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Service Offshoring and Firm Employment

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Abstract

Major technological advances have recently spurred a new wave of offshoring in services, which used to be non-tradable. Should service workers in developed countries worry about their jobs? Trade theory has given a nuanced answer to this question, suggesting that efficiency gains from offshoring may counteract direct job losses, which leaves the predicted net effect ambiguous. This paper investigates the employment effects of service offshoring in a newly combined and exceptionally detailed panel dataset, covering almost the entire universe of German firms' service imports over the years 2002-2013. It exploits firm-specific export supply shocks by partner countries and service types as an instrumental variable to find that service offshoring has *increased* firm employment. In line with the canonical trade in tasks model, the employment gains are greater in firms with higher initial levels of service offshoring.

JEL classifications: F16, F66, F14, J23.

Keywords: service offshoring, employment, firm-level data, service trade, trade in tasks.

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1 Introduction

There is a widespread fear in the developed world that domestic jobs are endangered by offshoring to low-wage countries. A popular narrative suggests that these anxieties played a decisive role in the US presidential elections of 2016.[1] More generally, the increasing public concern about offshoring is evidenced by the rising media attention to this topic over time, as documented by Amiti and Wei (2005) and Mankiw and Swagel (2006) for the UK and the US. In Germany, offshoring has been found to increase job loss fears, particularly among high-skilled workers (Geishecker et al., 2012). These workers may indeed be threatened by a 'new wave' of offshoring in services (Bardhan and Kroll, 2003) that has been triggered by groundbreaking innovations in information and communication technologies (ICT) over the past decades. The spread of portable computers, broadband internet, and the smartphone has made services tradable globally that could only be delivered locally before. As a consequence, the share of service trade in world GDP has doubled from around 3% to 6% between 1985 and 2007.[2] Since jobs in the newly tradable commercial services are typically skill-intensive and high-paying (Jensen, 2008), the question arises: Are these 'good' domestic jobs lost due to service offshoring?

In theory, the employment effects of offshoring are not as clear-cut as one might expect. While the relocation itself obviously reduces domestic employment, the associated cost savings entail a productivity effect that can help offshoring firms to expand their output, which ameliorates the job losses and might even turn them into job creation. The seminal 'trade in tasks' model by Grossman and Rossi-Hansberg (2008), formalizing these ideas, has typically been applied to offshoring of manufactured inputs, but similar effects can be expected for services.[3] The ambiguous theoretical predictions call for an empirical investigation into the employment effects of service offshoring.

The lack of appropriate micro data on service offshoring firms has severely restricted empirical investigations in the past, as establishing causality is difficult at the industry level. Only recently, firm-level data on service trade have become available and opened the door to a more rigorous causal analysis. Such a firm-level analysis can build on recent extensions of the trade in tasks model that show how the relocation and productivity effects of offshoring vary across heterogeneous firms.[4] However, the same models also predict that larger and more productive firms are more active in offshoring, raising an important endogeneity issue that needs to be addressed empirically.

This paper's goal is to estimate the causal effects of service offshoring on firm employment in Germany, the world's second largest importer of services. Service offshoring is measured by imports of tradable commercial services, reflecting the fact that the relocation of a required service to another country necessarily entails subsequent imports of that service. The paper builds on an exceptionally detailed panel dataset, covering almost the entire universe of German firms' service imports by partner countries and service types over the period from 2001 to 2013. These data are newly combined with firm-level employment and balance sheet information for the purpose of this analysis. The resulting firm panel provides an almost comprehen-

[1] For instance, the political analysis website *FiveThirtyEight* reports that Donald Trump received substantially more votes in counties with a higher share of routine jobs, which are potentially threatened by automation or offshoring (see https://fivethirtyeight.com/features/trump-was-stronger-where-the-economy-is-weaker/). Autor et al. (2017) stress the impact of imports from China on the election outcome.

[2] These numbers are based on service trade data from the World Trade Organization (WTO) and GDP data from the World Bank's World Development Indicators.

[3] Note that the early contribution by Jones and Kierzkowski (1990) focused on the offshoring of services, not manufactured inputs. The beneficial effects of cost savings for domestic labor have been investigated at least since Egger and Falkinger (2003) and Kohler (2004a,b). In addition to the negative relocation effect and the positive productivity effect, the trade in tasks model further predicts an ambiguous relative-price effect (see e.g. also Deardorff, 2001a,b).

[4] See Egger et al. (2015, 2016), Groizard et al. (2014), and Sethupathy (2013).

sive picture of German service offshoring. I use it to investigate the firm-level employment effects of both new offshoring (the extensive margin) and changes in the volume of offshoring (the intensive margin). The analysis at the extensive margin implements a difference-in-differences matching approach, which compares employment changes in firms that start service offshoring to changes in a matched control group with similar initial conditions. To investigate the intensive margin, I instrument for service offshoring using firm-specific export supply shocks by partner countries and service types in fixed effects regressions. This instrumental variable (IV) strategy follows Hummels et al. (2014), who use it to analyze offshoring of manufactured inputs by Danish firms. The IV exploits the fact that firms' importing behavior is highly firm-specific and stable over time. Holding the initial import mix of a firm constant, the time variation in its partner countries' exports to the rest of the world by service types is used as a revealed measure of changes in these countries' comparative advantage. The rationale behind this approach is that a German firm, which initially imports a given service from a given country, benefits disproportionately from an improvement in this country's comparative advantage in this particular service, and can thus expand its offshoring activities. The crucial assumption of the IV approach is that foreign exports to the rest of the world are uncorrelated with the German firm's employment growth, except through offshoring (conditional on the control variables, which include industry-year fixed effects). Under this exclusion restriction, it serves to identify the causal effect of service offshoring on firm employment.

The main findings are summarized as follows. Firms that start service offshoring for the first time experience non-negative employment effects, which are estimated to be small and not significantly different from the matched control groups in most years. More interestingly, the analysis at the intensive margin reveals that increased service offshoring has *increased* domestic employment in German firms over the years 2002-2013. The estimated elasticity of 5.2-7.6% suggests that this effect is economically sizable, and the IV approach ensures that the positive effect is not driven by simultaneity or omitted variables. Instead, the two-stage least squares estimates can be interpreted as evidence for a cost savings effect from offshoring, which allows firms to expand and hire more workers. In line with this interpretation, service offshoring is found to boost firm output and total factor productivity. Moreover, the employment gains are greater in firms with higher initial levels of service offshoring. This pattern is fully in line with the working of the productivity effect in Grossman and Rossi-Hansberg (2008) and formally predicted by recent extensions of their model featuring firm heterogeneity (see in particular Egger et al., 2016). Intuitively, if foreign services become cheaper, the firm can experience cost savings that tend to increase employment. However, these cost savings are zero for the first offshored worker, as they work only through the infra-marginal units, which have previously been offshored. Consequently, a firm that has previously offshored more services experiences greater cost savings, and hence more favorable employment effects. These predictions are borne out strongly in the data, suggesting that service offshoring entails substantial productivity effects that benefit domestic employment.

The analysis proceeds by digging deeper into the rich micro data to shed some light on three interesting features of the employment effects at the intensive margin of service offshoring. First, one might suspect that service imports reflect not only services that were previously conducted by the firm itself, but also those purchased from other German suppliers prior to offshoring. However, sample splits show that the employment effects are even more favorable in the service sector, where the phenomenon of domestic supplier substitution should be less relevant because firms engage in service activities themselves. Also, the positive effects are confirmed if service offshoring is measured relative to domestic purchases of services. These findings suggest that domestic supplier substitution is not of primary relevance. Second, splitting up service imports by source country reveals that offshoring to non-OECD countries tends to yield stronger positive employment effects. This result is in line with the expectation that services provided by low-income countries are more complementary to domestic employment in German firms in terms of task and skill requirements. Third, since the combined dataset includes information on all sizeable foreign direct investment (FDI) links of German firms, it can be used to shed some first light on possible differences in the employment effects between intra-firm and arm's length service offshoring. The data show that potential intra-firm service offshoring (to countries and industries where a firm has an FDI link) also entails non-negative employment effects, but the relevance of this phenomenon is very limited. A set of robustness checks presented at the end shows that the positive employment effects of increased service offshoring are confirmed when (i) accounting for possible selection into the dataset or into the service offshoring activity, (ii) allowing for dynamics in employment in a difference GMM model, and (iii) applying several modifications to the IV strategy.

This paper contributes to the literature estimating the labor market effects of offshoring, pioneered by Feenstra and Hanson (1996a,b, 1999). Focusing predominantly on the offshoring of manufactured inputs, numerous studies have applied their proxy for offshoring, which is based on industry-level imports and input-output tables, to the analysis of worker-level wages and employment.[5] The same approach has further been applied to the analysis of individual-level wages.[6] Recently, Hummels et al. (2014) have brought the analysis to firm-worker data for Denmark, which allows them to measure (and instrument for) offshoring using firm-level import data. An alternative firm-level approach has exploited the activities of multinational enterprises' foreign affiliates to measure offshoring.[7] Matching methods have been applied to analyze the employment effects at the extensive margin of offshoring by Monarch et al. (2017), using firm offshoring events in the US, and by Moser et al. (2015), exploiting qualitative information from an establishment-level survey in Germany. The majority of these studies find that offshoring of manufactured inputs has small adverse effects on low-skilled workers' domestic employment or wages.

Amiti and Wei (2005, 2009a) were the first to apply the approach developed by Feenstra and Hanson to services. They analyze the relationship between employment and service offshoring at the industry level in the UK and the US, where they find mixed evidence and rather small correlations. Crinò (2010b, 2012) further investigates the relative employment effects across skill groups in the US and Europe. He finds that service offshoring favors high-skilled employment, similar to offshoring of manufactured inputs.[8] These findings are confirmed in worker-level wage data for the UK by Geishecker and Görg (2013), who also rely on the industry-level service offshoring measure. The two studies most closely related to this paper are Crinò (2010a) and Hijzen et al. (2011), who examine the link between service offshoring and firm-level employment. Crinò (2010a) applies matching methods to a cross-section of Italian firms to investigate the employment effects at the extensive margin of service offshoring, which turn out to be insignificant as in this paper. While he compares firms that import services to those that do not, the German firm panel data allow

[5]This literature is reviewed by Feenstra and Hanson (2003). A recent contribution in this vein is by Wright (2014), who finds evidence for negative effects of offshoring on employment and positive effects on output in US industries.

[6]See Ebenstein et al. (2014, 2015) for the US and Geishecker and Görg (2008) and Baumgarten et al. (2013) for Germany.

[7]Examples include Head and Ries (2002) for Japan, Muendler and Becker (2010) for Germany, as well as Harrison and McMillan (2011) and Sethupathy (2013) for the US. Note that this approach restricts attention to intra-firm offshoring and is less suitable to study the offshoring of services, since my data reveal that the bulk of service offshoring by German firms is to unrelated parties, not to foreign affiliates or investors.

[8]Criscuolo and Garicano (2010), Jensen and Kletzer (2005, 2010), and Liu and Trefler (2011) develop alternative approaches for identifying tradable services from US occupational data to investigate the labor market implications of services offshoring. A related strand of the literature has studied the effect of service offshoring on productivity; see Amiti and Wei (2009b) and Winkler (2010) for industry-level studies and Crinò (2008) for a firm-level analysis.

me to extend this approach to a difference-in-differences setup, which examines *changes* in employment in firms that *start* importing services, thereby controlling for time-invariant confounding factors. Hijzen et al. (2011) use detailed import data for the UK to show that increasing service offshoring coincides with higher employment growth. However, their approach does not address the likely endogeneity of offshoring, so they conclude that the positive correlation may be explained either by efficiency gains from offshoring or by simultaneity. This paper applies an IV strategy to eliminate the second possibility, and thus provides first causal evidence for the firm-level employment gains from increased service offshoring.

This paper contributes more generally to the literature analyzing service trade (surveyed by Francois and Hoekman, 2010), which is small compared to the abundance of research on goods trade. A distinguishing feature is the intangible and non-storable nature of services, which traditionally could be delivered only face to face (Hill, 1977). By relaxing this requirement, advancements in ICT have contributed to a steep decline of international trade costs for many services over the past decades (see Hoekman and Braga, 1997; Freund and Weinhold, 2002). Despite the differences to goods trade, previous studies found that aggregate service trade is well explained by the traditional gravity equation (Kimura and Lee, 2006; Head et al., 2009). It was not until recent years that firm-level service trade data have become available, which have uncovered that service-trading firms are a small group, with a similar and even more pronounced heterogeneity in terms of performance than found for goods traders (see most prominently Breinlich and Criscuolo, 2011, for the UK).[9] The key advantage of the dataset used in this paper relative to previous studies is that it combines (close to) full coverage of German firms' service trade with information on firm employment and other key characteristics, which allows for a comprehensive causal analysis.

The paper is organized as follows: Section 2 describes the rich micro dataset of German firms' service trade compiled for this study. Section 3 describes the within-firm correlations between employment and service offshoring found in simple OLS estimations. Section 4 introduces propensity score matching to scrutinize the employment effects at the extensive margin of offshoring. Section 5 develops the IV strategy and uses it to investigate the employment effects at the intensive margin of service offshoring. The final section concludes with a brief discussion of the findings and their implications.

2 Data

2.1 Data sources

The panel dataset of German firms used in this paper combines information from three sources: the Statistics on International Trade in Services (SITS), the Corporate Balance Sheet Statistics (USTAN), and the Microdatabase Direct Investment (MiDi). These confidential micro datasets are provided by the Research Data and Service Centre (RDSC) of the Deutsche Bundesbank (the German central bank) on site for research purposes. Only recently, these datasets have been linked at the RDSC, and this paper is among the first to exploit information combined from all three sources.[10] This link is essential for the paper's objective because SITS does not contain information on firm employment, which is hence taken from USTAN and MiDi (see Section 2.2 on how the data are combined).

The Statistics on International Trade in Services (SITS, Biewen et al., 2013) provides highly detailed

[9] Similar patterns have been documented by Ariu (2016) for Belgium, Federico and Tosti (2016) for Italy, Gaulier et al. (2010) for France, Kelle and Kleinert (2010) for Germany (using the same service trade data as this paper), Morikawa (2015) for Japan, Wolfmayr et al. (2013) for Austria, and Damijan et al. (2015) for four other European countries.

[10] MiDi has previously been used in combination with USTAN (e.g. by Jäckle and Wamser, 2010; Muendler and Becker, 2010), or in combination with SITS (Biewen et al., 2012). Eppinger (2014) provides first explorations of a dataset linking all three sources.

panel data on imports and exports of services by firm, month, partner country, and service category for around 22,000-26,000 firms over the years 2001-2013 (a full list of all countries and service categories is provided in the documentation). Service trade flows are defined as transactions between German residents and non-residents, which correspond to the modes 1 (cross-border trade), 2 (consumption abroad), and 4 (presence of natural persons) according to the WTO's General Agreement on Trade in Services (GATS). Transactions via commercial presence (GATS mode 3), such as purchases by foreign affiliates, are not included in this definition, which is ideal given the paper's focus on domestic employment effects. The data contain the universe of German firms' service trade for all transactions exceeding the reporting threshold of € 12,500. The high level of detail and the comprehensive coverage of these data make them uniquely suited to address the research question posed in this paper. These features represent key advantages over the vast majority of firm-level service trade data previously used in the literature, which are based on firm surveys.[11] In particular, the SITS data allow me to construct a firm-specific instrument for service offshoring (see Section 5.1). The analysis in this paper focuses on tradable commercial services, which are typically the subject of the offshoring debate. Therefore, it excludes all service trade classified as government services, incidental payments, private transfers, royalties and license fees, as well as travel and transport services.[12] Any references to total service imports throughout the paper refer to these tradable commercial services. In anticipation of the IV strategy, the remaining service categories are grouped into ten service types – such as communications, engineering, or research and development services – according to Table A.1, which broadly follows Biewen et al. (2013). The SITS are also aggregated over months to combine them with the other annual datasets.

