The Arab Spring migrant wave and illegal labor on vineyards: Counting the uncountable

Stefan Seifert and Marica Valente*

Abstract About 2,7 million irregular migrants are estimated to work in Europe, of which up to 12% are illegally employed in Italian agriculture. Both farmers and illegal workers have incentives to match on the labor market, and irregular migrant waves may provide additional labor supply shocks. Using the exogenous variation of the 2011 Arab Spring migrant wave on southern Italian coasts and focusing on vineyards, this paper quantifies illegal employment caused by the migration-induced supply shock. Identification relies on abnormal increases in reported labor productivity due to underreported labor hours. Based on farm data at regional level and a dynamic panel model, labor productivity is estimated to abnormally increase by about 11% on average for 2011 and 2012 on vineyards of the landing regions Sicily and Apulia. We show that this corresponds to a total of around 10 million unreported work hours in each year, or 5,500 agricultural work units. We interpret this as an increase in illegal employment with displacement of legal labor due to the migration wave.

Keywords: Migration wave, Illegal employment, Labor productivity, Arab Spring
JEL Codes: F22, J61, J43

*Stefan Seifert – s.seifert@ilr.uni-bonn.de – University of Bonn, Institute for Food and Resource Economics, Bonn, Germany. Marica Valente – mvalente@ethz.ch – ETH Zurich, Zurich, Switzerland, and DIW Berlin (German Institute for Economic Research) Berlin, Germany. We thank Bernd Fitzenberger, Jeffrey Wooldridge, and Lorenzo Trapani for their invaluable advice. Also, we thank thank Pio Baake, Boris Bravo-Ureta, Tomaso Duso, Cristina Salvioni, and Angelo Zago for fruitful input. We thank the American Association of Wine Economists for financial support, and Mariusz Stefan Migas (European Commision), Mauro Santangelo (CREA), and Tiziana Sarnari (ISMEA) for providing data. The paper greatly benefited from discussions at the 2017 conferences of the American Association of Wine Economists (AAWE, Padua), the Agricultural Economics Society (AES, Dublin), the European Workshop on Efficiency and Productivity Analysis (EWEPA, London), and the Econometrics of Panel Data and Network Analysis Summer School (CRC, Berlin), as well as at the seminars of the Loughborough University, University of Verona, and DIW Berlin. We also thank the participants of the Fifth CReAM workshop for suggestions and discussion.

Introduction

European industries such as agriculture and construction have long relied on illegal employment and migrant workers to reduce factor costs, and to sustain international competition (Boswell & Straubhaar, 2004). In fact, employing irregular migrants for seasonal, labor-intense jobs is often the only option due to the lack of native labor supply. In this respect, labor supply shocks of irregular migrants may allow an easy match of seasonal labor demand to supply of individuals willing to work illegally.[1] Both irregular migration and illegal work are top priorities in the European Union (EU) due security, financial, and welfare concerns including, e.g., labor market effects on incumbent workers and employers, losses to national revenues, integration of irregular migrants, exploitation of illegal workers, and other violations of labor and human rights (Penal Code, 2009). Policies addressing such concerns need to account for the dimension of the phenomenon, however, the number of irregular migrants and illegal workers is unobserved, and difficult to estimate (Borjas, 2017).

In this paper, we propose a way to identify unreported labor, and we estimate the amount of illegal employment in the grape growing sector caused by a sudden irregular migrant wave. The latter is used as a source of exogenous variation to identify illegal employment under the assumption that an increase in illegal employment leads to an abnormal increase in labor productivity due to underreported work hours. Under plausible assumptions, this paper provides evidence on the displacement of legal by illegal labor in the agricultural sector, which, to our knowledge, is rarely addressed by the literature.[2]

In our empirical analysis, we study labor productivity on vineyards in Italian and French regions, and we analyze the impact of the 2011 Arab Spring migrant wave on illegal employment in the landing regions Sicily and Apulia. The identification of illegal labor uses vineyard labor productivity defined as the ratio of output over labor input. Contrary to its two components, this ratio is rather constant over time[3] and less sensitive to unobserved and time-varying factors such as weather, which allows us to better isolate the effect of the illegal labor supply shock on vineyards. Thereby, if illegal employment increases after the supply shock, vineyard labor productivity is expected to abnormally increase due to underreported work hours because the output is not impacted by the sudden availability of illegal labor. This is due to the fact that grape growers generally

