we can easily verify that this paper is the first of its kind.\textsuperscript{15}

\textit{Advantages of using the IAB dataset in the LBE context}

Our dataset offers a unique opportunity to examine firm responses to export participation for firms across all sectors in Germany. The examination of this particular dataset has two major advantages:

\textsuperscript{13} For ease of exposition, we will henceforth refer to establishments as firms.
\textsuperscript{14} Data access was provided remotely within project fdz1103 via JoSuA at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB).
\textsuperscript{15} In fact, Vogel (2011) does examine a very similar question, but bases his analysis primarily on data from the German Federal Statistical Office, and the data from the IAB is used only for robustness checks. His analysis differs in methodology, is narrower in scope (business services) and uses only years 2000-2005.
First, as outlined in the literature review above, we can jointly examine firm responses across the manufacturing and the services sector, which has not been done before for Germany. Those studies that do exist on Germany have relied on sectoral data, small samples and short time spans. In contrast, our dataset is representative of the entire German economy and covers the entire period after German reunification. In comparison with other datasets on German firms, the IAB panel stands out as the most comprehensive one. The research data center of the German Federal Statistical Office maintains a similar dataset (the "AFiD-Panel Strukturerhebung im Dienstleistungsbereich" (FDZ (2015))), but it is restricted in scope (only certain services industries), time (2003-2010, with a methodological break in 2007/2008) and includes only firms with a turnover of more than 250,000 Euros per year. The German Federal Statistical Office also maintains tax records for the universe of German firms, which can be accessed for research purposes. While that dataset is perhaps the most accurate of all, it only records exports of goods, as services exports are not tax-exempt. In contrast, the German Bundesbank maintains a very detailed record of all international services transactions of all German firms, tracking detailed industry affiliation, as well as type of service transaction and destination country over time. However, no other firm level characteristics are provided and the law prohibits matching that data with either datasets from the IAB or the German Federal Statistics Office.

The IAB panel offers a consistent set of key outcome variables over time that we can observe across a large panel of firms, without methodological break that could compromise comparability. The dataset merges survey data with administrative records on the classification of economic activity and includes a number of further measures to ensure high data quality for scientific purposes, as described later in this section.

Second, we can add to a better understanding of the under-researched case of Germany, and in doing so contribute to the literature on LBE in
advanced countries more generally. What makes the analysis of LBE in the German context most interesting is that the German economy is traditionally export-intensive and technologically advanced. As pointed out above in the literature review, the early studies on LBE have been largely devoted to the analysis of developing countries, both conceptually and empirically. Nevertheless, the question of whether access to foreign markets improves firms’ performance is equally important for developed economies and therefore highly relevant. The underlying hypothesis has often been one of “technological catch-up” through access to foreign markets, which would seem to apply less to developed economies such as Germany. However, there are at least two reasons why potential LBE effects should not be confined to developing countries only:

(i) Substantial variation of firm characteristics within countries and even industries have been the *raison d’être* for the emergence of the heterogeneous firms literature. Modern trade literature acknowledges that the departure from representative firms models has been a crucial element for the better understanding of economic realities. The literature on multi-product firms takes this a step further and allows for variation in productivity even across products within the same firm. This stylized pattern suggests unequal access to technology across firms regardless of the aggregate economy’s stage of industrialization and has led researchers to investigate individual firm responses to trade shocks. As such, the traditional argument for LBE effects can be transposed to the firm level and be equally applied to industrialized economies such as Germany. Indeed, evidence for such export-induced productivity gains and greater technology adoption has been found in the case of Canadian exporters into the US (Lileeva and Trefler (2010)).

(ii) There are a variety of mechanisms underlying the LBE hypothesis, as reviewed in section (2.1), which are not necessarily specific to developing country exporters. For instance, Mayer et al. (2014) stress prod-
uct adjustment and competitiveness channels through which exporting may result in productivity gains and find evidence for this channel in French exporting firms. Some dynamic "learning" models reviewed in section (2.1) identify mechanisms that relate exporters’ performances to uncertainty about foreign markets and adjustments to foreign preferences. While their predictions are generally tested against data from developing country exporters, there is no *a priori* reason why they should not hold for developed economies such as Germany, as long as export markets differ from the home market. A Colombian exporter to the US arguably faces greater adjustment than a German exporter, yet the latter will still be learning to survive and thrive in a foreign market as well. Ultimately, the question is an empirical one and comparing domestic producers with exporters of the same country should give us a clearer idea of the existence and the magnitude of these effects. Our dataset hence allows us to investigate this issue and contribute to the discussion of whether LBE effects can be detected in developed economies or not.

*Caveats of using the IAB dataset in the LBE context*

The advantages of our dataset must be discussed in the light of certain caveats that working with German data entails.

Our analysis will not focus on any particular mechanism that may underpin observed LBE effects. Rather, we aim at establishing a general causal link between entry into exporting and productivity, regardless of product-destination characteristics. This is simply because we cannot distinguish between a firm’s products and their characteristics, as well as its export destinations. More generally, we cannot distinguish between services and goods trade, such as eg in Malchow-Møller et al. (2015). This can be both an advantage and a disadvantage. On the one side, for analytical purposes it would be illuminating to have a better grasp on the type of export that a certain firm in a certain sector is associated with. For instance, we know
that services trade, goods trade, the manufacturing sector, and the services sector are closely intertwined. For Germany, we know that manufacturing firms account for almost 25% of services exports (Kelle and Kleinert (2010)). Conversely, 14% of services firms in Denmark appear to be exporting goods (Malchow-Møller et al. (2015)). On the other hand, it is increasingly complex to disentangle goods and services in general, as manufacturing firms both buy and produce more services in-house than before, but also sell and export more services than before (Lodefalk (2013)). Indeed, it appears that the services content of international trade in goods appears to have been systematically underestimated until recently (Cernat and Kutlina-Dimitrova (2014)). Our data hence take a rather agnostic approach toward the exact type of international transaction, but the fact that our panel is on an establishment level may therefore actually add confidence to associating sectoral exports with the corresponding type of export.

We do not have information on imports and hence cannot account for potential import-induced productivity effects that can result from increased import competition or greater availability of intermediate inputs (see eg. Goldberg et al. (2009), Amiti and Khandelwal (2013) and Halpern et al. (2015) for evidence on these channels). We are less concerned about this omission for two main reasons: 1) we do not exploit a specific change in the trade environment to identify the export decision and its productivity effect and 2) we rescale the time variable as described in 2.2.2, which would disperse any punctual change in the trade environment across different points in the time dimension we generate specifically for each firm we observe.

Lacking information on a firm’s capital stock, we cannot estimate traditional measures of total factor productivity (TFP). We discuss this issue and how we deal with it in greater length in section 2.2.4. At any rate, De Loecker (2011) argues that productivity outcomes need to be analyzed together with market power and prices, implying empirical studies need fur-
ther information on firms’ cost structures and markups. Questioning the large empirical literature that has estimated productivity based on proxying output with sales and exploited trade liberalization periods to identify changes in the trade environment, De Loecker (2011) argues that the relationship between measured productivity and trade liberalization may simply occur through the liberalization’s impact on prices and demand, implying that the impact on actual productivity cannot be identified. In order to address this bias, De Loecker et al. (2016) estimate a quantity-based production function using data that contain the prices and quantities of firms’ products over time, highlighting the need to additionally account for the allocation of inputs and their prices into the product mix of multi-product firms. Lacking information on quantities of firm inputs and outputs, we are hence not able to construct productivity measures based on quantities, products and their inputs. As such, our study relates most closely to traditional studies measuring revenue-based proxies of productivity without distinguishing between single- and multi-product firms. We are aware of this caveat, but aim to refine our results with a joint analysis on sectoral competitiveness after estimating our baseline model. We also seek support for the validity of our productivity measure by checking whether new exporters simultaneously increase their domestic sales, analogously to Lileeva and Trefler (2010), in our robustness checks.

An inherent weakness of survey data as we use it is that it relies on reported data and may hence display inaccuracies in comparison with administrative data. Such inaccuracies may persist despite substantial efforts undertaken by the IAB to ensure high quality of the data. For example, more than 70% of interviews are conducted face-to-face, by highly qualified interviewers, interviewing a member of the executive board in 47.8% of all cases and a head of department in 16.7% (Ellguth et al. (2014)).

Another inherent problem of survey data is that firms drop in and out of our sample for reasons we do not observe. We do not know whether
the firm has ceased to exist, or whether it simply discontinued answering the survey. By design, the survey is organized in such a way as to put significant resources into getting the same establishments to respond each year. The success of these efforts is evident in the large number of observations listed in table (2.2), listing—by construction—only observations on firms that appear at least in two consecutive years: As we want to observe firms over time, we delete all single observations from the sample. Given our topical focus, it is hence important to mention that we do not observe whether a firm has been exporting before it has been included in the survey. This shortcoming may potentially create a pro-LBE bias, as a firm that enters the survey at a time where it does (coincidentally) not export may tend to re-export during the period of observation and display higher productivity than a firm that has never been exporting before. However, we neither observe whether a firm will start exporting after having opted out of the survey (for reasons we do not observe), creating a potential bias in the opposite direction. Moreover, should cessation of activities be the prime reason for dropping out of the sample, we would expect declining productivity towards the end of each firm’s observation period. Arguably, declining productivity is an important factor leading to firm death. In light of the dynamic nature of our methodological setup, this bias would go against the effect we are seeking to uncover, as evidence for LBE would show in an increasing productivity trajectory.

At any rate, the selection of establishments into the sample follows explicit stratification schemes that are based solely on establishment size, sector of economic activity and geographical location (Ellguth et al. (2014)). Therefore, any establishment opting out of the sample will be replaced by another establishment that is highly similar along those three stratification variables only. Since the design of the IAB survey does in no way pay attention to export status when selecting the firms in the sample, we assume that the existence of either potential bias should not be overstated, as whatever bias that may exist would occur in both exporting and non-exporting
plants, and hence both in the treatment, as well as in the control group. The absence of a systematic bias between both should therefore not significantly affect our analysis. In addition, in our empirical strategy, we exploit differences across groups and time in within-firm variation, in order to isolate the net effect of exporting, correcting for any potential time invariant biases on the firm level (such as inherent productivity differences). Moreover, we define our main measure of switching into exporting status in a conservative way that rather biases against LBE, as explained in more detail in section 2.2.3. Finally, in our robustness checks, we employ an alternative technique to define the control group based on observable similarity, ultimately upholding our baseline results.

2.2.1 Industry Classifications

During the period of observation, the system of industrial classifications has undergone two changes, NACE Rev. 1.1 in 2003 and NACE Rev. 2 in 2008. In order to obtain time-consistent classifications of industry codes, we merge our dataset with correlation tables obtained from Eberle et al. (2011). Their identification strategy for the generation of time-consistent industry codes basically comes from the fact that in the years of conversion firms were required to indicate both their new and their old industry codes. We chose NACE 1.1 as our reference code and hence obtain time-consistent 5-digit codes, which we aggregate into the classification displayed in table (2.1).

We also do not observe an industry classification for firms before the year 2000, except for a self-reported more general branch affiliation (industry classifications are otherwise assigned based on administrative records). Here, we make the assumption that firms that are also observed in earlier
Table 2.1: Sectors and NACE Codes

<table>
<thead>
<tr>
<th>Sector</th>
<th>Rev. 1.1</th>
<th>Sector</th>
<th>Rev. 1.1</th>
</tr>
</thead>
<tbody>
<tr>
<td>Agriculture, Hunting, Fisheries</td>
<td>1,2,5</td>
<td>Telecommunication</td>
<td>643</td>
</tr>
<tr>
<td>Mining &amp; Quarrying</td>
<td>10,21,12,13,14</td>
<td>Transport, travel &amp; storage</td>
<td>66, 61, 62, 63, 641</td>
</tr>
<tr>
<td>Food Products, Beverages &amp; Tobacco</td>
<td>15, 16</td>
<td>Finance &amp; Insurance</td>
<td>65, 66, 67</td>
</tr>
<tr>
<td>Textile &amp; Leather</td>
<td>17, 18,19</td>
<td>Real Estate</td>
<td>70</td>
</tr>
<tr>
<td>Wood, Paper &amp; printing</td>
<td>20,21</td>
<td>Renting</td>
<td>71</td>
</tr>
<tr>
<td>Coke &amp; Refined petroleum products</td>
<td>23</td>
<td>R&amp;D</td>
<td>71</td>
</tr>
<tr>
<td>Chemical, Pharmaceutics</td>
<td>24</td>
<td>Legal, Accounting, Consulting &amp; advertising</td>
<td>744, 741</td>
</tr>
<tr>
<td>Rubber, Plastic &amp; Non-Metallic Minerals</td>
<td>25,26</td>
<td>Architecture &amp; Engineering</td>
<td>742, 743</td>
</tr>
<tr>
<td>Basic &amp; Fabricated metals</td>
<td>27, 28</td>
<td>Other professional, scientific or technical services</td>
<td>748</td>
</tr>
<tr>
<td>Machinery</td>
<td>29</td>
<td>Employment, Security &amp; Investigation,</td>
<td>745, 746, 747</td>
</tr>
<tr>
<td>Computer, Electronic &amp; Optical</td>
<td>30, 32, 33</td>
<td>Public Admin, Defense, Social Security</td>
<td>751, 752, 753</td>
</tr>
<tr>
<td>Electrical Equipment</td>
<td>31</td>
<td>Education</td>
<td>80</td>
</tr>
<tr>
<td>Motor Vehicles &amp; other Transport equipment</td>
<td>34, 35</td>
<td>Health</td>
<td>851</td>
</tr>
<tr>
<td>Furniture, Sport Goods, Toys, &amp; other</td>
<td>36</td>
<td>Veterinary</td>
<td>852</td>
</tr>
<tr>
<td>Utilities</td>
<td>37, 40, 41, 90</td>
<td>Social Services</td>
<td>853</td>
</tr>
<tr>
<td>Construction</td>
<td>45</td>
<td>Art, Entertainment &amp; Recreation</td>
<td>923, 925, 926, 927</td>
</tr>
<tr>
<td>Trade &amp; Repair</td>
<td>50, 51, 52</td>
<td>Other Services</td>
<td>93</td>
</tr>
<tr>
<td>Hotels and Restaurants</td>
<td>55</td>
<td>Households</td>
<td>95</td>
</tr>
<tr>
<td>Audiovisual Media and Broadcasting</td>
<td>22, 921, 922</td>
<td>Extra-Territorial Organizations</td>
<td>99</td>
</tr>
<tr>
<td>IT Services</td>
<td>72, 924</td>
<td>Unclassified</td>
<td>N/A</td>
</tr>
</tbody>
</table>

years belong to the same industry classification they belong to in 2000 and fill in the unobserved data accordingly.

2.2.2 Rescaling The Time Variable

We have a large unbalanced panel that ranges from 1993 to 2014. Since entry into exporting does not occur for all firms at the same time, we need to establish a common time scale along which we can compare firm performance. We hence create a time variable that counts the intervals in years from the moment a firm is first observed to export, which we denote as zero. A firm that is observed to export from the beginning hence appears only for time intervals > 0, counting the remaining years of observing that particular firm in the sample. On the other hand, a firm that is not initially an exporter will be observed for the time intervals < 0 until it is observed to export for the first time (at \( t = 0 \)), and all remaining \( t > 0 \) years. On the other hand, a firm that enters the sample with positive export values but subsequently quits the export market will be assigned the interval value of 0 at the moment the switch takes place, i.e. the first year in which it is
not an exporter anymore and all subsequent years.\textsuperscript{16} For strictly domestically operating firms that never start to export, the value zero is simply assigned to their rounded mean period of observation. For example, if a non-exporting firm is observed for 4 consecutive years, the first year will be assigned the value of -2, the second year will be -1, the third year will be 0 and the fourth year will be 1. As there is no particular or systematic importance to this moment of time, we do not worry about non-random peculiarities that may significantly confound the comparison of entry into exporting against non-exporters.

\textit{Controlling for Calendar Years}

While this common scale allows us to compare firms along this time dimension, we need to be aware that we may end up comparing firms at different calendar years. For example, if firm A enters into exporting in 1999 and firm B does so in 2013, both firms are assigned time interval 0. A number of issues may make such a comparison unacceptable without proper controls: Changes in the labor market environment, business cycle, trade policy and advancements in technology are just a few factors to be mentioned here. Therefore, we employ calendar year fixed effects in all our regression specifications, so as to be sure to control for all calendar year-specific factors that the rescaling of the time variable has cluttered. As hinted at in the data description above, the rescaling of the time variable has the added advantage of dispersing potentially confounding changes in the economic environment across time, which makes us more confident in the validity of using a dataset that does not contain information on (product-)export destinations and prices, as well as imported inputs.

\textsuperscript{16} In fact, we construct a separate time scale for the quitter variable, such that the switcher variable is defined on one scale and the quitter variable is defined on another, analogous scale. For the sake of readability, we only refer to one single time scale in the text.
2.2.3 Definition of firm groups

Armed with the rescaled time variable, we need to decide on an appropriate measurement for denoting a firm that has started to export. While this undertaking seems to be trivial, it is worth pondering its importance for a while. The early studies on the topic have usually looked at a dummy variable $EXP_{it}$, which indicate whether industry or firm $i$ has been observed to be an exporter in year $t$. A voluminous literature has since found support for the resulting finding of large exporter premia, in terms of productivity, average wages, size of the workforce etc. For our purposes, such a dummy indicating the moment a firm exports is not sufficient, as it would give us information only for the years that the firm is observed to actually export. Another frequently used indicator is a dummy variable that takes on the value of 1 if a firm exports in a given year, but has not been exporting in the previous year. Again, we believe that such a variable is not sufficient for our purposes, as a firm may well be classified as starting to export several times during the time it is observed.

We want to test whether a firm "learns" from exporting, i.e. whether we can observe any significant change of the dependent variable in response to a single change in the independent variable, namely the first switch into exporting. We hence generate the dummy variable $STARTER_{it}$ that takes on the value of 0 if a firm is never observed to export throughout its appearance in the panel, 1 for firms that export throughout all observations in the dataset. For firms that are initially observed not to export, their value of $STARTER_{it}$ is zero until the moment they first export (at time $t = 0$), and 1 for the remaining observations, regardless of its subsequent export status. The same applies to firms that export in the first year of observation, but then cease to export. This is our baseline characterization of export entrants for two main reasons: First, if learning from export markets were of any relevance, we would expect firms that have exported to display productivity gains regardless of whether they continue to export or not. Second, such
a conservative definition of export entrants hence also helps counter a potential pro-LBE bias that could be inherent in our dataset (see discussion in the data description above and the discussion on measuring productivity below).

