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Economic Sanctions and Informal Employment

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Abstract
This paper examines how economic sanctions affect the allocation of workers across formal and informal employment. We analyse the case of the unprecedented sanctions imposed on Iran in 2012. Employing a difference-in-differences approach, we compare the probability of being employed in the informal sector before and after 2012 for workers in industries with different pre-existing exposure to international trade. Our analysis reveals that, following the sanctions, workers in industries with higher trade exposure are significantly more likely to experience informal employment compared to workers in industries with lower trade exposure. These results remain robust when accounting for potential sorting issues by using an instrumental variable approach. Our findings suggest that the sudden shock to market access caused by the sanctions might have induced a decline in firms' productivity, especially in industries that heavily depend on imported inputs, and therefore an increase in firms' incentives to reduce the costs by shifting their employees to the informal sector. This sheds light on an important margin of labour market adjustment through which sanctions can affect the economy of the target country.

JEL Classification: E26, F16, F51, O17.

Keywords: Economic sanctions, Exposure to trade, Informal employment, Labor reallocation.

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1. Introduction

Informal employment constitutes a major component of developing countries' labour market. By one estimate, 70–80 per cent of employment within low-income countries can be attributed to informal, household-run small enterprises (Gollin 2002, 2008; Tybout 2000, 2014). These enterprises are substantially less productive than those operating in the formal sector (La Porta and Shleifer 2008, 2014; Nataraj 2011). The productivity gap between formal and informal firms contributes to explaining why countries with higher shares of informal employment are systematically associated with lower levels of economic development.[1]

The informal sector usually acts as a safety net, especially for low- and middle-income countries. When the economy experiences a negative shock, it is the informal sector that tends to absorb most of the displaced workers (Dix-Carneiro and Kovak 2019; Loayza and Rigolini 2011). The availability of the informal sector as a cushion for displaced workers' labour market outcomes can improve individual welfare, especially if unemployment is the only alternative. Yet, the reallocation of employment to firms with low tax compliance can reduce aggregate welfare.

The primary objective of this paper is to examine how sudden and extreme shocks to market access, such as those resulting from the imposition of economic sanctions, affects the allocation of workers across informal and formal employment. Our main hypothesis is that the economic hardship and the disruption to existing business relationships caused by the sanctions generate incentives for both firms and workers to shift their activities to the shadow economy.

Studies document that sanctions increase the criminalization of the state, economy, and civil society of both the target country and its neighbours, fostering illegal economic activities, such as smuggling (Andreas 2005; Bove et al. 2023; Crozet et al. 2021; Farzanegan 2013; Slavov 2007). There is also evidence that sanctions reduce gross domestic product (GDP) growth (Ghomi 2022; Hufbauer et al. 2009; Laudati and Pesaran 2022; Neuenkirch and Neumeier 2015), international trade and foreign direct investment (Afesorgbor 2019; Haidar 2017; Mirkina 2018), increase poverty and inequality (Afesorgbor and Mahadevan 2016; Neuenkirch and Neumeier 2016), and also have detrimental effects on human capital (Chakravarty et al. 2021; Moeeni 2022), firms' performance (Ahn and Ludema 2020), and formal employment (Etkes and Zimring 2015; Moghaddasi Kelishomi and Nisticò 2022). However, to the best of our knowledge no study has yet investigated the impact of sanctions on workers' probability of being employed in the informal sector. This is mainly because data constraints make it difficult to measure informal employment. In fact, both survey and

[1] The empirical evidence shows that these cross-country differences in aggregate income are attributable to the inefficient allocation of inputs across sectors and firms (e.g., see Hsieh and Klenow 2009; McMillan et al. 2014; Restuccia and Rogerson 2008).

administrative data rarely distinguish between formal versus informal employment. Furthermore, it is often challenging to separate the effects of the sanctions from other circumstances, such as conflicts that occur during the sanctions period.

We address these challenges by examining the case of Iran, which has a Labour Force Survey (LFS) that offers reliable measures of informal employment. Moreover, Iran was subject to an unexpected and unprecedented trade and financial embargo from 2012 to 2014[2], making it an ideal case for our analysis. In addition, focusing on the Iranian setting has the advantage that the estimation of the impact of the sanctions is not challenged by the potential confounding effects of the conflict, as Iran was a peaceful country both before and during the sanctions period. We conduct our empirical analysis at the individual level, combining repeated cross-section data on employment from the LFS with yearly data on import and export gathered from Iran's Customs Administration database for the period 2008–14. We exploit pre-existing industry-level differences in trade exposure and compare, in a difference-in-differences (DiD) setting, the probability that an individual works in the informal sector before and after the imposition of the sanctions in 2012. We also employ an event-study analysis, which allows us to examine not only the dynamics of the effects of sanctions on informal employment but also to assess the validity of the common trend assumption.

To measure whether an individual works in the informal sector (i.e. our outcome variable), we build on the widely used recommendations provided by the 15th and 17th International Conferences of Labour Statisticians (ILO 2000, 2003). According to the International Labour Organization (ILO) definition, a job is considered informal if it is characterized by an employment relationship that is "not subject to national labour legislation, income taxation, social protection or entitlement to certain employment benefits" (Hussmanns 2004: 7). Hence, our primary measure of informal employment is based on whether the worker has no social security coverage (i.e. is employed off the books).[3] Alternatively, we use an indicator for whether the individual works in a microenterprise and obtain qualitatively similar results.[4]

The results of our empirical analysis show that while sanctions have no effect on workers' probability to be employed, they increase their likelihood of transition to the informal sector. More specifically, we estimate that workers employed in industries with above-median exposure to trade

[2] The international economic sanctions imposed on Iran in 2012 are considered extraordinary in terms of its severity, scope, and non-discriminatory nature. These sanctions were substantially eased after 3 years with the Joint Comprehensive Plan of Action signed in July 2015.

[3] This measure has been extensively used in previous studies such as Acosta and Gasparini (2007), Acosta and Montes-Rojas (2014), Arias et al. (2018), Attanasio et al. (2004), Cisneros-Acevedo (2022), Pavcnik et al. (2004), Paz (2014), and Radchenko (2014), among others.

[4] Studies using informal employment measures based on the size of enterprise include Cunningham and Maloney (2001), Falco et al. (2011, 2015), Fiess et al. (2010), Günther and Launov (2012), Maloney (1999, 2004), and Williams et al. (2016), among others.

(i.e. treatment group) have a 5-percentage point higher probability of being employed informally, i.e. off the books, relative to workers in industries with below-median trade exposure before the sanctions (i.e. control group). This corresponds to a 9 per cent increase in the overall probability of working in the shadow economy. Importantly, we explore the heterogeneous effects of the sanctions by characterizing the individuals who are more susceptible to working in the informal sector when the economy is impacted by economic sanctions. We document that the sanctions led to a more significant rise in the likelihood of working in the informal sector for poorly educated workers. This highlights the unequal labour market consequences of economic sanctions. These findings are in line with those in McCaig and Pavcnik (2015), who show that poorly educated workers in Vietnam faced little prospects of formalization during the decade of rapid growth that started with the trade liberalization. Moreover, our results are consistent with the evidence in Ponczek and Ulyssea (2022), who find that "informality acts as an employment buffer in the face of negative economic shocks, but this seems to be the case only for low-skill workers".

Our empirical approach relies on the assumption that the trends in informal employment would have been the same in the treatment and control groups in the absence of the sanctions (i.e., the common trend assumption). To check the validity of this assumption, we use a dynamic DiD specification with lags and leads à-la Autor (2003). We show that while before the sanctions the trends in the share of informal employment for both groups are statistically indistinguishable from each other, the two paths begin to diverge immediately after the imposition of the sanctions in 2012, with the gap in the outcome becoming increasingly larger over the next two years. Moreover, through a placebo test, we demonstrate that our main result is not influenced by the potential delayed effects of the 2010 government subsidy plan. This subsidy plan aimed to provide targeted social assistance and to reallocate funds to people and the industrial sector. Taken together, these results provide credible evidence that the increase in the probability of being employed in the informal sector since 2012 for treated workers compared to untreated ones is likely a consequence of the sanctions.

One concern regarding our DiD estimates is the possibility of endogenous sorting of workers into treated versus untreated industries, which could bias the estimated effect of the sanctions on informal employment. This bias may stem from the variation in sorting patterns between the pre- and the post-sanctions periods, which cannot be differentiated using the DiD approach. To deal with this problem we adopt an instrumental variable strategy and use the pre-existing share of employment in treated industries in the worker's province of residence as an instrument for a worker's probability of working in treated industries. We thus exploit provinces with different initial industry mixes being differentially affected by the sanctions-induced trade shock (see e.g., Topalova 2010, Kovak 2013 and Ponczek and Ulyssea 2022). The first-stage results show that our instrument effectively predicts

the individual probability of being employed in treated industries. Importantly, the second-stage estimates confirm the positive and statistically significant effect of the sanctions on informal employment, thus reinforcing the confidence in the validity of our identification strategy. The estimated effect becomes larger (7 percentage points) than the ordinary least square (OLS) one, consistent with a local average treatment effect (LATE) interpretation (Imbens and Angrist 1994).

Our work relates to three different lines of literature. First, we contribute to a fast-growing literature on the labour market effects of economic sanctions, including Etkes and Zimring (2015) and Moghaddasi Kelishomi and Nisticò (2022). While these studies examine the impact of sanctions on (formal) employment reallocation across industries and sectors, here we study how sanctions affect the reallocation of workers from formal to informal employment.[5] Second, our paper relates to the recent literature on worker-level labour market effects of trade shock, such as Autor et al. (2014), Dix-Carneiro and Kovak (2019), and Utar (2018). A prominent feature of this literature is the reliance on trade liberalizations—hence, increased import competition—to identify causal impacts. In contrast, we use the unexpected trade embargo imposed on Iran in 2012 as a quasi-natural experiment to estimate how workers' labour market outcomes adjust to trade shocks. While sanctions are probably a more severe negative shock to the economy than a unilateral trade opening, we add to this literature by highlighting the importance of market access, especially for imported inputs, as an alternative mechanism behind the effect of trade shocks on informality. In addition, this is the first study to examine the effects of trade shocks on informal employment in the context of the Middle East. Third, we advance the literature on trade and informality. Previous studies in this line of research include Arias et al. (2018), Bosch et al. (2012), Goldberg and Pavcnik (2003), McCaig and Pavcnik (2018), and Ponczek and Ulyssea (2022), among others.[6] Our paper distinguishes itself from these works by investigating the effects of a foreign policy instrument (i.e. economic sanctions), utilised in international politics to alter the strategic decisions of governments that pose a threat to the interests of the imposing countries. However, it is crucial to note that these sanctions also affect the welfare of many individuals, especially those facing poorer economic conditions.

Overall, the empirical findings presented in this paper contribute to our understanding of how sanctions can affect the economy of the target country by shedding light on a significant aspect of

[5] The only study on the effects of sanctions on informality that we are aware of is Farzanegan and Hayo (2019), who examine the impact of the Iranian sanctions on the size of shadow economy at the province level.