Information on firm employment comes from two data sources. The first is the German Corporate Balance Sheet Statistics (USTAN, see Deutsche Bundesbank, 1998; Stöss, 2001). This dataset contains detailed balance sheets and income statements of a large number of non-financial German firms. Crucially for this paper, it includes information on the number of employees. The USTAN data are collected by the Deutsche Bundesbank for the purpose of credit assessments. The balance sheet data are used and carefully validated by central bank staff to assess the value of securitized, non-marketable claims or bills of exchange, which are presented as collateral to the central bank by commercial banks. The analysis excludes all consolidated balance sheets pertaining to corporations, as well as balance sheets referring to a short fiscal year. The full USTAN dataset used in this paper covers the years 1999-2013 and includes around 22,000-29,000 firms per year during the main period of analysis, 2002-2013.

The second source of employment data is the Microdatabase Direct Investment (MiDi, see Lipponer, 2011; Schild and Walter, 2017), which contains information on all German firms with inward or outward stocks of Foreign Direct Investment (FDI) above a reporting threshold. This threshold has been unchanged since 2002 at a minimum of 10% shares or voting rights in an affiliate with a balance sheet total exceeding € 3 million. Most importantly in the context of this paper, MiDi contains information on the number of employees and turnover for all firms involved in FDI, i.e., each multinational enterprise (MNE) and each foreign-invested enterprise (FIE) in Germany. The bulk of information contained in MiDi serves to provide a detailed picture of German firms' FDI links, including information on the country and industry of their foreign affiliates or investors. These data are exploited in this paper to identify German firms which have a related party in the country and industry to which they offshore services.

[11] Of the datasets used in all previous studies known to me, only the Belgian data used by Ariu (2016) are comparable to the SITS in terms of coverage.

[12] These payments cannot be thought of as offshoring, as is also argued by Head et al. (2009). Liu and Trefler (2011) make an analogous selection.

Bilateral data on international trade in services across country pairs, required to construct the IV (in Section 5.1), is taken from the UN Comtrade database.[13] Since information is frequently missing at more disaggregate levels, I use service trade data for a set of service codes corresponding largely to the first level of the Extended Balance of Payments Services (EBOPS) 2002 classification. The EBOPS codes are matched to the service types in the SITS data according to the correspondence reported in Table A.1.

2.2 Data preparation

The three micro-level datasets are combined via firm identification numbers, which are identical in SITS and MiDi. They are matched to USTAN via a correspondence table provided by the Deutsche Bundesbank (see Schild et al., 2017), resulting in the new combined dataset. Since USTAN does not cover the full population of German firms, and since by far not all firms are involved in service trade or FDI, the three data sources overlap only imperfectly. Nevertheless, the combined sample for which data on both firm employment and service imports are available (around 57,000 firms per year) includes on average 80% of the total service import value reported in SITS in a given year. This impressive coverage is due to the fact that the (almost fully covered) MNEs and FIEs in MiDi as well as the firms reporting to USTAN tend to be large compared to the average firm and hence account for a disproportionate share of total service imports. They also make up on average 25% of aggregate employment and 53% of aggregate turnover in the underlying non-financial private business sector per year.[14] Overall, the newly combined dataset provides an almost comprehensive picture of German firms' service offshoring activities and the firms in this dataset employ a substantial share of the entire German labor force.

Since SITS covers the entire universe of German firms' service trade (above a low reporting threshold), firms from USTAN and MiDi that do not show up in SITS have negligible service imports and exports, which can hence be set to zero. Similarly, one can be sure that a firm in USTAN or SITS is not involved in any economically significant inward or outward FDI if it does not report to MiDi.

Information on firm employment, the key outcome variable analyzed in this paper, is taken from either USTAN or MiDi, with preference given to the former data source, as it also contains the balance sheet information used to construct additional control variables in the empirical analysis. Due to the importance of firm employment, I implement a number of consistency checks to validate this variable and eliminate potential outliers, as described in Appendix A.2.

Two important variables, which are not readily observable in the data but required for parts of the empirical analysis, are physical capital stocks and total factor productivity (TFP). Physical capital stocks are constructed by the perpetual inventory method, closely following the procedure applied by Bachmann and Bayer (2014) to the USTAN data, which is outlined in Appendix A.3.

Firm productivity has been established in both theoretical and empirical work as an important determinant of firms' offshoring activities and their effects for firm employment (see Antràs and Helpman, 2004; Egger et al., 2015; Kohler and Smolka, 2014). Labor productivity (LP), defined as real value added over employment,[15] will be used as a simple proxy, but a more comprehensive and hence preferable measure of productivity is TFP. Since TFP is unobserved, it is estimated as the residual from industry-specific value added

[13] These data are obtained from https://comtrade.un.org/data/.

[14] These figures are based on data from the German national accounts from the German Statistical Office (see https://www.destatis.de/EN/). The non-financial private business sector is defined by excluding the sectors not covered in the micro data: private households, public administration, and the financial sector.

[15] Throughout the paper, wages are deflated by the consumer price index (CPI), while turnover, value added, and profits are deflated by the industry-level producer price index (complemented by the CPI whenever missing). These data are obtained from the German Statistical Office.

production functions. The estimation procedure implemented in this paper, described in Appendix A.4, resolves the well-known endogeneity issues by applying the estimator suggested by Ackerberg et al. (2015), building on the approach developed by Olley and Pakes (1996) and Levinsohn and Petrin (2003).

2.3 Stylized facts about the micro-structure of service offshoring

The subsequent analysis of correlations (in Section 3.2) and employment effects at the extensive margin of service offshoring (in Section 4) draws on the entire combined firm dataset whenever possible, subject to the availability of control variables and other required information. For the main analysis of employment effects at the intensive margin of service offshoring (in Section 5), the sample needs to be restricted to firms with positive service imports and employment information observed in several years over the period 2002-2013. The reason is that the identification strategy, which will be described in detail in Section 5.1, relies on time variation in imports. It further depends on the availability of aggregate service trade data from Comtrade, which reduces the estimation sample slightly. Since the main focus of the empirical analysis is on changes at the intensive margin, the full estimation sample used for this analysis (corresponding to the estimates reported in column 1 of Table 4) is described in more detail in this section.

The full estimation sample contains 7,100 firms (around 3,400 per year), which account on average for 71% of total service imports in SITS per year. Even though the number of firms in this sample is small compared to the original data sources, they account for the bulk of all service offshoring and around half of the aggregate employment and turnover in the combined USTAN and MiDi datasets. Hence, continuous service importers represent around 11% of aggregate employment and 25% of aggregate turnover in the German non-financial private business sector. These numbers are even higher in the manufacturing sector, where the full estimation sample accounts for 21% of aggregate employment and 27% of aggregate turnover (due to a higher coverage rate of USTAN).

Table 1 provides summary statistics of several key variables in the USTAN and SITS samples as well as the full estimation sample. The combined dataset created for this paper provides the first opportunity to examine the characteristics of German service importers compared to other firms. As pointed out by Kelle and Kleinert (2010), German firms from all sectors are engaged in importing and exporting services. These service importers are a small group, among which import volumes are heavily concentrated in even fewer firms, as the numbers cited in the previous paragraph suggest. This heterogeneity is also visible in several dimensions of firm size and performance. By comparing the USTAN sample with the subset of SITS for which additional firm information is available from the other sources, it can be seen that service importers are larger in terms of employment, capital stocks, and turnover; they pay higher wages, are more productive (in terms of LP) and more profitable (in terms of accounting profits) than other firms in USTAN. Furthermore, the table suggests that firms included in the estimation sample are even larger on average in terms of employment and turnover, which seems plausible since these firms are successfully offshoring services over several years. This fact can rationalize why the firms in the estimation sample account for the bulk of total service imports, as discussed in the previous paragraph.

The data further reveal that the service importing structure is highly firm-specific and stable over time within firms, two features that are important for the IV approach. Out of the possible 2,450 country-service type combinations, positive imports are observed for 2,030 combinations in the full estimation sample. The median firm in this sample imports ten different country-service type combinations. These import choices vary substantially across firms, such that only a small number of country-service type combinations

Table 1: Summary statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	USTAN sample			SITS sample			Full estimation sample		
Variable	Mean	SD	N	Mean	SD	N	Mean	SD	N
employment	216	2,204	276,357	619	4,255	99,305	1,017	6,129	40,905
ln *employment*	3.682	1.737	276,357	4.910	1.669	99,305	5.468	1.554	40,905
service importer	0.125	0.331	320,398	0.874	0.332	323,059	1.000	0.000	40,905
ln *service imports*	5.810	2.166	40,054	5.323	2.022	282,362	7.218	2.050	40,905
# countries	0.787	4.071	320,398	3.504	6.649	323,059	9.956	12.735	40,905
# service types	0.278	0.943	320,398	1.563	1.358	323,059	2.973	1.927	40,905
# country-service types	8.776	19.538	41,689	4.687	11.847	323,059	15.140	25.859	40,905
ln *narrow offshoring*	5.900	2.304	4,501	5.359	2.083	63,678	7.367	2.454	6,668
ln *intra-firm offshoring*	6.401	2.669	2,252	6.057	2.420	11,878	6.651	2.583	5,765
service exporter	0.037	0.189	320,398	0.317	0.465	323,059	0.440	0.496	40,905
ln *service exports*	6.924	2.435	11,865	6.223	2.257	102,428	7.865	2.400	17,990
ln *turnover*	8.947	2.009	307,217	10.722	1.654	97,814	11.289	1.587	40,753
goods exporter	0.287	0.452	320,398	0.075	0.263	323,059	0.285	0.452	40,905
ln *capital*	7.701	2.241	252,059	9.257	2.215	35,629	9.783	2.140	15,498
ln *wage*	3.734	0.568	267,276	4.062	0.457	39,868	4.077	0.373	16,868
ln *output*	4.346	2.013	307,049	6.316	1.645	39,911	6.864	1.559	16,828
ln *LP*	4.632	0.858	274,467	4.942	0.812	40,059	4.964	0.731	16,927
ln *profits*	5.582	2.115	241,046	7.579	2.069	28,704	8.031	2.028	11,361

Note: The table lists the mean, standard deviation (SD), and number of observations (N) for important variables used in the analysis for three different samples: The USTAN sample (51,926 firms, columns 1 to 3), the SITS sample (84,807 firms, columns 4 to 6), and the full estimation sample used in the analysis of employment effects at the intensive margin of service offshoring (7,100 firms, columns 7 to 9). Source: RDSC of the Deutsche Bundesbank, SITS, MiDi, and USTAN, 2001-2013, own calculations.

feature positive imports from many firms.[16] For instance, only six firms report positive imports for the median country-service type combination in 2007, the middle of the sample period. This fact illustrates that typically very few firms share the same service offshoring strategy. Instead, the importing structure is highly firm-specific, presumably driven by the specific needs of individual firms. As noted by Hummels et al. (2014) in the context of goods trade, the importing structure varies substantially across firms even within narrowly defined industries. This observation casts doubt on the standard approach in the offshoring literature, which applies the same input coefficients to all firms in an industry based on aggregate input-output tables. The German data reveal that this concern applies also to service trade, calling for a firm-level approach to measuring (and instrumenting) service offshoring, as the one pursued in this paper.

To illustrate the stability of firms' importing structure over time, I compute the share of the total service import value in the full estimation sample that is accounted for by the firm-country-service type combinations that are observed in the first importing year for each firm.[17] The data show that these firm-country-service type combinations from the first year account for 57% of the total service import value in the full estimation sample of all subsequent years. For the average firm in the estimation sample, these initial country-service type combinations even amount to 73% of their total import value. This observation reflects a high degree of persistence in the firms' service importing structure over time, which is comparable to the persistence observed in Danish goods trade (Hummels et al., 2014). These features of the data allow me to hold the initial import shares by country and service type constant when instrumenting for the intensive margin of service offshoring in the empirical analysis. Despite the stable importing structure, there is also

[16] In 2007, the most popular service imports are 'other business services' (in particular the category 'advertising, commercial, and administrative services') from Switzerland, followed by imports of the same service type from the UK and the US.

[17] The data from this first year for each firm constitute the pre-sample and will be omitted in the main empirical analysis for the reasons described in Section 5.1.

considerable within-firm variation in the value of service imports over time, reflected in a coefficient of variation of 0.429 (the standard deviation of service imports based on the within-firm variation relative to the mean) in the full estimation sample. The empirical analysis in Section 5 investigates to what extent this variation in service offshoring can explain changes in firm employment.

3 Econometric model and within-firm correlations

3.1 Econometric model

To investigate the relationship between service offshoring activities and employment of firm i, active in industry j and year t, the following econometric model is specified:

$$\ln \textit{employment}_{i,t} = \beta \cdot \textit{service offshoring}_{i,t} + \boldsymbol{\varphi} \cdot \mathbf{X}_{i,t-1} + \alpha_{j,t} + \alpha_i + \varepsilon_{i,t}, \qquad (1)$$

where the key explanatory variable $\textit{service offshoring}_{i,t}$ represents either a dummy variable indicating positive service imports (for the analysis at the extensive margin of offshoring), or the logarithm of the value of service imports $\ln \textit{service imports}_{i,t}$ (for the intensive margin), or alternative measures of service offshoring that will be introduced throughout the analysis. Firm employment is the main dependent variable of interest in this paper. Yet, other firm performance measures will also be considered as alternative dependent variables in equation (1). The main parameter of interest is β, the partial effect of service offshoring on firm employment.

Importantly, equation (1) includes firm fixed effects α_i, which absorb any time-invariant components of the firm's market environment and geographic location, its productivity, size, ownership structure, and other firm characteristics. In addition, the industry-year fixed effects $\alpha_{j,t}$ absorb any shocks to demand, factor markets, or technology that are common to all firms in an industry. The equation further includes the following firm-level control variables $\mathbf{X}_{i,t-1}$ lagged by one year (with associated coefficients $\boldsymbol{\varphi}$): dummy variables indicating whether the firm is a service exporter, an MNE, or an FIE (both defined by the MiDi thresholds, see Section 2.1), as well as $\ln turnover$.

Equation (1) is the firm-level analogue to the employment equations estimated by Amiti and Wei (2005, 2009a).[18] It can be derived as a conditional labor demand function from a standard cost minimization problem of the firm (see e.g. Hamermesh, 1993). For this purpose, it is assumed that wages are exogenous to firms and absorbed by industry-year fixed effects. This assumption is innocuous to the extent that wages are set at the industry level by unions, which play an important role in the German labor market. Nevertheless, it is relaxed in a robustness check in Section 5.7.

OLS regressions would yield consistent estimates of β only under the strong assumption that the error term $\varepsilon_{i,t}$ in equation (1) is uncorrelated with service offshoring (and the other explanatory variables), e.g. because it is due to random measurement error. While Section 3.2 does examine OLS estimates of β, I abstain from making this assumption, as it seems unlikely to hold in practice. In particular, the alternative measures of $\textit{service offshoring}_{i,t}$ may be correlated with the error term due to the simultaneity of employment and offshoring decisions, both of which are affected by output demand, technology, and labor supply shocks. Therefore, the OLS estimates presented below are interpreted as conditional correlations and causal analysis is relegated to Sections 4 and 5.