Nevertheless, we want to be able to also consider firms that quit exporting. Intuitively, we expect productivity gains to be stronger for continuing exporters once they are purged of quitters. Analogously to the construction of our STARTER variable, we hence construct a variable QUITTER that take on the value of 1 the first time a firm is observed to cease exporting, regardless of whether it resumes exporting at a later time or not. One can think of this variable as the inverse of the STARTER variable.

Altogether, we define three broad groups of firms, defined as:

1. Domestic firms if \( \sum_t \text{EXP}_{it} = 0 \)
2. Switchers if \( 0 < \sum_t \text{EXP}_{it} < T \)
3. International firms if \( \sum_t \text{EXP}_{it} = T \)

The group of Switchers contains firms for which the STARTER variable takes the value of 1 at least once during their period of observation, but also firms for which the value of QUITTER take the value of 1 at least once. The two subgroups are hence not mutually exclusive. Nevertheless, given our definition, each firm switches only once, whether into starting and/or quitting. Further, if we consider only those firms that are switchers, but who never display a positive value of QUITTER, we can focus on those new exporters that - once entering into exporting - remain exporting until the end of their observations, and which we will call SURVIVORS.

Given that not each firm is not observed in every time period, we end up with an unbalanced panel whose distribution is concentrated around time 0 and thins out towards the tails. What are the implications of this
concentration? Given the calculation of the STARTER and QUITTER variable and the rescaled time variable, we are left with a time span reaching from -21 to +21. Obviously, the further we go back in time \((t < 0)\) or ahead of time \((t > 0)\), we need to be careful about the judgments we make on the representativity of firm characteristics, as extreme values become more likely due to the lower number of sampled firms, which is why most of our analyses focus on a greatly reduced time span. Additionally, in order to minimize potential biases that accrue from this imbalance, we perform a propensity score matching technique on each time period to identify an average treatment effect in 2.4.3.

2.2.4 How to measure productivity?

The standard approach to proxy for firm productivity is to retrieve the residual of a production function, which then in turn is compared across exporting and non-exporting firms. Resulting productivity measures may be biased for four main reasons, of which the first two have a long tradition of debate in the literature (Van Beveren (2012): i) input choices may be endogenous, a function of firm efficiency, 2) a selection bias due to the exit of firms and input choices made conditional on survival, iii) an omitted price bias when revenue-based productivity estimation is used and prices not properly accounted for (De Loecker (2011)), and iv) a bias resulting from assuming identical production techniques and final demand across products manufactured by a single firm (Bernard et al. (2009). In the LBE context, De Loecker (2013) shows that estimating production functions that do not allow exporting status to affect productivity - as widely used in the literature on LBE - will eventually translate into a bias against the LBE hypothesis.

Unfortunately, values for the capital stock are not reported in our data. Since we do observe investment levels, we might obtain a measure of the
capital stock through applying the perpetual inventory method. However, given sometimes patchy investment data and short time spans of firm observations, we are doubtful of whether this approach would add value (see the discussion for this dataset in Müller (2010)). Instead, we use the different measure of labor productivity, constructing our variable as value-added per worker. Having value-added, rather than output, as the numerator of our labor productivity measure refines the indicator in that an important factor of production - intermediate inputs - is accounted for, at least in value terms. However, a serious drawback remains the lack of an adequate capital measure, in whose absence we cannot account or the contribution of capital to increased per worker value-added. Fixed effects estimation and controlling for investment in all our main specifications cannot fully remedy this shortcoming. We further take up the issue in the robustness checks towards the end of the study, but already caution that any generalization from labour towards TFP based on this study should be taken with a grain of salt. In constructing our productivity variable, we use the total number of workers. This choice is motivated by the fact that the shares of other available variables such as high-skilled, temporary and short-term employment remain remarkably invariant over intervals of observation $t$ within the three groups.

We can address some of the known issues in measuring productivity in the following ways: When estimating productivity in our main specification (2.3), we use a fixed effects estimator as in Pavcnik (2002) and Petrin and Levinsohn (2003). As such, we can to some extent account for unobserved factors such as time-invariant productivity shocks that may in part be a reflection of unobserved capital stocks that differ across firms. Such an approach also helps to attenuate the simultaneity bias resulting from endogenous input choices Ackerberg et al. (2007) and reduce selection bias resulting from endogenous firm exit in the sample, to the extent that it is related to the time-invariant productivity component. Furthermore, we explicitly take a possible correlation between exporting and productivity into
Table 2.2: Data description

<table>
<thead>
<tr>
<th>Variable</th>
<th>Obs</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min.</th>
<th>Max.</th>
</tr>
</thead>
<tbody>
<tr>
<td>exp</td>
<td>213931</td>
<td>.2298171</td>
<td>.4207161</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>starter</td>
<td>213931</td>
<td>.2923559</td>
<td>.454846</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>quitter</td>
<td>213931</td>
<td>.090403</td>
<td>.2867580</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>log productivity</td>
<td>156189</td>
<td>10.48702</td>
<td>.9910139</td>
<td>-.0497428</td>
<td>16.84333</td>
</tr>
<tr>
<td>log employment</td>
<td>213931</td>
<td>3.163625</td>
<td>1.697547</td>
<td>.6931472</td>
<td>10.97972</td>
</tr>
<tr>
<td>log investment</td>
<td>200839</td>
<td>4.438881</td>
<td>4.133074</td>
<td>0</td>
<td>16.65125</td>
</tr>
<tr>
<td>log wages</td>
<td>188037</td>
<td>7.086665</td>
<td>1.290652</td>
<td>0</td>
<td>10.59666</td>
</tr>
<tr>
<td>log dom sales</td>
<td>180886</td>
<td>11.17106</td>
<td>1.112905</td>
<td>-.0612103</td>
<td>18.35064</td>
</tr>
</tbody>
</table>

account by including exporting status directly in equation (2.3), similarly to igsten and Gebreeyesus (2009) or Van Biesebroeck (2005).

Concluding the data section, we list the summary statistics of the main variables used in table (2.2). Our baseline analyses will be based on 156189 observations, as per the productivity variable, which is defined only for firm-year data points for which data on both value-added and employment is available. Exporting status is observed in almost 23% of all cases, which increases to 29.2% for the starter variable, which remains 1 despite subsequent exit of previous exporters. The quitter variable is relatively low, indicating relatively little exit from exporting.

2.3 ANALYSIS

2.3.1 Pooled OLS

We now turn to a preliminary analysis of our dataset. In order to ensure comparability with similar studies, we start by following Bernard and

---

17 We use current values of the STARTER or QUITTER variables rather than lagged exporting. Section (2.3.5) will deal more specifically with dynamic variation.
Jensen (1999) and others in estimating variants of the following equation by Ordinary Least Squares:

\[
prod_{ikt} = \alpha + \beta I_{ikt} + \gamma l_{ikt} + \delta_{year} + \lambda_k + \epsilon_{ikt}
\]  

(2.1)

\( prod_{ikt} \) refers to the log of labor productivity of firm \( i \) in industry \( k \) at time \( t \). The \( l_{ikt} \) variable is replaced by \( EXP_{ikt} \) in a first step and then by our \( STARTER_{ikt} \) variable and \( l_{ikt} \) is the log of employment. Our coefficient of interest is \( \beta \). Given the rescaling of our time variable, we use calendar year dummies to keep track of year specific effects \( \delta_t \) (such as business cycle or other year-specific shocks), and finally \( \lambda_k \) is an industry specific fixed effects that controls for differential, time-invariant productivity tendencies across industries as classified in (2.1). Given the log specification of equation (3.2), we can interpret \( \exp(\beta) - 1 \) * 100 as the percentage difference between firms for which \( I_{ikt} = 1 \) and those for which \( I_{ikt} = 0 \).

**Table 2.3: Pooled OLS**

<table>
<thead>
<tr>
<th></th>
<th>1a</th>
<th>1b</th>
<th>2a</th>
<th>2b</th>
<th>3a</th>
<th>3b</th>
<th>4a</th>
<th>4b</th>
</tr>
</thead>
<tbody>
<tr>
<td>exp</td>
<td>0.493</td>
<td>0.327</td>
<td>0.479</td>
<td>0.333</td>
<td>0.390</td>
<td>0.297</td>
<td>0.170</td>
<td>0.117</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>starter</td>
<td>NO</td>
<td>YES</td>
<td>NO</td>
<td>YES</td>
<td>NO</td>
<td>YES</td>
<td>NO</td>
<td>YES</td>
</tr>
<tr>
<td></td>
<td>156189</td>
<td>156189</td>
<td>156189</td>
<td>156189</td>
<td>132039</td>
<td>132039</td>
<td>32716</td>
<td>32716</td>
</tr>
<tr>
<td></td>
<td>0.152</td>
<td>0.181</td>
<td>0.156</td>
<td>0.184</td>
<td>0.131</td>
<td>0.154</td>
<td>0.069</td>
<td>0.104</td>
</tr>
</tbody>
</table>

Columns (a) refer to regressions without the employment control variable, (b) columns refer to those with the employment variable. Columns (1) are over the entire sample of firms, columns (2) purge international firms, columns (3) are only over switchers. \( p \)-values in parentheses

We first estimate by pooled OLS, with both fixed effects and without the employment variable and display the results in the (a) columns of table (2.3). The first column tells us that exporting at any given year is associated with an average of roughly 63% higher productivity over the entire sample of firms. This figure is higher than the usual roughly 35% productivity pre-
The literature generally finds - mainly, because we do not control for firm level log employment. The moment we do so, displayed in columns (b) of table (2.3) - we find a highly comparable number of roughly 38% productivity premium for exporters.

In a next step, we regress equation (1) again over the entire sample of firms, but using the STARTER variable as the dependent variable, where - interestingly - the coefficient does not change much with respect to the first specification.

We still have no idea whether the productivity differences are inherent to the firm, or whether they are associated with the specific exporting status we have defined in the STARTER variable. We suspect that international firms may bias our coefficient upwards as they may be more productive in the first place. In order to get a better grasp on this question, we perform the same set of regressions on a restricted sample, which only includes domestic and switching firms (columns (3) of table (2.3)). As suspected, the coefficient decreases slightly in magnitude: Switching firms after their first observed year of exporting are still roughly 35% more productive than the average of domestic firms and switching firms before exporting, after controlling for firm-level employment. This result implies that we can now rule out large productivity differences between international firms and firms that have started to export. Nevertheless, we still do not know whether there are significant pre-export differences between domestic firms and switchers. Column (4) displays the results of the same set of regressions on the sub-sample of switching firms only; we hence compare the mean productivity of switching firms only before and after exporting. Controlling for employment, we find that the difference is still significant, but less than half as important as in the previous set of regression, suggesting substantial mean pre-export productivity differences with domestic firms.

In a second exploratory step, we split our group of switchers into its two components, survivors and quitters. We perform analogous regressions and display the results in table (2.4). The coefficients obtained on surviv-
ing firms (those who keep exporting after their first entry) are all larger than those obtained for the entire switcher group in (2.3), which makes intuitive sense. Likewise, the coefficients of quitters are all smaller, however still significantly large and positive, implying higher average post-quitting productivity even over the restricted sample in columns (3).

### Table 2.4: Pooled OLS: Switcher subgroups

<table>
<thead>
<tr>
<th></th>
<th>Survivors</th>
<th>Quitters</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1a</td>
<td>1b</td>
</tr>
<tr>
<td>starter</td>
<td>0.388</td>
<td>0.333</td>
</tr>
<tr>
<td>quitter</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>labor</td>
<td>NO</td>
<td>YES</td>
</tr>
<tr>
<td>N</td>
<td>130616</td>
<td>130616</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.172</td>
<td>0.195</td>
</tr>
</tbody>
</table>

Columns (a) refer to regressions without the employment control variable, (b) columns refer to those with the employment variable. Columns (1) are over the entire sample of firms, columns (2) purge international firms, columns (3) are only over the specific subgroup. $p$-values in parentheses.

#### 2.3.2 Comparing Means

In the previous section, we have established significant mean differences between domestic firms and both switchers, as well as international firms. We have also found significant differences among switchers before and after exporting, amounting to an average percentage difference of roughly 12%. We are still unsure as to how to interpret these results in the light of the self-selection, as well as the LBE hypothesis. Substantial pre-export differences between domestic firms and switchers suggest that self-selection is certainly at play, but how do we interpret the fact that post-export average productivity is even higher among switchers? Before resorting to more sophisticated econometric techniques to shed more light on these questions, we proceed with a simple graphical analysis. We compute the mean productivity levels of each of our three groups of firms for each interval of observation and plot the results in figure (2.1). The graph very
nicely reflects our regression results, but also gives illuminating insights on the phenomenon we want to explain. We indeed observe substantial pre-export productivity differences between domestic firms and switchers, but these appear to be relatively constant. The graph seems to suggest that relatively more productive firms do self-select into exporting, but not in that they increase their productivity in temporal proximity to their entry into export markets. If we drew a trend line for pre-export observations of both groups, they would both be quite flat. It is only once exporting has occurred (interval $t = 0$) that average productivity increases, to levels comparable with international firms. However, the post-entry trajectory is not increasing, which we would expect if LBE were present. Recall the identification problem we have at $t = 0$. In that context, let us also recall the quite restrictive characterization of our $STARTER$ variable, which groups all switchers together, regardless of whether they keep exporting or not. If we disentangle this potentially quite heterogeneous group, the trajectories look much more like what we would expect (figure (2.2)): Continuing exporters have a monotonically increasing post-entry productivity trajectory
(except for the last period), while that of quitters seems to be driving the relatively constant post-export entry slope of the aggregate group observed in figure (2.1). Notice also that quitters experience continuous export growth prior to quitting, i.e. while exporting.\footnote{The time scales for survivors and quitters are not the same. The \textsc{Starter} variable is defined on one time scale, the \textsc{Quitter} variable on another, since these characterizations are not mutually exclusive and a firm may be both a starter and a quitter.}

\begin{figure}[h]
\centering
\includegraphics[width=0.6\textwidth]{figure2.2}
\caption{Comparing Means: Labor Productivity by Subgroup}
\end{figure}

Opening a small parenthesis, it is instructive to examine labor productivity jointly with labor and external inputs purchased by the firm. We compute the means for employment and inputs and plot them in figures (2.3) and (2.4).\footnote{We do not include international firms here, as they are of significantly larger average size and would make the graph less readable.} It is interesting to note that the trends for average employment of switching firms is increasing throughout and at similar rates, while the average size of domestic firms does not display any particular trend, except for being smaller towards the tails. Seen in the light of rising employment throughout, the labor productivity increases that occur with exporting appear to be even more spectacular and not to be driven by re-
Figure 2.3: Comparing Means: Employment

![Graph showing employment trends]

Figure 2.4: Comparing Means: Inputs per worker

![Graph showing input trends]

ducing the average workforce. The stark average increase in purchased external inputs confirms larger demand for those per worker. Unreported figures for investment per worker draw a similar picture. Both metrics, however, do not display a substantial pre-exporting jump, which implies
that firms seem not to make conscious pre-export choices concerning the volume of these metrics.

2.3.3  

Pooled OLS with firm fixed effects

Our analysis so far has shown that there appear to be intrinsic differences between our groups of firms, and we cannot say much about those unless we control for more firm specific effects. We begin to do so by replicating the regressions in table (2.3a), without any firm level controls but with a year fixed effect and this time a firm fixed effect, as in equation (2.2)²⁰

\[ prod_{it} = \alpha + \beta I_{it} + \delta_{year} + \lambda_i + \epsilon_{it} \]  

(2.2)

If there are indeed intrinsic, firm-specific and time-invariant differences, the \( \lambda \) will pick those up and \( \beta \) will provide us with a more accurate estimation of the percentage difference each regression aims at uncovering.

A quick look at the results in table (2.5) confirms this intuition, notably

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>exp</td>
<td>0.027</td>
<td>0.027</td>
<td>0.035</td>
<td>0.036</td>
</tr>
<tr>
<td></td>
<td>(0.0011)</td>
<td>(0.0096)</td>
<td>(0.0013)</td>
<td>(0.0035)</td>
</tr>
<tr>
<td>starter</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>156189</td>
<td>156189</td>
<td>132039</td>
<td>32716</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.727</td>
<td>0.727</td>
<td>0.715</td>
<td>0.651</td>
</tr>
</tbody>
</table>

Table 2.5: Pooled OLS with firm fixed effects

Columns (1) and (2) are over the entire sample of firms, column (3) purges international firms, columns (4) is only over switchers. p-values in parentheses.

²⁰ An unreported Hausmann test confirms the appropriateness of fixed effects over random effects and the necessity for year fixed effects.

the high \( R^2 \) we obtain without any firm-level covariates except the dummy variable. We follow the same procedure as before, where column (1) and
(2) display the results of a regression over the entire sample, including international firms. The coefficients are again very similar and highly statistically significant, but much lower in value. Controlling for firm fixed effects (such as intrinsic productivity differences), we find that exporters and starters are now just 2.7% more productive than non-exporters and non-starters as we defined them. Interestingly, in column (3) we observe that removing international firms yields a higher coefficient $\beta$, reinforcing the LBE hypothesis in that it underlines the relevance of starting to export for productivity gains. The same is true for the value of $\beta$ obtained in a regression over switching firms (column 4), which tells us that switching firms are on average 3.7% more productive once they have started to export, controlling for their individual average pre-export productivity levels.

### 2.3.4 Fixed-effect estimation

Motivated by our discussion in (2.2.4), we refine our analysis further by focusing solely on within-firm variation, using a fixed effect estimator. This necessitates the addition of firm-level covariates that are reasonable for our purposes. We hence proceed to estimate a model of the following form:

$$ prod_{it} = \alpha + \beta \text{STARTER}_{it} + \gamma X_{it} + \pi_{year} + \lambda_i + \epsilon_{it} \quad (2.3) $$

where $X_{it}$ indicates a set of firm-level covariates. We control for firm size - or use of factor inputs - by including the log of employment, the log of investment per worker, as well as the payroll per worker. Additionally, we include the log of domestic sales per worker, in order to better isolate the effect of exporting, controlling for the purely domestic sources of productivity gains that may occur to firms regardless of their exporting status, gains that may accrue simply through domestic market expansion.