[6] The empirical findings in this literature are mixed and depend on the exact type of shock and context. Some find that trade integration is associated with a significant increase in informality - see e.g., Arias et al. (2018) for Brazil and Mexico, Cisneros-Acevedo (2022) for Peru, and Dix-Carneiro and Kovak (2017, 2019), Paz (2014) and Ponczek and Ulyssea (2022) for Brazil. Others find instead small or no increase in informal employment or even significant reductions in the share of informality - see e.g., Ben Yahmed and Bombarda (2020) for Mexico, Bosch et al. (2012) for Brazil, Erten et al. (2019) for South Africa, Goldberg and Pavcnik (2003) for Brazil and Colombia, McCaig and Pavcnik (2015, 2018) for Vietnam, and Nataraj (2011) for India.

labour reallocation. Moreover, the findings provide useful recommendations on the specific groups that the domestic policies should prioritise to mitigate the flow of workers from the formal to the informal sector.

The remainder of the paper is organized as follows. Section 2 provides background on the sanctions and discusses the conceptual framework and the various mechanisms through which sanctions can affect the informal labour market. Section 3 describes the data we use for the analysis. Section 4 describes the estimation methods used to estimate the impact of the sanctions on informal employment. Section 5 presents the empirical findings. Section 6 concludes with a discussion on the implications of our findings.

2. Background and Conceptual Framework

2.1. The 2012 Sanctions and the Iranian Economy

The history of the sanctions on Iran that we examine in this study can be tracked back to the International Atomic Energy Agency's Board of Governors' decision to report their concerns regarding Iran's nuclear activities to the United Nations (UN) Security Council in February 2006 (see Samore 2015). During 2006–10 the UN Security Council passed several resolutions against Iran's nuclear and military programme which were consequently followed by the European Union (EU) and the United States (US) in late 2011 and 2012. While the UN sanctions against Iran focused mainly on 'proliferation-sensitive nuclear activities', a new sanctions regime was devised and imposed on Iran in 2012, which was unprecedented in terms of its tools, severity, scope, and non-discriminatory nature. In fact, the sanctions prior to 2012 were limited in scope and often targeted designated individual or companies involved in the nuclear or military programme. On the contrary, the new set of sanctions targeted Iran's economy as a whole.

Figure A.1 in the Appendix depicts the timeline of the sanctions imposed on Iran by the United States and the European Union after 2006. For instance, the US sanctions under the Iran Freedom Support Act in September 2006 targeted Iranian advance conventional weapons and weapons of mass destruction. The Comprehensive Iran Sanctions, Accountability, and Divestment Act of 2010 was designed to restrict access to the US financial market for third-party institutions involved with Iran's petroleum sector or Iran's Islamic Revolutionary Guard Corps, a branch of the Iranian Armed Forces. However, sanctions imposed from December 2011 significantly expanded the scope and were intended to have broader impacts. In November 2011, in an unprecedented move the United States designated the Iranian financial sector as a jurisdiction of 'primary money laundering concern' under Section 311 of the USA PATRIOT Act for the first time. In December 2011, President Barack Obama

signed the National Defense Authorization Act (NDAA) for the fiscal year 2012, which led to severe unilateral sanctions against the Central Bank of Iran. Section 1245 of the NDAA codified the money laundering designation. The Act banned any activity of foreign financial institutions doing or facilitating any significant financial transaction with the Central Bank of Iran or any other Iranian financial institutions. This restriction also applied to the foreign central banks that engaged in financial transactions for the sale or purchase of petroleum or petroleum products to or from Iran, thus causing a blockade in Iranian oil exports (Gladstone and Castle 2012). The NDAA gave private foreign financial institutions 60 days after the date of enactment of the NDAA, for non-petroleum product transactions, for the sanctions to become effective. The commencement date was 180 days for the sale or purchase of petroleum and petroleum products. To enforce the secondary sanctions imposed by the United States, President Obama issued Executive Order 13608 in May 2012.

The European Union, Iran's more important trade partner, has been pursuing sanctions since 2007. The sanctions imposed by the European Union before 2012 mostly targeted Iran's nuclear and missile activities (Council Common Position 2007/140/CFSP), banned arms sales and exports of oil and natural gas technology (Council Decision 2010/413/CFSP). In January 2012, the European Union went along with the United States in broadening the scope of the sanctions. The EU Council Decision 2012/35/CFSP imposed an oil embargo, prohibiting import, purchase, or transport of Iranian crude oil, natural gas, and petrochemical products, and prohibited provision of related financing, insurance, or reinsurance. In March 2012, with the EU Council Decision 2012/635/CFSP froze the assets of the Central Bank of Iran and prevented the entire financial system from accessing the SWIFT messaging service to paralyse the Iranian financial sector, including the Central Bank of Iran, making them unable to operate in international business. This measure was unprecedented and unexpected. In August and October 2011, the governor of the Central Bank of Iran, excluded the possibility of any sanctions against the bank, claiming not only that this would be illegal and against all the principles but also that the sanctioning countries 'will be ridiculed by the world' (IRNA 2011a, 2011b). These sanctions were followed by "secondary sanctions" and other extraterritorial measures aimed at discouraging companies and individuals of other countries to conduct business with Iran (Stoll et al. 2020). Thus, the effect of enforcement went beyond the sanctioning countries, generating a major obstacle in processing international payments and curbing other bilateral economic flows (Van Bergeijk 2015).

Iran reacted to the sanctions by threatening to block the Strait of Hormuz in the Persian Gulf. The oil embargo occurred at the time when the oil price was above US$100 per barrel for the whole of 2011. The Iranian currency depreciated by around 40 per cent against the dollar in January 2012 and again in October 2012 after the EU boycott of Iranian oil exports came into effect. Moreover,

Iran's economic activity declined substantially because of the sanctions. For instance, the non-oil real GDP dropped by 3.1 per cent in 2012 and by and 1.1 per cent in 2013, relative to an increase of 3.2 per cent in 2011 (IMF 2014). Following the oil embargo, oil exports fell from 2.1 million barrels per day in 2011 to 1.4 million barrels per day in 2012 and to 1.1 million in 2013, while the current account declined from 10.4 per cent of GDP in 2011 to an average of 4.7 per cent of GDP over the post-sanctions period. Iran built up massive foreign reserves, which amounted to US$104 billion in 2012, due to the high crude oil price. However, the access to the foreign reserves were limited during the post-sanctions period because of the financial sanctions that were in place.

Given the timing and the scope of the sanctions imposed by the United States and the European Union, we define 2012 as our treatment year. As discussed, although the United States imposed several primary sanctions before 2012, the most damaging secondary sanctions that targeted the Iranian financial system altogether were imposed towards the end of 2011 with commencing dates in 2012. The impact of US initial primary sanctions on Iran would have been limited given the size of trade between Iran and the United States before sanctions. Prior to 2012, Iran's imports from the United States represented, on average, about 0.5 per cent of Iran's total imports compared with 30 per cent from the European Union. Similarly, Iran's exports to the United States averaged 0.3 per cent of Iran's total exports, as opposed to 8 per cent for the European Union.

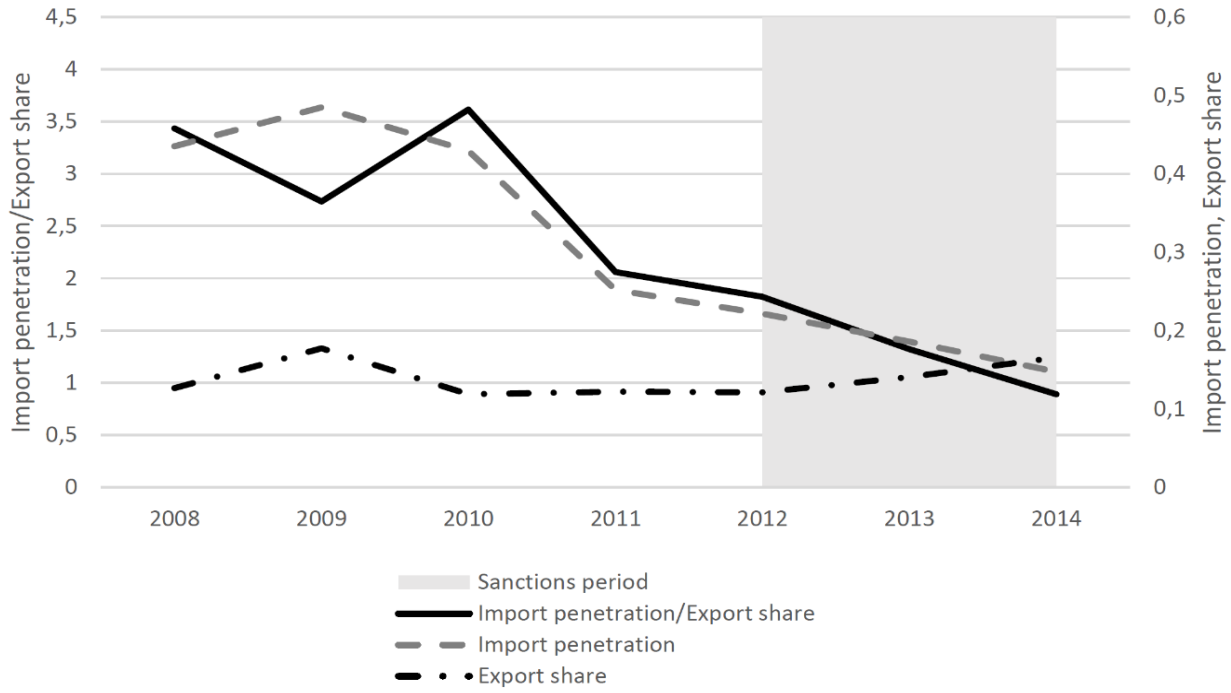
Figure 1 illustrates the trends in average import penetration, export share, and overall exposure to trade in Iran's manufacturing sector. Following previous literature (e.g., see Acemoglu et al. 2016; Feenstra et al. 2019), we compute import penetration as the ratio of import of manufactured goods to the initial domestic absorption in Iran and export share as export of manufactured goods divided by initial production in 2011.[7] Overall exposure to trade, measured as the ratio of import penetration to export share, is the main variable that we use in the empirical analysis to define industries in treatment and control groups, as we discuss in more detail in Section 3. While the average import penetration depicts a downward trend since 2008, the decline continued following the imposition of the new sanction regime in 2012. The average import penetration dropped from 45 per cent in 2008 to 25 per cent in 2011 and to 15 per cent in 2014.