[18] In contrast to their approach of first-differencing, I prefer to estimate the equation using the within transformation to consider also medium-term employment effects, maintain a larger sample, and facilitate the analysis of interaction effects below. A first-differenced version of equation (1) is considered in Section 5.6.

3.2 Within-firm correlations

Table 2 demonstrates that various measures of service offshoring activity are positively correlated with employment and other dimensions of performance in German firms. It reports the estimated conditional correlations between various firm variables, indicated in the header (all in logs), and several alternative measures for service offshoring, indicated in each row. Each cell reports an estimate of β based on a different variant of the within-transformed equation (1).[19] The estimate shown in the top cell in column 1 examines the correlation of employment with the firm extensive margin of service offshoring, as measured by the service importer dummy. The FE estimate suggests that firms do not experience a significant increase or decrease in terms of employment as they start (or stop) service offshoring. There is also no significant correlation of the wage per employee with the importer dummy (see column 2). However, the remaining estimates reported in the first row (columns 3-6) show that service offshoring firms are larger in terms of output, more productive as measured by TFP, and more profitable (in terms of real accounting profits) than non-offshoring firms. These findings extend the results of Breinlich and Criscuolo (2011), who have estimated performance 'premia' of service importers in similar regressions using cross-sectional variation across UK firms. Table 2 reveals that these performance differences in terms of output and productivity are significant (though much smaller) even when identified from time variation within firms in Germany.

Table 2: Within-firm correlations of firm performance and service offshoring

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Employment	Wage	Output	LP	TFP	Profits	# firms range
service importer	0.00267	-0.00162	0.0374***	0.00961**	0.00820**	0.0315**	30,437-41,755
	(0.00497)	(0.00287)	(0.00464)	(0.00479)	(0.00413)	(0.0154)	
# service types	0.0168***	0.000509	0.0195***	0.00346	0.00351*	0.0159**	30,437-41,755
	(0.00202)	(0.00131)	(0.00224)	(0.00226)	(0.00206)	(0.00734)	
# countries	0.00881***	-0.000277	0.00829***	0.00148	0.00246***	0.00966***	30,437-41,755
	(0.000858)	(0.000469)	(0.000917)	(0.000898)	(0.000768)	(0.00257)	
# country-service types	0.00407***	0.0000579	0.00365***	0.000671	0.00116***	0.00334**	4,362-12,233
	(0.000486)	(0.000210)	(0.000543)	(0.000433)	(0.000350)	(0.00145)	
ln *service imports*	0.0252***	0.00126	0.0343***	0.0124***	0.0119***	0.0329***	4,193-11,872
	(0.00242)	(0.00160)	(0.00268)	(0.00275)	(0.00228)	(0.00862)	
ln *narrow offshoring*	0.0280***	-0.00468	0.0421***	0.0119	0.00687	0.0347	434-2,049
	(0.00625)	(0.00672)	(0.01000)	(0.0119)	(0.00924)	(0.0275)	

Note: The table reports FE estimates of equation (1). Each cell corresponds to one single regression, where the dependent variable is the log of the variable indicated in the header and the explanatory variable measuring service offshoring is indicated in each row. The last column reports the range of the number of firms per row. All regressions control for lagged dummy variables indicating service exporter, MNE, and FIE status, lagged $\ln turnover$, as well as fixed effects by firm and by industry-year. Standard errors reported in parentheses are robust to heteroskedasticity and autocorrelation within firms. Asterisks indicate significance levels: * p<0.1, ** p<0.05, *** p<0.01. Source: RDSC of the Deutsche Bundesbank, SITS, MiDi, and USTAN, 2001-2013, own calculations.

The second, third, and fourth row of Table 2 extend the analysis by considering several within-firm extensive margins of service offshoring as explanatory variables in equation (1). They examine one by one the correlations of firm performance with the number of source countries, service types, and country-service type combinations in which positive service imports are observed. The estimates reveal that firms which start offshoring to another country, or those adding another service type or another country-service type combination to their offshoring activities, become larger in terms of output and employment, more productive in terms of TFP, and more profitable. Plausibly, the correlations are higher at the levels of service

[19] The remaining coefficient estimates are not reported to conserve space. The last column lists the range of the number of firms per row, which varies slightly depending on data availability. These estimations, as well as most others in this paper, are implemented using the Stata command `reghdfe` (Correia, 2014).

types or countries compared to the more disaggregate country-service type combinations. The estimates suggest that, for example, a firm experiences larger performance changes if it starts offshoring IT services for the first time (to any country), compared to the situation in which it is already importing IT services from one country (e.g. India) and starts to import them additionally from another country (e.g. China). The correlations with wages and LP are very small and insignificant throughout.

The intensive margin of service offshoring is examined in in the fifth row of Table 2, which uses $\ln \textit{service imports}$ as an explanatory variable. Note that the requirement of positive imports in at least two years reduces the sample substantially compared to the full sample of USTAN and MiDi firms that was used to examine the firm extensive margin. The estimate reported in column 1 suggests that an increase in service imports by 10% coincides on average with a small increase in employment by 0.252%. The estimated coefficient is insignificant in the wage regression, while output, productivity, and profits are positively correlated with the intensive margin of offshoring. The sixth row applies an alternative, narrow definition of offshoring by focusing on imports in the service type that corresponds to the firm's own 2-digit NACE Rev. 1.1 industry code (similar to the narrow measure by Feenstra and Hanson, 1999). Since only firms in the service sector can have positive values of narrow offshoring, the sample is drastically reduced. The estimated correlations with narrow offshoring are positive for employment and output, but insignificant for all other performance variables.

Due to the endogeneity issues discussed in Section 3.1, the OLS estimates in Table 2 cannot be interpreted as causal effects of offshoring. In particular, the positive estimates for employment and output may reflect a positive efficiency gain from offshoring, but they could also be driven by demand shocks or productivity shocks inducing simultaneity bias. These issues are addressed in detail in the next sections. First, Section 4 scrutinizes the effect of starting service offshoring (the firm-level extensive margin) on employment through matching methods. Second, Section 5 investigates the employment effect of increasing service offshoring (the intensive margin) through an IV.

4 Employment effects at the extensive margin of service offshoring

4.1 Combining difference-in-differences and propensity score matching

In order to estimate the employment effect at the extensive margin of service offshoring, this section adopts a difference in differences (DiD) propensity score matching (PSM) approach. This approach compares the employment changes in offshoring firms to those in a suitably defined control group of non-offshoring firms. Formally, I am interested in the average effect of starting service offshoring on the employment of offshoring firms, i.e., the average treatment effect on the treated (ATT):

$$ATT_{\tau,t} = (\Delta_{\tau,t} L_{1,i} - \Delta_{\tau,t} L_{0,i} | O_{i,t} = 1), \qquad \tau = \{1, 2, 5\}. \tag{2}$$

In this equation, $\Delta_{\tau,t} L_{o,i} \equiv \left(\ln \textit{employment}_{o,i,t-1+\tau} - \ln \textit{employment}_{o,i,t-1}\right)$ denotes the relative change in employment of firm i with offshoring status $o = \{0, 1\}$ between the pre-treatment year $t-1$ and year $t-1+\tau$. The treatment variable $O_{i,t}$ is an indicator for offshoring firms that start to import services in year t for the first time (within the sample period 2001-2013). The ATT in equation (2) is estimated for three different time horizons. In the first specification, I set $\tau = 1$ to examine the immediate employment effect in the first offshoring year. Since domestic employment may need time to adjust, I examine also the medium-term effects after two and five years ($\tau = 2, 5$). The latter two exercises adopt the convention that a firm is classified as offshoring only if it continues to import services in two (five) consecutive years.

The obvious challenge in estimating the ATT in equation (2) is that the employment changes of an offshoring firm i are observed only for the situation in which it is offshoring $(\Delta_{\tau,t}L_{1,i})$, but not for the *counterfactual* situation in which it is *not* offshoring $(\Delta_{\tau,t}L_{0,i})$. One way to address this challenge is by using the employment changes of a suitable control group to impute the counterfactual employment changes of the offshoring firms. As a potential control group, I consider all firms which have not previously imported any services within the sample period and do not start offshoring in the same year. However, some of these non-offshoring firms differ in important ways from the treated firms and may hence not be suitable to estimate $\Delta_{\tau,t}L_{0,i}$. To illustrate these differences, Figure 1 shows the evolution of average employment over time in unmatched samples of offshoring and non-offshoring firms for four exemplary configurations of treatment and control groups. The graphs on the left examine firms that start service offshoring in 2006 (compared to those that do not), while the graphs on the right consider the starting year 2010. The top graphs only restrict the sample to those firms that did not import services before, and the bottom graphs additionally require that offshorers consecutively import services for at least two years. It is apparent from all four configurations that service offshoring firms are larger in terms of employment throughout the observation period, including the years before they start offshoring.[20] This pattern may be explained by the fact that larger firms self-select into offshoring, for instance, because they are more productive and hence better able to cover the associated fixed costs (see e.g. Antràs and Helpman, 2004).

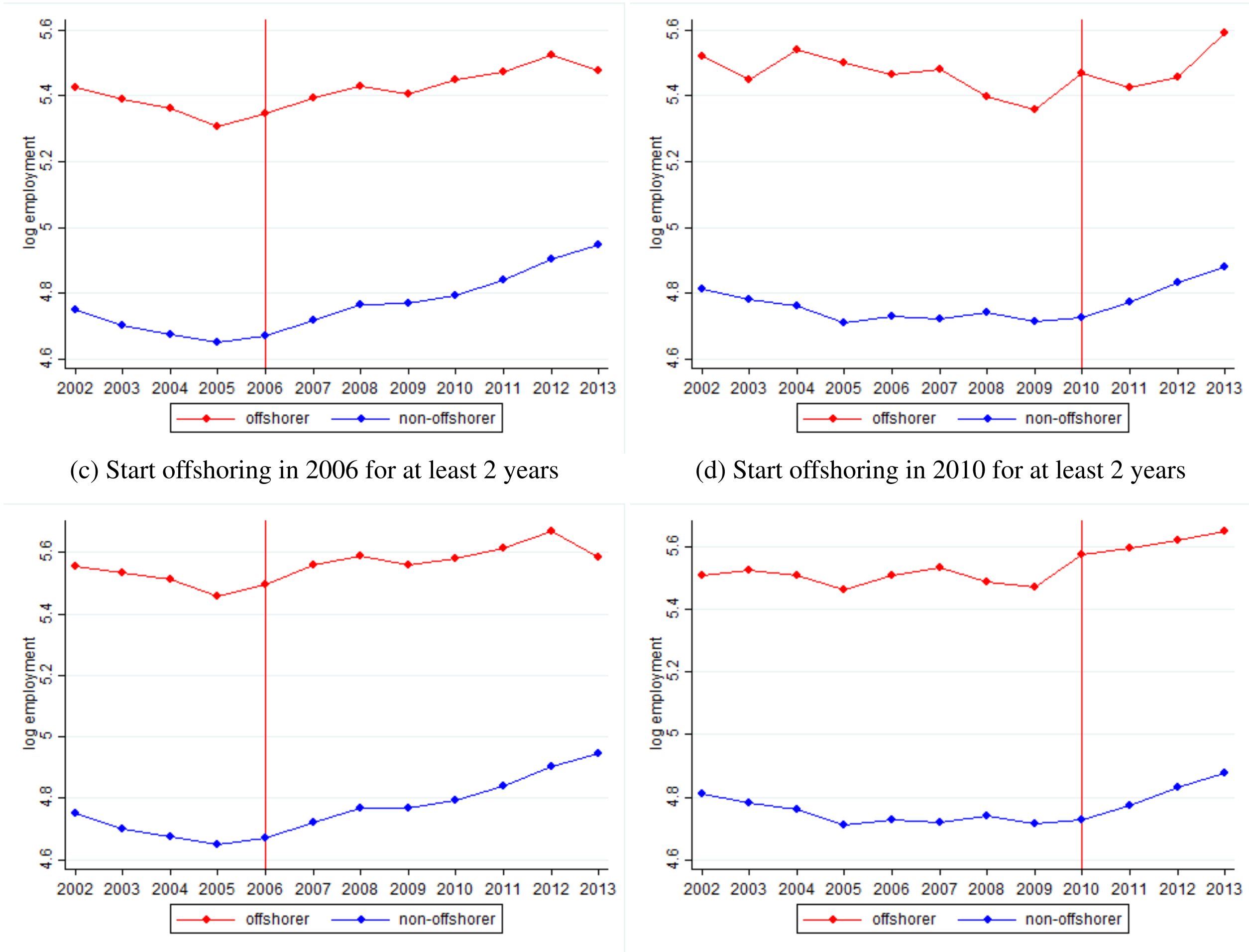
Figure 1 further suggests that employment in offshoring and non-offshoring firms follows a similar trend in the pre-offshoring period for the examples depicted. The assumption of a common pre-treatment trend is necessary for consistently estimating the ATT in a standard DiD setup, so this observation is reassuring. Also, there seems to be a small positive deviation from the trend for offshoring firms in the year in which they start importing services, in particular for those starting to offshore in 2010. However, these observations are merely indicative and may be confounded relative to the true ATTs by a variety of factors, e.g. if the size differences across offshoring and non-offshoring firms discussed above have dynamic implications.

To provide a more complete and rigorous assessment of the ATT, I implement propensity score matching (PSM). The idea behind PSM is to determine a control group of firms that are as comparable as possible to the treated firms based on a set of observed covariates $\mathbf{M}_{i,t-1}$. In their seminal work, Rosenbaum and Rubin (1983) show that this objective can be achieved by matching observations based on the treatment probability predicted by $\mathbf{M}_{i,t-1}$, the propensity score $p_{i,t} \equiv Pr(O_{i,t} = 1|\mathbf{M}_{i,t-1})$. This approach allows for consistently estimating the ATT under three assumptions. First, the 'conditional mean independence' assumption requires that the average outcome (in the present application, the change in employment) is independent of the treatment conditional on the covariates, which is formally stated as $E\left[\Delta_{\tau,t}L_{0,i}|\mathbf{M}_{i,t-1}, O_{i,t}\right] = E\left[\Delta_{\tau,t}L_{0,i}|\mathbf{M}_{i,t-1}\right]$.[21] Loosely speaking, this assumption is fulfilled if on average the observed pre-treatment characteristics $\mathbf{M}_{i,t-1}$ account also for unobserved factors that influence selection into treatment. While standard in the matching context, the conditional mean independence assumption is crucial for identification and fundamentally untestable. However, covariate balancing tests discussed below provide reassuring evidence that the PSM approach successfully accounts for differences between treated and untreated firms. Second, the 'stable unit treatment value' assumption requires that the treatment affects all treated firms similarly and does not affect the non-treated firms. Arguably, this assump-

[20] Similar differences in terms of various firm characteristics are apparent from a comparison of the full USTAN sample with the preferred estimation sample, which includes only the subset of firms with positive imports (see Table 1).

[21] This assumption, sometimes referred to as unconfoundedness or ignorability in means (Wooldridge, 2010), is weaker than strict ignorability of the treatment, as the treatment is required to be ignorable only on average.