At this point, it is important to recall that the RHS variables enter the equation solely for the purpose of controlling for time-varying firm-specific
characteristics that may matter in terms of productivity. The previous analyses have shown the need to take account of firm specific characteristics, such as in table (2.3), where we alternate variants of equation (3.2) with and without inclusion of the employment variable. In the original productivity sense, we are interested in the extent to which firms are able to change their ability to transform inputs into output, or value added in our case. Conditioning on current employment is hence necessary to control for variation in value added that is immediately caused by variation in labor, as the focus of this study lies elsewhere, namely in uncovering the role of exporting. Hence, the coefficient \( \beta \) can be interpreted as a productivity shifter of exporting, conditional on the other controls. With this background, it is crucial to note that having the employment variable on both sides of the equations (3.2) and (2.3) does not affect the statistical validity of our analysis. To see this, let us first expand our estimating equation (2.3) as follows:

\[
\ln \left( \frac{VA_{it}}{L_{it}} \right) = \alpha + \beta \text{STARTER}_{it} + \gamma_1 \ln(L_{it}) + \gamma_2 \ln \left( \frac{I_{it}}{L_{it}} \right) + \gamma_3 \ln \left( \frac{W_{it}}{L_{it}} \right) + \gamma_4 \ln \left( \frac{D_{it}}{L_{it}} \right) + \pi_{year} + \lambda_i + \varepsilon_{it}
\]

Now consider the following equation:

\[
\ln VA_{it} = \alpha + \beta \text{STARTER}_{it} + \delta \ln(L_{it}) + \gamma_2 \ln(I_{it}) + \gamma_3 \ln(W_{it}) + \gamma_4 \ln(D_{it}) + \pi_{year} + \lambda_i + \varepsilon_{it}
\]

The labor variable \( L_{it} \) now enters solely the right hand side of the equation. Nevertheless, the two equations are econometrically equivalent, if we let \( \delta \equiv (\gamma_1 - \gamma_2 - \gamma_3 - \gamma_4 + 1) \).
In a panel setting like ours, idiosyncratic errors are likely to be serially correlated. Bertrand et al. (2004) show that the usual standard errors of the fixed effects estimator are drastically under-estimated in the presence of serial correlation. As suggested by Stock and Watson (2008), we cluster standard errors on the firm level to control for both heteroskedasticity as well as within-firm serial correlation.

Finally, we group firms into a manufacturing and a services sector and proceed within these groups as above, regressing over a) all three types of firms (domestic, switchers and international), b) only domestic and switchers, c) only switchers. We plot the results in table (2.6).

Table 2.6: Fixed Effects Estimation: Sectors

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Manufacturing</th>
<th>Services</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>a</td>
<td>b</td>
<td>c</td>
</tr>
<tr>
<td>starter</td>
<td>0.087</td>
<td>0.107</td>
<td>0.088</td>
</tr>
<tr>
<td>(0.0000)</td>
<td>(0.0000)</td>
<td>(0.0000)</td>
<td>(0.0000)</td>
</tr>
<tr>
<td>firm controls</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>firm fe</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>year fe</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>N</td>
<td>142448</td>
<td>121025</td>
<td>29896</td>
</tr>
<tr>
<td>R²</td>
<td>0.157</td>
<td>0.208</td>
<td>0.102</td>
</tr>
</tbody>
</table>

Columns (a) are over the entire sample of firms, columns (b) purges international firms, columns (c) is only over switchers. Manufacturing comprises sectors 3-14, Services comprises 16-37. Standard errors are clustered on the firm level and p-values are given in parentheses.

Qualitatively, the results are similar to what we have established so far, except that we are now looking at within-variation only, which enables us to get rid of self-selection effects that may occur as the result of inherent differences in firm productivities. Quantitatively, the coefficients we estimate are larger. Looking at column (b) of the regression over all firms, we find that switchers are on average almost 11% more productive once they export, compared to domestic firms and before exporting. Looking at switchers only before and after starting to export, we find a significant starting premium of over 9%, which is both statistically and economically highly significant.
Furthermore, we can now for the first time look at differences between firms in the manufacturing and in the services sector. Overall, sectoral results resemble the aggregate results. Starting to export is associated with higher productivity gains in manufacturing than in services, but the effect is statistically and economically significant in both sectors. Compared to the aggregate analysis and the one on manufacturing, the analysis on services firms displays an interesting peculiarity: The coefficient in column (c) is higher than in column (b), suggesting that the productivity differences between domestic firms and switchers are less substantial than in the manufacturing sector.

**Table 2.7: Fixed Effects Estimation: Subgroups**

<table>
<thead>
<tr>
<th></th>
<th>Survivors</th>
<th></th>
<th>Quitters</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>a</td>
<td>b</td>
<td>c</td>
<td>a</td>
</tr>
<tr>
<td>starter</td>
<td>0.188</td>
<td>0.250</td>
<td>0.084</td>
<td>0.042</td>
</tr>
<tr>
<td>quitter</td>
<td>(0.0000)</td>
<td>(0.0000)</td>
<td>(0.0040)</td>
<td>(0.0019)</td>
</tr>
<tr>
<td>firm controls</td>
<td>yes</td>
<td></td>
<td></td>
<td>yes</td>
</tr>
<tr>
<td>firm fe</td>
<td>yes</td>
<td></td>
<td></td>
<td>yes</td>
</tr>
<tr>
<td>year fe</td>
<td>yes</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>118987</td>
<td>97564</td>
<td>6435</td>
<td>142448</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.169</td>
<td>0.268</td>
<td>0.042</td>
<td>0.157</td>
</tr>
</tbody>
</table>

Columns (a) are over the entire sample of firms, columns (b) purges international firms, columns (c) is only over the subgroup in question. Standard errors are clustered on the firm level and p-values are given in parentheses.

Analysis of figure (2.2) suggests taking account of different productivity trajectories between surviving exporters and quitters within the switcher group. Varying the control groups analogously to the results in table (2.6), we perform the analysis separately for survivors and quitters in table (2.7). The coefficients we obtain for survivors are largely consistent with previous results on switchers. The results for quitters are more interesting. In particular, having added firm level covariates and exploiting within-firm
dynamics, we see a reversal of the signs of the coefficients when compared to the specification in table (2.4). However, this effect is only significant in specifications (a) and (b), where the control group comprises the more productive international and surviving firms. Column (c) attests no notable post-exporting productivity differences within the group of quitters only.

**Sectoral Decomposition and Market Structure**

Given these results, we dig deeper into detailed industry classifications to get an idea of which sectors are those where starting to export is associated most closely with productivity gains. To this end, we estimate equation (2.3) over firms in each subset of industry classifications as generated in table (2.1). We find that indeed not all industries seem to be associated with LBE effects. We list those industries where we find a statistically significant coefficient $\beta$ in table (2.8).

<table>
<thead>
<tr>
<th>LBE Manufacturing</th>
<th>LBE Services</th>
</tr>
</thead>
<tbody>
<tr>
<td>Wood, paper and printing</td>
<td>Construction</td>
</tr>
<tr>
<td>Chemical and pharmaceutical products</td>
<td>IT services</td>
</tr>
<tr>
<td>Rubber, Plastic and non-metallic mineral products</td>
<td>R&amp;D</td>
</tr>
<tr>
<td>Basic metals and fabricated metal products</td>
<td>Architecture and engineering</td>
</tr>
<tr>
<td>Electrical Equipment</td>
<td>Other professional, scientific or technical services</td>
</tr>
<tr>
<td>Furniture, jewellery, sport goods, toys, and other</td>
<td>Education</td>
</tr>
<tr>
<td></td>
<td>Transport, Travel and Storage</td>
</tr>
<tr>
<td></td>
<td>Health</td>
</tr>
<tr>
<td></td>
<td>Art, Entertainment and Recreation</td>
</tr>
<tr>
<td></td>
<td>Real Estate</td>
</tr>
</tbody>
</table>

Intuitively, we fail to detect an immediate reason for why these precise sectors display the LBE effects we find. While this question is beyond the immediate scope of this paper, we nevertheless ponder it for a moment, to the extent that the limitations we face in our dataset allow us to do so. In fact, firm-level workforce characteristics we have not yet accounted for seem not to be important determinants of these different behaviours, even
those that vary across time and, hence, are not picked up by the individual fixed effects employed in our regressions. Importantly, hiring and firing decisions, the share of qualified workers, part-time and temporary employment in total employment are not significantly associated with post-export productivity increases. We have seen in 3.1.2. that management decisions such as investment or purchase of external inputs per worker are significantly associated with productivity increases, comparing switching firms with domestic ones across sectors. However, LBE sectors do not display significant average differences with non-LBE sectors along those lines.

Theoretically, we expect to find such effects primarily in relatively export-oriented sectors. The average propensity to export is very heterogeneous across sectors, which is in part a reflection of differences in intrinsic exportability of certain goods or services over others. For example, the services sector has long been regarded as non-tradable as a whole. It is through revolutions in technology and transport that this sector is getting increasing attention in the international trade literature. Calculating the potential tradability of different services sectors in the US on grounds of their geographic concentration, ensen et al. (2011) obtains a ranking of these sectors according to their ‘tradability’. While our sectors listed in table(2.8) are much more aggregated (in an effort to ensure time-consistent classifications of economic activity, as well as an adequate trade-off between sectoral precision and meaningful numbers of observations within these sectors), there is a striking overlap with the sectors identified by Jensen.

Likewise, there are also differences in propensity to export across manufacturing sectors. These can result from a whole variety of factors, ranging from traditional explanations of comparative advantage to differences in consumer valuation of some goods over others. In both cases, our data confirm the heterogeneity across sectors in export propensity.

Table (2.9) shows that (i) the average propensity to export is, as expected, much lower in services than in manufacturing. While export propensities
Table 2.9: Comparing sectors

<table>
<thead>
<tr>
<th></th>
<th>Manufacturing</th>
<th>Services</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Non-LBE</td>
<td>LBE</td>
</tr>
<tr>
<td>Export Propensity</td>
<td>50.3%</td>
<td>53.4%</td>
</tr>
<tr>
<td>Market Concentration</td>
<td>0.059</td>
<td>0.074</td>
</tr>
<tr>
<td>Export Concentration</td>
<td>0.086</td>
<td>0.116</td>
</tr>
</tbody>
</table>

of above 11% are quite low, it is certainly not the case that the services sector *per se* is not tradable, but scope for exporting is on average much lower than in manufacturing, which in turn may be part of an explanation for lower LBE effects in services as established in table (2.6). At the same time, table (2.9) shows that (ii), if we compare LBE and non-LBE sectors within services and manufacturing each, average propensity to export is higher in the former in both cases (roughly 6% in manufacturing and 10% in services), reinforcing our conjecture that export orientation matters for LBE effects.

Apart from export orientation, and hence the simple capacity to tap into foreign markets, we suspect that the degree of competition matters as well. Increased competition is widely considered as a major driver of firm productivity (see e.g. Aghion et al. (2015)) and we hence expect those productivity gains resulting from exporting to be relatively higher in domestic sectors with relatively low levels of competition. The intuition here follows from basic microeconomic theory, which establishes that firms in uncompetitive markets tend to be relatively unproductive as they face little competition. Entry into exporting hence entails productivity upgrades, as firms operating in a formerly uncompetitive sector find themselves in competition on the world market. In order to investigate this channel further, we thus need a measure of the degree of competition within a sector and choose to compute a normalized Herfindahl index per sector as follows:

\[ NH_s = \frac{(H_s - 1)/N_s}{(1 - 1/N_s)} \]
where

\[ H_s = \sum_{i}^{N_s} \left( \frac{Rev_{is}}{Rev_s} \right)^2 . \]

The index ranges from 0 to 1, where a higher index indicates higher market concentration. Plotting the results in (2.9), we do indeed find evidence for higher market concentration, and hence less competition, in LBE sectors, as compared to non-LBE sectors. Again, the difference is less pronounced in the services sector, but these results are suggestive of taking the analysis a step further.

**Figure 2.5: LBE and Market Concentration**

In figure (2.5) we plot the Herfindahl index we obtain for each sector against the (statistically significant)\(^{21}\) coefficients we obtain from the individual regression results obtained in table(2.8). The low number of observations notwithstanding, we have strong suggestive evidence in favor of the hypothesis that LBE effects increase with uncompetitive market structures as proxied by the Herfindahl index. The underlying OLS regression of each

\(^{21}\) p-value < 0.05
sector’s $\beta$ coefficient on its Herfindahl measure yields a coefficient of 0.931, with a 0.260 standard error. A similar hypothesis was put forward by Manjón et al. (2013), who found lower LBE effects for firms in Spain than De Loecker (2007) did for Slovenia, despite using a very similar method. The authors hypothesize that this difference is due to the higher potential of productivity gains in post-Communist Slovenia, without further substantiation, however. Our result is not inconsistent with this hypothesis.

Our result is also robust to the omission of the negative coefficient obtained for "Real Estate”. While the sign of this coefficient is somewhat of a puzzle and would require a more in-depth analysis, we believe that the peculiarities of this sector are responsible for the negative association between starting to export and labor productivity. In particular, the real estate sector requires significant local expertise and interaction with clients, which may set it apart from other sectors.

In order to complete our picture, contrasting service sector performance with manufacturing, we also calculate a Herfindahl concentration index for exporting shares only. A higher measure hence indicates the concentration of export revenue in few firms. Unlike the simple measure of export propensity, the concentration index measures the distribution of export revenues among exporting firms. If analyzed jointly with the market concentration index, this metric may point to restrictive access to foreign markets. The last row in table (2.9) reports the numbers for our sectoral classification. The differences between LBE and non-LBE sectors mimic the differences established earlier between both sectors with respect to market concentration. Intuitively, this result makes sense and has a mechanical component, as export revenue is part of overall market revenue. The higher level of export concentration as compared to market concentration is also readily rationalizeable by the positive correlation between exporting and firm size. What is striking in these results, however, is the sizeable higher concentration in service sector exports, as opposed to its market concentra-
tion measures. While the latter are broadly comparable to manufacturing concentration measures, export revenue is highly concentrated in few service sector firms, pointing to highly uncompetitive foreign market access. Not only does the German services sector exhibit a generally lower propensity to export, but even within the group of exporting firms, revenues are highly concentrated.

2.3.5 Testing for temporal proximity of self-selection vs LBE

Taken together, the previous results suggest that the average post-exporting productivity of switchers is higher than both their average pre-export productivity and domestic firms’ average productivity before and after their median observation. This seems to be true for firms in both manufacturing and services industries, where some industries appear to be more predisposed to experience such productivity gains than others. The size of productivity gains on average appears to be higher in the manufacturing sector than in services, which may be the result of differences in the degree of competitiveness of the underlying market structures. We also observe substantial differences within the group of switchers. As such, firms that continue exporting after their first entry display a greater average post-entry productivity gains that firms that quit exporting.

We have so far attested self-selection to the extent that future exporters are on average more productive than their domestic counterparts. We have also firmly established the result that average post-entry productivity of switchers is higher than average pre-entry productivity. However, we cannot ascertain yet whether our results are driven by the potentially endogenous year of entry into exporting (see discussion in section (2.1)).

In order to better account for each firm’s productivity trajectory, we follow the method developed by Autor (2003) and augment equation (2.3)

---

22 This result is strengthened for continuing exporters, as seen in table (2.4)
with leads and lags of the $\text{Starter}_{it}$ and $\text{Quitter}_{it}$ variables, replacing those variables with a set of dummies as follows: We add a dummy for $t_{-k}$, where $k$ denotes the intervals a firm is observed before entry into (exit from) exporting, as well as a dummy for $t_0$ and $t_{+j}$, where $j$ denotes the intervals a firm is observed after exporting (exit from exporting). These dummies each take the value of one only for the year of their corresponding time period and are zero otherwise, which allows us to isolate the average effect in each time period that is being considered. We also include a dummy that takes on the value of 1 for all observations $> k$, starting in $t_{+(k+1)}$. Note that all international firms will not enter the sample, since their value of $t_{-j}$ for $j > 0$ is undefined. We therefore regress only over those firms whose time dimensions range from at least -1 to at least +2. We test different values for $k$ and $j$, with very similar results across specifications. As the number of firms observed drops significantly with larger values of $k$ and $j$ (as we increase the required number of consecutive observations), we display the results of a regression with $k = 1$ and $j = 1$ in table (2.10), implying that we regress over all firms that are observed at least for a period of four consecutive years. All firms that do not satisfy the criterion of at least one observation prior to exporting (exit from exporting), as well as at least two observations after the year of exporting (exit from exporting), do not enter the regression. We regress over all firms in column (1), over manufacturing and services firms in columns (2) and (4) respectively, and finally over those subsectors we identified in table (2.8) as being particularly prone to LBE effects in manufacturing (3) and services (5).

If we would observe an anticipation effect in the sense that a firm makes a conscious effort to upgrade productivity prior to entering into exporting, we would observe a positive coefficient on $t_{-1}$. The interpretation of that coefficient would be that its productivity at that time exceeds its average productivity when $t_{-1} = 0$, meaning all other years of observation of the firm. In contrast, an LBE effect would be supported by positive and increas-
ing coefficients on $t \geq 0$.

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Manufacturing</th>
<th>LBE Manufacturing</th>
<th>Services</th>
<th>LBE Services</th>
</tr>
</thead>
<tbody>
<tr>
<td>$t_{-1}$</td>
<td>-0.076</td>
<td>-0.058</td>
<td>-0.055</td>
<td>-0.08</td>
<td>-0.078</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.073)</td>
<td>(0.249)</td>
<td>(0.000)</td>
<td>(0.062)</td>
</tr>
<tr>
<td>$t_0$</td>
<td>0.108</td>
<td>0.095</td>
<td>0.163</td>
<td>0.116</td>
<td>0.216</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.005)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>$t_{+1}$</td>
<td>0.087</td>
<td>0.124</td>
<td>0.166</td>
<td>0.052</td>
<td>0.11</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.023)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>$t_{+(2)}$</td>
<td>0.056</td>
<td>0.161</td>
<td>0.241</td>
<td>-0.020</td>
<td>0.090</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.297)</td>
<td>(0.043)</td>
</tr>
</tbody>
</table>

firm controls | yes
firm fe | yes
year fe | yes

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Manufacturing</th>
<th>LBE Manufacturing</th>
<th>Services</th>
<th>LBE Services</th>
</tr>
</thead>
<tbody>
<tr>
<td>$N$</td>
<td>10121</td>
<td>18642</td>
<td>9998</td>
<td>63402</td>
<td>29078</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.253</td>
<td>0.191</td>
<td>0.198</td>
<td>0.242</td>
<td>0.286</td>
</tr>
</tbody>
</table>

Standard errors are clustered on the firm level. p-values in parentheses.