[1]We use "irregular" to refer to unauthorized/undocumented persons on the European Union territory, and "illegal" to any worker, regular or not, without legal employment.
[2]This is usually due to the lack of data on illegal labor, and, generally, on the informal sector.
[3]See, e.g., discussions in Jorgenson & Griliches, 1967; Lamouria *et al.*, 1963.

harvest all the output. As a result, an abnormal increase in labor productivity after the shock indicates displacement of legal with illegal employment. This, however, does not necessarily indicate that incoming irregular migrants displace incumbent workers. Since identification only requires that illegal labor can substitute unskilled labor flexibly without sacrificing learning effects, former legally employed workers might become illegally employed due to increased competition.

We use the exogenous variation from the 2011 Arab Spring migrant wave that hit the southern Italian regions of Apulia, Calabria and, primarily, Sicily. This was the largest wave of the last decades crossing the central Mediterranean with about 64,000 detected and further undetected landings (FRONTEX, 2016; INEA, 2014). In particular, in the first half of 2011 about 50,000 migrants landed illegally, compared to an average of about 4,000 persons in the same period in the two prior years. This labor supply shock can be considered as exogenous because neither Arab Spring migrants nor southern Italian coasts self-selected into treatment, and labor markets of origin- and host-countries are not interdependent. Indeed, the migrant wave was so unexpected that Italy declared the state of emergency already in February 2011. Specifically, Italy lacked resources for continuous coastal monitoring as well as migrants' first aid, identification, and detention (Gola, 2015).

In our analysis, the "treatment" is the migration-induced supply shock of illegal labor, and the "treated" regions are Sicily and Apulia[4] which are the only regions where large numbers of incoming, undetected migrants may have supplied illegal labor to local farmers – in our case, to grape growers.[5] As a control group we use Italian and French regions where migrant landings, namely, irregular migrant shocks, do not take place. We find it plausible that irregularly landed migrants may travel to control regions, though not in masses. In fact, travelling is difficult due to the lack of documents and the risk of immediate expulsion if caught. Therefore, we assume that an irregular migrant shock occurs only in the landing regions, and not in other Italian and French regions.[6]

The illegal labor supply shock is expected to be absorbed by the grape growing sectors of Sicily and Apulia due to the large demand for seasonal, cheap labor as well as sizable informal agricultural labor markets. The demand side is characterized by a large number

[4]Calabria, a region located between Sicily and Apulia that likely received a large number of migrants as well, is excluded due to missing data, but it also has only a very small grape sector.

[5]Background information on illegal labor channels for undetected migrants in the treated regions is provided in Section 1.1.

[6]We cannot extend our sample to regions of other European countries because they are either recipients of large migrant waves, or have very different viticultural traditions, grape sectors, climatic conditions, and regulations of agricultural labor markets (Section 2). Sensitivity of our results is tested against different choices of the control group (Section 4).

of on average very small farms (80% with less than 5 hectares) that are, however, worldwide leaders in table grape production, and also produce large quantities of wine, mostly low-quality (INEA, 2012). Since the 2000s, increasing international competition, shrinking demand globally for wine, climatic shocks, and failed agricultural policies leading to deregulation of the EU wine sector (CMO, 2008) required grape growers - and in particular, (small) less favored vineyards - to reduce factor costs (Gaeta & Corsinovi, 2014). One option is to rely on illegal and migrant workforce. In particular, in Sicily (Apulia), there are about 114,000 (110,000) agricultural workers of which about 32,000 (26,000) foreigners (non-EU 15) employed mostly in tree crops (over 30%) with seasonal contracts (98%), fully legal for only 30%, and with 90% of the wages below the minimum admitted by law (INEA, 2012).