Figure 1: Employment over time for offshoring and non-offshoring firms



(a) Start offshoring in 2006

(b) Start offshoring in 2010

(c) Start offshoring in 2006 for at least 2 years

(d) Start offshoring in 2010 for at least 2 years

Note: The graphs plot the log of average employment for different groups of firms over time. The top left (right) graph defines offshoring firms as those starting to import services for the first time in 2006 (2010). The bottom left (right) graph defines offshoring firms as those starting to import services for the first time in 2006 (2010) and consecutively import services for at least two years. Source: RDSC of the Deutsche Bundesbank, SITS, MiDi, and USTAN, 2001-2013, own calculations.

tion may be questioned in any setting in which treated firms are competing with untreated firms in imperfect product or factor markets and may hence affect their performance through spillovers. However, since service imports make up a small share of firms' purchases on average, and given that firms are not matched within narrowly defined industries or regions, any violations of this assumption should have negligible effects in the present application. Third, the 'common support' assumption requires overlap between treated and untreated firms, i.e., $Pr(O_{i,t} = 1|\mathbf{M}_{i,t-1}) < 1 \,\forall\, \mathbf{M}_{i,t-1} \in \mathcal{M}_{t-1}$, where $\mathcal{M}_{t-1}$ is the support of the covariates. Intuitively, for each treated firm and each covariate, there must exist at least one untreated firm with the same value for the covariate. This condition is enforced below by excluding the few treated firms off the common support.

The propensity score $p_{i,t}$ is estimated separately for each offshoring year t using logit regressions of the treatment dummy $O_{i,t}$ on the matching covariates $\mathbf{M}_{i,t-1}$.[22] The variables included in $\mathbf{M}_{i,t-1}$ are chosen to make firms comparable in terms of their initial service trade and foreign investment linkages, size, and key employment characteristics. The preferred set of covariates $\mathbf{M}_{i,t-1}$ includes the following firm

[22] Since the sets of treated and control firms vary by the selected time horizon $\tau = \{1, 2, 5\}$, three different logit regressions are implemented for each year t (and strictly speaking, the propensity score should have an index τ, omitted for brevity).

characteristics measured in the pre-treatment year $t-1$: dummy variables indicating whether the firm is a service exporter, an MNE, or a FIE, as well as log values of the firm's turnover, employment, and average wage per employee. This choice is the result of solving a trade-off between data availability and match quality, and it resembles the sets of covariates used in the previous matching literature on offshoring (Crinò, 2010a; Monarch et al., 2017; Moser et al., 2015). Also, the main conclusions obtained below are insensitive to small variations in this set. The logit regressions reveal that the preferred covariates are individually significant predictors of treatment in most years and jointly yield a pseudo R^2 of around 18% on average.

Several alternative matching algorithms may be used in combination with the estimated propensity score to construct an appropriate counterfactual. In my preferred specification, I employ the kernel-based matching estimator proposed by Heckman et al. (1997), so the ATT is formally computed as:

$$ATT_t = \frac{1}{N_t}\sum_{i=1}^{N_t}\left[\Delta_{\tau,t}L_{1,i} - \sum_{j\in J_{i,t}} w(p_{i,t},p_{j,t})\Delta_{\tau,t}L_{0,j}\right] \text{ if } |p_{i,t}-p_k| < 0.01, \qquad (3)$$

where N_t denotes the number of offshoring (treated) firms in year t, and $w(p_{i,t},p_{j,t})$ is the weight of each matched firm j from the control group $J_{i,t}$:

$$w(p_{i,t},p_{j,t}) = \frac{K\left[(p_{i,t}-p_{j,t})/b\right]}{\sum_{j\in J_{i,t}} K\left[(p_{i,t}-p_{j,t})/b\right]}.$$

The preferred specification uses an Epanechnikov kernel function $K[\cdot]$ and a bandwidth $b = 0.01$, so $J_{i,t}$ is defined as the set of all non-offshoring firms j for which $|p_{i,t}-p_{j,t}| < 0.01$. This matching algorithm is chosen because it proves to be superior to several alternative procedures in terms of the subsequent covariate balancing tests (discussed below). The DiD PSM estimator in equation (3) effectively compares employment changes in offshoring firms to the weighted average of employment changes of multiple firms in the control group, where the narrow bandwidth ensures that only very similar firms are included in the control group, and the kernel function ensures that the most similar firms receive the highest weights. The matching procedure is implemented using the Stata command `psmatch2` (Leuven and Sianesi, 2003) and standard errors are bootstrapped based on 200 replications.[23]

Note that there is a crucial difference between the DiD PSM estimator employed in this paper and cross-sectional matching estimators, previously used for instance by Crinò (2010a) in the context of service offshoring. Crinò (2010a) uses PSM to compare employment *levels* across firms that import services and those that do not in a single cross-section. By contrast, this paper defines offshorers as firms that start importing services and exploits *time variation* within firms to identify the employment effects. Thereby, the DiD PSM estimator accounts for time-invariant unobservable firm characteristics and is hence superior to simple cross-sectional matching approaches (see Heckman et al., 1997; Smith and Todd, 2005). Furthermore, the panel data at hand allow for distinguishing the immediate impact of service offshoring from medium-term changes to shed some light on the persistence of the employment effects.

4.2 Employment effects of starting service offshoring

The results of implementing the DiD PSM procedure year by year are summarized in Table 3. Panel A reports the immediate ATT of service offshoring on firm employment in the first offshoring year. Panels B

[23] While Abadie and Imbens (2008) argue that the bootstrap is not generally valid in the case of nearest neighbor matching, they expect that it is valid in the case of the kernel-based matching algorithm employed in this paper, for which the number of matches increases in the sample size.

and C consider the medium-term effects in firms that start and consecutively offshore services over two and five years, respectively. The estimated ATTs fluctuate around zero and are never significantly negative. Instead, firms starting to offshore in the years 2005, 2006, and 2010 even increase their employment significantly compared to the matched control group in their first offshoring year. These positive ATTs are also confirmed over the medium term for firms that consecutively offshore for at least two years. For the small set of firms that starts and consecutively offshores for five years, the estimated ATTs tend to be positive over this time horizon. However, the effects are only significant for firms that start offshoring in 2003 or 2006. Overall, the estimated ATTs in Table 3 are rather unstable and insignificant in the majority of years. Hence, these findings are best summarized as evidence for non-negative employment effects at the extensive margin of service offshoring.

Table 3: Employment effects of starting service offshoring in DiD PSM estimations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
A. Immediate employment effects in the first year												
$ATT_{1,t}$	-0.00134	0.0168	-0.00927	0.0554**	0.0560**	0.0191	0.000728	0.0142	0.0537**	-0.0111	0.00346	0.0142
	(0.0191)	(0.0204)	(0.0183)	(0.0250)	(0.0257)	(0.0154)	(0.0196)	(0.0176)	(0.0230)	(0.0133)	(0.0155)	(0.0142)
Firms	16,302	14,633	13,872	13,407	13,388	13,455	13,622	13,765	14,220	14,392	14,050	13,114
Treated	1,120	886	866	792	840	842	800	812	930	880	806	750
MABR	0.93	0.85	0.95	0.87	0.90	0.91	0.92	0.86	0.94	0.90	0.96	0.87
Pseudo R^2	0.00145	0.00354	0.0031	0.00524	0.00259	0.00327	0.0015	0.00522	0.0016	0.00582	0.0012	0.00266
B. Employment effects over two years (consecutive service offshoring)												
$ATT_{2,t}$	0.00909	0.0419	-0.00118	0.0848**	0.104***	0.0353	0.0295	0.0315	0.109***	-0.0350	0.0207	
	(0.0295)	(0.0338)	(0.0508)	(0.0375)	(0.0385)	(0.0399)	(0.0373)	(0.0358)	(0.0374)	(0.0262)	(0.0224)	
Firms	12,935	12,198	11,855	11,893	11,936	12,131	12,226	12,665	12,911	12,975	12,294	
Treated	566	480	424	422	454	456	380	418	482	494	418	
MABR	0.90	0.92	0.95	0.87	0.92	0.93	0.96	0.97	0.93	0.86	0.97	
Pseudo R^2	0.00299	0.00217	0.00147	0.00812	0.00421	0.00129	0.0015	0.00173	0.00163	0.00559	0.00183	
C. Employment effects over five years (consecutive service offshoring)												
$ATT_{5,t}$	0.0213	0.285***	0.0812	0.139	0.230***	0.0970	-0.232					
	(0.0871)	(0.0900)	(0.159)	(0.112)	(0.0782)	(0.112)	(0.258)					
Firms	7,748	7,705	7,838	8,269	8,602	8,822	8,618					
Treated	178	104	58	47	66	54	36					
MABR	0.92	0.78	0.74	0.55	0.69	0.62	0.17					
Pseudo R^2	0.00355	0.0359	0.0281	0.101	0.0305	0.0436	0.0318					

Note: The table reports the estimated $ATT_{\tau,t}$ for firms that start offshoring services in year t (indicated in the header) on changes in employment over a time period of $\tau = \{1, 2, 5\}$ years, based on the preferred DiD PSM estimator (Epanechnikov kernel matching with a bandwidth of $b = 0.01$). The bottom of each panel reports the median absolute bias reduction (MABR) in terms of the pre-treatment matching covariates as well as the pseudo R^2 from regressing these covariates on the estimated propensity score. Standard errors reported in parentheses are bootstrapped based on 200 replications. Asterisks indicate significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Source: RDSC of the Deutsche Bundesbank, SITS, MiDi, and USTAN, 2001-2013, own calculations.

The covariate balancing tests reported at the bottom of each panel suggest that the PSM procedure is very effective at making the treated and control groups comparable. For the immediate and two-year effects in panels A and B, the medium absolute bias reduction (MABR) in terms of the pre-treatment matching variables is 85-97%. Also, logit regressions of the estimated propensity score on the matching variables yield pseudo R^2 values in the range of 0.001-0.008. For the rather small set of firms in the bottom panel, the same tests indicate a lower, though satisfactory match quality with a MABR of 64% and a pseudo R^2 of 0.039 on average. Overall, the PSM seems to successfully account for observable pre-treatment differences in the matching covariates. To the extent that these covariates also account for unobserved heterogeneity, the DiD PSM estimator can serve to identify the ATTs.

Since a variety of alternative matching algorithms and potential covariates are available, I explore the sensitivity of my results to these choices in robustness checks (which are not reported to save space). The estimated ATTs are almost identical for radius matching (with a caliper of 0.01), and they have very similar magnitudes for single nearest-neighbor matching (without caliper). I have also experimented with varying the set of matching covariates by excluding lagged employment or wages, or by adding the lagged change in employment. Such variations slightly reduce the match quality (in terms of the MABR), but leave the main conclusions largely unchanged. All of these robustness checks confirm the main insight from Table 3: The employment effects at the extensive margin of service offshoring are non-negative within German firms and may even be positive in some years.

5 Employment effects at the intensive margin of service offshoring

5.1 Identification through instrumental variable

Estimates of equation (1) by OLS to investigate the intensive margin of service offshoring (as shown in Table 2) may be subject to bias from (at least) three sources. First, any firm-specific productivity or output demand shock that is not captured by the industry-year fixed effects can be expected to bias the estimate of β upward due to simultaneity. The reason is that firms which get more productive or face increasing demand should both grow larger in terms of output and employment *and* find it easier to overcome the costs associated with offshoring (see, e.g. Egger et al., 2016; Sethupathy, 2013). Second, it can be expected that firm-specific (or regional) labor supply shocks bias the estimate of β downward. To see this, consider a positive shock to labor supply, e.g. due to the moderate negotiation strategy of a firm-level union that demands rather low wages. This increases firm-level employment and simultaneously discourages offshoring, which is ultimately motivated by international wage differences. The result is a downward bias in β. Third, the value of imports is an imperfect proxy for the amount of service offshoring, and this measurement error leads to attenuation bias towards zero as long as it is not systematic. While the net direction and magnitude of these combined biases is a priori not clear, they all seem to be of first-order relevance.

In order to address the aforementioned endogeneity issues, I exploit world export supply (WES) by the firms' trading partners as an IV for the intensive margin of service offshoring. More precisely, the IV is the firm-specific mix of countries' export supply to the rest of the world by service type. Intuitively, a German firm importing a given service from a given partner country benefits disproportionately from an improvement in this country's comparative advantage, reflected in its growing exports of this service. Thus, the firm can respond to the positive supply shock by increasing its amount of service offshoring. Arguably, the service exports of foreign countries to the rest of the world have no other direct or indirect effect on firm-level employment, nor are they affected by the German firm itself or by other determinants of the firm's employment decision (conditional on the control variables, which include industry-year fixed effects). Based on this exclusion restriction, WES can be used to instrument for service imports in equation (1). This idea was first proposed by Hummels et al. (2014) for Danish firms' goods trade, and I closely follow their approach in constructing the IV for German firms' service trade.[24] The availability of international data on bilateral service trade imposes a constraint on the level of disaggregation at which the IV can be constructed (see Section 2.1). As a result, the IV exploits variation in service exports from 233 partner countries (or

[24]The scope, structure, and level of detail in the German service trade data is comparable to that of the Danish goods trade data used by Hummels et al. (2014). Autor et al. (2013) use a similar approach at a more aggregate level, which concentrates on the impact of Chinese imports on local labor markets in the US and has been applied to German goods trade by Dauth et al. (2014).

territories) across ten service types over twelve years.

The firm-specific and time-varying instrument $IV_{i,t}$ is computed as the following weighted sum:

$$IV_{i,t} = \sum_{s} \sum_{c} s_{i,c,s,0} WES_{c,s,t}, \tag{4}$$

where world export supply $WES_{c,s,t}$ is the export supply by country c of service type s in year t to all countries in the world except Germany. The weights are firm-level import shares $s_{i,c,s,0}$, defined as the firm's share of imports by country and service type in its total service imports in year $t = 0$, which indicates the first year in which positive service imports by the firm are observed. The first year is 2001 for the majority of all firms in the estimation sample, but it is a later year for firms that started to import during the sample period. All of the observations from $t = 0$ form the pre-sample, which is subsequently omitted in all estimations. As demonstrated in Section 2.3, German firms' service importing structure is very specific and stable over time. The fact that very few firms share the same service import mix ensures that there is substantial firm-level variation in the IV, even though $WES_{c,s,t}$ by itself is not firm-specific. The stability of the importing structure further allows me to hold the import shares fixed at their pre-sample values, thereby avoiding a potential endogeneity issue due to adjustments in these shares over time. Following Hummels et al. (2014), the first-stage estimation uses a log-log specification, regressing $\ln \textit{service imports}_{i,t}$ on $\ln IV_{i,t}$.

5.2 Employment effects of increasing service offshoring

Table 4 presents one of the main findings of this paper: Increased service offshoring has significantly boosted firm-level employment in Germany over the period 2002-2013. Panel A of the table reports estimates from two-stage least squares (2SLS) regressions of equation (1), instrumenting for service offshoring by world export supply as described in Section 5.1. The first column considers the full available estimation sample of firms importing services in multiple years. In this sample, the instrumented service offshoring is estimated to increase firm-level employment by an elasticity of 7.6%, which is statistically significant at the 1% level. This positive estimate provides evidence for efficiency gains from service offshoring that lead to firm-level employment gains. These gains are also economically significant. The estimate suggests that an increase in service offshoring by 10% (or approximately € 2 million at the mean) would create around eight new jobs at an average firm in this sample, which has 1,000 employees.