Our results in table (2.10) are remarkably clear, in that the coefficient on $t_{-1}$ is never positive. This gives us confidence that we can reject the null hypothesis that firms self-select into exporting by upgrading their productivity just prior to starting to export. Conversely, we find ample backing for the LBE hypothesis. The coefficients on $t \geq 0$ are very interesting when we compare sectors. The manufacturing sector, and notably LBE manufacturing, displays the predictions of LBE to the letter. At $t = 0$, the average manufacturing firm is almost 10% more productive than its average (18% in LBE manufacturing). At $t = 1$, that firm will be already 13% more productive ( 18% for LBE manufacturing). For $t \geq 2$, average productivity rises further to 17% (27% in LBE manufacturing). These results suggest that firms literally "learn" from exporting, in terms of productivity gains, as time passes.

In the services sector, the results are not as clear-cut. The coefficients on $t \geq 0$ are also positive, but decrease in magnitude as $t$ rises. For the services sector as a whole, the coefficient on the forward variable $t_{+(k+1)}$ becomes
insignificant, whereas it remains significant in the LBE services sector. The same pattern holds when $k = 2$. These results still support the LBE hypothesis, as firms remain more productive than prior to exporting. However, it seems that the learning effect is not progressive and more short-lived than in the manufacturing sector, reflecting underlying differences in competitiveness of market structures as established in section (2.3.4).

We now decompose the analysis by considering the two subgroups of switching firms separately. We display the results of the same regression with leads and lags for survivors in table (2.11) and for quitters in table (2.12). Consider first table (2.11). Except for the general services sector, all coefficients reflect LBE to the letter in that each post-entry period is associated with an increasing gain in productivity. Even if endogeneity in the entry year were present, we can confirm productivity gains in each subsequent year. In order to strengthen this result, we compare the lead and lag coefficients in table (2.13) and report the corresponding p-value of rejecting the null hypothesis of equality of coefficients. The first row tests for equality of the $t_{-1}$ coefficient with the contemporaneous coefficient $t_0$. In

We now decompose the analysis by considering the two subgroups of switching firms separately. We display the results of the same regression with leads and lags for survivors in table (2.11) and for quitters in table (2.12). Consider first table (2.11). Except for the general services sector, all coefficients reflect LBE to the letter in that each post-entry period is associated with an increasing gain in productivity. Even if endogeneity in the entry year were present, we can confirm productivity gains in each subsequent year. In order to strengthen this result, we compare the lead and lag coefficients in table (2.13) and report the corresponding p-value of rejecting the null hypothesis of equality of coefficients. The first row tests for equality of the $t_{-1}$ coefficient with the contemporaneous coefficient $t_0$. In

### Table 2.11: Leads and Lags: Survivors

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Manufacturing</th>
<th>LBE Manufacturing</th>
<th>Services</th>
<th>LBE Services</th>
</tr>
</thead>
<tbody>
<tr>
<td>$t_{-1}$</td>
<td>-0.120</td>
<td>-0.053</td>
<td>-0.088</td>
<td>-0.147</td>
<td>-0.115</td>
</tr>
<tr>
<td></td>
<td>(0.026)</td>
<td>(0.442)</td>
<td>(0.448)</td>
<td>(0.065)</td>
<td>(0.338)</td>
</tr>
<tr>
<td>$t_0$</td>
<td>0.132</td>
<td>0.158</td>
<td>0.305</td>
<td>0.087</td>
<td>0.286</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.001)</td>
<td>(0.359)</td>
<td>(0.029)</td>
</tr>
<tr>
<td>$t_{+1}$</td>
<td>0.208</td>
<td>0.246</td>
<td>0.308</td>
<td>0.178</td>
<td>0.305</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.029)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>$t_{+(2)}$</td>
<td>0.248</td>
<td>0.315</td>
<td>0.404</td>
<td>0.104</td>
<td>0.341</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.225)</td>
<td>(0.002)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Manufacturing</th>
<th>LBE Manufacturing</th>
<th>Services</th>
<th>LBE Services</th>
</tr>
</thead>
<tbody>
<tr>
<td>firm controls</td>
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<td>yes</td>
<td>yes</td>
<td></td>
<td></td>
</tr>
<tr>
<td>firm fe</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td></td>
<td></td>
</tr>
<tr>
<td>year fe</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$N$</td>
<td>88691</td>
<td>14917</td>
<td>7901</td>
<td>56567</td>
<td>26091</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.273</td>
<td>0.197</td>
<td>0.184</td>
<td>0.266</td>
<td>0.334</td>
</tr>
</tbody>
</table>

Standard errors are clustered on the firm level. p-values in parentheses

---

23 Results are not reported
Table 2.12: Leads and Lags: Quitters

<table>
<thead>
<tr>
<th></th>
<th>All Manufacturing</th>
<th>LBE Manufacturing</th>
<th>Services</th>
<th>LBE Services</th>
</tr>
</thead>
<tbody>
<tr>
<td>$t_{-1}$</td>
<td>0.053 (0.002)</td>
<td>0.036 (0.190)</td>
<td>0.080 (0.006)</td>
<td>0.062 (0.018)</td>
</tr>
<tr>
<td>$t_0$</td>
<td>-0.040 (0.08)</td>
<td>0.018 (0.524)</td>
<td>0.059 (0.130)</td>
<td>-0.062 (0.020)</td>
</tr>
<tr>
<td>$t_{+1}$</td>
<td>0.011 (0.560)</td>
<td>0.062 (0.034)</td>
<td>0.114 (0.002)</td>
<td>-0.027 (0.314)</td>
</tr>
<tr>
<td>$t_{+(2)}$</td>
<td>-0.016 (0.383)</td>
<td>0.038 (0.211)</td>
<td>0.087 (0.013)</td>
<td>-0.055 (0.949)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th>firm controls</th>
<th>firm fe</th>
<th>year fe</th>
<th>N</th>
<th>R²</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td></td>
<td></td>
<td>130224</td>
<td>0.159</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>36419</td>
<td>0.105</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>19918</td>
<td>0.106</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>71040</td>
<td>0.167</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>32579</td>
<td>0.200</td>
</tr>
</tbody>
</table>

Standard errors are clustered on the firm level. p-values in parentheses

Subsequent rows, we test with respect to the other two post-export (post exit from export) coefficients. In the case of both Switchers and Survivors, we can reject the null hypothesis of pairwise equality of coefficients. Looking at quitters in table (2.12), the exit from exporting does not seem to be strongly related with systematic productivity effects across subsectors. The last period of exporting $t_{-1}$ does indicate higher productivity, but the periods after exit do not display a consistent and significant pattern, notably across sectors. It is important to keep in mind that, analogously to the group of switchers, the group of quitters also comprises firms that start exporting again at a later point in time. By definition, however, the Quitter variable remains 1 once a firm exits from exporting.

Table 2.13: Testing equality of coefficients

<table>
<thead>
<tr>
<th></th>
<th>Switchers</th>
<th>Survivors</th>
<th>Quitters</th>
</tr>
</thead>
<tbody>
<tr>
<td>$t_{-1} - t_0 = 0$</td>
<td>0.0000 0.0000 0.0000</td>
<td>0.0000 0.0021 0.0124</td>
<td>0.0000 0.5187 0.0000</td>
</tr>
<tr>
<td>$t_{-1} - t_{+1} = 0$</td>
<td>0.0000 0.0000 0.0000</td>
<td>0.0000 0.0000 0.0000</td>
<td>0.0020 0.3448 0.0014</td>
</tr>
<tr>
<td>$t_{-1} - t_{+(2)} = 0$</td>
<td>0.0000 0.0000 0.0741</td>
<td>0.0000 0.0000 0.0100</td>
<td>0.0002 0.9407 0.0000</td>
</tr>
</tbody>
</table>
2.4 ROBUSTNESS CHECKS

2.4.1 Marginal Labor Productivity

Our analysis has consistently focused on average productivity effects. Here we estimate a simple production function to check for differential marginal productivity effects of entry into exporting. We therefore need an employment variable that captures employment before having exported and after having done so for the first time, analogously to our previous analysis. To obtain this, we generate a nonstarter variable that takes the opposite values of our starter variable and is hence 1 for any firm that does not export or has not done so yet, and 0 else. We interact both the starter and the nonstarter variable with firm employment, take logs and estimate the following production function again by fixed-effect estimation:

\[ \ln V_{Ai} = \alpha + \beta_1 \text{nonstarter}_{it} + \beta_2 \text{starter}_{it} + \gamma \text{cap}_{it} + \pi_{year} + \lambda_i + \epsilon_{it} \quad (2.4) \]

where \( \ln V_{Ai} \) is log value-added and \( \text{cap}_{it} \) is a set of dummies we create to proxy for capital that we do not observe. In fact, at each survey, firms are asked to rate the state of their technical equipment on a scale from 1 to 5, where 1 is the best. Creating four dummies for each score other than the worst will hence give us a vague indication of a firm’s capital intensity, which may proxy for the capital stock in a production function.

The results displayed in table (2.14) are broadly consistent with our earlier findings. Throughout the subsamples we use for our analysis, we find that the output elasticity of employment is higher once firms have begun to export (the coefficient on variable starter). Looking at sectoral differences, we find an increase of 3 (4.5) percentage points in the manufacturing sector (LBE manufacturing), whereas this increase is 0.6 (2) percentage points in the (LBE) services sector. Unreported results for a regression over switching firms only yield even higher differences in all sectors. The full set of
Table 2.14: Output Elasticities of Employment

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Manufacturing</th>
<th>LBE Manufacturing</th>
<th>Services</th>
<th>LBE Services</th>
</tr>
</thead>
<tbody>
<tr>
<td>starterl</td>
<td>0.689 (0.0000)</td>
<td>0.784 (0.0000)</td>
<td>0.755 (0.0000)</td>
<td>0.620 (0.0000)</td>
<td>0.629 (0.0000)</td>
</tr>
<tr>
<td>nonstarterl</td>
<td>0.668 (0.0000)</td>
<td>0.754 (0.0000)</td>
<td>0.710 (0.0000)</td>
<td>0.614 (0.0000)</td>
<td>0.609 (0.0000)</td>
</tr>
<tr>
<td>cap1</td>
<td>0.196 (0.0004)</td>
<td>0.092 (0.2708)</td>
<td>0.045 (0.7283)</td>
<td>0.237 (0.0338)</td>
<td>0.360 (0.2537)</td>
</tr>
<tr>
<td>cap2</td>
<td>0.192 (0.0005)</td>
<td>0.076 (0.3554)</td>
<td>0.040 (0.7582)</td>
<td>0.236 (0.0347)</td>
<td>0.267 (0.2447)</td>
</tr>
<tr>
<td>cap3</td>
<td>0.157 (0.0043)</td>
<td>0.050 (0.5382)</td>
<td>0.017 (0.8978)</td>
<td>0.205 (0.0654)</td>
<td>0.330 (0.294)</td>
</tr>
<tr>
<td>cap4</td>
<td>0.057 (0.2912)</td>
<td>-0.021 (0.7884)</td>
<td>-0.056 (0.6534)</td>
<td>0.119 (0.2825)</td>
<td>0.227 (0.4646)</td>
</tr>
<tr>
<td>firm fe</td>
<td>yes</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>year fe</td>
<td>yes</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>122541</td>
<td>24997</td>
<td>13459</td>
<td>74371</td>
<td>33600</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.085</td>
<td>0.101</td>
<td>0.107</td>
<td>0.072</td>
<td>0.100</td>
</tr>
</tbody>
</table>

Regressions over domestic and switching firms only. P-values in parentheses.

dummies for capital intensity yields statistically significant and economically reasonable results only when regressing over the entire set of firms and in part for the services sector.

2.4.2 Mark-ups and post-export domestic expansion

As discussed above, our approach is inherently prone to productivity mismeasurement, notably due to the lack of data on capital stock. One way to check for the implications of this omission is to check for post-export domestic expansion, as proposed by Lileeva and Trefler (2010). The reasoning is as follows: If we were to pick up higher measured productivity through higher prices fetched in foreign markets and hence charging higher mark-ups, then - absent underlying differences in TFP performance - this would cause exporters to lose customers domestically. In this context, De Loecker and Warzynski (2012) find evidence for increasing mark-ups with export entry of Slovenian firms that may hence be mistaken for productivity improvements, if not properly accounted for. However, within exporters, they
do not find significant differences between mark-ups charged domestically and abroad. This is important for the validity of our robustness check, because it suggests that the intuition developed by Lileeva and Trefler (2010) holds, since firms do not apply different mark-ups at home and abroad.

**Table 2.15: Domestic Sales Expansion**

<table>
<thead>
<tr>
<th></th>
<th>Switchers</th>
<th>Quitters</th>
<th>Survivors</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>a</td>
<td>b</td>
<td>c</td>
</tr>
<tr>
<td>starter</td>
<td>1.456</td>
<td>1.112</td>
<td>0.457</td>
</tr>
<tr>
<td>qitter</td>
<td>(0.0000)</td>
<td>(0.0000)</td>
<td>(0.0000)</td>
</tr>
<tr>
<td>industry fe</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>year fe</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>180877</td>
<td>153791</td>
<td>37376</td>
</tr>
<tr>
<td>R²</td>
<td>0.212</td>
<td>0.170</td>
<td>0.120</td>
</tr>
</tbody>
</table>

(a) All firms, (b) No international firms, (c) only subgroup under consideration (Switchers, Quitters, Survivors). Standard errors are clustered on the firm level and p-values are given in parentheses.

Table (2.15) displays the result across the now familiar control groups, of a regression of log domestic sales on the Starter and Quitter variable respectively. Our switchers (and notably survivors) have increased their domestic market sales relative to non-exporters, as well as relative to their pre-export levels. This result is inconsistent with rising mark-ups, but consistent with higher TFP. It suggests that switchers have indeed gained in TFP and were therefore able to outperform their domestic competitors. The rise in productivity we observe is also reflected in a rise in domestic sales across all subsamples, which strengthens our confidence in the validity of our estimations and our productivity measures.

### 2.4.3 Propensity Score Matching

Finally, our last robustness check addresses the possibility of having chosen inadequate control groups, addressing the selection problem in our analysis from a different angle. Recall that in most of our specifications, we compare the effect of export entry (exit from exporting) relative to non-
exporters. However, this comparison may not be valid if non-exporters display fundamentally different key characteristics. By employing firm-level fixed effects, we do account for time-invariant heterogeneity across firms. However, a propensity score matching approach based on Heckman et al. (1997) allows us to match exporters with non-exporters on observable characteristics, which helps us proxy for the counterfactual of what an exporter’s characteristics would be had it not started exporting. The average treatment effect we seek to uncover is given by:

$$E[prod^1_{it} - prod^0_{it}|Exporter_{it} = 1] = E[prod^1_{it}|Exporter_{it} = 1] - E[prod^0_{it}|Exporter_{it} = 1]$$ (2.5)

where $prod^0_{it}$ and $prod^1_{it}$ stand for our measure of labor productivity before and after entry into (exporting from) exporting respectively. The variable $Exporter_{it}$ equals 1 for all switchers both before and after their entry into, or exit from exporting, so as to allow comparisons at intervals $t < 0$, where our standard Starter and Quitter variables would take on the value of zero. We do not consider international firms for this analysis. The unobservable last term, the counterfactual for each firm, is hence proxied with $E[prod^0_{it}|Exporter_{it} = 0]$, where the corresponding control firm is matched based on the nearest neighbor in terms of its propensity score. We estimate the latter for each time interval, so as to account for the changing composition of firms that is the result of having an unbalanced panel and our rescaled time variable as per 2.2.2. Our matching variables are the same that we use as controls throughout our regressions, but we match within each industry as per 2.2.1 only, so as to maximize comparability in terms of heterogeneous industry-level export potential.24 The latter is warranted by the diverging propensities of exporting we observe empirically across industries. The average treatment effects for each group of firms in each interval is displayed in table (2.16).

---

24 Sectors "Households" and "Extra-territorial Organizations" cannot be considered due to insufficient numbers of observations.
Table 2.16: Average Treatment Effects

<table>
<thead>
<tr>
<th></th>
<th>-2</th>
<th>-1</th>
<th>0</th>
<th>1</th>
<th>2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Switchers</td>
<td>-0.002</td>
<td>0.001</td>
<td>0.247</td>
<td>0.190</td>
<td>0.180</td>
</tr>
<tr>
<td></td>
<td>(0.970)</td>
<td>(0.970)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Survivors</td>
<td>-0.084</td>
<td>-0.031</td>
<td>0.110</td>
<td>0.346</td>
<td>0.408</td>
</tr>
<tr>
<td></td>
<td>(0.403)</td>
<td>(0.464)</td>
<td>(0.013)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Quitters</td>
<td>0.148</td>
<td>0.227</td>
<td>0.009</td>
<td>0.102</td>
<td>0.073</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.808)</td>
<td>(0.000)</td>
<td>(0.011)</td>
</tr>
</tbody>
</table>

P-values are given in parentheses

Altogether, the results from matching corroborate our previous results in 2.3.5 quite neatly. Pre-entry differences are insignificant for both Switchers and Survivors, whereas they become significant at the year of entry. For Survivors, the difference is increasing over time. For Quitters, the differences are significant and increasing prior to exit ($t < 0$), but become inconsistent thereafter.

2.5 CONCLUSION

Our study has revisited the self-selection vs learning-by-exporting debate using detailed data on German firms across all economic sectors. We have exploited variation within and across firms of entering into exporting to gauge whether firms self-select into exporting through higher pre-exporting productivity levels and/or whether firms upgrade their productivity prior to or after entry into exporting. We have also investigated the channels through which productivity effects may occur. We find that future exporters do display higher productivity levels than firms that never export, lending strong support to the self-selection hypothesis. However, average pre-exporting productivity levels remain relatively constant up to entry into exporting, upon which point we register strong increases in productivity. These productivity gains in turn lend strong support to the learn-
ing by exporting hypothesis, in that productivity growth picks up only after entry into exporting. This effect is stronger for manufacturing firms than for services firms, in that the former exhibit persistent growth in productivity past entry into exporting, whereas this effect is limited in time (2 years on average) for services firms. In contrast, we find strong evidence in favor of the hypothesis when considering continuing exporters across both sectors. We also find that not all sectors display this effect to the same extent. In fact, we have identified a number of subsectors in both manufacturing and services, in which learning by exporting holds, while this effect is not significantly present in others. We explain the different performances of the manufacturing and services sector with significant inherent differences in average propensities to export, which are substantially lower for the services sector. Furthermore, we are able to show that across sectors the size of the LBE effect depends on the level of within-sector competition. In line with basic microeconomic theory, productivity gains are higher for entrants into exporting, which operate in relatively uncompetitive domestic sectors, pointing to an important competitiveness channel for increased productivity through LBE. Moreover, we explain the lower scope for LBE effects in the services sector by uncovering substantially more restrictive access to foreign markets in that sector, which effectively maintains export revenues in only few firms.