There is significant heterogeneity across industries. The import penetration ranges from above 80 per cent for different varieties of machinery, for instance in 'manufacture of agricultural and forestry machinery', International Standard Industrial Classification (ISIC) 2921, to around 50 per cent in industries such as 'manufacture of railway and tramway locomotives and rolling stock', ISIC 3520, to 1 per cent in 'carpets and rugs', ISIC 1724. In contrast, the export share was stable in the

[7] All nominal values are Iranian rials and are deflated to their 2011 equivalent. The values are the averages over 105 four-digit (ISIC, Revision 3.1) manufacturing industries in Iran.

run-up to the sanctions but increased in the years after the sanctions, from 12 per cent in 2011 to 17 per cent in 2014, partly because of a substantial depreciation in the value of Iranian rials. 'Processing and preserving of fruit and vegetables', ISIC 1519, 'Manufacture of chemicals and chemical products', ISIC 241, and 'Manufacture of carpets and rugs', ISIC 1724, are among the industries with the highest export ratio, while machinery industry is among the lowest. Figure 1 also shows a downward trend in average exposure to trade. Trade exposure decreased from 3.4 in 2008 to 0.89 in 2014, mainly reflecting the import penetration trend.

Figure 1: Trends in industry-level trade exposure, import penetration and export share.

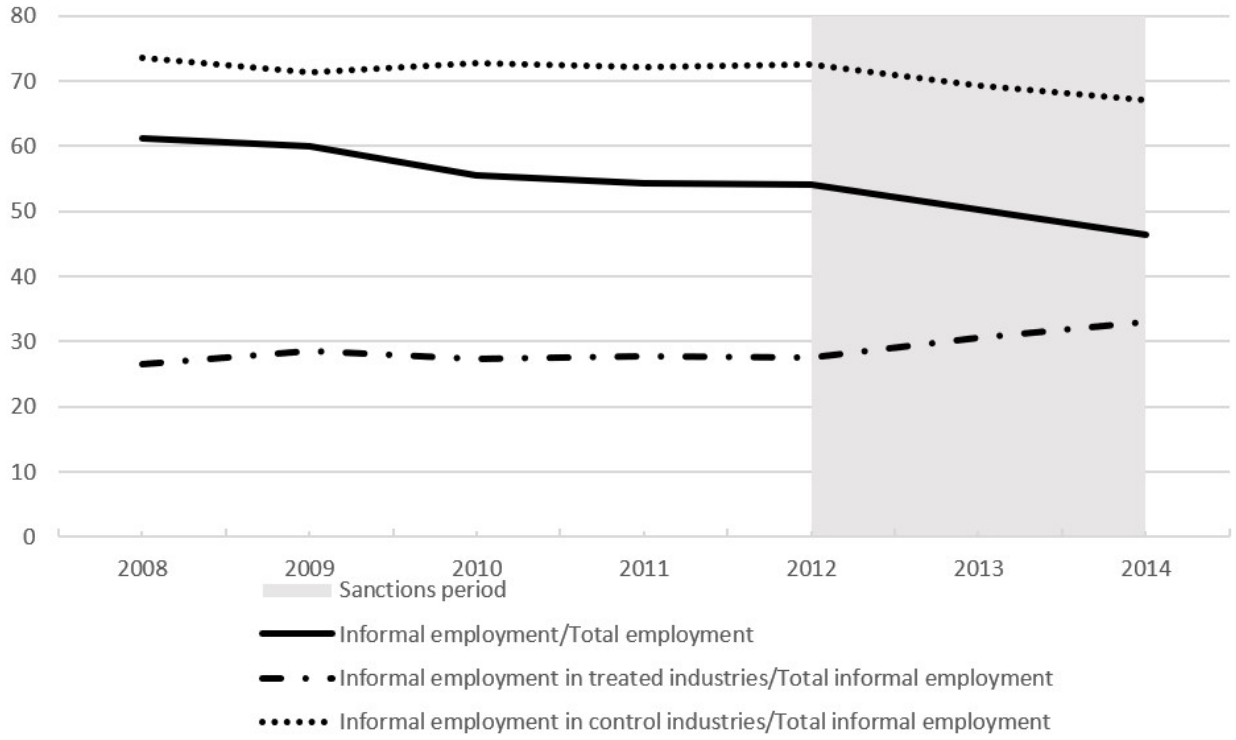


Source: authors' compilation based on study data.

Figure 2 illustrates the trend in the share of informal employment in manufacturing, calculated as the ratio of informal employment to the total employment in manufacturing, between 2008 and 2014. Although the figure indicates a secular declining trend in informal employment over the period covered by this study, the rate of decline sped up after the sanctions in 2012. This is consistent with the negative impact of the sanctions on overall manufacturing employment documented in Moghaddasi Kelishomi and Nisticò (2022). However, as it emerges from the other two distinct trends shown in Figure 2, industries with a pre-sanction level of openness to international trade above and below median (i.e. respectively, our treated and control groups, as defined in Section 3) display opposite patterns following the imposition of sanctions in 2012. Indeed, we observe an increase in the share of informal employment in relatively open industries and a decline in relatively closed

industries, providing suggestive evidence that workers in industries more exposed to the sanctions faced higher probability of transition to the informal sector.

Figure 2: Trend in informal employment shares.



Source: authors' compilation based on study data.

2.2. Conceptual Framework and Mechanisms

Theoretically, there are different mechanisms whereby economic sanctions may affect the informal labour market. First, the sharp reduction in import and export typically caused by trade embargos can lead to significant job losses in export-competing industries as well as in industries that largely rely on imported inputs (Moghaddasi Kelishomi and Nisticò 2022). This can also cause the reallocation of employment across industries differentially exposed to the trade shock. Etkes and Zimring (2015) show, for the case of the Gaza blockade during 2007–10, that the labour market adjusts via the reallocation of workers away from manufacturing and into services. Part of this reallocation can occur from the formal to the informal sector, depending on workers' expected employment prospects (Dix-Carneiro and Kovak 2019; Goldberg and Pavcnik 2003; McCaig and Pavcnik 2018). As shown in Figure 1, the significant reduction in trade exposure observed in Iran's manufacturing sector after the imposition of the sanctions was mainly driven by the drop in import penetration. Interestingly, when we break down imports to discern their purpose, categorized as gross fixed capital formation,

intermediate consumption, and final consumption (following the UN's broad economic categories), we find that approximately half of Iran's imports predominantly serve as inputs into other economic activities, falling under the category of intermediate consumption. For instance, in the year 2011, only 17 percent of the total imports were classified as intended for final consumption, while 49 per cent was identified as being allocated to intermediate consumption, and the remaining 34 per cent as capital formation. Thus, we are inclined to think that the sanctions primarily manifested their impact by disrupting domestic production, as opposed to adhering to the conventional channel underscored in the trade liberalization literature, which predominantly centers on the reduction of foreign competition.

Second, the contraction in FDI and in the presence of foreign firms may foster domestic production in the short run, potentially leading to an increase in formal employment in industries that are more exposed to FDI. Yet, the slump in FDI can hamper the technology spillover induced by the presence of foreign firms, and negatively affects the growth and survival of domestic firms in the long run (Kosová 2010). We believe that this channel might play a minor role in the context of Iran. Foreign Direct Investment (FDI) in the Iranian economy was primarily channelled into the oil and gas extraction sector, accounting for more than half of the total FDI inflows. Other key sectors that attracted significant FDI included oil and gas related industries such as petrochemicals, chemicals, plastics industries, which are usually very capital intensive, and other manufacturing industries. Prior to the imposition of economic sanctions, the average annual FDI into Iran stood at approximately $3.2 billion. This represented a 2% of the gross fixed capital formation in the country, or roughly 0.65% of Iran's GDP. In comparison, the FDI inflow as a percentage of GDP for the Middle East and North Africa region averaged around 3% over the same period (UNCTAD 2023).

Third, the increased political instability and uncertainty caused by the sanctions (Adam and Tsarsitalidou 2019; Allen 2008; Hultman and Peksen 2017; Marinov 2005) can increase the relative costs of credit (the instability can signal increased risk of insolvency), thus fostering informal employment through its negative effects on investment and economic growth. We note that this is unlikely to be a major mechanism in our setting as Iran has historically exhibited a relatively modest dependence on external debt, both the public and private sectors, particularly when compared to other developing countries. For instance, in 2010, Iran’s total external debt stocks was 20% of its exports and 4.1% of its Gross National Income (GNI).[8] This stands in marked contrast to the significantly higher ratios seen in other countries: Turkey at 185% (39% of GNI), Thailand at 45% (32.5% of GNI), Mexico at 94.7% (29.4% of GNI), Colombia at 132% (23.3% of GNI), Indonesia at 117.6%

[8] Total external debt stock is defined as the sum of public, publicly guaranteed, and private nonguaranteed long-term debt, short-term debt, and use of IMF credit.

(27% of GNI), and India at 81% (17.5% of GNI) when considering their respective ratios of external debt to exports and GNI. Moreover, it is worth emphasizing that the Iranian private sector displays limited involvement in non-guaranteed external debt. In 2010, for instance, the private sector's external debt stock guaranteed by the public sector amounted to a mere $12 million, while its non-guaranteed debt remained at zero. This observation underscores the fact that nearly the entire burden of long-term external debt, which is already relatively low compared to other countries, falls within the domain of the public sector or publicly guaranteed sector (World Bank 2023).

Our study focuses on the financial sanctions imposed on Iran and the trade-inducing effects of these sanctions. As noted in Section 2.1, although the United States and the European Union both have imposed partial trade sanctions on goods and services related to Iran's nuclear, missile, and energy sector activities, the 2012 sanctions regime was financial in nature. The sanctions laid out by the United States in NDAA Section 1245 and the European Union prohibiting Iran's access to SWIFT were aimed at preventing Iran from doing normal business with the rest of the world. Financial sanctions are expected to reduce bilateral trade by raising the transaction cost and the cost of entering foreign markets. Indeed, empirical evidence shows that financial sanctions reduce trade. For example, Crozet et al. (2021) show that the 2012 sanctions on Iran lowered the number of French exporters to Iran by 39.2 per cent. In fact, they observed that the impact was most pronounced for firms using trade finance instruments such as letters of credit. More generally, Felbermayr et al. (2020) estimate that the sanctions imposed on Iran reduced Iran's trade with the sanctioning countries by about 55 per cent. They also show that the effect on exports and imports has been symmetric. Thus, we expect the effect of the 2012 sanctions on informal employment that we estimate in this study to be mainly driven by the financial sanctions and their resulting trade shock. By contrast, as our focus is on informal employment in the non-oil manufacturing sector, we expect the oil embargo imposed in late 2011 to play a minor role in our empirical analysis.

Moghaddasi Kelishomi and Nisticò (2022) estimate that sanctions imposed on the Iranian economy in 2012, deemed as the most severe multilateral measures ever imposed on a country, caused a significant reduction in the growth rate of manufacturing formal employment (16.4 percentage points). The overall negative effect on employment estimated in that study is attributable to a large fraction of Iran's manufacturing industries being heavily dependent on imported inputs (as it is often the case in the context of developing countries). As it is unlikely that domestic production could entirely replace imports in the short run, our prediction is that the sanctions would necessarily entail a decline in productivity in these industries. Firms can respond to such decline in productivity by adjusting their labour demand (i.e. reducing formal employment, as documented in Moghaddasi

Kelishomi and Nisticò 2022) or by shifting part of their workforce to the informal sector (i.e. via transition to informal employment).