The positive employment effect of service offshoring is confirmed in each of two subsamples defined by the source of the employment data – MiDi (column 2, comprising MNEs and FIEs) and USTAN (column 3). The effect is larger in the sample of MNEs and FIEs compared to the USTAN sample, but it maintains its statistical significance and a similar magnitude in both subsamples. Restricting the sample to USTAN further allows me to include the following firm-level control variables that may affect employment (in column 4): a dummy variable indicating goods exporter status, the log of the physical capital stock, and the log of the real wage per employee, all lagged by one year. The estimated effect of service offshoring changes only slightly through the inclusion of these additional control variables and implies an elasticity of 5.2%. The subsequent analysis concentrates on the preferred specifications from columns 1 and 4 of Table 4, with the former maximizing the sample size and the latter maximizing the set of relevant control variables.

In all of the 2SLS regressions, $\ln IV$ proves to be a strong instrument for $\ln$ *service imports*, as evidenced by the first-stage regression diagnostics in panel B of Table 4. The instrument is a significant predictor of service offshoring in all samples, characterized by very high F-statistics and a partial R^2 of 12-14%, based on the variation within firms and within industry-years. Given the persistence of the firm-specific sourcing pattern, it is not surprising that the firm-specific export supply shocks reflected in the IV explain a substantial part of the time variation in service offshoring.[25] Hence, under the assumption that partner countries' world export supply is exogenous to employment in German firms, the 2SLS estimates can be interpreted as causal effects of increasing service offshoring.

Table 4: Employment effects of increasing service offshoring in 2SLS estimations

	(1)	(2)	(3)	(4)
A. Second-stage estimates. Dependent variable: ln *employment*				
	Full sample	MiDi	USTAN	
ln *service imports*	0.0734***	0.0730***	0.0559***	0.0505***
	(0.00861)	(0.00986)	(0.00928)	(0.00834)
lag *service exporter*	-0.00592	-0.00652	-0.00230	-0.00456
	(0.00938)	(0.0107)	(0.0124)	(0.0111)
lag *MNE*	0.0279**	0.0336**	0.0187	0.0256**
	(0.0124)	(0.0159)	(0.0130)	(0.0123)
lag *FIE*	-0.0468**	-0.0415*	-0.0209	-0.0156
	(0.0184)	(0.0213)	(0.0193)	(0.0171)
lag ln *turnover*	0.269***	0.276***	0.205***	0.264***
	(0.0147)	(0.0159)	(0.0183)	(0.0250)
lag *goods exporter*				-0.00161
				(0.00788)
lag ln *capital*				0.153***
				(0.0154)
lag ln *wage*				-0.338***
				(0.0469)
Observations	40,905	32,954	16,574	14,785
Firms	7,100	5,697	2,919	2,688
B. First-stage estimates. Dependent variable: ln *service imports*				
ln *IV*	0.261***	0.262***	0.261***	0.253***
	(0.00705)	(0.00797)	(0.0103)	(0.0109)
F-statistic (excl. IV)	1,374.5	1,081.2	636.2	538.5
p-value of F-test	0.000	0.000	0.000	0.000
Partial R^2 (excl. IV)	0.122	0.119	0.137	0.132

Note: The table reports 2SLS estimates of equation (1). The top panel reports second-stage estimates with ln *employment* as the dependent variable. All regressions control for fixed effects by firm and by industry-year. The bottom panel reports results from the first-stage regressions with ln *service imports* as the dependent variable. Standard errors reported in parentheses are robust to heteroskedasticity and autocorrelation within firms. Asterisks indicate significance levels: * p<0.1, ** p<0.05, *** p<0.01. Source: RDSC of the Deutsche Bundesbank, SITS, MiDi, and USTAN, 2001-2013, own calculations.

It is instructive to compare the 2SLS estimate of β in the first column of Table 4 to the corresponding OLS estimate in the first column (row 5) of Table 2. The 2SLS estimate is substantially greater, suggesting a downward bias in the OLS estimate. This finding may seem surprising to readers who expect a positive simultaneity bias arising from productivity or demand shocks, which would increase both offshoring

[25]The strong first-stage results reported in Table 4 mirror the significant power of the instruments found by Hummels et al. (2014) even when they consider multiple interaction terms at the firm-worker level. The high F-statistics in my application accommodate any potential concerns related to weak instruments.

capabilities and firm employment. Three considerations may reconcile the estimation results with these expectations. First, it seems plausible that firm fixed effects account well for inherent productivity differences and industry-year fixed effects capture output demand shocks to a large extent, so these sources of bias do not dominate in the OLS regression. Second, the apparent downward bias may be explained by labor supply shocks, which simultaneously boost employment and discourage offshoring, as discussed in Section 5.1. For instance, a firm-level union practicing wage moderation would maintain employment at a relatively high level and simultaneously reduce the incentive for offshoring. A regional migration inflow decreasing the wage in the local labor market would have similar effects. The negative correlation between service offshoring and employment implied by such labor supply shocks biases the OLS estimate of β downward if they are not adequately controlled for. Third, given that the true employment effect is positive, any non-systematic measurement error in service offshoring implies attenuation bias that contributes to the downward bias. The IV approach can eliminate this downward bias, resulting in a larger 2SLS estimate for β compared to the OLS regression.

The positive employment effects estimated in Table 4 extend and strengthen previous findings by Amiti and Wei (2005) and Hijzen et al. (2011) for the UK. In particular, Amiti and Wei (2005) uncover mostly positive correlations between service offshoring and employment at the industry level, and Hijzen et al. (2011) show that this positive correlation is remarkably robust at the firm level. The new results presented in Table 4 show that the employment effect of service offshoring is positive and significant in German firms when IV techniques are applied to resolve the obvious endogeneity issues. My findings indicate that the positive correlations of service offshoring and firm employment that can also be found in Germany (see Table 2) are not driven by confounding factors, but instead, they even seem to be smaller than the positive causal effects. The 2SLS estimates suggest the conclusion that efficiency gains from service offshoring can indeed boost firm-level employment. These findings constitute novel firm-level evidence for the mechanism described as a productivity effect by Grossman and Rossi-Hansberg (2008), which can rationalize employment gains due to cost savings from offshoring. The subsequent sections investigate this productivity effect as a source of the employment gains in more depth.

5.3 Effects on wages, output, and productivity

To better understand the changes that service offshoring triggers within the firm, this section investigates its impact on other firm outcomes. It considers the wage per employee, output, LP and TFP (all in real terms and in logs) as alternative dependent variables in equation (1), estimated with and without the additional firm-level control variables from column 4 of Table 4. Table 5 summarizes the results of 2SLS estimations, in which $\ln$ *service imports* is instrumented by $\ln IV$ throughout.

How does service offshoring affect wages? In fact, a substantial part of the offshoring literature has concentrated on wage effects. On the one hand, one would expect the direct relocation effect to depress wages because it frees up domestic labor (Grossman and Rossi-Hansberg, 2008). On the other hand, the average wage within the firm may be positively affected by the productivity effect, rent-sharing mechanisms (Sethupathy, 2013), and compositional effects in the labor force.[26] The 2SLS wage regressions in the first two columns of Table 5 find none of these effects dominating, as the estimated coefficient is close to zero in both specifications. The data do not allow for rejecting the hypothesis of a zero effect of increased service offshoring on wages in German firms. This ambiguous result is not surprising in light of the different countervailing forces just discussed. In combination with the employment gains from Table 4, these find-

[26] Service offshoring has been found to increase the relative demand for high-skilled labor (Crinò, 2010b,a, 2012).

ings suggest that service offshoring indeed benefits the overall domestic workforce within the firm, which experiences increased employment without wage cuts on average.

The analysis proceeds by investigating the output effect of service offshoring. A positive output effect is an important precondition for positive employment effects. The firm can be expected to hire more domestic labor only if the efficiency gains from service offshoring induce it to increase its optimal scale of production. Hence, if the productivity effect is responsible for the positive employment effect identified above, there should also be direct evidence for a positive impact of service offshoring on firm-level output. The estimates reported in columns 3 and 4 of Table 5 confirm this expectation. Service offshoring significantly increases firm-level output, and the estimated output gains are substantial, implying an elasticity of 7.1-8.8$\%$. To account for the fact that employment and output may be jointly driven by service offshoring, I have also experimented with estimating the equations for employment and output jointly in three-stage least squares regressions, similar to the approach developed by Wright (2014) in the context of US industry-level offshoring. These (unreported) estimation results confirm the positive effects on both output end employment.

Table 5: Effects on wages, output, and productivity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Wage		Output		LP		TFP	
ln *service imports*	0.00174 (0.00532)	0.00656 (0.00452)	0.0847*** (0.0122)	0.0685*** (0.0106)	0.0134 (0.00993)	0.0104 (0.00905)	0.0210** (0.00881)	0.0181** (0.00856)
Additional controls	no	yes	no	yes	no	yes	no	yes
Observations	16,659	14,222	16,910	14,239	16,718	14,233	15,059	13,831

Note: The table reports 2SLS estimates of equation (1), where the dependent variable is the logarithm of the variable indicated in the header. All regressions control for the basic set of lagged firm control variables and fixed effects by firm and by industry-year. The even columns further account for the additional firm-level control variables from column 4 of Table 4. Standard errors reported in parentheses are robust to heteroskedasticity and autocorrelation within firms. Asterisks indicate significance levels: * p<0.1, ** p<0.05, *** p<0.01. Source: RDSC of the Deutsche Bundesbank, SITS, MiDi, and USTAN, 2001-2013, own calculations.

The last four columns of Table 5 examine the effects of offshoring on productivity by using $\ln LP$ and $\ln TFP$ as alternative dependent variables in equation (1). For LP as a crude proxy for productivity, the estimated effect of service offshoring is insignificant, though the point estimates are positive in both specifications. Apparently, service offshoring increases both value added and employment, so the ratio between these two variables does not increase significantly. However, when considering estimated TFP as a more comprehensive measure of productivity, the 2SLS regressions uncover a significantly positive effect of service offshoring in both specifications (columns 7 and 8). These novel, firm-level results extend the existing evidence on a positive association between service offshoring and productivity that has been found in industry-level data by Amiti and Wei (2009b) for the US and by Crinò (2008) for Western Europe. In sum, Table 5 provides direct evidence for the fact that increased service offshoring raises firm output and productivity, which can rationalize the employment gains identified in the main analysis.

5.4 Heterogeneous employment effects

Table 6 shows interesting patterns of firm heterogeneity, which support the view that the cost savings from offshoring are responsible for the firm-level employment growth. The employment effects are expected to increase in the initial scale of offshoring because the productivity effect works through the infra-marginal units already offshored. To see this, note that there will be no positive output effect on employment (but only a negative relocation effect) in a firm that starts to offshore the first job. By contrast, a firm that has already offshored some service activities experiences the beneficial productivity effect from further reductions in offshoring costs in all infra-marginal units that have already been offshored. Hence, a positive shock to offshoring should have more favorable employment effects in firms that have already offshored more service. This prediction follows intuitively from the original trade in tasks model by Grossman and Rossi-Hansberg (2008), and it is formally derived in recent extensions of their model with heterogeneous firms by Egger et al. (2015, 2016).

Panel A of the table directly tests this prediction by considering interaction terms of offshoring with alternative measures of lagged offshoring. The first two columns show 2SLS estimates of equation (1), with and without additional firm-level control variables, adding the one-year lag of $\ln$ *service imports* and an interaction term of this lag with the contemporaneous $\ln$ *service imports*. Excluded instruments are $\ln IV$ and the interaction between $\ln IV$ and the lag of $\ln$ *service imports*. In both specifications, the interaction term is positive and (at least weakly) significant, substantiating the idea that the employment effects increase in the lagged level of service offshoring.

Columns 3 and 4 of panel A take a closer look at the distribution of initial service imports. For this purpose, I define four dummy variables indicating quartiles of the firm distribution of lagged imports ($Q1 - Q4$ in the table), which are included and interacted with $\ln$ *service imports*. These interaction terms are instrumented by interactions of the quartile dummies with $\ln IV$. The estimation results in the full sample reveal employment effects of service offshoring that are monotonically increasing in lagged offshoring. The specification with additional control variables shows no significant effect in the bottom quartile, positive effects of similar magnitude in the second and third quartiles, and the highest point estimate in the top quartile. In both specifications, the employment effect is significantly greater in the top compared to the bottom quartile by a factor of two to four. These estimates suggest that the employment effects of service offshoring are small at low initial levels of imports, and they increase in the value of services that have already been offshored. This is exactly the pattern expected based on the productivity effect of offshoring from the trade in tasks model. A higher initial level of offshoring implies greater cost saving and hence more beneficial employment effects from a positive shock to foreign service exports, as captured by the IV. The estimates in columns 3 and 4 of panel A provide strong support for this theoretical prediction.

The fact that the estimated employment effects in column 4 of Table 6 are not strictly monotonic around the middle of the lagged service imports distribution raises the question of appropriate measurement. In particular, a given value of service imports may correspond to very different shares of employment in large compared to small firms. Hence, an alternative measure of lagged service imports scaled by firm size may be better able to capture a firm's initial exposure to offshoring cost shocks (and hence the relevance of the productivity effect). Columns 5 and 6 of panel A repeat the previous exercise, but in this case, the dummy variables $Q1 - Q4$ distinguish quartiles of the distribution of lagged $\frac{\textit{service imports}}{\textit{turnover}}$. The resulting point estimates for the interaction terms increase monotonically in both specifications, with firms in higher quartiles experiencing greater employment gains from service offshoring. Note that the 2SLS estimates in columns 3-6 each require four first-stage regressions for the four endogenous interaction terms. Nevertheless, in all of these estimations, the instruments are strong as evidenced by high Wald rk F-statistics for the cluster-robust rank test proposed by Kleibergen and Paap (2006), which seem reassuringly high compared to conventional thresholds for weak IV tests (see Stock and Yogo, 2005), with the minor exception of column 4.