Our results are robust to different specifications and, importantly, the overall productivity gains we find are on average not labor-saving, but rather generate increased demand for workers, while basic metrics of working conditions such as the share of temporary and part-time work and average wages do not display particular changes in trend. While we do not investigate policy measures per se, it is safe to conclude from our work that policies aiming at increasing market access may be particularly beneficial for relatively uncompetitive domestic sectors, in terms of productivity gains and employment generation. Notably the services sector displays large asymmetries in available access to foreign markets, which directly
translates into lower export-induced productivity gains. While we can make informed statements about the extent of barriers to market access, our data does not allow us to identify their nature. Given the increasing importance of the services sector in generating value-added and employment, further research to highlight what policies contribute to lowering these barriers to foreign market access is of key importance.
REFERENCES


RIETI Discussion Paper, 15-E-053.


LABOUR MARKET EFFECTS OF CURRENCY APPRECIATION: THE CASE OF SWITZERLAND

ABSTRACT

The labor market effects of international trade in developed economies are the subject of intense public scrutiny and a major argument of anti-globalization movements. However, insights on these effects have been drawn almost exclusively on the basis of the manufacturing sector, whose decline is a stylized fact of structural economic transformation. This study contributes to filling this gap by examining the broader effects of external exposure on the labor market in Switzerland. We find strong evidence for three channels of antagonistic employment effects of currency appreciation: negative employment growth induced by increasing export uncompetitiveness and higher import competition, and positive employment growth induced by cheaper availability of foreign inputs. Importantly, the latter channel appears to trump the former two, and the Swiss economy appears to successfully have hedged itself through integration in global value chains. Our findings hence make a strong case for open trade policy and corroborate recent insights on the implications of global value chains for trade policy.
3.1 INTRODUCTION

After then US presidential candidate Ross Perot warned of a "giant sucking sound" in 1992, referring to the loss of jobs to Mexico through the North American Free Trade Agreement (NAFTA), the conclusion of NAFTA and the establishment of the World Trade Organization in the same decade rather pointed towards an emerging consensus about the mutual benefits of international trade. Few empirical evidence existed at the point to seriously counter that view. With empirical work necessarily lagging current developments, the tides changed significantly in the following decades. Evaluating the Canada-US Free Trade Agreement, Trefler (2004) ascribed the loss of 100,000 jobs (5%) to Canadian tariff concessions through the agreement, or 1 in 8 jobs in those industries most impacted by increased import competition. These numbers are high, especially given that trade liberalization occurred between two industrialized countries, which theoretically should have little disruptive effects on labor markets (Krugman (2008)). Indeed, the negative aggregate employment effects predicted for NAFTA could not be corroborated through subsequent empirical evidence, even though important distributional effects do show up across industries and regions (Hakobyan and McLaren (2016)). In an early study on low-wage country import competition including China as its biggest member, but using data that predates China’s accession to the WTO, Bernard et al. (2006) associate import competition with a variety of plant-level adjustment mechanisms, including lower survival rates and employment growth. They estimate that import competition from these countries has accounted for 14% of the total 674,689 of employment losses observed in their sample of manufacturing plants during the period under study. A similar study with more recent data (1995-2007) by Auer et al. (2013) on the effects of low wage import competition, confirms similar results for European countries, linking these imports to a reduction of manufacturing employment in Germany, Italy, France, Sweden and the UK of the order of 10% (1.3 million workers in the considered industries). Generally, however, only a minority
of empirical studies has focused on aggregate employment effects of trade, emphasizing rather distributional issues that have been found to be pervasive (Harrison et al. (2011)).

Nevertheless, following China’s accession to the WTO and its subsequent surge in exports, a number of empirical studies reviewed by Autor et al. (2016) has since taken up the issue of rising import competition in the Chinese context and its labor market implications in terms of (negative) absolute employment effects. Pierce and Schott (2016) link the significant decline in US manufacturing after 2000 specifically to changes in US trade policies towards China on the industry level, Autor et al. (2014) find evidence for adjustment costs on the worker level, and Ebenstein et al. (2014) - without singling out China - identify negative employment effects of import competition and offshoring on the occupational level. Iacovone et al. (2013) relate Chinese import competition to increased firm exit in Mexican manufacturing and Bloom et al. (2016) associate employment losses in European low-tech firms, basing their analysis on the AMADEUS database. Our study relates most closely to studies that use geographical units as units of analysis and draws notably on the methodology developed in Autor et al. (2013 a,b) to transpose industry level data on the regional level, depending on initial industrial structure. We depart from these studies in four important and interrelated ways.

First, we are the first study to undertake a similar analysis of Switzerland. The main focus of existing studies on rising import competition may be warranted by the specific US case, as the surge in bilateral trade with China was largely unidirectional. The results of Autor et al. (2013 a,b), however, do not necessarily hold in other countries. Balsvik et al. (2015) find the impact of the China shock on the Norwegian manufacturing sector to be only half as important as in the US. Dauth et al. (2014), find positive aggregate employment gains from trade relations with China that are driven by increased export opportunities, while finding evidence for
employment losses in import-competing regions. Analyzing local labor markets in Spain, Donoso et al. (2015) find no significant association between exposure to Chinese imports and unemployment altogether.

Second, we attempt to capture a broader set of channels, through which trade may have an effect on employment and include all three channels explicitly in the main analysis. Autor et al. (2013 a) take different channels into account in robustness checks. They respectively incorporate exports directly into their measure of import competition in one step and also redefine their measure of import competition net of intermediate inputs in another, basing the calculation on the US input-output table of 1992, and - given data constraints - assuming identical external structure of the domestic input-output relationships. This definition of intermediate inputs, however, is still based on gross trade in goods statistics (COMTRADE). In a world of global value chains, the effect of intermediates may be better captured in terms of trade in tasks (Grossman and Rossi-Hansberg (2008)), whose greater availability through trade may have important effects that a focus on traditional datasets cannot account for. In fact, identification in the baseline specifications is based on the assumption of imbalanced trade between the US and China. Koopman et al. (2014) point out that value-added decomposition shows that a significant portion of China’s trade surplus to the US in gross terms reflects indirect value-added exports that China does on behalf of Japan, Korea and Taiwan. Recent evidence suggests that China’s trade surplus with the United States was actually one-third smaller in 2011 when measured in value-added terms than in gross terms (OECD-WTO (2015)).

This brings us to our third point of departure, which is the explicit consideration of the services sector, as well as its trade relations. Building on Acemoglu et al. (2012), Acemoglu et al. (2016) effectively augment the approach in Autor et al. (2013 a,b) to account for up- and downstream

linkages, exploiting the domestic input-output structure. They find that
the negative employment effects are thereby magnified, as they propagate
through domestic linkages with the rest of the economy. While this ap-
proach certainly refines the domestic interlinkages, it still relies on defining
international trade on the industry level solely through correspondence ta-
bles with internationally traded goods and commodities, which effectively
classifies the entire service sector as non-tradable. According to the latest
numbers released by the BEA, the value of US service imports was 23% of
goods imports and 52% of goods exports in 2016, running a surplus in
services trade (BEA (2017)). The focus on goods trade does not allow to
account for services trade that manufacturing industries engage in either.
For example, we know that services trade, goods trade, the manufacturing
sector, and the services sector are closely intertwined. For Germany, we
know that manufacturing firms account for almost 25% of services exports
(Kelle and Kleinert (2010)). On the other hand, 14% of services firms in
Denmark appear to be exporting goods (Malchow-Møller et al. (2015)). We
address this issue by using the OECD (2015) Inter-Country Input-Output
database, which allows for the indiscriminate identification of trade flows
across intermediate and final consumption sectors across all industries and
60 countries.

Fourth, and finally, we exploit recent exogenous exchange rate move-
ments to identify trade-induced shocks, using detailed employment data
on the entirety of Swiss firms. The Swiss Franc appreciation is a rare op-
portunity to do so, given the paucity of natural experiments in international
trade. It has appreciated considerably against major foreign currencies over
the past decade, notably from 2008 onwards (figure (3.1)). While the Swiss
industrial structure plays an important role in the long term tendency of
the Swiss Franc to appreciate, the recent appreciation has been the result
of the Swiss Franc’s "safe haven" status during the financial turmoil of the
late 2000s (Baltensperger and Kugler (2016)). This largely exogenous varia-

Pierce and Schott (2016) also account for input-output linkages, albeit in a different empirical
set-up that is based on double and triple differencing. They also make use of customs data,
registering trade in goods, to identify trade intensities.
tion in the CHF’s value is key to our identification strategy. Its continued appreciation has raised fears about loss of international competitiveness and resulting losses for Swiss exporters, which has prompted the Swiss National Bank to impose a currency floor against the Euro in 2011.27 Our study aims to take a broader view on the implications of this CHF appreciation, in terms of the various channels that link it to domestic labor outcomes.

Figure 3.1: Bilateral nominal exchange rates, 1999=100

Source: UNCTAD Stat. Measured as foreign currency per unit of CHF

As such, our study also relates to the general literature examining labor market effects of currency fluctuations, such as Campa and Goldberg (2001), Goldberg and Tracy (2000), Nucci and Pozzolo (2010), Nucci and Pozzolo (2014), Huang and Tang (2015). We notably draw on the methodology developed in Campa and Goldberg (2001) to reflect industry exposure to trade simultaneously through three transmission channels, namely industry import penetration, export orientation, and imported input use.

27 see e.g. http://www.nzz.ch/meinung/kommentare/jetzt-ist-die-politik-gefordert-1.18632980
Also following Campa and Goldberg (2001), we interact these variables with a measure of industry-specific exchange rate fluctuations whose weights are determined by the the trade intensities along each of the three transmission channels. Our main contribution lies in establishing a novel nexus with the approach followed in Autor et al. (2013 a,b), transposing the industry specific channels on the municipal level in Switzerland, and augmenting it to using international input-output data to take comprehensive account of all trade relations between industries of all sectors. Moreover, rather than focusing solely on initial exposure, we account for variation in exposure across time in a panel-like structure, taking the evolving composition of national industrial structure explicitly into account.

Our study hence also relates to the Swiss-specific studies that have recently investigated the effects of CHF fluctuations on Swiss economic performance in a number of dimensions. Using the KOF enterprise survey, Lassmann (2013) finds a negative impact of the CHF appreciation on firm profits. From an economic perspective, a temporary currency appreciation may even have permanent adverse impacts on exports. This “hysteresis effect” has been studied by Fauceglia et al. (2015). At the same time, a high level of integration into global value chains (GVCs) could potentially mitigate these negative effects by simultaneously rendering foreign inputs cheaper. This “natural hedging” has been confirmed in Fauceglia et al. (2014, 2015). Relative export resilience may also be explained by very low price elasticities of major Swiss exports such as pharmaceuticals and machinery, as suggested by Auer et al. (2011). However, Auer and Sauré (2012) caution that the failure to account for the simultaneous rebound of global demand and the strong Franc appreciation may lead to the wrong assumption that exchange rate fluctuations matter little for Swiss export performance. To the best of our knowledge, the only study that explicitly looks at the link between Swiss Franc fluctuation and employment outcomes is Kaiser and Siegenthaler (2016). Using a panel of Swiss manufacturing firms based on the KOF innovation survey, the authors find that an appreciation
increases high-skilled and reduces low-skilled employment in most firms, though the aggregate impact of the Swiss Franc appreciation seem to be muted in their findings.

Our empirical strategy is informed by the theoretical predictions of Campa and Goldberg (2001). Conceptually, there are three ways in which the exchange rate may impact firms and hence workers. First, an appreciation makes domestic firms less competitive, which may translate into job losses within export-oriented firms. Appreciation makes domestic products more expensive relative to foreign products, leading to a decline in demand for domestic products and labour. On the import side, predictions depend on whether imported inputs are substitutes or complements to domestic labour. The former case occurs in import-competing sectors. In this case, an appreciation of the CHF makes imports of foreign goods cheaper, which ultimately results in the downsizing of these firms’ workforces. In contrast, firms that rely heavily on foreign inputs may expand operations as their relative prices fall when the domestic currency appreciates, resulting in an increase in the demand for labour.

We find strong evidence for all three channels of antagonistic employment effects of currency appreciation: negative employment growth induced by increasing export uncompetitiveness and higher import competition, and positive employment growth induced by cheaper availability of foreign inputs. Importantly, the latter channel appears to trump the former two, and the Swiss economy appears to successfully have hedged itself through integration in global value chains. In highlighting the importance of the intermediate input channel, our findings make a strong case for open trade policy and complements recent studies such as Blanchard et al. (2016), who make a similar case by looking at the effects of tariffs on final goods in a world of global supply chains.

Section 3.2 will offer a brief link between the Campa-Goldberg model sketched in 3.2.1 and our estimation strategy previewed in 3.2.2. Section
3.3 gives a detailed account of the datasets used in our study, as well as the construction of variables. Section 3.4 presents the main results of our study and we run robustness checks in section 3.5, before concluding in section 3.6.

3.2 CONCEPTUAL FRAMEWORK AND PREVIEW

3.2.1 The Campa-Goldberg model

The theoretical setup that motivates our empirical strategy has been developed by Campa and Goldberg (2001) and therefore, we focus only on the essential part of the model. The mechanisms through which exchange rates may impact industry level employment root in a simple framework, where labor demand is obtained from the first order conditions for a representative firm’s profit maximization problem, who sells domestically and abroad and sources intermediate inputs domestically and abroad. There are hence three potential sources of shocks a firm might face: through aggregate domestic demand, aggregate foreign demand and through the exchange rate. They derive an expression for the elasticity of equilibrium employment $\tilde{L}$ with respect to the exchange rate $x_{rate}$ (defined as domestic currency per unit of foreign currency) of the following form:

$$\frac{\partial \tilde{L}_t}{\partial x_{rate_t}} \frac{x_{rate_t}}{L_t} = \lambda_t[(1 + \eta^{-1})\gamma \times me_{ct}$$

$$+ (1 + \eta^*-1) \times xe_t$$

$$- (\partial Q_t / \partial Z_t^*)^{-1} \times me_{it}]$$

where $\lambda_t$ and $\gamma$ cover industry-specific characteristics, $\eta$ and $\eta^*$ are domestic and foreign product elasticities facing producers in their industries and $(\partial Q_t / \partial Z_t^*)$ is the marginal product of the foreign input $Z^*$. Equation (3.1) clearly highlights the three channels through which optimal labor de-
mand is exposed to exchange rate movements and which we will focus our analysis on. All else equal,

1. greater import penetration of domestic markets \((mec_i)\) raises the sensitivity of labor demand to exchange rates

2. greater export-orientation \((xe_i)\) raises the sensitivity of labor demand to exchange rates

3. greater reliance on imported inputs \((mei_i)\) can offset or even reverse the adverse consequences of a stronger currency (for example) on industry labor demand

3.2.2 Estimation

Based on the predictions of the Campa-Goldberg setup, which hold on the industry level, we will transpose its implications on the Swiss municipality level \((c)\), drawing on the methodology of Autor et al. (2013 a,b) that exploits initial industrial structures that vary across municipalities. We will discuss this transposition extensively and in great detail throughout section 3.3, but for the sake of readability, we already outline the broad steps. First, as equation (3.1) suggests, the impact of exchange rate movements will impact industries differentially. The effects of exchange rate fluctuation will be magnified according to industries’ level of exposure to the three channels we have identified, which should be picked up empirically in simple interaction terms. Unlike Campa and Goldberg (2001) and Nucci and Pozzolo (2010) who were not able to disentangle import penetration and reliance on foreign inputs, the richness of our dataset yields sufficient variation that allows us to include both import channels in our analysis (see figure (3.3), which makes it more consistent with equation (3.1). Second, noting that a common exchange rate fluctuation would be observed by all industries, we recognize that each industry’s trading relations - be they exporting, import competition, or sourcing of intermediate inputs
- are not equally distributed geographically (see also Goldberg and Tracey (2010)). We hence identify each industry’s exposure along each channel with a trade-weighted, industry-specific exchange rate that takes due account of the importance of each trading partner and its bilateral exchange rate and define it in accordance with our data sources and standard empirical convention - unlike in 3.2.1 - in terms of foreign currency per unit of Swiss Franc, i.e. \(1/xrate\). The construction of this industry- and transmission channel-specific exchange rate measure allows us to include it directly in our econometric analysis, with the predicted signs of its coefficients directly flowing from 3.2.1. Third, we will transpose all industry-level variables onto the municipal level, reflecting the local industrial structure in doing so. This strategy of scaling industry level observations onto the municipal level generates strong correlations of the constructed variables with municipality size. While we discuss the implications more deeply in 3.4 and notably 3.4.3, we stress at this point that, while affecting the goodness of fit of our regressions, the signs of the coefficients we obtain should not be affected.

A such, the empirical counterpart of equation (3.1) is strongly reminiscent of the estimation equation derived by Campa and Goldberg (2001) and Nucci and Pozzolo (2010), but is adapted to the municipal level \(c\) as the unit of analysis, instead of industry \(i\):

\[
\Delta L_{c,t} = \beta_0 + (\beta_{1x} + \beta_{2x}xe_{c,t}) \Delta e_{c,t}^x + (\beta_{1mi} + \beta_{2mi}mei_{c,t}) \Delta e_{c,t}^{mi} + (\beta_{1mc} + \beta_{2mc}mec_{c,t}) \Delta e_{c,t}^{mc} + \beta_1 \Delta GDP^c_{c,t} + \gamma_{t} + \gamma_{c} + e_{c,t}
\]
Our dependent variable takes the form of period changes in employment per Swiss municipality \(c\) and will be subject to different definitions in 3.4. We include municipality-level or cantonal fixed effects \(\gamma_c\), depending on the specification, as well as time fixed effects \(\gamma_t\) to control for idiosyncratic shocks at each point in time that are assumed to equally affect all industries, such as domestic demand.