Therefore, based on all these considerations, we hypothesize that workers in industries originally (i.e. before the 2012 sanctions) more exposed to international trade, that is, industries with higher dependence on imported inputs, should face higher probability of working in the informal sector than their counterparts in less-exposed industries.

3. Data

We combine data from two main sources. First, we use the labour force survey (LFS) data from the Statistical Centre of Iran (SCI).[9] LFS uses a 2–2–2 rotating panel sample design. Each participant is interviewed for two consecutive quarters, then is left out for two consecutive quarters and is back to sample for the last two interviews. Our sample expands from 2008 (1387 in the Iranian calendar) to 2014 (1393 in the Iranian calendar). Second, data on trade for 2008–2014 are from Iran's Customs Administration database obtained from the SCI. The dataset includes import and export in local currency for a six-digit harmonized system product level. The data are converted to four-digit ISIC (Revision 3.1) by the SCI. We then aggregate and merge this data into 105 four-digit ISIC industries to match the trade data to the LFS employment data. All nominal values are converted to 2011 constant prices using the relevant price indices. For export, we use the Export Price Index from the Central Bank of Iran. For import, we use SCI's Import Price Index. However, this index is not available before 2011. The World Bank Import Value Index is used to impute pre-2011 values. To construct our main explanatory variables, trade exposure, we use the production data in 2008 from the Annual Survey of Manufacturing Firms with more than 10 workers of the SCI. We also use the Producer Price Index from SCI to deflate production.

Our sample consists of 149,439 individuals aged 15–65 years, who report having a job at the time of the interview. We focus on the manufacturing sector only. As informality is not an option for public sector employees, our sample excludes individuals who report being employed in the public sector. To measure informal employment, we exploit a unique question of the Iranian LFS, which asks employed workers, typically wage earners, salaried and self-employed workers, whether they are covered by the so-called Social Security Organization (SSO), which provides insurance for major services, including retirement, disability, death, unemployment, health, and parental leave. We therefore construct our dependent variable, *Uninsured*, as a dummy taking value 1 if the worker is

[9] All the data are annual and collected according to the Iranian calendar that begins within a day of March 21 of the Gregorian calendar. The analysis is carried out based on the Iranian calendar and the specific Gregorian date, for instance 2012, refers to the period 20 March 2012–20 March 2013 in this study.

not covered by SSO (i.e. is employed off the books), and 0 otherwise.[10] Since 2019, the SCI has added additional questions to the LFS questionnaire to help identify informal employment in the Iranian labour market. To assess the credibility of our definition of informal employment, we used our measure to calculate the share of employment in the informal sector in 2019 and 2020. This measure was then compared with the corresponding statistic provided by the SCI using the additional questions (SCI 2021). The comparison reveals that the two measures are remarkably similar. We calculate the share of informal employment at 60 per cent as opposed to 59.27 per cent in the SCI calculation for 2019 and 58.23 per cent compared to 58.01 per cent in the SCI calculation for 2020. We also probe the robustness of our main results using employment in a microenterprise as alternative proxy for informal employment.[11]

Moreover, we define treatment and control groups on the basis of a worker's industry pre-sanctions level of exposure to international trade. We measure trade exposure at the industry level as the ratio of import penetration to the export share to capture the total effect of the sanctions (Campbell and Lusher 2019). We then define treated workers as those employed in industries with above-median levels of trade exposure. More specifically, *Treated* is a dummy variable that takes value 1 if the individual works in an industry with an ex-ante (i.e. as of 2008) ratio of import penetration to the export share above the median, and 0 otherwise. In our DiD estimation strategy, that we discuss in detail in Section 4, we use workers in below-median industries (i.e. the control group) as a counterfactual for treated ones. Moreover, we define the variable *Post* as an indicator for respondents interviewed in the sanctions period, that is, in 2012 or later.

Our dataset also includes the following variables: age, educational attainment (five levels), residence (urban versus rural), married, immigrant (no Iranian citizenship), job seniority (in years), four job types (employer, self-employed, family firm employee, private wage employee), 105 four-digit (ISIC, Revision 3.1) manufacturing industries, 7 years, and 30 provinces. The summary statistics of the variables used in the empirical analysis are reported in Appendix Table A.1. In our sample, 54 per cent of workers have no social security insurance (i.e. are employed informally). The percentage of workers employed in treated industries (i.e. with above-median exposure to trade as of 2008) is 36 per cent. Individuals interviewed after 2012 account for 44 per cent of our sample, while those interviewed in the post-sanction period and working in treated industries account for 16 per cent.

[10] Previous studies measuring informal employment as lack of social security insurance include Acosta and Gasparini (2007), Acosta and Montes-Rojas (2014), Attanasio et al. (2004), Cisneros-Acevedo (2022), Pavcnik et al. (2004), Paz (2014), and Radchenko (2014).

[11] According to the ILO's definition of employment in the informal sector, enterprises are considered as informal sector enterprises if 'their size in terms of employment is below a certain threshold to be determined according to national circumstances' (Hussmanns 2004). In our analysis, this is taken as fewer than 10 employees, which is the standard definition of a microenterprise in Iran, based on the definition provided by the Statistical Centre of Iran (SCI).

Sampled workers are predominantly male (76 per cent), and they are relatively young (34 years), with an average of 8 years of tenure. The percentage of workers with tertiary education (i.e. with university degree or higher education) is 36 per cent. About 27 per cent are single (never married) and 79 per cent live in urban areas, while only 3 per cent are immigrants (no Iranian citizenship). The table also shows that 5 per cent are employers, 27 per cent are self-employed, 5 per cent are family firm employees, while the vast majority (62 per cent) are private wage employees. Trade exposure, which is used as our treatment measure, exhibits a significant degree of heterogeneity between the control and treatment groups. The average imported-input intensity, which we employ as an alternative measure for identifying treated and control industries, also varies remarkably across the two groups. On average, 26 per cent of production inputs in treated industries are sourced from international market (with a minimum value of 10 per cent and a maximum value of 70 per cent), while this figure is less than 0.5 per cent for control industries.

To provide a better picture of cross-industry variation in our primary treatment variable and their respective activities, Table A.2 in the Appendix presents the mean trade exposure for a sample of industries from both the treatment and control groups. It is evident that industries involved in the manufacturing of advanced products are notably more exposed to trade. These industries tend to exhibit a higher dependence on international markets for sourcing inputs.

4. Estimation Strategy

We compare the probability that a worker works in the informal sector before and after the 2012 sanctions across industries differentially exposed to trade using a DiD approach and estimating the following model:

$$Y_{ijt} = \alpha + \delta\, Treated_j * Post_t + \gamma\, X_{ijt} + \lambda_j + \theta_t + \varepsilon_{ijt} \qquad (1)$$

where Y_{ijt} is an indicator for whether a worker *i* in industry *j* interviewed at time *t* works in the informal sector. To capture informal employment, we mainly employ the variable *Uninsured*, that we discussed in previous section, and that measures whether the worker is covered by social security insurance.[12] $Treated_j$ is, as described in Section 3, a dummy variable varying at the industry level, which is meant to capture a worker's industry exposure to international trade, and hence to the trade shock caused by the sanctions. $Post_t$ is a dummy for the post-sanction years 2012 to 2014. X_{ijt} is a vector of individual characteristics, including gender, age, age squared, education level (primary, lower secondary, upper secondary and tertiary, with no education used as the omitted category), type

[12] As an alternative outcome variable, in the robustness analysis we also use the variable *Enterprise size<10*, which is defined as a dummy equal to 1 for individuals working in microenterprises (as defined by the SCI), and 0 otherwise.

of residence (i.e. urban versus rural), marital status, immigrant status, and tenure (in years), as well as a set of province dummies that are meant to absorb any time-invariant province-level factors influencing labour market conditions in a province. The inclusion of these demographics allows to control for differences in worker composition across industries and years that could simultaneously affect a worker's probability of working in the informal sector and be spuriously correlated with the treatment. The specification also includes industry (λ_j) and year (θ_t) dummies, which capture all time-invariant industry characteristics correlated with the treatment and the outcome and any aggregate-level adjustments in informal employment over time, respectively.

The main parameter of interest is δ, the coefficient on the interaction term $Treated_j * Post_t$. This coefficient measures the impact of the sanctions on the likelihood of working in the informal sector for treated workers relative to untreated ones. A negative coefficient implies, for instance, that workers in industries initially facing higher exposure to international trade experience an increase in the probability of working in the informal sector after the sanctions. This would signify that the sanctions caused the reallocation of labour away from the formal sector. Finally, ε_{ijt} is an error term. Standard errors are clustered by industry to account for general forms of heteroskedasticity and serial correlation in the error term within an industry.

5. Results

As preliminary evidence, in Table A.3 in the Appendix, we report simple descriptive statistics in a standard 2×2 matrix and explore whether individuals' likelihood of working in the informal sector changed differently after the imposition of the sanctions in treated and control industries. For individuals interviewed before the sanctions (during the years 2008–11), the probability of being uninsured is on average 45 per cent for treated workers (i.e. in industries with higher initial exposure to international trade) and 65 per cent for workers in the control group (i.e. in industries with lower initial exposure to international trade). In contrast, for individuals interviewed after the sanctions (during the period 2012–14), the probability of being uninsured is 41 per cent for treated workers and 56 per cent for control workers. Our descriptive evidence shows that while the informality rate declined following the sanctions in both the treated and the control group, the difference in the share of informality between the two groups shrunk over time, moving from 20 percentage points in the pre-2012 period to 14 percentage points in the post-2012 period. This suggests that the sanctions slowed down the negative trend in informal employment for treated industries.

Table 1 shows the main results of our empirical analysis, obtained estimating equation 1 in Section 4, and using *Uninsured* as outcome variable. Column 1 reports the results of a basic regression (without controls) using only $Treated_j$, $Post_t$, the interaction term $Treated_j * Post_t$, and the two

separate measures for import penetration and export share at the industry level. The inclusion of import penetration and export share as separate explanatory variables allows to disentangle the effects of decreasing imports from decreasing exports. Moreover, it overcomes the potential concern that industries may have the same value in the ratio between import penetration and export share (i.e. our treatment variable) but different levels of exposure to trade.[13] The estimates in Column 1 indicate that workers in the post-2012 period have on average a 10-percentage points lower probability of working in the informal sector than workers in the pre-2012 period. This is consistent with the overall trend in employment rate, that, as documented in Moghaddasi and Nisticò (2022), steadily increased over the analysed sample period, notwithstanding the sanctions.