Panel B of Table 6 takes an alternative view on firm heterogeneity in the employment effect of offshoring by distinguishing firms in terms of their productivity. Firm productivity is the key determinant of

Table 6: Heterogeneous employment effects by lagged offshoring and firm productivity

	(1)	(2)	(3)	(4)	(5)	(6)
A. Interactions with lagged offshoring						
	Lag ln *service imports*		Quartiles of lag imports		Quartiles of lag $\frac{imports}{turnover}$	
ln *service imports*	0.0379**	0.0278			0.0502***	
	(0.0173)	(0.0171)			(0.00980)	
ln *service imports* × lag ln *service imports*	0.00622**	0.00451*				
	(0.00243)	(0.00259)				
lag ln *service imports*	-0.0566***	-0.0385**				
	(0.0182)	(0.0185)				
$Q1$ × ln *service imports*			0.0518***	0.0215		0.0461***
			(0.0143)	(0.0131)		(0.00979)
$Q2$ × ln *service imports*			0.0568***	0.0679***	0.0261**	0.0563***
			(0.0146)	(0.0161)	(0.0107)	(0.0120)
$Q3$ × ln *service imports*			0.0919***	0.0678***	0.0407***	0.0617***
			(0.0216)	(0.0259)	(0.0124)	(0.0132)
$Q4$ × ln *service imports*			0.136***	0.0841**	0.0661***	0.0730***
			(0.0249)	(0.0369)	(0.0171)	(0.0172)
$Q2$			-0.0409	-0.280**	-0.190***	-0.0813
			(0.124)	(0.129)	(0.0677)	(0.0761)
$Q3$			-0.294*	-0.294	-0.306***	-0.132
			(0.179)	(0.205)	(0.0838)	(0.0935)
$Q4$			-0.700***	-0.452	-0.533***	-0.227*
			(0.221)	(0.312)	(0.129)	(0.135)
Additional controls	no	yes	no	yes	no	yes
Observations	39,939	14,313	40,905	14,785	40,905	14,785
F-statistic (Kleibergen-Paap)	605.6	227.8	180.0	18.23	230.9	90.06

	(1)	(2)	(3)	(4)	(5)	(6)
B. Interactions with productivity						
	Lag LP		High vs. low LP		High vs. low TFP	
ln *service imports*	-0.0683	-0.168**	0.0479***	0.0384***	0.0454***	0.0408***
	(0.0938)	(0.0720)	(0.0108)	(0.00884)	(0.00919)	(0.00835)
ln *service imports* × lag ln *LP*	0.0244	0.0443***				
	(0.0191)	(0.0151)				
lag ln *LP*	-0.462***	-0.580***				
	(0.134)	(0.116)				
ln *service imports* × high productivity			0.0203*	0.0598***	0.0596***	0.0536***
			(0.0108)	(0.00916)	(0.0106)	(0.00964)
high productivity			-0.277***	-0.257***	-0.197***	-0.164***
			(0.0772)	(0.0601)	(0.0608)	(0.0538)
Additional controls	no	yes	no	yes	no	yes
Observations	16,880	14,775	16,718	14,233	15,059	13,831
F-statistic (Kleibergen-Paap)	227.6	193.5	322.9	257.3	284.6	252.0

Note: The table reports estimates of equation (1) with ln *employment* as the dependent variable, including additional interaction terms as regressors. In panel A, $Q1 - Q4$ denote dummy variables indicating quartiles of lagged *service imports* (columns 3-4) or of lagged $\frac{service\ imports}{turnover}$ (columns 5-6), respectively. In panel B, high productivity is a dummy variable indicating above-median LP (TFP) in columns 3-4 (columns 5-6). All regressions control for the basic set of lagged firm control variables and fixed effects by firm and by industry-year. The even columns further account for the additional firm-level control variables from column 4 of Table 4. Standard errors reported in parentheses are robust to heteroskedasticity and autocorrelation within firms. Asterisks indicate significance levels: * p<0.1, ** p<0.05, *** p<0.01. Source: RDSC of the Deutsche Bundesbank, SITS, MiDi, and USTAN, 2001-2013, own calculations.

firms' offshoring activities in models featuring heterogeneous firms (such as Egger et al., 2016). Since more productive firms engage more actively in offshoring, they can be expected to experience stronger productivity effects and hence greater employment gains by the same mechanism as discussed above. Panel B provides empirical evidence in line with this conjecture. The regressions presented here are based on equation (1) augmented by an interaction term between $\ln$ *service imports* and three alternative proxies for firm productivity (as well as the productivity variable itself). Both $\ln$ *service imports* and the interaction term are instrumented by $\ln IV$ and an interaction of $\ln IV$ with the same productivity variable.

Columns 1-2 of panel B start by differentiating the effects of service offshoring by labor productivity, continuously measured as the one-year lag of $\ln LP$. The 2SLS regressions yield negative estimates of $\ln$ *service imports* and positive estimates of the interaction effect, which are however only significant after including additional control variables. These results indicate that the marginal effect of service offshoring is zero or even negative for very unproductive firms and increases in labor productivity. To allow for non-linear effects, I proceed by considering alternatively dummy variables indicating above-median productivity by industry and year in terms of LP (columns 3-4) or TFP (columns 5-6). For both measures, the interaction term of service offshoring with the high productivity dummy is always positive and (at least weakly) significant in both specifications of the 2SLS regression. The estimated interaction effects in columns 4-6 further suggest that the elasticity for firms with above-median productivity is more than twice as large compared to firms with a productivity below the industry median. As expected, more productive firms experience larger employment effects from service offshoring. The Kleibergen-Paap F-statistics well above the standard critical values indicate strong instruments in all regressions in panel B. Overall, the estimates presented in Table 6 describe an empirical pattern of firm heterogeneity that is fully in line with the employment effects being driven by the productivity effect of offshoring.

5.5 Deeper analysis of employment effects

This section provides deeper investigations into three interesting aspects of the employment effects at the intensive margin of service offshoring by further exploiting the rich available micro data. First, to shed some light on the role of supplier substitution in the domestic economy, the employment effects are distinguished by sector (services vs. other), and service imports are considered relative to total purchases (from domestic and foreign suppliers). Second, service offshoring is split up by OECD and non-OECD source countries to examine differences in offshoring to high-income vs. low-income countries. Third, the section analyzes specifically the service offshoring activities of firms with multinational investment links to characterize the effects of potential intra-firm service offshoring.

In principle, an increase in service imports may reflect one of three cases: It may (i) substitute for activities previously done within the firm, or (ii) substitute for activities previously purchased from other domestic suppliers, or (iii) constitute the purchase of a service that the firm did not use at all before. Since most firms' sourcing requirements are rather stable over time, as indicated by the persistent international sourcing patterns discussed in Section 2.3, the following argument abstracts from the third possibility. Among the other two cases, the first corresponds to a widespread understanding of service offshoring both in the general public and among economists, and may be called 'genuine offshoring'. The second case does not accord well with this common definition of offshoring, but may instead be labeled 'supplier substitution'. Since supplier substitution does not involve any relocation of tasks previously conducted in-house, there is no reason to believe that it should entail any negative employment effects within the firm, but instead jobs may be lost in other domestic firms. Supplier substitution may however have beneficial productivity effects within the firm similar to genuine offshoring, provided that the same services are delivered from abroad at a lower cost. Hence, the possibility that service imports reflect supplier substitution rather than genuine offhoring implies a risk of overstating the employment effects of genuine service offshoring, which is the main subject of this paper. While supplier substitution may be of interest in itself, the available data do not allow for a comprehensive assessment of its employment effects since domestic service providers cannot be identified. However, information on firms' sector affiliations and their total purchases can be exploited to gauge the relevance of supplier substitution for the positive employment effects of service offshoring identified above.

Panel A of Table 7 uses firms' sector affiliations to provide indicative evidence that supplier substitution is not the driving force behind the positive employment effects. It starts by splitting the sample into firms in the primary and industrial sectors (columns 1 and 2) and those in the service sector (columns 3 and 4). Since service sector firms by definition deliver some services themselves, it seems more likely that their service imports substitute for activities that could in principle be delivered within the firm. Hence, in comparison to firms in the primary and industrial sectors, it seems likely that a larger part of service imports by service sector firms represents genuine offshoring. Furthermore, to the extent that genuine offshoring may have less favorable effects than supplier substitution, the estimated employment effect of service offshoring should be lower in service sector firms. In contrast to this logic, the sectoral sample split reveals that the employment effects are in fact greater for service sector firms compared to the primary and industrial sectors in both specifications.[27] While these sectoral differences may have a variety of explanations, they do not suggest that the employment effects of genuine offshoring are overstated due to supplier substitution.

The last two columns of panel A take the previous approach one step further by applying a narrow definition of offshoring along the lines of Feenstra and Hanson (1999). As in the OLS regressions of Section 3.2, narrow offshoring is defined as imports in the service type corresponding to the firm's own industry. For instance, imports of IT services by a software firm qualify as narrow offshoring, but imports of IT services by a consultancy or a manufacturer do not. The idea behind this definition is that imports within the firm's own industry are more likely to represent activities that could be done by the firm itself, and hence reflect more narrowly the common understanding of genuine offshoring (rather than supplier substitution). As noted earlier, this approach reduces the estimation sample to the few service sector firms which import in several years the same type of service as they deliver. Interestingly, the estimated elasticity is even much higher for narrow offshoring (instrumented by $\ln IV$) compared to the broad definition in the largest available sample from MiDi and USTAN (column 5). The estimate is rendered insignificant and the IV is much weaker in the specification with additional firm control variables (column 6), however, the point estimate remains positive also in this small sample. These regressions reveal that even for a narrow definition of service offshoring the employment effects are non-negative.

Panel B presents an alternative approach to disentangling the effect of genuine service offshoring from that of supplier substitution. The regressions reported in the first four columns split the sample into firms that experienced a contemporaneous increase in their *service import share*, defined as the ratio of service imports over 'other operating charges', and those for which it decreased. The item 'other operating charges' from the firm's profit and loss account in USTAN includes (a large part of the) expenses on both domestic and foreign purchases of services. Hence, firms increasing their import share are likely to import services that they did *not* purchase domestically before, representing genuine offshoring. The estimates reported in columns 1 and 2 confirm that service offshoring had a significantly positive effect on employment in these firms. Columns 3 and 4 further reveal that higher service imports also increased employment in

[27]This observation is in line with the correlation patterns found by Hijzen et al. (2011) across UK sectors.

firms with a contemporaneous decrease in their import shares, and the effect has a similar magnitude in such firms. These regressions reveal that service offshoring has boosted firm employment regardless of contemporaneous changes in domestic sourcing activities. The last two columns in panel B employ ln *service import share* as an explanatory variable (instrumented by $\ln IV$) in equation (1) and confirm the positive employment effects also for this relative service offshoring measure. Overall, the results in panels A and B suggest that supplier substitution effects cannot explain the key finding of employment gains from increased service offshoring.

Panel C of Table 7 investigates differences in the employment effects of service offshoring to different countries. Since the existing literature has found the employment and wage effects of offshoring in the US to differ between high-income and low-income source countries (Harrison and McMillan, 2011; Ebenstein et al., 2014), imports are broadly split up into OECD and non-OECD countries. Since the other commercial services examined in this paper often involve high-skill intensive tasks, this analysis may reveal interesting differences compared to offshoring of manufactured inputs, which are typically thought of as low-skill intensive. The data show that the majority of service offshoring by German firms is to OECD countries, both in terms of the number of firms and in terms of the value of imports. This observation is not surprising given the high skill abundance of the European OECD member countries surrounding Germany and the fact that the gravity equation provides a good description of trade in services (Kimura and Lee, 2006).

The analysis in panel C of the table starts by considering separately service imports from OECD (columns 1 and 2) and non-OECD countries (columns 3 and 4) as service offshoring variables in equation (1). Each of these endogenous variables is instrumented by an IV constructed as described in equation (4), including only trade with OECD or non-OECD partner countries c, respectively. The 2SLS estimates identify positive employment effects of offshoring to both country groups across all specifications. They further suggest that offshoring to non-OECD countries benefits domestic employment in German firms more strongly. Since the IVs rely on time variation, including both OECD and non-OECD imports in the regressions (columns 5 and 6) reduces the sample size substantially and results in only marginally strong IVs. The point estimates in these regressions are positive but imprecisely estimated and only significant for offshoring to non-OECD countries in the larger sample. Overall, the results of panel C suggest that service offshoring to low-income countries, though less prevalent among German firms, entails stronger positive employment effects than offshoring to OECD countries. This finding seems to indicate a higher complementarity between German firms' domestic workforce and their imports from low-income countries as opposed to high-income countries. It is in line with the expectation that relatively low skill-intensive commercial services imported from non-OECD countries are more likely to complement high-skilled employment in German firms. These conclusions contrast with those obtained for offshoring of manufactured inputs by US MNEs in Harrison and McMillan (2011) and Ebenstein et al. (2014), who find that offshoring to high-income countries complements, while offshoring to low-income countries substitutes for domestic employment in US MNEs. The evidence for service offshoring in Germany suggests a reversed pattern and generally more favorable employment effects.

To what extent is service offshoring conducted via intra-firm service trade within multinational firms' boundaries or through international outsourcing? The MiDi data, covering all German inward and outward FDI (above a low reporting threshold), can be exploited to provide a tentative answer to this question. These data also serve to relate the findings in this paper to the literature that has exclusively focused on the foreign activities of MNEs for measuring offshoring. To approach this question, I apply two definitions of intra-firm service offshoring. First, I classify firms' service imports in all countries and service types in which

Table 7: Deeper analysis of employment effects

	(1)	(2)	(3)	(4)	(5)	(6)
A. Sector split and narrow offshoring						
	Non-service sectors		Service sector		Narrow offshoring	
ln *service imports*	0.0540***	0.0333***	0.0826***	0.0524***		
	(0.00755)	(0.00755)	(0.0166)	(0.0193)		
ln *narrow offshoring*					0.169***	0.0444
					(0.0313)	(0.0546)
Additional controls	no	yes	no	yes	no	yes
Observations	23,818	10,822	16,840	3,880	6,351	1,056
F-statistic (Kleibergen-Paap)	912.0	412.7	441.9	114.8	110.9	14.59
B. Imports relative to domestic purchases						
	Increasing import share		Decreasing import share		Continuous import share	
ln *service imports*	0.0610***	0.0617***	0.0611***	0.0461***		
	(0.0158)	(0.0138)	(0.0122)	(0.0104)		
ln *service import share*					0.0574***	0.0472***
					(0.0102)	(0.00868)
Additional controls	no	yes	no	yes	no	yes
Observations	7,770	6,768	6,965	6,041	16,698	14,215
F-statistic (Kleibergen-Paap)	236.4	204.0	378.5	287.6	587.1	480.2
C. Effects by source country of imports						
	OECD		Non-OECD		OECD and non-OECD	
ln *service imports* OECD	0.0650***	0.0460***			0.00666	0.0297
	(0.00899)	(0.00901)			(0.0428)	(0.0495)
ln *service imports* non-OECD			0.107***	0.0547***	0.140**	0.0644
			(0.0282)	(0.0197)	(0.0686)	(0.0479)
Additional controls	no	yes	no	yes	no	yes
Observations	35,068	12,264	10,811	4,765	8,140	3,413
F-statistic (Kleibergen-Paap)	1291.2	502.9	112.0	67.79	17.35	7.092
D. Intra-firm imports and service count						
	FDI by country-service type		FDI by country		Service count by country	
ln *intra-firm offshoring*	0.291***	0.0258	0.184***	0.0326		
	(0.0875)	(0.0676)	(0.0432)	(0.0862)		
# *country-service types*					0.00851***	0.00484***
					(0.000929)	(0.00107)
Additional controls	no	yes	no	yes	no	yes
Observations	5,388	1,063	5,388	1,063	47,970	17,014
F-statistic (Kleibergen-Paap)	18.24	13.24	135.6	20.77	946.3	445.3

Note: The table reports 2SLS estimates of equation (1) with ln *employment* as the dependent variable. All regressions control for the basic set of lagged firm control variables and fixed effects by firm and by industry-year. The even columns further account for the additional firm-level control variables from column 4 of Table 4. In panel A, columns 1-2 restrict the sample to non-service sectors (NACE codes <45), columns 3-4 restrict it to the service sector (NACE codes ≥ 45), and columns 5-6 consider only narrow offshoring of services corresponding to the firm's own industry. In panel B, columns 1-2 restrict the sample to firms with an increasing import share, columns 3 and 4 restrict it to firms with a decreasing import share, and columns 5-6 consider the import share as an explanatory variable. Panel C distinguishes service offshoring to OECD and non-OECD countries. In panel D, columns 1-2 consider intra-firm service offshoring defined as imports from a country-service type combination where the firm has a foreign affiliate or parent, columns 3-4 consider intra-firm service offshoring more broadly defined as imports from a country where the firm has a foreign affiliate or parent, and columns 5-6 examine the number of country-service type combinations as an explanatory variable. Standard errors reported in parentheses are robust to heteroskedasticity and autocorrelation within firms. Asterisks indicate significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Source: RDSC of the Deutsche Bundesbank, SITS, MiDi, and USTAN, 2001-2013, own calculations.

they also have a foreign affiliate or parent (according to MiDi) as intra-firm service offshoring. Second, I drop the service type requirement and include in the definition all service imports from a country where the firm reports any foreign investment link, to allow for the possibility that a related party active in a different industry also provides services. Both definitions overstate the amount of actual intra-firm offshoring. The second definition furthermore provides an upper bound for the relevance of this phenomenon, since the existence of a related party in the source country of service imports is a necessary condition for intra-firm trade. Based on these considerations, the data allow for the conclusion that intra-firm service offshoring is of limited relevance, as only 1,076 firms in the full estimation sample satisfy this necessary condition and may potentially engage in intra-firm service offshoring.