\(x_{c,t}, mei_{c,t},\) and \(mec_{c,t}\) denote the degree of export orientation, reliance on foreign inputs and import competition respectively for each Swiss municipality. In order to avoid simultaneity issues, they are lagged by one period. Similarly, \(\Delta e_{x_{c,t}}, \Delta e_{mei_{c,t}}\) and \(\Delta e_{mec_{c,t}}\) are municipality-specific trade weighted exchange rate changes that municipalities face depending on their industry composition. A trade weighted measure of foreign GDP change \(\Delta GDP*_{ct}\) serves as a municipality-specific gravity measure to correct for the intensity of trade relations that arises from economic strength.

As discussed in 3.2.1, our theoretical setup predicts negative coefficients \(\beta_{1x}\) and \(\beta_{2x}\), as well as \(\beta_{1mc}\) and \(\beta_{2mc}\). \(\beta_{1x}\) captures the export-specific exchange rate effect, whereas \(\beta_{2x}\) captures its interaction with export exposure of the municipality, which we expect to magnify the effect observed in \(\beta_{1x}\). The same reasoning allows us to interpret the other coefficients, but we expect the coefficient on the second block to be positive, as a positive employment effect through the availability of cheaper inputs is expected to prevail in those municipalities relying heavily on foreign intermediate goods.

Unlike Campa and Goldberg (2001), who find significant effects only for wages, we find evidence for significant sensitivity effects of exchange rate movements with respect to employment. These results are consistent with theories that predict more sluggish responses of employment to trade shocks (Artuç et al. (2010)). At least two factors may hence explain our results: 1) We allow for adjustment between time periods in our panel (3-4 years) and 2) we exploit a more significant exchange rate shock. Further issues that may have a bearing on the inter-temporal variation that we pick
up and that may drive our results are taken up in 3.5.

3.3 EMPIRICAL METHODOLOGY

3.3.1 Data

Our identification strategy requires detailed information about firms’ exposure to exports, foreign sourcing and import competition. Unfortunately, no single Swiss data source provides sufficient information for our purposes. We hence need to make use of various data sources in order to construct accurate measures that reflect municipality level heterogeneity along those three lines. We will discuss the various data sources and how we use them in this section.

Swiss employment according to the BZ and STATENT

We are interested in the micro employment and industrial structure of the Swiss economy and hence our main datasets are the STATENT dataset and the Swiss business census (BZ), which is its predecessor. These datasets contain establishment-level data on the universe of Swiss firms and establishments, notably total employment, full-time equivalents (FTE), detailed location and industry classification of main economic activity (NOGA), as well as juridical form. The years of interest for the BZ dataset are 2001, 2005 and 2008, which is the last year before the strong and persistent CHF appreciation. We propose to make use of the STATENT dataset for the years 2011 and 2014. There are important methodological differences in the construction of the BZ and STATENT, which we need to account for. The main difference is that STATENT relies on administrative data, while BZ data comes from compulsory business censuses. The BZ has a somewhat higher threshold for registering employees and establishments (all employees that work at least 6 hours a week and all establishments in which at least 20
hours per weeks are being worked) than STATENT (all employees and establishments subject to social security contributions).

**Figure 3.2: Share of FTE and total FTE when \( FTE \geq 0.5 \)**

![Graph showing share of FTE and total FTE over years]

Source: BFS STATENT

In figure (3.2), we plot different statistics of the entire database if we restrict the sample to firms with \( fte \geq 0.5 \). The dotted line is measured on the right scale and shows the evolution of Swiss FTEs over time. We observe a sharp increase that has started in 2008, i.e. with the methodology of the old BZ dataset. There is no obvious indication of a strong structural break when moving to the Statent methodology in 2011. The structural break, however, is visible when looking at shares, as reflected on the left axis. The dashed line tracks the evolution of the share of firms with \( FTE \geq 0.5 \) over the total number of firms registered in the respective datasets. This share drops significantly from over 95% in BZ to up to 85% in Statent, suggesting a higher sampling rate of micro-enterprises. However, in terms of FTE, the difference is rather marginal (the solid line), suggesting the presence of a large number of Swiss firms (15% of the total) that generate less than 2% of total full time equivalent employment. Figure (3.2) hence suggests that we can effectively correct for the sampling bias by restricting the sample to
those firms with at least 0.5 FTE.

When following municipalities over time, we face another challenge in that a large number of municipalities has undergone mutations over the time of observation. Most have been fusions, which we can track over time. BZ and STATENT each provide consistent identification codes for their respective datasets. However, in order to compare STATENT data with BZ data we, for now, simply work with time-consistent municipalities across both datasets, yielding a total of 2284 municipalities that comprise more than 97% of Swiss employment.

The economic sectors given follow the NOGA classification, which has undergone a substantial change in nomenclature in 2008. The NOGA08 codes are reported for all firms, while the old NOGA02 code is reported only until 2008. Since the OECD ICIO data that we will use comes in broad categories based on 2-digit ISIC Rev.3 classification, we need to obtain time consistent NOGA codes that indicate the sector of economic activity in conformity with the industry level data in ISIC Rev.3. As the latter is more aggregated (2-digit level) and we cannot disaggregate it without imposing strong assumptions, we rather build the concordance from the highly disaggregated Swiss NOGA data. In order to do so, we first aggregate each firm’s economic activity into the corresponding 2-digit NOGA08 code. We then compute the corresponding NOGA02 code using correlation tables obtained from the Federal Statistic Office, taking due account of transition probabilities when codes do not map into each other one-to-one.\textsuperscript{28} The conversion into NOGA02 is necessary, as NOGA02 is analogous to ISIC Rev. 3 on the two digit level, which allows us to obtain exact correspondences between employment and industry output and trade data. As the latter is aggregated into 33 distinct industries (excluding households), we follow the same methodology to map NOGA02 codes into the 33 industries used

\textsuperscript{28} The transition probabilities are calculated based on differences in reporting transitions in the year 2008, when some firms from the same previous NOGA02 sector reported their transition into different NOGA08 sectors.
in OECD input-output tables. We provide the mapping into these industries in columns 1) and 2) of table (3.3.1).

Table (3.3.1) also displays a number of key summary statistics. On aggregate, Swiss employment has grown by 20.45% from 2001 to 2014. This aggregate growth, however, masks important structural changes on the industry level. In particular, the manufacturing sector (shaded) has shed labor relative to other sectors, as well as in absolute terms. While there were roughly 635’162 FTEs active in manufacturing in 2001, that number has been reduced by 2’779 to 632’383 FTEs in 2014. While its share in total employment was almost 20% in 2001, that share has been reduced to 16.5% in 2014 (The sums of columns 3 and 4 over the shaded area). In fact, the only manufacturing industries that have experienced positive growth (column 4), while contributing substantially to overall employment growth (column 5) are "food products, beverages and tobacco", "chemicals and chemical products", as well as "computer, electronic and optical equipment". These
are the usual suspects of the Swiss manufacturing scene and include local food giants, the pharmaceutical, as well as the emblematic Swiss watch industry.

On the other hand, employment growth has been driven almost entirely by the services sector. Without counting "electricity, gas and water supply" and "construction", the services sector has accounted for more than 95% of total employment growth in Switzerland. More than 33% of total employment growth has originated from "R&D and other business activities", of which most expansion has originated in the business service sector (NOGA02 code 74). This sector alone has expanded by 82% since 2001, adding 216'646 new FTE jobs to the economy, which corresponds to almost 7% of total employment in 2001. The second largest contribution to total employment growth (25%) has been in the "health and social work" sector, which has, however, grown less dynamically (56%). Except for postal and telecommunication, as well as hotels and restaurants, each services industry has contributed positively to employment growth.

While structural transformation from manufacturing into services is a common phenomenon in developed economies, analyses that study the relationship between trade and employment generally focus their attention solely on the manufacturing sector. Given the preceding discussion of employment trends in Switzerland, it should be clear that such studies paint only a partial picture of modern economic realities.

**Data on Swiss industry output, trade and bilateral exchange rates**

Both BZ and STATENT do not record any data on output or trade activity. We hence need to combine our dataset with external data. The way this is usually done in the literature, is through customs records, which generally maintain detailed records on products flowing into and out of a country. Alas, we believe that we cannot follow this approach for two main reasons:
First, customs record only data on trade in goods. The historical reason is that international goods exchanges were subject to tariff revenue, whereas services are generally not subject to tariffs or other types of duties. Nations hence had an intrinsic interest in maintaining detailed collections of trade data, which offered a wealth of information to researchers. Today, however, it is clear that trade in goods accounts for only part of international trade. As mentioned in the introduction, Swiss services trade amounts to roughly half of Swiss goods trade, according to Swiss balance of payments data. Simply excluding the services sector - which we have seen accounts for over 95% of employment gains in Switzerland - hence seems implausible.

Second, the product classification used to register goods does not have a direct correspondence with the classification of economic activity of the firm that participates in international trade. Using correspondence tables to map these products to the industry of origin, which are widely used in the trade literature and readily available from eg EUROSTAT, we can associate the entirety of Swiss traded products exclusively with manufacturing sectors, from which they originate. As such, we can use goods trade as a proxy for export exposure of manufacturing firms, as well as the degree of import competition. However, we would completely miss out on the channel of reliance on foreign inputs. Given that customs data is recorded in gross terms, as opposed to value added, this shortcoming substantially distorts any indicator that is measured in such a way. Moreover, we have reason to believe that the correspondence obtained through such tables is far from being exact. For example, when calculating the degree of export exposure as yearly total (manufacturing) industry exports as a share of industry gross output, as obtained from Swiss Value Added Tax data, indicators range from 0 to over 100 in some cases, while, per definition, that number cannot be greater than 1.

In order to work with a unified framework, and given the above-mentioned shortcomings of the standard approach in the literature, we decide to make use of the recent OECD Inter-Country Input-Output (ICIO) database (OECD,
which provides the basis for the joint WTO-OECD Trade in Value Added project. The ICIO data provide us with an excellent basis for our purposes. First, it comprises bilateral trade data for both trade in goods and services, for years that mainly overlap with our Swiss employment data. Second, the ICIO database differentiates 34 industrial activities, which we can map one-to-one to Swiss employment data, given the consistent use of ISIC Rev.3 classifications (see above). The sectors comprise all manufacturing and services industries and we are hence able to include the entirety of Swiss industries in the analysis. Third, unlike national input-output tables, ICIO provides for detailed inter-industry and final demand relationships across 61 economies, allowing us to construct very precise measures of industry-level exposure to changes in bilateral exchange rates, GDP and trade. We obtain bilateral data on Swiss trade with 60 countries, as well as one "Rest of the World" aggregate economy. As the construction of our indicators requires detailed information on industry- and country-specific weights, we decide to drop the aggregate economy in our analysis, as it combines all remaining countries with various levels of exchange rates and GDP into one. Concerning trade flows, this aggregate economy accounts for typically less than 7% of Swiss trade flows over the period of observation.

---

29 ICIO is not available for the year 2001. After verifying the consistency with Swiss Federal Customs trade data, we simply combine 2000 ICIO data with 2001 BZ data
30 We will not use the "Households" sector, as it is not producing, reducing the effective number of industries to 33
Table 3.1: Trade Intensities of ICIO Industries

<table>
<thead>
<tr>
<th>Industry</th>
<th>Export</th>
<th>Import</th>
<th>Inputs</th>
</tr>
</thead>
<tbody>
<tr>
<td>Agriculture, hunting, forestry and fishing</td>
<td>0.01</td>
<td>0.05</td>
<td>0.13</td>
</tr>
<tr>
<td>Mining and quarrying</td>
<td>0.12</td>
<td>0.13</td>
<td>0.11</td>
</tr>
<tr>
<td>Food products, beverages and tobacco</td>
<td>0.14</td>
<td>0.12</td>
<td>0.13</td>
</tr>
<tr>
<td>Textiles, textile products, leather and footwear</td>
<td>0.62</td>
<td>1.34</td>
<td>0.34</td>
</tr>
<tr>
<td>Wood and products of wood and cork</td>
<td>0.08</td>
<td>0.01</td>
<td>0.14</td>
</tr>
<tr>
<td>Pulp, paper, paper products, printing and publishing</td>
<td>0.20</td>
<td>0.07</td>
<td>0.17</td>
</tr>
<tr>
<td>Coke, refined petroleum products and nuclear fuel</td>
<td>0.30</td>
<td>4.53</td>
<td>0.17</td>
</tr>
<tr>
<td>Chemicals and chemical products</td>
<td>0.73</td>
<td>0.16</td>
<td>0.17</td>
</tr>
<tr>
<td>Rubber and plastics products</td>
<td>0.47</td>
<td>0.08</td>
<td>0.32</td>
</tr>
<tr>
<td>Other non-metallic mineral products</td>
<td>0.16</td>
<td>0.04</td>
<td>0.18</td>
</tr>
<tr>
<td>Basic metals</td>
<td>0.76</td>
<td>0.04</td>
<td>0.32</td>
</tr>
<tr>
<td>Textiles, textile products, leather and footwear</td>
<td>0.23</td>
<td>0.03</td>
<td>0.22</td>
</tr>
<tr>
<td>Machinery and equipment, nec</td>
<td>0.60</td>
<td>0.26</td>
<td>0.21</td>
</tr>
<tr>
<td>Computer, Electronic and optical equipment</td>
<td>0.53</td>
<td>0.18</td>
<td>0.20</td>
</tr>
<tr>
<td>Electrical machinery and apparatus, nec</td>
<td>0.38</td>
<td>0.13</td>
<td>0.28</td>
</tr>
<tr>
<td>Motor vehicles, trailers and semi-trailers</td>
<td>0.77</td>
<td>4.76</td>
<td>0.39</td>
</tr>
<tr>
<td>Other transport equipment</td>
<td>0.39</td>
<td>0.57</td>
<td>0.25</td>
</tr>
<tr>
<td>Manufacturing nec; recycling</td>
<td>0.68</td>
<td>0.51</td>
<td>0.27</td>
</tr>
<tr>
<td>Electricity, gas and water supply</td>
<td>0.15</td>
<td>0.02</td>
<td>0.08</td>
</tr>
<tr>
<td>Construction</td>
<td>0.00</td>
<td>0.02</td>
<td>0.14</td>
</tr>
<tr>
<td>Wholesale and retail trade; repairs</td>
<td>0.25</td>
<td>0.11</td>
<td>0.08</td>
</tr>
<tr>
<td>Hotels and restaurants</td>
<td>0.21</td>
<td>0.15</td>
<td>0.08</td>
</tr>
<tr>
<td>Transport and storage</td>
<td>0.28</td>
<td>0.20</td>
<td>0.11</td>
</tr>
<tr>
<td>Post and telecommunications</td>
<td>0.08</td>
<td>0.03</td>
<td>0.08</td>
</tr>
<tr>
<td>Financial intermediation</td>
<td>0.32</td>
<td>0.01</td>
<td>0.02</td>
</tr>
<tr>
<td>Real estate activities</td>
<td>0.02</td>
<td>0.01</td>
<td>0.01</td>
</tr>
<tr>
<td>Renting of machinery and equipment</td>
<td>0.15</td>
<td>0.02</td>
<td>0.11</td>
</tr>
<tr>
<td>Computer and related activities</td>
<td>0.00</td>
<td>0.01</td>
<td>0.04</td>
</tr>
<tr>
<td>R&amp;D and other business activities</td>
<td>0.03</td>
<td>0.00</td>
<td>0.05</td>
</tr>
<tr>
<td>Public admin. and defence; compulsory social security</td>
<td>0.03</td>
<td>0.00</td>
<td>0.03</td>
</tr>
<tr>
<td>Education</td>
<td>0.01</td>
<td>0.00</td>
<td>0.05</td>
</tr>
<tr>
<td>Health and social work</td>
<td>0.03</td>
<td>0.00</td>
<td>0.08</td>
</tr>
<tr>
<td>Other community, social and personal services</td>
<td>0.12</td>
<td>0.11</td>
<td>0.09</td>
</tr>
</tbody>
</table>

Table (3.1) summarizes key statistics of our ICIO trade data for Switzerland. The first column shows each industry’s export intensity, defined as total exports over total output. We observe the highest ratios in the manufacturing sector, where exports often consist of a majority of sales. The service sector is on average much less export oriented, but nevertheless displays substantial outward orientation in a few subsectors such as "Wholesale and retail trade and repairs", "Hotels and restaurants", "Transport and storage", as well as "Financial intermediation". The second column displays a measure of import competition, defined as Swiss final consumption expenditure abroad as a share of the respective industries’ total output. Very high shares are obtained in sectors that Switzerland does not have important industrial capacities (anymore) in, such as "Coke, refined Petroleum
products and nuclear fuel", "Textiles, leather and footwear", and "Motor Vehicles". The final column lists a measure of reliance on foreign inputs, defined as the expenditure on industry inputs purchased abroad over total industry output. On average, Swiss industries display a high level of reliance on foreign inputs, which may be due to its relatively small domestic market. Column 3 shows that all Swiss industries rely to varying degrees on foreign inputs, the service sector is no exception here.

Finally, we combine the information on trade partners from ICIO with data on bilateral exchange rates from UNCTADStat and on GDP from the World Development Indicator database of the World Bank. We complete lacking information on GDP for Taiwan from the Taiwanese National Statistics Office.

3.3.2 Construction of industry-level variables

Industry-level exchange rate measures

While we are interested in local heterogeneities, we first proceed by constructing industry-level measures. Given that different industries trade more or less intensively with different countries, we need an exchange rate measure that reflects this fact. Aggregate trade weighted exchange rate indices are not sufficient for our purpose, as they do not reflect the industry-specific exposure to foreign currencies. Building on Goldberg (2004), we propose to construct an industry-level exchange rate index of the following form:

\[
\Delta e^{i}_{t} = 100 * \sum_{j} \frac{x_{i,j,t-1} e_{j,t} - e_{j,t-1}}{e_{j,t-1}}
\]

where \(e_{j,t}\) is the bilateral nominal exchange rate between Switzerland and country \(j\) defined per unit of CHF, \(\Delta e^{i}_{t}\) is the export weighted exchange rate industry \(i\) is facing at time \(t\), taking into account its previous exposure to exchange rate fluctuations with country \(j\). The weighting term reflects
industry $i$’s trade exposure to country $j$, in this case in terms of its export portfolio. We weight by previous period trade shares, so as to avoid any contemporaneous correlation, as yearly trade flows may have adjusted for exchange rate fluctuations during the same year. Imports of consumption goods and inputs do not necessarily follow the same pattern. Depending on the product in question, these are likely to being sourced from countries other than the export destination. The calculation of the imported input weighted exchange rate change $\Delta e_{i,t}^{mi}$ follows the same pattern with imported input data. For the analogous calculation of $\Delta e_{i,t}^{mc}$, we calculate weights on the basis of imports undertaken by Swiss final demand sectors such as households and government purchases, instead of Swiss industry imports.