In addition, we find that workers in industries with higher ex-ante exposure to trade have on average a 17-percentage point lower probability of working in the informal sector in the years prior to the sanctions. Importantly, we find that, after the sanctions, the probability of working in the informal sector for treated workers is 6.5 percentage points higher than for untreated workers. This suggests that the sudden shock to market access caused by the sanctions might have induced a decline in productivity, and therefore in labour demand, especially in industries that heavily depend on imported inputs. As a result of the decline in productivity, firms might have faced higher incentives to reduce the costs by shifting their employees to the shadow sector.

Column 2 of Table 1 controls for a large set of individual characteristics to ensure balanced comparisons between treated and control workers. The coefficients of $Treated_j$ and $Post_t$ are now substantially lower in magnitude, with the former turning insignificant. Importantly, this implies that after controlling for individual characteristics, treated and control workers have the same pre-sanctions probability of working informally. Yet, we still find that in the post-2012 period treated workers have a 5.8-percentage point higher probability of working in the informal sector than untreated workers. For the control variables, we find that (i) female workers have a higher likelihood of working informally than their male counterparts; (ii) age is convexly related to informal employment; (iii) education reduces the likelihood of being uninsured; (iv) workers in urban areas have a lower probability of being employed in the informal sector; (v) ever married workers have a smaller likelihood of being employed in the shadow economy than single workers; and (vi) immigrants are significantly more likely to work in the informal sector than native workers. Finally, we find no significant effect of tenure on a worker's informal employment probability.

Column 3 of Table 1 adds industry dummies to account for time-invariant unobserved heterogeneity at the industry level. This allows to control for important industry-level start-of-period

[13] For example, consider two industries with a ratio equal to 1, but industry 1 has an import penetration and an export share equal to 0.5 and industry 2 has an import penetration and an export share equal to 0.05.

factors, such as the intensity of the use of production labour or the average wage. However, the set of industry dummies absorbs the variable $Treated_j$, as well as the two separate variables for import penetration and export share, which vary only across industries but not over time. Controlling for industry-specific factors, the estimated effect of interest is slightly lower (4.7 percentage points) and remains strongly significant.

Table 1: Main results.

	(1)	(2)	(3)	(4)	(5)
Treated * Post	0.065***	0.058***	0.047***	0.048***	0.048***
	(0.024)	(0.019)	(0.017)	(0.017)	(0.018)
Treated	-0.166*	-0.091			
	(0.095)	(0.068)			
Post	-0.094***	-0.053***	-0.052***		
	(0.017)	(0.013)	(0.015)		
Import penetration	0.115	0.186*			
	(0.236)	(0.101)			
Export share	0.129	0.001			
	(0.220)	(0.071)			
Female		0.324***	0.232***	0.231***	0.217***
		(0.028)	(0.031)	(0.031)	(0.029)
Age		-0.038***	-0.030***	-0.030***	-0.028***
		(0.005)	(0.003)	(0.003)	(0.003)
Age squared		0.000***	0.000***	0.000***	0.000***
		(0.000)	(0.000)	(0.000)	(0.000)
Primary		-0.061***	-0.067***	-0.067***	-0.052***
		(0.012)	(0.013)	(0.013)	(0.007)
Lower secondary		-0.108***	-0.104***	-0.103***	-0.083***
		(0.015)	(0.015)	(0.015)	(0.009)
Upper secondary		-0.151***	-0.120***	-0.118***	-0.099***
		(0.035)	(0.031)	(0.031)	(0.024)
Tertiary		-0.268***	-0.191***	-0.189***	-0.167***
		(0.026)	(0.025)	(0.025)	(0.016)
Urban		-0.062***	-0.067***	-0.067***	-0.054***
		(0.010)	(0.006)	(0.006)	(0.009)
Ever Married		-0.069***	-0.056***	-0.055***	-0.057***
		(0.016)	(0.012)	(0.012)	(0.013)
Immigrant		0.288***	0.254***	0.254***	0.266***
		(0.049)	(0.045)	(0.045)	(0.049)
Tenure (in years)		0.001	-0.003**	-0.003**	-0.003***
		(0.001)	(0.001)	(0.001)	(0.001)
Industry dummies	NO	NO	YES	YES	YES
Year dummies	NO	NO	NO	YES	YES
Province dummies	NO	NO	NO	NO	YES
Observations	149,439	149,434	149,434	149,434	149,434

Notes: Dependent variable, *Uninsured*, is a dummy that takes value 1 if the worker is not covered by social security insurance, and 0 otherwise. *Treated* is defined as a dummy equal to 1 for individuals working in industries with above-median exposure to trade. Exposure to trade is measured as the ratio of Import penetration to the export share, where Import penetration is computed as total import divided by initial absorption and Export share is measured as total export divided by initial production. The sample covers the years 2008-14. Standard errors in parentheses are clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: authors' compilation based on study data.

Columns 4 and 5 progressively add year and province dummies to control for time-invariant unobserved geographical factors and year fixed effects, respectively. Reassuringly, the estimates are virtually unchanged (4.8 percentage points). The size of the impact we detect is significant. Based on the results in Column 5, our preferred specification, our estimated effect (0.048) corresponds to an increase in the probability of working in the informal sector of about 9 per cent (=0.048/0.54) of the mean (0.54).

In Table 2, we probe the robustness of the results to additional controls in the main specification. In Column 1 we control for province and year dummies to account for any unobserved time-varying heterogeneity at the local level. This allows, for instance, to account for local labour market unemployment rates, or GDP growth, or population growth. Results are unchanged relative to those reported in Table 1, Column 5. In Column 2 we include sector-specific (linear) time trends to allow, for example, for different secular trends in the growth rate of aggregate labour and productivity across sectors, defined at the three-digit ISIC level. Once again, results are unchanged. In Column 3 we include a set of dummies for worker's type of job: self-employed, employed in family firm, private firm wage employee, and employer (used as the omitted category). The estimated coefficient is strongly significant and slightly larger in magnitude than that in our main specification. This reassures us that our main result is not confounded by different shares of informality across different job types.

Table 2: Robustness results.

	(1) Including province* year dummies	(2) Including sector time trends	(3) Including job type dummies	(4) Excluding inter-province movers	(5) Excluding bottom 10% in export share	(6) Using alternative treatment definition	(7) Using alternative outcome definition
Treated * Post	0.045***	0.042**	0.047**	0.048***	0.047***	0.064***	0.037**
	(0.015)	(0.018)	(0.018)	(0.018)	(0.018)	(0.021)	(0.016)
Observations	149434	149434	149434	148649	146620	74482	149434

Notes: In columns 1-6 the dependent variable is *Uninsured*, a dummy that takes value 1 if the worker is not covered by social security insurance, and 0 otherwise, while in column 7 is *Enterprise size<10*, a dummy that takes value 1 if the individual works in an enterprise with less than 10 employees, and 0 otherwise. *Treated* is defined as a dummy equal to 1 for individuals working in industries with above-median exposure to trade. Exposure to trade is measured as the ratio of Import penetration to the export share, where Import penetration is computed as total import divided by initial absorption and Export share is measured as total export divided by initial production. Job types are self-employed, employed in family firm, private firm wage employee, and employer (omitted category). Sectors are defined at 3-digit ISIC level. The sample covers the years 2008-14. All specifications include individual controls plus year, industry, and province dummies as in column 5 of Table 1. Standard errors in parentheses are clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: authors' compilation based on study data.

Next, in Column 4 of Table 2 we check that results are not driven by the potential sorting of individuals across provinces. In fact, the sanctions might have induced individuals to move away

from their province in search of better employment prospects. Thus, in Column 4 we exclude from the sample those individuals who moved across provinces within the last year (0.5 per cent of our sample). The estimates are similar to those in our main specification.

In Column 5 we check that our result does not change when we exclude industries with very low pre-sanction export share. Recall that our treatment indicator is defined on the basis of the trade exposure variable, which is measured as the ratio between import penetration and export share. While we showed in Figure 1 that the trend in trade exposure mostly reflects the trend in import penetration (the numerator), it might still be the case that high values in trade exposure are associated with industries at the bottom of the export share distribution. This would imply that industries with low export share would likely end up being defined as treated industries according to our treatment definition. Thus, in Column 5 we replicate our main specification excluding industries in the bottom 10 per cent of the export share distribution. Reassuringly, results are not affected by such exclusion.[14]

In Column 6 of Table 2 we verify that our main estimate does not hinge on the way we define our treatment and control groups. Indeed, it could well be argued that industries with trade exposure below median (i.e. the control group) might also be partially affected by the sanctions. This might imply that the Stable Unit Treatment Value Assumption condition is not met, thus questioning the precision of our counterfactual. To mitigate this concern, we replicate our main specification using industries in the top quartile of the trade exposure distribution as treated and those in the first quartile as control, while excluding industries between the 25th and the 75th percentile. Reassuringly, the estimate in Column 6 remains strongly significant and increases in magnitude (as expected). Still, we cannot fully exclude the possibility that control industries might be indirectly affected by the sanctions via the input–output linkages with treated industries.

Furthermore, in Column 7 we check that results are not driven by our definition of informal employment. As documented in previous studies, low- and middle-income countries have large shares of employment in small enterprises operating in the informal sector (Gollin 2002, 2008; Tybout 2000, 2014). Thus, as an alternative proxy for informal employment we use an indicator for working in a small enterprise (i.e. with fewer than 10 employees). Remarkably, the treatment effect is also strongly significant in such case, although the economic magnitude of the effect shrinks somewhat, moving from 9 per cent (=0.048/0.54) to 6 per cent (=0.038/0.69) of the mean.

Another concern with our results is that they could be driven by a few industrial provinces, such as Tehran, Isfahan, and Khuzestan. To explore this issue, we estimate our main specification by excluding one province at a time. Figure A.2 in the Appendix shows the estimated coefficient of

[14] Results are similar when we exclude industries in the bottom 25 per cent of the export share distribution.

interest for any of the 30 separate regressions. Notably, our main result remains stable in size and highly significant in all these exercises.