Panel D of Table 7 reports 2SLS estimates of regressions including the two alternative definitions of intra-firm service offshoring (instrumented by $\ln IV$) in equation (1). Whether restricting the definition to countries and service types in which related parties are present (columns 1 and 2) or more broadly to FDI partner countries (columns 3 and 4), the employment effects are estimated to be positive, and significantly so in the larger sample. The estimates in the smaller sample with additional control variables are based on only 205 firms and imprecise due to a rather weak IV. Overall, this analysis suggests that intra-firm service offshoring plays a negligible role in Germany, where services are typically imported at arm's length.[28] Hence, datasets restricted only to MNEs do not seem suitable for investigating service offshoring, as they are likely to miss the bulk of relevant activity. Irrespective of the organizational mode, increases in service offshoring tend to have positive effects on domestic firm employment.

Finally, the last two columns in panel D reexamine a measure of the extensive margin of service offshoring to estimate the effect of adding another service type from the same country to the offshoring portfolio. At this level of disaggregation, the number of country-service type combinations in which the firm conducts offshoring can be instrumented by an IV similar to the main instrument from equation (4). The IV is the world export supply by partner country c and year t, weighted by the firm's pre-sample import shares. Compared to the main instrument, this approach ignores the variation across service types, so it is valid only under somewhat more restrictive assumptions, but it allows for investigating one dimension of the extensive margin of service offshoring by the same means as the intensive margin. The 2SLS estimates confirm the positive employment effects also for this measure of service offshoring, and they are also larger than the corresponding FE estimates.

5.6 Sample selection

Two margins of selection are relevant for the main analysis of the employment effects at the intensive margin of service trade. First, since information on employment (and other covariates) is only available for the firms reporting to MiDi or USTAN, there may be sample selection into these databases. Second, firms select into the service offshoring activity, as discussed in detail in Section 4. To the extent that these margins of selection are non-random and their determinants are correlated with the error term $\varepsilon_{i,t}$ in equation (1), they may bias the estimates of β in the previous analysis at the intensive margin. Therefore, both of these selection issues are addressed in turn.

To correct for possible sample selection bias in the panel, I follow the method proposed by Wooldridge (2010).[29] It involves estimating probit selection equations à la Heckman (1979) year by year and includ-

[28] Note that all the results in this study are restricted to services delivered via cross-border trade or movement of natural persons in the sense of GATS modes 1, 2, and 4. Service imports via commercial presence (GATS mode 3) are not captured, as they involve domestic transactions between the foreign affiliate and a service provider within the foreign country.

[29] See also Harrison and McMillan (2011) for an application in the offshoring context.

ing the inverse Mills ratios $IMR_{i,t}$ predicted by these equations, interacted with year dummies δ_t, in the following first-differenced (FD) version of equation (1):

$$\Delta \ln \textit{employment}_{i,t} = \gamma \cdot \Delta \ln \textit{service imports}_{i,t} + \boldsymbol{\psi} \cdot \boldsymbol{\Delta X}_{i,t-1} + \rho \cdot \delta_t \times IMR_{i,t} + \delta_{j,t} + \nu_{i,t}. \quad (5)$$

In this equation, Δ denotes the FD operator (so $\Delta Y \equiv Y_{i,t} - Y_{i,t-1}$ for any variable Y) that takes care of firm-specific effects, γ, ψ, and ρ denote the parameters to be estimated, $\delta_{j,t}$ are industry-year fixed effects, and $\nu_{i,t}$ is the error term. The model is estimated by 2SLS, instrumenting for $\Delta \ln \textit{service imports}_{i,t}$ by $\Delta \ln IV_{i,t}$, and standard errors are bootstrapped (based on 200 replications) to account for the generated regressors $IMR_{i,t}$, of which there are two sets.

First, to correct for selection into the sample, the annual selection equations explain a dummy variable for whether the observation is included in the full estimation sample in column 1 of Table 4. They include as explanatory variables a set of dummy variables indicating whether the firm was a service importer, service exporter, MNE, or FIE, and whether it reported to USTAN in the previous year, as well as industry fixed effects. These variables are pre-determined, well defined for all observations in the panel, and they are related to selection into the three underlying databases. The motivation for including these explanatory variables is that firms typically show high persistence in their trading status, FDI links, and reporting behavior, so these lagged variables can partly predict future appearance in the sample. The estimates are used to compute the first set of IMR terms.

Second, to correct for selection into the service offshoring activity, a service importer dummy is explained in annual cross-sections by the same variables $\mathbf{M}_{i,t-1}$ which have been identified as relevant predictors of the extensive margin of offshoring in Section 4. The predicted values are used to compute the second set of IMR terms. This twofold approach to correct for selection exploits the fact that the three underlying data sources are overlapping in terms of their firm coverage. Due to this feature of the data, different sets of firms are used to estimate the different first-stage equations for selection into the sample and selection into the service offshoring activity. In the absence of useful exclusion restrictions, the selection models are identified by assuming joint normality of the error terms in the selection equations and $\nu_{i,t}$.

Table 8: First differencing and correcting for sample selection

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	First differencing		Sample selection		Offshoring selection		Both selection margins	
Δ ln *service imports*	0.0354***	0.0307***	0.0428***	0.0360***	0.0429***	0.0360***	0.0428***	0.0358***
	(0.00767)	(0.00912)	(0.00748)	(0.00694)	(0.00715)	(0.00726)	(0.00794)	(0.00743)
Additional controls	no	yes	no	yes	no	yes	no	yes
Observations	32,356	11,473	32,356	11,473	32,360	11,477	32,356	11,473
χ^2 IMR			20.88	18.78	21.24	21.17		

Note: The table reports 2SLS estimates of equation (5) with Δ ln *employment* as the dependent variable. All regressions control for the basic set of lagged firm control variables and fixed effects by firm and by industry-year. The even columns further account for the additional firm-level control variables from column 4 of Table 4 (in first differences). The first two columns report estimates of the simple FD model, columns 3-4 correct for selection into the estimation sample by adding interaction terms $\delta_t \times IMR_{i,t}$, columns 5-6 analogously correct for selection into service offshoring status, and columns 7-8 correct for both selection margins. The χ^2 IMR statistic refers to a test for joint significance of the interaction terms $\delta_t \times IMR_{i,t}$. Standard errors reported in parentheses are robust to heteroskedasticity and autocorrelation within firms in columns 1-2 and bootstrapped based on 200 replications in columns 3-8. Asterisks indicate significance levels: * p<0.1, ** p<0.05, *** p<0.01. Source: RDSC of the Deutsche Bundesbank, SITS, MiDi, and USTAN, 2001-2013, own calculations.

Table 8 reports the results of estimating equation (5) for the two specifications with and without the additional firm-level control variables from column 4 of Table 4. It starts in columns 1 and 2 by estimating the FD model in equation (5) without any IMR terms. The strength of the IV is confirmed and the estimated effect of service offshoring on employment is positive and highly significant in the FD model. However, the estimates of γ are smaller compared to the main FE estimates of β. This result seems plausible provided that some of the employment gains from service offshoring take more than one year to materialize, and are hence not captured by the FD model.[30] In columns 3 and 4, the interaction terms $\delta_t \times IMR_{i,t}$ for $t = \{2002, \ldots, 2013\}$ are added to the FD model in order to correct for selection into the dataset. Columns 5 and 6 proceed analogously for selection into service offshoring, and columns 7 and 8 include all 24 interaction terms to correct for both margins of selection. The χ^2 statistics from tests of joint significance of the $\delta_t \times IMR_{i,t}$ terms suggest that both selection margins are only marginally relevant for explaining changes in firm employment, and only few of the interaction terms are individually significant (not reported). The estimated employment effect of service offshoring is positive and significant in all of the regressions correcting for selection.

5.7 Dynamic labor demand

The analysis in this paper, as well as most of the existing international economics literature investigating the employment effects of offshoring, has so far abstracted from dynamic aspects of labor demand.[31] In practice, however, firm employment may depend on its past realizations due to adjustment costs (Hamermesh, 1993), such as search costs, notice periods, and severance pay. To take these aspects into account, equation (1) is modified in this section to include lagged employment as an additional explanatory variable. Estimating such a dynamic model poses the well-known challenge that lagged employment is likely to be correlated with unobserved firm characteristics, which causes dynamic panel bias (Nickell, 1981) and calls for a generalized methods of moments (GMM) approach (Hansen, 1982). This challenge is addressed below by applying the difference GMM estimator, which exploits longer lags of the endogenous variable along with lags of the exogenous regressors as instruments in the FD model. The difference GMM estimator was originally proposed by Holtz-Eakin et al. (1988) and is best known as the Arellano-Bond estimator, named after the contribution by Arellano and Bond (1991). This estimator is well-established as a tool for consistently estimating dynamic labor demand equations, and its one-step version is implemented in this paper by the Stata command `xtabond2` (Roodman, 2009).

Table 9 reports the results of estimating the dynamic panel data models. The first column starts by adding the lagged dependent variable to the standard specification of equation (1), including only the basic set of firm-level control variables, and estimating this model by 2SLS in the sample pooled over all years (treating the firm fixed effect as random). This naïve pooled 2SLS estimator is subject to the Nickell bias. In particular, it is well-known that the coefficient of the lagged dependent variable in this model is biased upward. The estimates reported in the second column apply the within-transformation to the same model, resulting in the 2SLS-FE estimator (used in most of the previous analysis). Note that the estimates in column 2 are still biased since the lagged dependent variable is correlated with the error term also in the within-transformed model. In particular, the estimated coefficient of the lagged dependent variable can be expected to be biased downward. Hence, the two estimates 0.961 and 0.583 provide approximate upper and lower bounds to the true effect of lagged employment.

[30] Note that the within-transformed (FE) model is preferred precisely because the IV has been designed to capture medium-to-long-term changes in service offshoring (see the discussion in Hummels et al., 2014).

[31] Two notable exceptions are Görg and Hanley (2005) and Lo Turco and Maggioni (2012).

Table 9: Dynamic labor demand

	(1)	(2)	(3)	(4)	(5)
	Pooled 2SLS	2SLS-FE	Difference GMM		
ln *service imports*	0.0192***	0.0491***	0.0387***	0.0209***	0.0203***
	(0.00285)	(0.00610)	(0.00775)	(0.00699)	(0.00639)
lag ln *employment*	0.961***	0.583***	0.606***	0.722***	0.349***
	(0.00334)	(0.0170)	(0.0913)	(0.183)	(0.118)
ln *wage*					-0.590***
					(0.0849)
Additional controls	no	no	no	yes	yes
Observations	43,057	40,717	32,246	11,538	11,148
p-value of AR1 test			0.000	0.001	0.005
p-value of AR2 test			0.242	0.586	0.235
p-value of Hansen test			0.133	0.000	0.000

Note: The table reports estimates of equation (1) with ln *employment* as the dependent variable, including the lagged dependent variable as an additional regressor. All regressions control for the basic set of lagged firm control variables and fixed effects by firm and by industry-year. Columns 4-5 further account for the additional firm-level control variables from column 4 of Table 4. Column 1 uses the 2SLS estimator in the pooled sample and column 2 uses the 2SLS estimator in the within-transformed model. Columns 3-5 employ the one-step difference GMM estimator, using all available higher-order lags of the exogenous regressors and of the dependent variable as well as $\ln IV$ as instruments in the first-differenced model. Standard errors reported in parentheses are robust to heteroskedasticity and autocorrelation within firms. Asterisks indicate significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Source: RDSC of the Deutsche Bundesbank, SITS, MiDi, and USTAN, 2001-2013, own calculations.

Columns 3 and 4 of Table 9 report difference GMM estimates of the dynamic model, which use the second lag and all available higher-order lags of the dependent variable, all lags of the exogenous variables, as well as $\ln IV$ as instruments in the FD model. The regression in column 3 includes the basic set of control variables as before, while column 4 includes the additional firm-level control variables. Conditional on the validity of these instruments, the difference GMM approach yields consistent estimates of the parameters of interest. The resulting estimates for the employment effect of service offshoring are positive and highly significant in both specifications. Again, the point estimates are smaller than in the main analysis, as was the case for the FD estimates reported in Table 8. The final column includes as an additional regressor the firm-level wage per employee $\ln wage$, which is treated as endogenous and similarly instrumented by all available lags. Adding this regressor does not change the estimated effect of service offshoring and yields a plausible estimate for the wage elasticity well within the range typically found in the literature (see e.g. Lichter et al., 2015). These exercises further validate the approach and ensure that the positive effects of service offshoring identified above cannot be explained by dynamic panel bias in labor demand or endogeneity in firm-level wages.

A number of standard plausibility checks support the validity of the difference GMM estimates. First, the estimated coefficient of lagged employment of 0.606 in column 3 lies in the plausible range identified by the corresponding pooled 2SLS and 2SLS-FE regressions. Second, in all the estimations reported in columns 3-5, the Arellano-Bond test for zero autocorrelation of order one (AR1) is rejected at the 1% significance level, whereas the test for zero autocorrelation of order two (AR2) cannot be rejected. Third, the Hansen J test (obtained from a consistent two-step estimate) is rejected at any conventional significance level for the regression reported in column 3, suggesting that the overidentifying restrictions are valid. However, it cannot be rejected for the estimations including additional control variables (columns 4 and 5).[32] Overall, these regression diagnostics suggest that the difference GMM approach seems suitable to estimate the dynamic model, which supports the main conclusions of the previous analysis.

5.8 Robustness checks

Table 10 explores the robustness of the estimated employment effects at the intensive margin of service offshoring to addressing various concerns that might challenge the exclusion restriction of the IV. As before, the estimates in this table are based on the main specification of equation (1) with and without additional firm control variables, corresponding to columns 1 and 4 of Table 4.