We plot the average exchange rate change across all time periods obtained for each industry in figure (3.3).

**Figure 3.3:** Average industry-specific exchange rate change

![Graph showing average industry-specific exchange rate change](image)

Source: Authors’ calculations

The dispersion of values within each industry is key to our identification strategy and we observe at times significant differences between the
average exchange rate changes within industries that are specific to exporting \((de_x)\), import competition \((de_{mc})\) and reliance on foreign inputs \((de_{mi})\). Per definition, these differences are the result of differing intensities of each industry’s geographical sourcing or export destinations. The diamond-shaped dots appear to display the least variation across industries, which suggest relatively similar geographical sourcing of intermediate inputs across Swiss industries. Significant variation, however, can be observed for both export and import competition specific exchange rate appreciation measures, which suggests higher industry-specificity in trade partners for goods other than intermediate inputs. Overall, the observed variation in figure (3.3) justifies the calculation of industry-specific exchange rates, as aggregate exchange rate measures would not pick up these differences. Interestingly, the Swiss manufacturing sector appears to have experienced a much larger export-specific exchange rate appreciation, while the services sector displays relatively higher import-specific appreciation measures. Table (3.2) reports these measures as aggregated on the sectoral level. We observe that the manufacturing sector has experienced an average appreciation of 10.55%, whereas this number has been only 8.81% for the services sector, suggesting less external competitiveness pressures for its exports. The roles are inversed, however, when looking at the values of the import-specific measures, notably the import-competition specific exchange rate appreciation.
Industry-level foreign demand

The timing of the CHF appreciation coincides with the financial crisis that broke out in 2009. In order to disentangle the effects of the CHF appreciation from those of the global slowdown in demand, we propose to construct an industry specific, trade weighted measure of world demand, in addition to year fixed effects in all regression specifications. The inclusion of such a control variable is warranted by the predominant role of countries’ GDP in determining trade relations. Since we look at both imports and exports, this variable captures more than just foreign demand, but rather the trade effects net of the exchange rate variation, due to variation in foreign economic activity. We proceed analogously to the measure on industry exchange rates and calculate:

\[ \Delta GDP_{i,t}^* = 100 \times \sum_j w_{i,j,t} \frac{GDP_{j,t} - GDP_{j,t-1}}{GDP_{j,t-1}} \]

where \( GDP_{j,t} \) is the trading partner’s GDP and \( GDP_{i,t}^* \) is the trade weighted foreign GDP industry \( i \) is facing at time \( t \) and

\[ w_{i,j,t} = \frac{x_{i,j,t-1} + m_i_{i,j,t-1} + m_c_{i,j,t-1}}{x_{i,t-1} + m_i_{i,t-1} + m_c_{i,t-1}} \]

Industry-Level Exposure to Export and Import

We then turn to calculating the degree of industries’ export orientation \((xe_{i,t})\), reliance on foreign inputs \((mei_{i,t})\) and import competition \((mec_{i,t})\) as in table(3.1), which we define as

\[ xe_{i,t} = \sum_j \frac{x_{i,j,t-1}}{Out_{i,t-1}} \]

where \( x_{i,j,t-1} \) denotes industry \( i \)'s exports to country \( j \) and \( Out_{i,t-1} \) denotes its total output;

\[ mec_{i,t} = \sum_j \frac{x_{i*,j,t-1}}{Out_{i,t-1}} \]
where $x_{i^*,j,t-1}$ denotes Swiss final consumption of foreign industry $i^*$’s exports, and

$$mei_{i,t} = \sum_{i^*,j} \frac{x_{i^*,j,t-1}}{Out_{i,t-1}}$$

where we consider industry $i$’s total imports of foreign intermediate inputs from across industries $i^*$.

### 3.3.3 Constructing local measures

Armed with the full set of industry-specific variables, we propose moving to estimating their local counterparts. The BZ and STATENT offer a detailed information on local industrial structures that we propose to exploit to this end. Specifically, we propose to scale national estimates at the industry level to the Swiss municipal level, the identifying assumption being that the typical firm in an industry in a municipality shares the same features as the national industry, proportionally to its size relative to the entire industry. The choice of scaling parameter is hence consistent with the stylized fact that firms engaging in international trade are typical very large (Bernard et al. (2011)). The industry level exposure to foreign exchange rate fluctuations and trade is hence proportionally apportioned to local size, as in Autor et al. (2013). Specifically, we calculate a municipality-specific scaling variable that reflects both its industry composition, as well as its relative size:

$$I_{c,t} = \frac{1}{33} \sum_i \frac{L_{c,i,t-1}}{L_{i,t-1}} I_{i,t}$$

where indicator variable $I_{i,t} = \{\Delta e_{i,t}^x, \Delta e_{i,t}^{mi}, \Delta e_{i,t}^{mc}, \Delta GDP_{i,t}^*, xe_{i,t}, mec_{i,t}, mei_{i,t}\}$, $I_{c,t}$ is its local counterpart.

We display the descriptive statistics for the set of our constructed municipality-variables in table (3.3). Our original employment dataset had observations for 2001, 2005, 2008, 2011, and 2014. Our industry trade dataset has ob-
Table 3.3: Descriptive Statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Obs</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td>year</td>
<td>9136</td>
<td>2009.5</td>
<td>3.354286</td>
<td>2005</td>
<td>2014</td>
</tr>
<tr>
<td>$\Delta e_{c,t}^x$</td>
<td>9136</td>
<td>.0042421</td>
<td>.0274679</td>
<td>-.0133255</td>
<td>1.679724</td>
</tr>
<tr>
<td>$\Delta e_{c,t}^{mc}$</td>
<td>9136</td>
<td>.0040031</td>
<td>.0283389</td>
<td>-.0222461</td>
<td>1.738849</td>
</tr>
<tr>
<td>$\Delta e_{c,t}^m$</td>
<td>9136</td>
<td>.0039543</td>
<td>.0283452</td>
<td>-.0042345</td>
<td>1.754438</td>
</tr>
<tr>
<td>$xe_{c,t}$</td>
<td>9136</td>
<td>.0001176</td>
<td>.0004455</td>
<td>9.83e-10</td>
<td>.0115742</td>
</tr>
<tr>
<td>$mec_{c,t}$</td>
<td>9136</td>
<td>.0001819</td>
<td>.0020382</td>
<td>6.63e-09</td>
<td>.1034906</td>
</tr>
<tr>
<td>$mei_{c,t}$</td>
<td>9136</td>
<td>.0000675</td>
<td>.0002277</td>
<td>1.40e-08</td>
<td>.0057888</td>
</tr>
<tr>
<td>$\Delta GDP_{c,t}^*$</td>
<td>9136</td>
<td>.0102382</td>
<td>.0637368</td>
<td>-.0020827</td>
<td>3.510124</td>
</tr>
</tbody>
</table>

Table 3.4: Top Ten Municipality Export Exposure $xe_{c,t}$ Ranking

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Zürich</td>
<td>2</td>
<td>2</td>
<td>2</td>
<td>1</td>
</tr>
<tr>
<td>Basel</td>
<td>1</td>
<td>1</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>Genève</td>
<td>7</td>
<td>4</td>
<td>4</td>
<td>3</td>
</tr>
<tr>
<td>Kreuzlingen</td>
<td>10</td>
<td>6</td>
<td>6</td>
<td>4</td>
</tr>
<tr>
<td>Mendrisio</td>
<td>32</td>
<td>20</td>
<td>9</td>
<td>5</td>
</tr>
<tr>
<td>Bern</td>
<td>6</td>
<td>3</td>
<td>3</td>
<td>6</td>
</tr>
<tr>
<td>St. Gallen</td>
<td>9</td>
<td>5</td>
<td>5</td>
<td>7</td>
</tr>
<tr>
<td>Sevelen</td>
<td>21</td>
<td>7</td>
<td>7</td>
<td>8</td>
</tr>
<tr>
<td>Biel/Bienne</td>
<td>14</td>
<td>14</td>
<td>11</td>
<td>9</td>
</tr>
<tr>
<td>Baden</td>
<td>17</td>
<td>15</td>
<td>24</td>
<td>10</td>
</tr>
</tbody>
</table>

Table (3.4) gives us a good example of what our indices capture. We see the top ten Swiss municipalities in terms of export exposure in 2014, as well as their ranks in earlier periods. While the biggest Swiss municipalities in terms of employment head this ranking, we also find a number of
relatively small municipalities that display similarly high levels of export exposure. For example, Kreuzlingen, Mendrisio and Sevelen together account for less than 7% of total employment in Zurich alone. Nevertheless, they have similar levels of export exposure, as per $x_{c,t}$. Sevelen alone has less than 2000 FTEs, corresponding to a little more than half a percent of employment in Zurich. The similar level of export exposure comes from the fact that Sevelen provides a large share of total Swiss employment in one particular industry that makes a large part of its revenues abroad. The table hence illustrates well the fact that our variables reflect municipality exposure along two lines: 1) the share of municipal industry employment in total industry employment, irrespectively of the total size of the community, and 2) the trade intensities of the respective industries that a particular municipality houses.

3.4 RESULTS

3.4.1 Total municipal employment change

Before turning to the estimation of equation (3.2), we need to define the measurement of the outcome variable of interest. Previous studies have generally regressed either employment growth of tradable industries, or the share of those in total employment. In our set-up, all industries participate in international trade. In a first step, we are hence interested in total municipal employment change and simply define:

$$\Delta L_{c,t}^1 = L_{c,t} - L_{c,t-1}$$

In order to get a first feel for municipal employment growth in Switzerland, we plot our measure of total municipal employment change $\Delta L_{c,t}^1$ in boxplots in figure (3.4). The median Swiss municipality has experienced positive employment growth in all periods except for 2001-2005. This result
is remarkable, given the unprecedented global economic downturn, which seems not to have had an important negative effect on municipal employment changes in Switzerland.

Figure 3.4: Municipal employment change $\Delta L_{ij}^1$ by year

In order to investigate the issue econometrically, we start by estimating a preliminary version of equation (3.2). We estimate a linear panel regression with year fixed effects to account for the time dimension of our short panel and year-specific aggregate shocks. We also control for idiosyncratic, time-invariant fixed effects that may influence employment growth across regions, such as geographical and other locational determinants, as well as heterogeneity in policies. Economic policy is made at the municipal, cantonal, as well as federal level in Switzerland, which complicates the choice of entity for the fixed effect. Corporate taxes, which may be argued to have a significant impact on employment choices of firms, vary both across municipalities and cantons. We hence proceed by displaying the results for both regressions in table (3.5). Throughout all specifications, we cluster standard errors on the level of the respective fixed-effect, so as to control for
heteroskedasticity and serial correlation. Column (1) lists the results for the case with a cantonal fixed effect and column (2) uses a municipality fixed effect. Specification (a) includes only changes in exchange rate measures and foreign demand, whereas in column (b) we also include interactions with respective trade exposures as regressors.

Table 3.5: Total change in FTEs

<table>
<thead>
<tr>
<th></th>
<th>(1) Canton FE</th>
<th></th>
<th>(2) Municipal FE</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(\Delta L_{1c,t})</td>
<td>(\Delta L_{1c,t})</td>
<td>(\Delta L_{1c,t})</td>
<td>(\Delta L_{1c,t})</td>
</tr>
<tr>
<td>(\Delta e_{x,c,t})</td>
<td>-18781.7*</td>
<td>-12478.3</td>
<td>-87641.2*</td>
<td>-7632.7*</td>
</tr>
<tr>
<td>(\Delta e_{x,c,t})</td>
<td>(-2.69)</td>
<td>(-1.21)</td>
<td>(-2.46)</td>
<td>(-2.33)</td>
</tr>
<tr>
<td>(\Delta e_{m,c,t})</td>
<td>120300.7**</td>
<td>108972.5**</td>
<td>192059.7***</td>
<td>173928.3***</td>
</tr>
<tr>
<td>(\Delta e_{m,c,t})</td>
<td>(2.90)</td>
<td>(3.31)</td>
<td>(4.14)</td>
<td>(3.95)</td>
</tr>
<tr>
<td>(\Delta GDP_{c,t})</td>
<td>-84367.5*</td>
<td>-81302.1*</td>
<td>-101337.1***</td>
<td>-97705.6***</td>
</tr>
<tr>
<td>(\Delta GDP_{c,t})</td>
<td>(-2.12)</td>
<td>(-2.24)</td>
<td>(-4.92)</td>
<td>(-4.13)</td>
</tr>
<tr>
<td>(\Delta e_{m,c,t} \times x_{c,t})</td>
<td>886.0**</td>
<td>1158.8**</td>
<td>-4099.9*</td>
<td>-3907.4</td>
</tr>
<tr>
<td>(\Delta e_{m,c,t} \times x_{c,t})</td>
<td>(2.67)</td>
<td>(3.72)</td>
<td>(-2.14)</td>
<td>(-1.87)</td>
</tr>
<tr>
<td>(\Delta e_{m,c,t} \times x_{c,t})</td>
<td>-1072554.9**</td>
<td>-2408200.6**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(\Delta e_{m,c,t} \times x_{c,t})</td>
<td>(-2.82)</td>
<td>(-3.29)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(\Delta e_{m,c,t} \times x_{c,t})</td>
<td>4434137.1***</td>
<td>6255084.3***</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(\Delta e_{m,c,t} \times x_{c,t})</td>
<td>(6.02)</td>
<td>(4.26)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(\Delta e_{m,c,t} \times x_{c,t})</td>
<td>-1530643.6***</td>
<td>-696658.9*</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(\Delta e_{m,c,t} \times x_{c,t})</td>
<td>(-6.79)</td>
<td>(-2.10)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

|                  | (a)          |                | (b)            |                |
| Constant         | -37.23       | -40.41*        | 189.8*         | 184.7*         |
|                  | (-2.48)      | (-2.29)        | (2.35)         | (2.06)         |
| Observations     | 9136         | 9136           | 9136           | 9136           |
| \(R^2\)          | 0.547        | 0.598          | 0.614          | 0.645          |

\(t\) statistics in parentheses, SEs clustered on the level of the fixed effect
* \(p < 0.05\), ** \(p < 0.01\), *** \(p < 0.001\)

The results in 1a) and 2a) back up our theoretical intuition of 3.2.1 empirically. We estimate statistically significant negative coefficients on the export specific exchange rate, as well as the import-competition specific exchange rate, suggesting negative employment effects of the Swiss Franc appreciation through these channels. Everything else equal, an appreciation of the Swiss Franc renders Swiss exports less competitive and fosters a substitution in consumption towards relatively cheaper foreign goods. However, we also estimate a very large and precisely estimated positive coefficient on the intermediate-inputs specific exchange rate, which is consistent with the "natural hedging" mechanism found in Fauceglia et al. (2014). Being
a small open economy with a relatively limited domestic economy, Swiss firms are closely interwoven in global value chains and source important parts of their inputs from abroad. Everything else equal, the relative fall of prices of these foreign inputs gives them a competitive edge, which leads to positive employment growth.

In fact, when comparing the magnitudes of the estimated coefficients, we find a much larger positive effect on the intermediate-imports channel than the negative effect on both other channels combined. Potential losses in competitiveness through exchange rate appreciation seem to be entirely hedged by the availability of relatively cheaper foreign inputs, at least on aggregate.

In columns b), we include the interaction terms that reflect the exposure of each municipality to our three channels of exchange rate change transmission. The reduction of the magnitude of our previous coefficients is as expected, because the interaction term picks up differential exposure to exchange rate fluctuation, in terms of industrial structure. Employment growth in municipalities increases with an appreciation of the imported input-specific Swiss Franc as municipalities host larger shares of employment in industries that use foreign inputs. On the other hand, the Swiss Franc appreciation is associated with negative employment growth for municipalities that host larger shares of employment in import-competing sectors ($\beta_{2mc}$). This finding is consistent with the previous result, indicating a substitution of consumer spending away from domestic production and towards relatively cheaper foreign final goods. The same mechanism is confirmed for municipalities that host a relatively larger share of export-oriented industries. The negative coefficient on the interaction term ($\beta_{2x}$) indicates that municipalities with relatively greater exposure to exchange rate appreciation experience negative employment gains. Throughout all specifications, the magnitude of coefficient $\beta_{1x}$ is relatively low, indicating that the negative export exchange rate effect is less important than the other exchange rate effects. This finding puts the debate on adverse effects of currency appreciations into a larger, less worrisome context. In column
1b), the coefficient even loses statistical significance, which may, however, also be the result of strong collinearity between the regressors we use. We will get back to this point later in the analysis.