5.1 DiD validity checks

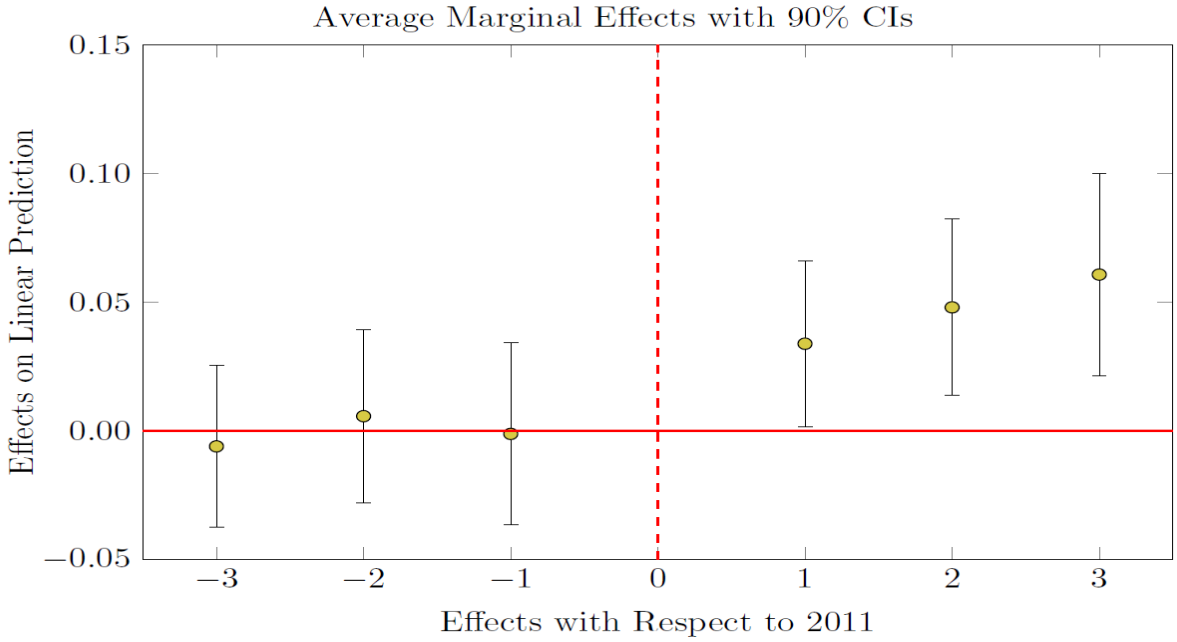
The validity of the DiD approach relies on the common trend assumption, that is, the hypothesis that, without the sanctions, treated and control individuals would have followed similar trends in the outcome. Although this hypothesis cannot be tested directly because the counterfactual is not observable, we can verify whether before 2012 workers in industries with high versus low exposure to trade were following similar trends in the probability of working in the informal sector. Using data covering the period 2008–14, we run an event-study analysis with lags and leads à-la Autor (2003) and regress our indicator for working in the informal sector, Uninsured, on the full set of interactions between Treated and the year dummies. We estimate a specification that includes all individual characteristics as well as the full set of industry, year, and province dummies. The results are shown in Appendix Table A.4. While in Column 1 we use 2011 (i.e. the year before the sanctions) as the reference category, in Columns 2, 3, and 4, we use years 2010, 2009, and 2008, respectively, as alternative reference. Reassuringly, we find no evidence of different trends for treated versus control workers in the pre-sanction period. The coefficients of the interaction terms for any year prior to 2012 are never statistically significant, thus ruling out the possibility that before the sanctions the share of informal employment in industries with high exposure to trade was already following a different pattern from that in industries with low exposure.

For visual inspection, Figure 3 plots the coefficients reported in Column 1 of Appendix Table A.4, where we use 2011 as reference year. As the figure clearly shows, the trend in the share of informal employment for treated industries during the years before the sanctions is statistically indistinguishable from that for untreated industries. Instead, the two trends start to significantly diverge immediately after the imposition of the sanctions, providing evidence that the increase in the probability of being employed in the shadow economy since 2012 is likely attributable to the labour demand shock caused by the sanctions. Moreover, the event-study analysis allows us to estimate the dynamics of the effect of sanctions on informal employment. Figure 3 suggests that the effect is positive and statistically significant on impact (i.e. in 2012) and becomes increasingly larger in magnitude during the subsequent 2 years.

We also follow Pei et al. (2019) and test our identifying assumption using the covariates on the left-hand side of Equation 1. According to this test, our design is presumed to be valid if we systematically find null effects, implying that the observable characteristics are not affected by the interaction term $Treated_j * Post_t$, that is, there are no compositional effects induced by the

sanctions. Reassuringly, the results in Appendix Table A.5 show that, except for female, the estimated coefficient of interest (δ in Equation 1) is never statistically significant; that is, our estimated effect does not depend on specific sub-groups of the sample we analyse. The significant effect on female might be due to the disproportionate gender balance across treated and untreated industries.

Figure 3: Estimated differences in treated versus control industries before and after the sanctions.



Source: authors' computation based on study data.

As a final validity check, we use data for the period before the sanctions and run two placebo tests, hypothesizing that a fictitious sanctions regime was imposed on Iran in the years 2010 and 2009. We therefore build two dummy variables, *Fake Post 2010* and *Fake Post 2009* as well as their respective interactions with *Treated*. In Table A.6 in the Appendix we report the results of the two separate regressions. The estimates in Columns 1 and 2 show that the coefficients of the interaction terms are never statistically significant. This again confirms that the effect of the sanctions-induced trade shock on informality is not a spurious correlation. Importantly, the results reported in Column 1 for *Fake Post 2010* are informative of the short-run impact of the Iranian targeted subsidy plan introduced by the government in 2010 and suspended in 2012.[15] These estimates show no significant effects on informal employment, indicating that our main results are not confounded by the delayed effect of pre-sanctions policies.

[15] The government plan meant to replace subsidies on food and energy with targeted social assistance, and to reallocate funds to people and the industrial sector.

5.2. Heterogeneous responses

In Table 3, we investigate whether workers' response to the trade shock differs in relation to various individual characteristics. First, we focus on gender and test whether the effect of the sanctions on the probability of working in the informal sector for female workers differs from that observed for their male counterparts. We find no differential effects across gender (Column 1). Second, we examine heterogeneous effects by age. Given that, as shown in Table 1, the relationship between age and informal employment is nonlinear and convex, we split the sample in three age groups: 15–30, 30–50, and 50+ years. The results in Column 2 show that the impact of the sanctions does not vary across workers of different ages.

Table 3: Heterogeneous results.

	(1)	(2)	(3)	(4)	(5)
Treated * Post	0.052**	0.053***	0.064***	0.049***	0.049***
	(0.016)	(0.020)	(0.023)	(0.017)	(0.018)
Treated * Post * Female	-0.040				
	(0.040)				
Treated * Post * Age15-30		-0.025			
		(0.022)			
Treated * Post * Age50+		-0.056			
		(0.039)			
Treated * Post * Tertiary			-0.081**		
			(0.036)		
Treated * Post * Urban				0.001	
				(0.022)	
Treated * Post * Immigrant					0.061
					(0.046)
Observations	149,434	149,434	149,434	149,434	149,434

Notes: Dependent variable, *Uninsured*, is a dummy that takes value 1 if the worker is not covered by social security contributions, and 0 otherwise. *Treated* is defined as a dummy equal to 1 for individuals working in industries with above-median exposure to trade. Exposure to trade is measured as the ratio of Import penetration to the export share, where Import penetration is computed as total import divided by initial absorption and Export share is measured as total export divided by initial production. The sample covers the years 2008-14. All specifications include individual controls plus year, industry, and province dummies as in column 5 of Table 1. Standard errors in parentheses are clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: authors' compilation based on study data.

Third, we explore whether the effect varies by the level of education achieved. As high-skilled workers have better employment prospects than low-skilled workers, one would expect the sanctions to hit harder for poorly educated workers than for highly educated ones. In Column 3 we verify whether workers with university degree or higher education are differentially affected than workers with a level of education up to high school. The estimates in Column 3 show that the sanctions asymmetrically affect workers with and without tertiary education. More thoroughly, we find that the effect is negative for individuals without tertiary education, but it turns positive for workers with tertiary education. This finding is consistent with the evidence in McCaig and Pavcnik (2015).

Finally, in Columns 4 and 5 we respectively check for the presence of heterogeneous effects by workers' residence type (urban versus rural area) and immigrant status. We find no significantly different results across these two dimensions.

Next, we assess whether the effects vary across industries with different labour intensity in the production process. To classify industries as labour intensive or capital intensive, we use the United Nation Industrial Development Organization classification from Van Beers (1998). We find no significant differential effects for workers in labour-intensive industries relative to workers in capital-intensive ones.[16]

5.3. Effects on unemployment and labour supply

In this section we explore the effects of the sanctions on other margins: unemployment and hours worked per week. Table 4 shows the results of our main specification using the probability of being employed and the total number of hours worked per week as dependent variable in Columns 1 and 2, respectively. The estimates in Table 4 indicate that: 1) the sanctions have no differential effect on the probability of being employed for treated as opposed to untreated workers, 2) treated workers tend to work almost 1 hours less than their non-treated counterparts in the period after the sanctions. Moreover, Table A.7 in the Appendix shows that the negative effect on hours worked is larger for low-educated workers and for workers in rural areas, pointing again to the unequal labour market consequences of the sanctions.

Table 4: Additional results on unemployment and hours worked.

	(1) Employed	(2) Hours worked
Treated * Post	0.005	-0.977*
	(0.010)	(0.511)
Observations	169,965	149,004

Notes: Dependent variable in column 1, *Employed*, is a dummy that takes value 1 if the worker is employed, and 0 otherwise. Dependent variable in column 2, *Hours worked*, measures the worker's total number of hours worked per week. *Treated* is defined as a dummy equal to 1 for individuals working in industries with above-median exposure to trade. Exposure to trade is measured as the ratio of Import penetration to the export share, where Import penetration is computed as total import divided by initial absorption and Export share is measured as total export divided by initial production. The sample covers the years 2008-11. All specifications include individual controls plus year, industry, and province dummies as in column 5 of Table 1. Standard errors in parentheses are clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: authors' compilation based on study data.

Taken together, these results indicate that although the sanctions had no effect on workers' likelihood of being employed, they produced a negative effect on the intensive margin of labour supply. This finding, coupled with the positive effect on informal employment we discussed above,

[16] Results are not shown for sake of brevity but are available upon request.

suggest that Iranian firms did not alter the overall workforce as a response to the trade shock caused by the sanctions, but they did adjust their labour demand by lowering it at the intensive margin and by shifting part of their workforce off the books.

5.4. Dealing with sorting issues

One final concern with the DiD estimates presented so far is that they might be affected by sorting bias. This bias derives from the fact that the potential endogenous sorting of workers into treated versus untreated industries can vary between pre- and post-sanctions periods, and therefore cannot be differentiated by the DiD approach. To deal with this problem, we adopt an instrumental variable approach and use as an instrument for Treated the start-of-period (i.e. as of 2008) share of employment in treated industries in the same province in which the worker is currently employed, *Percentage treated in province*.[17] Analogously, as our model includes the interaction term *Treated * Post*, we instrument this variable with the interaction term *Percentage treated in province * Post*.

The main idea underlying our instrument is that whether an individual ends up being employed in a treated or control industry depends to some extent on the productive structure (i.e. the industry mix) of their province of residence. This is especially true if inter-province mobility is not so common, as we show above. The variable *Percentage treated in province* is meant to precisely capture this mechanism. The higher the percentage of workers in industries highly exposed to import penetration in each province, the lower the individual probability of ending up working in those industries. This is because firms in treated industries might face higher competition—hence, lower labour demand—and therefore have greater incentives to shift their activities into the informal sector. The exclusion restriction relies on the assumption that the pre-existing share of employment in industries with above-median trade exposure in a worker's province does not affect their probability of informal employment directly. We can think that, once all individual observed factors are controlled for, our instrument would indeed affect informal employment only via its effect on the likelihood of being employed in a treated industry.