An important assumption underlying the IV approach is that the time variation in the export supply of partner countries to the rest of the world reflects supply-side shocks. In particular, the exclusion restriction requires that world export supply (WES) may not be correlated with demand shocks in German firms (conditional on the covariates). In general, this assumption seems plausible, since the estimation is implemented at the highly disaggregate level of individual firms, Germany itself is excluded from WES in the IV construction, and all specifications control for industry-year fixed effects absorbing industry-wide demand shocks. There might, however, be a slight possibility that the assumption is violated through reverse causality if a very large German firm is a significant buyer of a particular service type in the world market, and thereby affects demand by other countries. To eliminate this possibility, the robustness checks summarized in columns 1 and 2 of panel A exclude from the sample all firms whose service imports make up more than 0.1% of the partner country's exports to the rest of the world in a given service type and year (according to Comtrade). The employment effects estimated in this sample are almost identical to the baseline estimates for both specifications, which ameliorates any concerns related to reverse causality.[33]

Another possible threat to the exclusion restriction might lie in German demand shocks that are correlated with demand shocks in other countries, which affect the time variation in WES. If such shocks are not perfectly captured by industry-year fixed effects in the regressions, they might impair the validity of the IV. To mitigate this concern, an alternative variant of the IV is constructed by restricting the set of destination countries for partner countries' export supply in equation (4) to a few countries that are comparable to Germany but not subject to the same demand shocks. To be precise, the set of destination countries for the construction of this IV is restricted to eight non-neighboring, non-Eurozone countries with an average income level similar to Germany, following Dauth et al. (2014).[34] Since not all partner countries export services to these eight countries in several years, using the alternative IV reduces the sample size. Nevertheless, the estimates reported in columns 3 and 4 of panel A are almost identical to the main estimation results.

The third set of robustness checks presented in Table 10 addresses an issue related to missing data for aggregate service trade flows. Unfortunately, the availability of service trade data lags behind the standard for goods trade (see e.g. Francois and Hoekman, 2010). Due to imperfect coverage, aggregate service trade data in Comtrade are missing for some country-service type-year observations despite positive imports reported by German firms in SITS. Since the IV defined in equation (4) is constructed as a weighted sum of aggregate trade flows, this issue induces measurement error in the IV. To address this problem, an alternative

[32] Note that "the validity of the overidentifying restrictions is neither sufficient nor necessary for the validity of the moment condition" (Parente and Silva, 2012, p. 314), but should rather be viewed as a test for whether the instruments coherently identify the same parameters.

[33] Similar conclusions are obtained when large firms are excluded by alternative criteria, e.g. firms that account for more than 0.01% of aggregate German service imports in a given year, or firms whose real turnover exceeds € 5 billion (in values of 2010).

[34] The eight destination countries are: Australia, Canada, Japan, Norway, New Zealand, Sweden, Singapore, and the UK.

variant of the IV is constructed, which uses only data on the country-service type combinations for which aggregate trade data are available in all of the years 2001-2013. While this IV can explain only a smaller share of the time variation in firms' service offshoring, it does not vary due to missing aggregate data. Columns 5 and 6 of panel A show that 2SLS regressions relying on the alternative IV yield even larger point estimates for the employment effect of service offshoring compared to the main analysis.

Table 10: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)
A. Excluding large firms and using alternative IVs						
	Excluding large importers		IV based on 8 countries		IV data in all years	
ln *service imports*	0.0719***	0.0492***	0.0783***	0.0500***	0.117***	0.0688***
	(0.00861)	(0.00828)	(0.0114)	(0.0132)	(0.0181)	(0.0183)
Additional controls	no	yes	no	yes	no	yes
Observations	40,401	14,626	30,952	11,270	21,112	7,302
F-statistic (Kleibergen-Paap)	1,399.3	548.1	714.6	280.1	452.1	122.6
B. Considering service exports and size effects						
	Imports and exports		Only exports		Size-industry-year FE	
ln *service imports*	0.0963	0.0792			0.0657***	0.0357***
	(0.0708)	(0.0529)			(0.00848)	(0.00787)
ln *service exports*	0.00971	-0.0190	0.119***	0.0483**		
	(0.0749)	(0.0471)	(0.0206)	(0.0196)		
Additional controls	no	yes	no	yes	no	yes
Observations	13,315	3,898	17,524	5,173	30,121	12,464
F-statistic (Kleibergen-Paap)	5.232	5.925	210.7	60.05	1,048.3	449.9

Note: The table reports 2SLS estimates of equation (1) with ln *employment* as the dependent variable. All regressions control for the basic set of lagged firm control variables and fixed effects by firm and by industry-year. The even columns further account for the additional firm-level control variables from column 4 of Table 4. In panel A, columns 1-2 restrict the sample by excluding large firms that account for more than 0.1% of a given country's service exports by service type and year, columns 3-4 use a variant of the IV based on eight industrialized destination countries, and columns 5-6 use a variant of the IV based only on the country-service type combinations for which aggregate service trade data is available in all of the years 2001-2013. In panel B, columns 1-4 consider instrumented service exports, and columns 5-6 include a full set of fixed effects for all combinations of industry, size group, and year. Standard errors reported in parentheses are robust to heteroskedasticity and autocorrelation within firms. Asterisks indicate significance levels: * p<0.1, ** p<0.05, *** p<0.01. Source: RDSC of the Deutsche Bundesbank, SITS, MiDi, and USTAN, 2001-2013, own calculations.

Panel B of Table 10 addresses the concern that service exporting activity, similar to offshoring, can also affect firm employment, and it may similarly be endogenous to employment. For this reason, the estimations of Hummels et al. (2014) include as an additional explanatory variable firms' goods exports, for which they construct instruments in analogy to the import side. I apply a similar approach to service trade in Germany by including the log of firm-level service exports in equation (1) and instrumenting this variable by the weighted world import demand by partner countries and service types, constructed in analogy to the IV in equation (4). As shown in columns 1 and 2 of panel B, the resulting point estimates for the effect of service imports are positive and greater than in the main specifications without service exports. However, these estimates are very imprecisely estimated and not reliable, since the hypothesis of weak instruments cannot be rejected at standard critical values based on the Kleibergen-Paap F-statistics of 5-6. The weak IV problem is caused by the need to estimate two first-stage regressions in combination with the implied, drastic reduction of the sample to firms which import *and* export services in several years. Hence, these estimates are unfortunately not informative for the true employment effects of service offshoring and exporting. However, given the point estimates for the employment elasticity in columns 1 and 2 and the small number of service importing firms that also export services, it seems unlikely that the bias that might arise from endogenous exporting behavior is of economic significance in the main estimations. In 2SLS regressions considering only service exports (columns 3 and 4), the exporting IV is strong and the estimates suggest that service exporting also increases domestic employment.

The final robustness check reported in Table 10 relates to the firm's initial importing structure, which is used to weight partner countries' WES in the IV. In general, this importing structure may vary systematically across different firms, for instance because larger firms have better access to more distant offshoring locations. Note that the weights in the IV are held constant based on the pre-sample importing structure to avoid any reverse causality issues. Furthermore, any time-invariant correlates of employment and the importing structure have been fully accounted for by the within-transformation (or first-differencing) throughout the analysis. However, if firms with a particularly favorable initial importing structure are also inherently more dynamic, this might threaten the validity of the IV. For instance, larger firms might be characterized by higher employment growth rates in general and might initially offshore services to countries that subsequently experience particularly positive export supply shocks. In this case, the higher employment growth of larger firms might in part be attributed to service offshoring through the IV, resulting in an upward bias of β. To address these and related concerns, I control for a full set of time-varying fixed effects for all combinations of firms' industries and size groups, defined by categories of their real turnover in the pre-sample year.[35] These fixed effects do not only control for size-group-specific employment trends, but more generally allow the size-group specific effects to vary arbitrarily across industries and over time. The estimates reported in the last two columns of Table 10, conditional on these fixed effects, confirm the positive and significant employment effects of service offshoring.

6 Concluding discussion

Fear of job losses in developed countries has spurred a public debate about service offshoring. The existing literature studying this phenomenon has found small positive correlations between employment and service offshoring, but establishing causality has remained a challenge due to endogeneity issues and the lack of detailed micro data. To address this challenge, the paper introduces a newly combined dataset covering almost the entire universe of German firms' service trade over the period from 2001 to 2013. Using this unique dataset, I conduct a comprehensive analysis of the causal effects of service offshoring (both at the extensive and at the intensive margin) on firm employment.

The analysis demonstrates that firms which started service offshoring did not decrease their domestic employment in the subsequent years compared to a matched control group. Instead, further increases in service offshoring have even contributed to increasing employment within German firms, as IV regressions reveal. These results provide strong support for the productivity effect of offshoring predicted by the canonical trade in tasks model. In line with this theory, service offshoring has also boosted output and productivity, and the employment gains are stronger in firms with higher initial offshoring values and higher productivity.

These findings indicate several promising avenues for future research. Since the analysis in this paper has concentrated explicitly on firm-level employment, it does not capture possible repercussions outside the firm in the domestic economy, e.g. through supplier substitution and spillover effects. Investigating the economy-wide impact of offshoring thus remains an important task. The robust positive employment effects of service offshoring found in this paper obviously contrast with the less favorable effects typically found

[35]The eight categories are <2, $2-5$, $5-10$, $10-20$, $20-50$, $50-100$, $100-500$, and >500 million € in values of 2010.

for the offshoring of manufactured inputs in other studies (notably Hummels et al., 2014, who use a very similar methodology). A deeper investigation into the fundamental differences between these two types of offshoring seems warranted.

What can we expect from service offshoring in the future? Estimates from the mid-2000s suggest that around 25% of all US jobs are potentially offshorable, including many service activities that used to be non-tradable (see Blinder, 2006, 2009; Blinder and Krueger, 2013). It also seems reasonable to expect that the range of tradable services is going to expand further with technological progress. Therefore, the bulk of service offshoring may yet lie ahead. In light of the results found for the past decade in Germany, the average worker employed in a firm that considers expanding its service offshoring activities should have little reason to worry about these future changes.

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A Appendix

A.1 Service types correspondence

Table A.1: List of Services

Service type	SITS service categories	Comtrade EBOPS 2002 codes
Computer & IT	513	262
Communications	518, 591	245
Insurance	400, 401, 410, 420, 440-445, 450, 451, 460	253
Financial	533	260
Construction	561, 570, 580	249
Engineering	512	280, 283
Research & development	511	279
Other business services	516, 519, 523, 530, 531, 540	274, 284, 269, 278, 272
Personnel	514, 517, 521	310
Other services	510, 534, 562, 594, 595	282, 287

The table lists the correspondence between the broad service types used in the analysis, the detailed service categories recorded in SITS (see Biewen et al., 2013), and the Extended Balance of Payments Services (EBOPS) 2002 classification of the UN Comtrade data.

A.2 Consistency checks for employment

A variety of checks are implemented to validate information on firm employment, the main outcome variable of interest in this paper. Note that reporting employment in USTAN is not mandatory, and this variable is not crucial for the main purposes for which both MiDi and USTAN data are collected. As a result, information on employment is missing for some firm-year observations in both datasets and the information provided may be subject to reporting errors. Therefore, the employment variable is carefully cross-validated in three ways. First, for firms reporting employment in both datasets, the numbers of employees can be compared. These numbers are identical or deviate by less than 2 employees or 2% of employment in 64% of these firm-year observations, suggesting no major measurement problems for the majority of observations. For these firms, the employment information from USTAN is preferred to MiDi because USTAN is also the source of the balance sheet data used in the analysis. Second, for firms that report employment only in USTAN, I exploit for validation the total wage bill, which is reported as part of the firm's profit and loss statement and carefully checked by central bank staff. Outliers with a real wage per employee below € 1,200 or above € 2.2 million in 2010 values (approximately the minimum and maximum values observed in the overlap of USTAN and MiDi) are excluded from the analysis. Third, for firms that report employment only in MiDi, information on the wage bill is unavailable, so I exploit data on real turnover to validate employment. Potential outliers are excluded if the real turnover per employee is below € 1,000 or above € 10 million in 2010 values. A stricter plausibility requirement is applied for firms with a single employee, where the upper bound is set to € 5 million in 2010 values. For the analysis in the paper, information on turnover is generally taken from the same data source as information on employment.

A.3 Constructing physical capital stocks

It is well-known that deflated accounting capital stocks tend to underestimate firms' physical capital stocks because book values, measured at historical prices, differ from current market values and accounting depreciation is typically higher than real depreciation for tax reasons. To construct physical capital stocks, I therefore implement the perpetual inventory method (PIM) as described by Bachmann and Bayer (2014), who apply the PIM to earlier years of the USTAN data.[36] Since the procedure is described in detail in their Web Appendix, the exposition here is brief, highlighting the few differences from their approach.

The PIM is implemented separately for two types of capital goods: (i) buildings and structures and (ii) equipment and machinery. In the first step, nominal investment for each of these capital goods is inferred from changes in the accounting capital stock and accounting depreciation. The second, main step of the PIM starts from the the initial capital stock in the first year of a non-interrupted series of observed investments, deflated to the year 1999 by the investment price deflator. The physical capital stock in all subsequent years is computed by successively adding similarly deflated investments and subtracting real depreciation.[37]

This procedure is useful to correct the capital stock in these subsequent years, but it does not affect the initial capital stock for each firm, which remains to be underestimated. Therefore, the initial capital stock is iteratively adjusted by a correction factor, which measures by how much the real capital stock constructed for later years exceeds the deflated accounting capital stock. As a minor deviation from Bachmann and Bayer (2014), I use the median value of this correction factor by industry, which is less prone to outliers

[36] I am grateful to Christian Bayer for providing valuable advice and code for implementing the PIM.

[37] Data on the investment price indices and depreciation rates by type of capital good are obtained from the same time series as in Bachmann and Bayer (2014), provided by the German Statistical Office. For the PIM and the subsequent estimation of production functions, each firm is assigned a constant 2-digit NACE industry code, corresponding to the industry code it reports most frequently in USTAN (and the latest one of these values in case of several modes).

than the mean. The procedure is iterated in each industry until either the median correction factor falls below 1.1 or the number of rounds reaches ten. In each round, the procedure corrects for outliers in terms of the correction factor, which may indicate unusual events at the level of the firm (such as a fire or an accident). As a result, the physical capital stock is missing for these outliers, but the observations are nevertheless used in the regression analysis in the paper, as long as it does not rely on the capital stock. Note that even after implementing the PIM, the initial capital stock is imperfectly approximated. This concern is ameliorated by the fact that the PIM is implemented using USTAN data as far back as 1999, while the main analysis in the paper starts in 2002.

Finally, to obtain the total physical capital stock, the real stocks for both capital goods obtained from the PIM are aggregated and added to the net present value of rented capital. The latter is constructed as the reported value of rented capital divided by the average investment price deflator and a measure of the user cost of capital, defined as in Bachmann and Bayer (2014).

A.4 Estimating total factor productivity

To obtain a measure of total factor productivity (TFP), Cobb-Douglas production functions are estimated by industry using the procedure suggested by Ackerberg et al. (2015). The log-linear production functions specify real value added as a function of the firm's employment and the physical capital stock (constructed by the PIM described in Appendix A.3), while using real material inputs (also deflated by the industry-level producer price index) as a proxy variable. The estimations exploit the full available USTAN sample over the years 1999-2013, where each firm is assigned a single 2-digit industry, and a few small industries are grouped together to obtain a sufficient number of observations for each estimation.

At least since Olley and Pakes (1996), it is well-known that estimation of such production functions suffers from bias due to the fact that the firm may base its input choice on the observed productivity, which is unobserved to the econometrician. To address this issue, a control function approach à la Olley and Pakes (1996) is implemented, which exploits material input purchases as a proxy for unobserved productivity, as suggested by Levinsohn and Petrin (2003). This procedure assumes that material inputs are strictly increasing in productivity, and employment is treated as a freely adjustable input, while the physical capital stock enters as a state variable. Following the critique by Ackerberg et al. (2015), no coefficient is identified in the first stage of this procedure, which regresses the log of value added on a third-order polynomial in the inputs (all in logs). Instead, the labor and capital coefficients are identified in the second stage GMM estimation by the appropriate moment conditions.