3.4.2 Total municipal employment growth

Table 3.6: Total growth in FTEs

<table>
<thead>
<tr>
<th></th>
<th>(1) Canton FE</th>
<th></th>
<th>(2) Municipal FE</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(a)</td>
<td>(b)</td>
<td>(a)</td>
<td>(b)</td>
</tr>
<tr>
<td></td>
<td>$\Delta L_{c,t}^2$</td>
<td>$\Delta L_{c,t}^2$</td>
<td>$\Delta L_{c,t}^2$</td>
<td>$\Delta L_{c,t}^2$</td>
</tr>
<tr>
<td>$\Delta c_{i,t}$</td>
<td>-0.158</td>
<td>-0.257</td>
<td>-0.552</td>
<td>-0.618</td>
</tr>
<tr>
<td></td>
<td>(-0.46)</td>
<td>(-0.68)</td>
<td>(-1.08)</td>
<td>(-1.15)</td>
</tr>
<tr>
<td>$\Delta c_{i,t}$</td>
<td>3.141**</td>
<td>3.321**</td>
<td>3.791***</td>
<td>3.596***</td>
</tr>
<tr>
<td></td>
<td>(3.18)</td>
<td>(3.17)</td>
<td>(4.25)</td>
<td>(4.16)</td>
</tr>
<tr>
<td>$\Delta c_{i,t}$</td>
<td>-2.976**</td>
<td>-2.964**</td>
<td>-3.301***</td>
<td>-3.225***</td>
</tr>
<tr>
<td></td>
<td>(-3.61)</td>
<td>(-3.52)</td>
<td>(-4.45)</td>
<td>(-4.56)</td>
</tr>
<tr>
<td>$\Delta GDP_{c,t}$</td>
<td>0.0886**</td>
<td>0.0896**</td>
<td>0.0734**</td>
<td>0.0742**</td>
</tr>
<tr>
<td></td>
<td>(3.15)</td>
<td>(3.30)</td>
<td>(2.67)</td>
<td>(2.77)</td>
</tr>
<tr>
<td>$\Delta c_{i,t} \times x_{c_{i,t}}$</td>
<td>8.913</td>
<td>12.72</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.03)</td>
<td></td>
<td></td>
<td>(0.74)</td>
</tr>
<tr>
<td>$\Delta c_{i,t} \times me_{c_{i,t}}$</td>
<td>-24.77</td>
<td>29.64</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-1.05)</td>
<td></td>
<td></td>
<td>(0.72)</td>
</tr>
<tr>
<td>$\Delta c_{i,t} \times mec_{c_{i,t}}$</td>
<td>-13.04*</td>
<td>-4.438</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-2.53)</td>
<td></td>
<td></td>
<td>(-0.61)</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.0167*</td>
<td>-0.0168*</td>
<td>-0.0159***</td>
<td>-0.0155***</td>
</tr>
<tr>
<td></td>
<td>(-2.60)</td>
<td>(-2.61)</td>
<td>(-3.75)</td>
<td>(-3.65)</td>
</tr>
<tr>
<td>Observations</td>
<td>9136</td>
<td>9136</td>
<td>9136</td>
<td>9136</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.028</td>
<td>0.028</td>
<td>0.034</td>
<td>0.035</td>
</tr>
</tbody>
</table>

$t$ statistics in parentheses, SEs clustered on the level of the fixed effect
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In a second step, we repeat the analysis by scaling the outcome variable over initial total municipal employment, i.e. municipal employment growth:

$$\Delta L_{c,t}^2 = \frac{L_{c,t} - L_{c,t-1}}{L_{c,t-1}}$$

The analogous results are plotted in table (3.6). Given that these computed growth rates do not take account of municipalities’ sizes, the explanatory power of our regressors shrinks drastically (the $R^2$ falls to roughly 0.03). Nevertheless, the results are similar, at least with respect to our exchange
rate change variables. Both coefficients on foreign inputs and import competition take the expected sign and are statistically significant. However, the coefficient on export-specific exchange rate fluctuations remains insignificant across all specifications, mirroring the less precise measurement we already witnessed above. Except for the import competition interaction term in 1b), the coefficients on all industry exposure interactions remain insignificant in this specification. We suspect that this outcome is due to the weighting scheme of the dependent variable, which masks municipal heterogeneities in terms of employment size. In fact, constructing \( \Delta L_{c,t}^2 \) to reflect aggregate municipal employment growth entails significant loss of information. The aggregate number does not allow us to track where this growth originates, or which industries have contributed to it. For example, suppose that municipality A has grown entirely through additional restaurants, whereas municipality B’s growth has originated entirely in the chemical products industry. Suppose further that these two municipalities have both grown by 5%. Obviously, our measures of exposure to foreign trade are not able to distinguish between the source of employment growth in each municipality and will hence provide imprecise coefficients. The construction of our municipality-level exposure hinges critically on the its relative size in the overall economy, which is better captured by absolute employment change, as in table (3.5). But even then, we lose information on which industries have contributed how much to this total employment change.

3.4.3 Total industry-weighted municipal employment change

In order to construct another growth measure that better reflects the industrial structure that drives total municipal growth, as well as the relative size of the municipality, we redefine the dependent variable consistently with our regressors as follows:
\[ \Delta L_{c,t}^3 = \frac{1}{33} \sum_i \frac{L_{i,c,t} - L_{i,c,t-1}}{L_{i,t-1}} \]

Defined as such, \( \Delta L_{c,t}^3 \) reflects the overall growth contribution of each industry in municipality \( c \), taking account of the size of the given municipality in the total Swiss economy. Given that these characteristics correspond to our empirical strategy most closely, this specification is our preferred one. We display the results keeping the previous structure of specifications of the estimating equation in table (3.7).

**Table 3.7: Industry weighted municipal employment growth**

<table>
<thead>
<tr>
<th></th>
<th>Canton FE</th>
<th>Municipal FE</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(a)</td>
<td>(b)</td>
</tr>
<tr>
<td>( \Delta L_{c,t}^3 )</td>
<td>( \Delta L_{c,t}^3 )</td>
<td>( \Delta L_{c,t}^3 )</td>
</tr>
<tr>
<td>( \Delta e_{c,t} )</td>
<td>(-1.432^{**} )</td>
<td>(-1.050^{*} )</td>
</tr>
<tr>
<td>( \Delta e_{c,t} )</td>
<td>((-3.47) )</td>
<td>((-3.27) )</td>
</tr>
<tr>
<td>( \Delta e_{c,t} \times e_{c,t} )</td>
<td>(0.0619^{***} )</td>
<td>(0.0687^{***} )</td>
</tr>
<tr>
<td>( \Delta GDP_{c,t} )</td>
<td>(0.1766^{**} )</td>
<td>(0.1766^{**} )</td>
</tr>
<tr>
<td>( \Delta e_{c,t} \times mc_{c,t} )</td>
<td>(-31.33^{**} )</td>
<td>(-31.33^{**} )</td>
</tr>
<tr>
<td>( \Delta e_{c,t} \times mc_{c,t} )</td>
<td>((-3.57) )</td>
<td>((-3.57) )</td>
</tr>
<tr>
<td>( \Delta e_{c,t} \times mc_{c,t} )</td>
<td>((-2.33) )</td>
<td>((-2.44) )</td>
</tr>
<tr>
<td>( \Delta GDP_{c,t} )</td>
<td>(-68.65^{**} )</td>
<td>(-138.7^{***} )</td>
</tr>
<tr>
<td>( \Delta GDP_{c,t} )</td>
<td>((-3.12) )</td>
<td>((-3.12) )</td>
</tr>
<tr>
<td>( \Delta GDP_{c,t} )</td>
<td>((-2.04) )</td>
<td>((-2.04) )</td>
</tr>
<tr>
<td>( \Delta GDP_{c,t} )</td>
<td>((-2.53) )</td>
<td>((-2.53) )</td>
</tr>
<tr>
<td>Constant</td>
<td>(-0.0000125^{*} )</td>
<td>(-0.0000152 )</td>
</tr>
<tr>
<td>R²</td>
<td>0.345</td>
<td>0.385</td>
</tr>
<tr>
<td>R²</td>
<td>0.345</td>
<td>0.385</td>
</tr>
</tbody>
</table>

\( t \) statistics in parentheses, SEs clustered on the level of the fixed effect

* \( p < 0.05 \), ** \( p < 0.01 \), *** \( p < 0.001 \)

The additional variation in \( \Delta L_{c,t}^3 \) now allows for a better identification of the various trade channels we are investigating. In fact, the estimated coefficients again correspond exactly to our theoretical predictions, both in the case of cantonal fixed effects, as well as in the case of municipal fixed effects. All coefficients of interest bear the expected sign and are statistically significant. Importantly, the inclusion of our interaction variables does not significantly alter our exchange rate coefficients \( \beta_{1,c} \), while yield-
ing precisely estimated coefficients $\beta_2$. In comparison to our results on aggregate municipal growth, we can now disentangle the sources of employment growth and do find a negative association between the export-specific exchange rate appreciation and the corresponding industry employment growth. Moreover, this effect is magnified for municipalities that host relatively large shares of export-oriented industry. The import competition-specific exchange rate variation yields similar effects and confirms the previous result, in that we find evidence for a substitution effect in favor of imported final consumption, which the CHF appreciation has rendered relatively cheaper. The negative coefficient $\beta_{2mc}$ confirms that the corresponding employment growth effect is stronger for municipalities that host a relatively larger share of import-competing industries. Nevertheless, the perhaps most important result here is again that we maintain the positive employment effects of the intermediate import-specific channel: in fact, across all specifications, coefficients $\beta_{1mi}$ and $\beta_{2mi}$ are by far the largest, confirming the earlier result that Swiss industries effectively "hedged" themselves from negative exchange rate shocks through integration in global value chains.

The robustness of our results and stability of coefficients across these different specifications is even more remarkable if we consider that - given the nature of our regressors - we have strong indications of the presence of high degrees of multicollinearity. Both our exchange rate measures and the industry exposure measure are highly collinear, given that we can distinguish only 33 industries and those industries that participate in international trade generally both import and export (see table (3.1)). Nevertheless, when running robustness checks by omitting collinear variables, we maintain the sign of each coefficient of interest, while losing significance rather than gaining it. These exercises make us confident that our regressors, while being substantially collinear, do matter in a statistically significant way at the margin.
3.5 ROBUSTNESS

3.5.1 Average year-on-year exchange rate fluctuations

We have so far calculated exchange rate changes as the percentage change over two points in time. This approach may be problematic, as it coincides with the two points in time that we use to construct employment changes. As such, our exchange rate change measures may pick up fluctuations that are year-specific, as our exchange rate data display great volatility, despite being yearly averages (see figure (3.1)). This concern is less pronounced for our employment variable, as employment is generally much less volatile than exchange rates, given that hiring and notably firing decisions are subject to regulatory and procedural discipline.

Figure 3.5: Average yoy nominal exchange rate growth

Source: Authors’ calculations based on UNCTAD Stat

We hence proceed by recalculating all our exchange rate measures as the mean year-on-year percentage exchange rate change between the periods under consideration, i.e. 2001-2005, 2005-2008, 2008-2011, and 2011-2014.
We plot the measures we obtained for the CHF against four major currencies in figure (3.5). Notice that the flatter slope of the curves, relative to figure (3.1), stem from the fact that we compare exchange rate levels at each base year of each period with its subsequent development, as we are interested in currency fluctuations, rather than long-term trends.

We re-estimate our equation with the yoy exchange rate measures and display the results in table (3.8). Our results are very similar to the ones we obtain in our preferred specification with the previous definition of exchange rate changes in table (3.7). The only notable difference is the greater magnitude of coefficients obtained here. The reason for these larger estimates comes from the definition of the exchange rate measure: Since we are averaging over the number of years between periods, the magnitude of the percentage change is on average lower. The unreported plot of our previous measures of exchange rate fluctuations as in figure (3.5) shows that percentage changes of the nominal exchange rate in our earlier definition closely follow the curves plotted in figure (3.5), albeit at a larger scale.

3.5.2 Long term exchange rate trend

Our analysis has until now focused exclusively on exchange rate fluctuations and relatively short term employment effects (up to four years). It could be argued that our results are, however, driven by long term exchange rate trends. Given data constraints, we are not able to decompose exchange rate fluctuations into a transitory and a permanent part, as is often done in the literature (see eg Klein et al. (2003) or Black and Mc Millan (2004)). Our trade related analysis requires bilateral nominal exchange rate data on 60 partner countries over time, which are not available on a monthly, or even daily basis. The UNCTAD dataset on average yearly bilateral exchange
Table 3.8: Industry weighted municipal employment growth

<table>
<thead>
<tr>
<th></th>
<th>Canton FE</th>
<th>Municipal FE</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(a)</td>
<td>(b)</td>
</tr>
<tr>
<td>( \Delta L_{ct}^{3} )</td>
<td>( -4.634^{***} )</td>
<td>( -7.429^{*} )</td>
</tr>
<tr>
<td></td>
<td>( -4.95 )</td>
<td>( -2.41 )</td>
</tr>
<tr>
<td>( \Delta e_{ct}^{ex} )</td>
<td>18.62**</td>
<td>17.19**</td>
</tr>
<tr>
<td></td>
<td>( 3.11 )</td>
<td>( 3.39 )</td>
</tr>
<tr>
<td>( \Delta e_{ct}^{mic} )</td>
<td>( -13.08^{*} )</td>
<td>( -12.99^{*} )</td>
</tr>
<tr>
<td></td>
<td>( -2.13 )</td>
<td>( -2.17 )</td>
</tr>
<tr>
<td>( \Delta GDP_{ct}^{1} )</td>
<td>0.342***</td>
<td>0.356***</td>
</tr>
<tr>
<td></td>
<td>( 4.72 )</td>
<td>( 4.64 )</td>
</tr>
<tr>
<td>( \Delta e_{ct}^{mc} \times x_{ct} )</td>
<td>-247.6**</td>
<td>-520.0***</td>
</tr>
<tr>
<td></td>
<td>( -3.29 )</td>
<td>( -7.79 )</td>
</tr>
<tr>
<td>( \Delta e_{ct}^{mic} \times x_{ct} )</td>
<td>635.0***</td>
<td>1158.1***</td>
</tr>
<tr>
<td></td>
<td>( 4.17 )</td>
<td>( 9.15 )</td>
</tr>
<tr>
<td>( \Delta e_{ct}^{mic} \times x_{ct} )</td>
<td>-107.4**</td>
<td>-97.35***</td>
</tr>
<tr>
<td></td>
<td>( -3.42 )</td>
<td>( -3.45 )</td>
</tr>
<tr>
<td>( \text{Constant} )</td>
<td>-0.00186**</td>
<td>-0.00198**</td>
</tr>
<tr>
<td></td>
<td>( -3.18 )</td>
<td>( -2.79 )</td>
</tr>
<tr>
<td>Observations</td>
<td>9136</td>
<td>9136</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.357</td>
<td>0.400</td>
</tr>
</tbody>
</table>

All exchange rate measures redefined as average year-on-year change between periods (see 3.5.1) 

<table>
<thead>
<tr>
<th></th>
<th>( t ) statistics in parentheses, SEs clustered on the level of the fixed effect</th>
</tr>
</thead>
<tbody>
<tr>
<td>( * ) ( p &lt; 0.05 ),  ( ** ) ( p &lt; 0.01 ),  ( *** ) ( p &lt; 0.001 )</td>
<td></td>
</tr>
</tbody>
</table>

rates is hence the most suitable for our analysis, despite its shortcomings. We can still test for a potential effect of long term exchange rate appreciation using a different methodology. If we posit that it is rather long term changes in the exchange rate that matter, we make use of a different model specification, that is, we replicate our analysis on the basis of municipal employment and exchange rate levels. For this purpose, we redefine our exchange rate measure as an index, which equals 100 for the base period of 2001. We hence do not compare period-by-period changes in exchange rate movement, but focus solely on the evolution of the exchange rate with respect to its baseline level, capturing the long term evolution of the Swiss Franc appreciation. Similarly, for each municipality, we use the level of employment as the dependent variable, rather than its change. We are hence interested in the evolution of within-municipality employment levels and use a municipality fixed effect in this specification.
Table 3.9: FTE’s and long term exchange rate movement

<table>
<thead>
<tr>
<th></th>
<th>(1) $fte_{ct}$</th>
<th>(2) $fte_{ct}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\Delta e_{ct}^f$</td>
<td>-26432.2</td>
<td>-14110.7</td>
</tr>
<tr>
<td></td>
<td>(-1.93)</td>
<td>(-1.36)</td>
</tr>
<tr>
<td>$\Delta e_{ct}^{mi}$</td>
<td>39516.8</td>
<td>24104.2</td>
</tr>
<tr>
<td></td>
<td>(0.81)</td>
<td>(0.59)</td>
</tr>
<tr>
<td>$\Delta e_{ct}^{mc}$</td>
<td>2538.1</td>
<td>-1958.2</td>
</tr>
<tr>
<td></td>
<td>(0.06)</td>
<td>(-0.06)</td>
</tr>
<tr>
<td>$\Delta GDP_{ct}^*$</td>
<td>6087.8***</td>
<td>6036.6***</td>
</tr>
<tr>
<td></td>
<td>(3.90)</td>
<td>(4.27)</td>
</tr>
<tr>
<td>$\Delta e_{ct}^f \times xe_{ct}$</td>
<td>904616.9</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.89)</td>
<td></td>
</tr>
<tr>
<td>$\Delta e_{ct}^{mi} \times me_{ct}$</td>
<td>-54658.0</td>
<td>(-0.14)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\Delta e_{ct}^{mc} \times me_{ct}$</td>
<td>7419.7</td>
<td>(0.16)</td>
</tr>
<tr>
<td>Constant</td>
<td>273.9</td>
<td>522.8***</td>
</tr>
<tr>
<td></td>
<td>(1.49)</td>
<td>(4.25)</td>
</tr>
<tr>
<td>Observations</td>
<td>9136</td>
<td>9136</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.773</td>
<td>0.81</td>
</tr>
</tbody>
</table>

Exchange rate variation is here defined with respect to the baseline year, as per 3.5.2
$t$ statistics in parentheses, year and municipality fixed effects, SEs clustered on the municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table (3.9) shows our regression results. In contrast to our previous regressions, none of the coefficients of interest is of any notable statistical significance, except for our foreign GDP measure. This holds despite the higher explanatory power, reflected in an $R^2$ of 0.77 in column (1) (no interaction terms), and 0.81 in column (2) (with interaction terms). If considered jointly with our previous results, the findings in table (3.9) suggest that the long term appreciation of the Swiss Franc we can observe in figure (3.1) has no discernible on Swiss employment, while more short-term fluctuations do.

### 3.6 Conclusion

We have exploited exogenous exchange rate movements to identify trade-induced shocks across all sectors of the Swiss economy and transposed industry-level exposures to the municipal level, using detailed employment
data on the entirety of Swiss firms. In line with theoretical predictions, we find strong evidence for three channels of antagonistic employment effects of currency appreciation in the short- to medium-term: negative employment growth induced by increasing export uncompetitiveness and higher import competition, and positive employment growth induced by cheaper availability of foreign inputs. Importantly, the latter channel appears to trump the former two, and on aggregate, the Swiss economy appears to have successfully hedged itself through integration in global value chains. We do not find evidence for any employment effects of long term exchange rate trends. While not investigating trade policies per se, our the results of our analysis extend to other policies that alter relative external prices as well. As such, our findings make a strong case for open trade policies and corroborate recent insights on the implications of global value chains for trade policy. Further research may investigate the qualitative dimension of labor market adjustments more closely. Notably an extension towards wages and part-time work may build upon the methodology developed in this paper and provide evidence beyond adjustment in total hours worked.
3.7 References


Lassmann, A. (2013). Exchange rate transmission and export activity at the firm level (No. 331). *KOF Working Papers, KOF Swiss Economic Insti-


STATUTORY DECLARATION

Hereby I declare,

- that I wrote this dissertation without any illicit assistance and without using any other aids than stated and that this dissertation was neither presented in equal nor in similar form at any other university;

- that I cited all references that were used respecting current academic rules.

Place and date of issue:
Eichhölzli, July 2017

Signature:

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