Table 5 reports the results obtained from the instrumental variable DiD estimation strategy, using the two-stage least squared (TSLS) estimator. Columns 1–3 replicate the specifications reported in Table 1, Columns 1–3, respectively. We could not replicate the specifications in Columns 4 and 5 of Table 1 because the inclusion of industry and province dummies would absorb all the variation in the *Treated* and the *Percentage treated in province* variables, respectively. Results from the first-stage regressions indicate that our instruments are strongly significant in explaining workers' choice

[17] Since the instrument is defined in a pre-sanctions period, our instrumental variable strategy also helps deal with the potential sorting of firms into industries with various levels of exposure to trade.

of working in treated versus control industries.[18] That our instrumental variable is a strong predictor of the likelihood of ending up working in a treated industry is confirmed by the value of the first-stage F-statistic that is always above 100.

Table 5: Two Stage Least Squared results.

	(1)	(2)	(3)
Second-stage regressions			
Treated * Post	0.091***	0.074***	0.074***
	(0.031)	(0.025)	(0.025)
Treated	-0.289**	-0.097	-0.097
	(0.132)	(0.069)	(0.069)
Post	-0.107***	-0.062***	
	(0.019)	(0.015)	
First-stage regressions for *Treated*			
Percentage treated in province	-0.020***	-0.020***	-0.020***
	(0.001)	(0.001)	(0.001)
Kleibergen-Paap F statistic	102.600	111.912	112.013
Individual controls	NO	YES	YES
Year dummies	NO	NO	YES
Observations	149,439	149,434	149,434

Notes: Dependent variable, *Uninsured*, is a dummy that takes value 1 if the worker is not covered by social security contributions, and 0 otherwise. *Treated* is defined as a dummy equal to 1 for individuals working in industries with above-median exposure to trade. Exposure to trade is measured as the ratio of Import penetration to the export share, where Import penetration is computed as total import divided by initial absorption and Export share is measured as total export divided by initial production. *Percentage treated in province* is defined as the start-of-period share of employment in treated industries in the same province in which the worker is currently employed. The sample covers the years 2008-14. Individual controls are those in column 5 of Table 1. Standard errors in parentheses are clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: authors' compilation based on study data.

The second-stage estimates of our coefficient of interest (i.e. the interaction Treated * Post) confirm the significant positive effect of the sanctions on informal employment. The size of the impact we estimate in Column 3 of Table 5 (7.4 percentage points) is larger than the one we estimate in Table 1, Column 5 (4.8 percentage points), and corresponds to an increase in formal employment by 13 per cent of the mean.[19] This is because in the presence of heterogeneous effects of the treatment on informal employment, our TSLS estimator captures a weighted average of LATE defined for each pair of levels of the instrument and for each value of the covariates (Cornelissen et al. 2016; Heckman and Vytlacil 2001, 2005). However, the estimation of a weighted average of LATEs requires the validity of the monotonicity assumption. In our case, this would imply that an increase in the share of employment in treated industries in a worker's province does not increase the probability of being employed informally, which we think is a tenable assumption. Overall, the TSLS estimates in Table 7 lend further credibility to our main results in Table 1.

[18] In Table 5 we only report the estimates of the first-stage regression for *Treated* and omit those for *Treated * Post.*

[19] This is consistent with Imbens and Angrist (1994), who show that in the presence of heterogeneous effects the instrumental variable estimates can be larger than the ordinary least square ones.

5.5. Alternative Treatment

In section 2, we underscored the significance of market access for imported inputs as the primary mechanism underpinning the impact of economic sanctions on informal employment, thus highlighting an alternative channel with respect to foreign competition whereby trade shocks can affect informality. In this section, we investigate the validity of this argument by going a step further and directly defining our treatment variable based on imported input intensity.

We define imported-input intensity of industry *j* as the share of material spending allocated to imported inputs in 2008 in that industry.[20] We subsequently define treated workers as those employed in industries with ex-ante (as of 2008) level of imported-input intensity above the median. Table 6 presents the results obtained by estimating the models specified in columns 1 to 5 of Table 1, now using the alternative treatment variable. These results provide robust support for our hypothesis that the disruption of Iranian companies' access to essential production inputs from the international market is fundamental mechanism through which economic sanctions affect informal employment. Note that the size of the effect in column 5 of Table 6 is very similar to that estimated in column 5 of Table 1. This evidence reaffirms the notion that constraining the import of crucial inputs plays a fundamental role in driving the observed effects of economic sanctions on informal employment.

Table 6: Additional results using imported-input intensity to define treated and control industries.

	(1)	(2)	(3)	(4)	(5)
Treated * Post	0.048*	0.044**	0.046**	0.046**	0.045**
	(0.029)	(0.020)	(0.018)	(0.018)	(0.019)
Treated	-0.065	0.015			
	(0.129)	(0.058)			
Post	-0.104***	-0.059***	-0.060***		
	(0.025)	(0.017)	(0.018)		
Industry dummies	NO	NO	YES	YES	YES
Year dummies	NO	NO	NO	YES	YES
Province dummies	NO	NO	NO	NO	YES
Observations	149,439	149,434	149,434	149,434	149,434

Notes: Dependent variable, *Uninsured*, is a dummy that takes value 1 if the worker is not covered by social security insurance, and 0 otherwise. *Treated* is defined as a dummy equal to 1 for individuals working in industries with above-median level of imported-input intensity, defined as the share of material spending allocated to imported inputs in 2008 in that industry. The sample covers the years 2008-14. Standard errors in parentheses are clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: authors' compilation based on study data.

6. Conclusions

This paper estimates the short-run effects of economic sanctions on informal employment. We focus on the case of the unprecedented sanctions jointly imposed by the EU, the UN, and the US on the Iranian economy in 2012 to investigate how trade shocks affect the reallocation of labour between

[20] The data is from the annual Survey of Manufacturing Firms with more than 10 workers of the SCI.

the formal and the informal sector. Our empirical strategy exploits the variation in the level of exposure to international trade across industries prior to the sanctions. We therefore employ a DiD approach, which allows us to compare the probability of working in the informal sector before and after the sanctions for workers in industries that were differentially exposed to international trade before the sanctions. Our analysis reveals that the sanctions increased informal employment by 9 per cent of the mean. Notably, this effect was significantly larger for workers with lower level of education. Furthermore, when we employ an instrumental variable DiD approach, the positive effect of the sanctions on informal employment reaches 13 per cent of the mean. These findings consistently demonstrate a positive impact of the sanctions on informal employment.

One potential caveat of our empirical analysis is the possibility that the estimated impact of the sanctions might partly reflect the effects of uncontrolled countervailing government policies introduced concurrently with the sanctions in 2012. For these policies to be a concern, they would need to have disproportionally affected industries with different pre-existing exposure to international trade. Although we are not aware of any such government policies being implemented in 2012 that differentially targeted workers in industries with different pre-existing openness to trade, in our analysis this concern should be mitigated to a substantial extent for two reasons. First, we focus on a relatively short sample period of three years before and three years after the sanctions. The institutional, political, and social environment, which typically changes slowly, is less likely to undergo significant changes over such a restricted timeframe. Second, our sample excludes the immediate post-sanctions periods, as sanctions were eased in 2015. This is mainly because the institutional, political, and social setting in the period following the lifting of sanctions may differ substantially from the period during the sanctions. Overall, these considerations should strengthen the confidence in the validity of our findings.

The findings of this study offer key insights into the analysis of the economic effects of sudden and extreme shocks to market access, highlighting the role of the informal sector in absorbing a substantial number of displaced individuals. This finding aligns with the role of the informal sector as a buffer, as previously documented in theoretical (Dix-Carneiro et al. 2021) and empirical (Dix-Carneiro and Kovak 2019; Goldberg and Pavcnik 2003; Ponczek and Ulyssea 2022) works. However, the loss of access to input markets can lead to resources reallocation towards informal and less productive activities, ultimately affecting aggregate welfare and economic growth. In this regard, our findings also provide insights into a potentially important margin of labour market adjustment through which sanctions can affect the target country's economy. Moreover, the results presented here carry relevant policy implications for addressing informal employment and providing support to trade-displaced workers. Our heterogeneity analysis offers useful recommendations on the specific groups

that the domestic policies should prioritise to mitigate the flow of workers from the formal to the informal sector.

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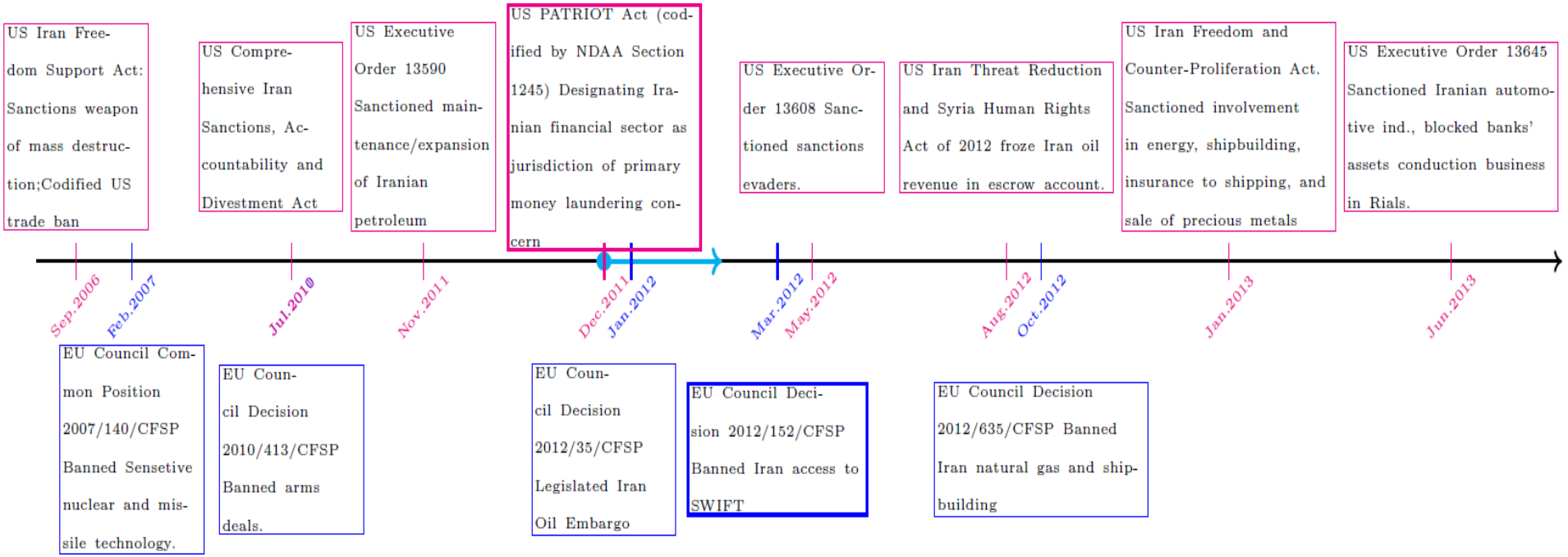
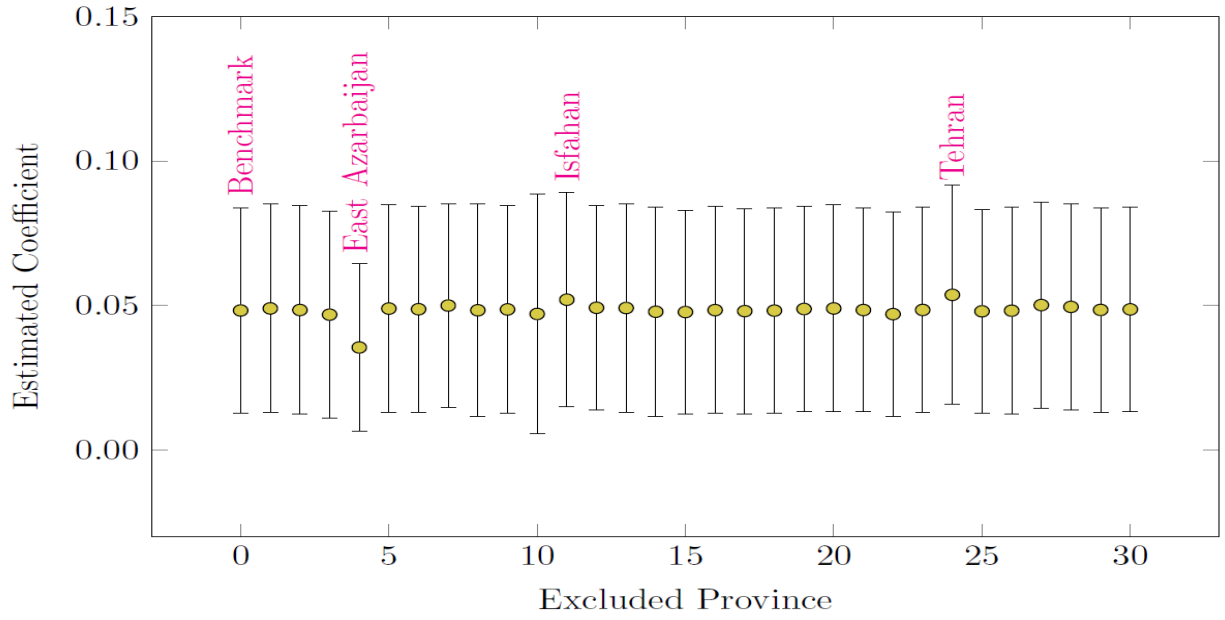
Appendix

Figure A.1: Timeline of the key sanctions imposed on Iran.



Source: authors' compilation.

Figure A.2: Estimated effects excluding one province at a time.



Source: authors' computation based on study data.

Table A.1: Summary statistics

	(1) Mean	(2) SD	(3) Min	(4) Max
Uninsured	.54	.50	0	1
Enterprise size<10	.69	.46	0	1
Treated	.36	.48	0	1
Post	.44	.50	0	1
Treated * Post	.16	.36	0	1
Female	.24	.43	0	1
Age	34.09	10.53	15	65
No education	.02	.14	0	1
Primary education	.29	.45	0	1
Lower Secondary education	.26	.44	0	1
Upper Secondary education	.01	.09	0	1
Tertiary education	.36	.48	0	1
Urban	.79	.41	0	1
Ever married	.73	.45	0	1
Immigrant	.03	.16	0	1
Tenure (in years)	8.46	8.60	0	55
Employer	.05	.23	0	1
Self employed	.27	.45	0	1
Family firm employee	.05	.21	0	1
Private wage employee	.62	.48	0	1
Trade exposure (control group)	2.71	2.40	0.11	8.63
Trade exposure (treatment group)	70.35	127.8	8.68	673.17
Imported-input intensity (control group)	0.04	0.03	0	0.09
Imported-input intensity (treatment group)	0.26	0.14	0.09	0.71

Notes: The sample consists of 149,439 observations and covers the period 2008-14. It includes individuals aged 15 to 65 with a job at the time of interview (excluding public sector employees). *Source*: authors' compilation based on study data.

Table A.2: Summary statistics: industry samples in treatment and control groups.

Treatment				Control			
Industry	Activity	Trade exposure	Imported-input intensity	Industry	Activity	Trade exposure	Imported-input intensity
3312	Manufacture of machinist's precision tools	161.96	0.22	1520	Dairy products	2.25	0.06
3230	Manufacture of consumer electronics	88.20	0.31	2696	Plastic products	1.41	0.005
3120	Manufacture of electricity distribution and control apparatus	23.70	0.17	2411	Manufacture of basic chemicals, except fertilizers	0.82	0.02
3430	Manufacture of pharmaceuticals, medicinal chemicals and …	15.32	0.21	1911	Dressing and dyeing fur	0.34	0.06
2912	Manufacture of pumps, compressors, taps and valves	10.02	0.28	1545	Manufacture of bakery products	0.11	0.05

Notes: Treated industries are those with above-median exposure to trade. Exposure to trade is measured as the ratio of Import penetration to the export share, where Import penetration is computed as total import divided by initial absorption and Export share is measured as total export divided by initial production. *Source*: authors' compilation based on study data.

Table A.3: Informality shares in treated and control industries before and after the sanctions.

	Pre	Post	Post-Pre Difference
Treated	0.452	0.411	-0.041***
	(0.003)	(0.003)	(0.004)
Control	0.653	0.556	-0.097***
	(0.002)	(0.002)	(0.003)
Treated-Control Difference	-0.201***	-0.145***	0.056***
	(0.004)	(0.004)	(0.005)

Notes: Statistics refer to the variable *Uninsured*, a dummy that takes value 1 if the worker is not covered by social security insurance, and 0 otherwise. Treated is defined as a dummy equal to 1 for individuals working in industries with above-median exposure to trade. Exposure to trade is measured as the ratio of Import penetration to the export share, where Import penetration is computed as total import divided by initial absorption and Export share is measured as total export divided by initial production. The sample covers the years 2008-14. Standard errors in parentheses are clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: authors' compilation based on study data.

Table A.4: Estimated differences in treated-control individuals before and after the 2012 sanctions.

	(1)	(2)	(3)	(4)
Treated * Year 2008	-0.006	-0.005	-0.012	
	(0.019)	(0.017)	(0.014)	
Treated * Year 2009	0.006	0.007		0.012
	(0.020)	(0.018)		(0.014)
Treated * Year 2010	-0.001		-0.007	0.005
	(0.021)		(0.018)	(0.017)
Treated * Year 2011		0.001	-0.006	0.006
		(0.021)	(0.020)	(0.019)
Treated * Year 2012	0.034*	0.035	0.028	0.040*
	(0.019)	(0.026)	(0.022)	(0.023)
Treated * Year 2013	0.048**	0.049*	0.042*	0.054**
	(0.021)	(0.027)	(0.023)	(0.026)
Treated * Year 2014	0.061**	0.062**	0.055**	0.067**
	(0.024)	(0.027)	(0.023)	(0.026)
Observations	149,434	149,434	149,434	149,434

Notes: Dependent variable, *Uninsured*, is a dummy that takes value 1 if the worker is not covered by social security insurance, and 0 otherwise. *Treated* is defined as a dummy equal to 1 for individuals working in industries with above-median exposure to trade. Exposure to trade is measured as the ratio of Import penetration to the export share, where Import penetration is computed as total import divided by initial absorption and Export share is measured as total export divided by initial production. The sample covers the years 2008-14. All specifications include individual controls plus year, industry, and province dummies as in column 5 of Table 1. Standard errors in parentheses are clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: authors' compilation based on study data.

Table A.5: Pei et al. (2019) test of identifying assumption.

	(1) Female	(2) Age	(3) Education	(4) Urban	(5) Married	(6) Immigrant	(7) Tenure
Treated * Post	0.011*	-0.004	0.007	0.002	-0.010	0.002	-0.137
	(0.007)	(0.008)	(0.011)	(0.011)	(0.021)	(0.004)	(0.263)
Observations	149,439	149,439	149,439	149,439	149,439	149,439	149,434

Notes: Dependent variable, *Uninsured*, is a dummy that takes value 1 if the worker is not covered by social security insurance, and 0 otherwise. *Treated* is defined as a dummy equal to 1 for individuals working in industries with above-median exposure to trade. Exposure to trade is measured as the ratio of Import penetration to the export share, where Import penetration is computed as total import divided by initial absorption and Export share is measured as total export divided by initial production. The sample covers the years 2008-14. All specifications include individual controls plus year, industry, and province dummies as in column 5 of Table 1. Standard errors in parentheses are clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: authors' compilation based on study data.

Table A.6: Placebo tests in sample period 2008-11.

	(1)	(2)
Treated * Fake Post 2010	-0.006	
	(0.020)	
Treated * Fake Post 2009		0.011
		(0.022)
Observations	81,436	81,436

Notes: Dependent variable, *Uninsured*, is a dummy that takes value 1 if the worker is not covered by social security contributions, and 0 otherwise. *Treated* is defined as a dummy equal to 1 for individuals working in industries with above-median exposure to trade. Exposure to trade is measured as the ratio of Import penetration to the export share, where Import penetration is computed as total import divided by initial absorption and Export share is measured as total export divided by initial production. The sample covers the years 2008-11. All specifications include individual controls plus year, industry, and province dummies as in column 5 of Table 1. Standard errors in parentheses are clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: authors' compilation based on study data.

Table A.7: Heterogeneous results on hours worked.

	(1)	(2)	(3)	(4)	(5)
Treated * Post	-0.309	-0.779	-1.221**	-2.062***	-0.880*
	(0.768)	(0.597)	(0.539)	(0.393)	(0.502)
Treated * Post * Female	-1.110				
	(1.791)				
Treated * Post * Age15-30		-0.596			
		(1.010)			
Treated * Post * Age50+		1.730*			
		(1.034)			
Treated * Post * Tertiary			2.534**		
			(1.229)		
Treated * Post * Urban				1.514**	
				(0.731)	
Treated * Post * Immigrant					1.392
					(3.528)
Observations	149,000	149,000	149,000	149,000	149,000

Notes: Dependent variable, *Hours worked*, measures the worker's total number of hours worked per week. *Treated* is defined as a dummy equal to 1 for individuals working in industries with above-median exposure to trade. Exposure to trade is measured as the ratio of Import penetration to the export share, where Import penetration is computed as total import divided by initial absorption and Export share is measured as total export divided by initial production. The sample covers the years 2008-14. All specifications include individual controls plus year, industry, and province dummies as in column 5 of Table 1. Standard errors in parentheses are clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: authors' compilation based on study data.