Farm migrant labor in Sicily and Apulia is often absorbed through *caporalato*, an increasingly widespread informal system to recruit and exploit underpaid workforce through intermediaries (Flai-Cgil, 2016).[7] There is considerable anecdotal evidence that shows the link between migration waves and illegal farm work: For example, in August 2011, the first self-organized revolt of farm pickers took place, leading to investigations that revealed illegal migrant trafficking between Apulia, Calabria, and Sicily (Spagnolo, 2017). However, there is little empirical evidence on the amount of illegal labor in agriculture and its effects on the agricultural labor markets, especially in the European Union (EU). This paper contributes to filling the gap.[8]

For estimation, we use farm-level data aggregated at the regional level for Italy and France between 1999 and 2012 supplied by the Farm Accountancy Data Network (EC, 2017). We analyze causal effects using a Difference-in-Differences (DiD) framework in a dynamic linear panel regression model. Thereby, using a lagged dependent variable as an exogenous predictor accounts for the persistence that characterizes vineyard labor productivity while simultaneously controlling for time-varying effects of unobserved heterogeneity. Various tests support this model, and indicate that fixed effect approaches are likely not suited to account for the underlying dynamics in the data. As a robustness check against the residual presence of unobserved fixed (time-invariant) effects in the dynamic model specification, we implement an Anderson-Hsiao type regression (Anderson & Hsiao, 1981). To further validate our results under different choices of the control group, we use

[7]Over 400,000 irregular workers, most often undocumented migrants, are estimated to be part of such system, for a 1.8 billion euro loss to national revenues each year (Flai-Cgil, 2018).

[8]More about the existing literature on the identification of illegal labor, and its effect on legal employment is discussed in Section 1.

k nearest neighbor matching to identify the best control matches for each treated region, and we re-estimate the DiD model for different values of k. Finally, we check for model misspecification and allow for more flexible functional forms, and we estimate the causal effect on the selected model using the post-lasso estimator as introduced by Belloni *et al.* (2012, 2013).

Our results point to an increase of illegal employment on vineyards after the 2011 migrant wave. Indeed, we find that this labor supply shock has a statistically significant average causal effect on labor productivity on Siciliy's and Apulia's vineyards, increasing by around 11% on average over the post-treatment period (2011 and 2012). Under further assumptions, we show that this effect corresponds to around 10 million unreported hours in the treated regions in each year – or around 5,500 unreported agricultural work units (each defined as working 1800 hours a year; EC, 2017). In other terms, the migration-induced supply shock caused the average farm to illegally employ about one full-time grape picker for its harvest (about 30 days for one grape variety). As mentioned before, these workers can be undocumented migrants from the 2011 migration wave, and/or other workers available to work illegally, including former legally employed workers. Further, due to high labor intensity and low mechanization of the grape growing sector of the treated regions, a substitution effect of capital with illegal labor is rather unlikely. Given the absence of any technological shock in grape harvesting, price shock of the harvested grapes, or additional labor market shocks in the analyzed period, our findings are likely due to the displacement of legal with illegal workforce caused by the 2011 inflow of migrants.

It should be noted that our estimates of the treatment effect on reported labor productivity is only a lower bound of the actual treatment effect. The reason for this is that reported labor productivity is already upward biased before the treatment due to the employment of illegal labor. Thus, also estimates of the displacement of legal workforce is only a lower bound. Moreover, the treatment could have also caused displacement of former illegal labor with incoming illegal labor which cannot be captured by our identification strategy.

The remainder of the paper is organized as follows: Section 1 provides further background information about this study and summarizes related literature. Section 2 outlines our empirical strategy. The data are described in Section 3. Section 4 presents the results, and Section 5 concludes.

1 Background and Literature

1.1 Farm labor and the 2011 migrant wave

The Italian Association for Juridical Studies on Immigration (ASGI, 2015) reports numerous violations of the EU Directive on illegal immigration and illegal employment of migrants without the required legal status in the EU (2009/52/EC). In Italy, estimates report over 400,000 illegal farm workers of which around 80% are migrants, often undocumented (Assosomm, 2016; Flai-Cgil, 2014). Indeed, about 30% of total agricultural employment in Italy, and up to 70% in Sicily and Apulia at local level, is illegal (Flai-Cgil, 2012). These estimates are in strong contrast to the only 5,200 agricultural labor contracts found to be illegal by the Labor Inspectorate in 2018 (Flai-Cgil, 2018). In addition, illegal workers in agriculture seem to be increasing over time, with up to 50,000 more from 2016 to 2018, producing an overall value of 4.8 billion euros and causing losses to national revenues for around 1.8 billion euros (Flai-Cgil, 2018).[9] Often, illegal migrant labor is exploited with wages below the legal minimum thresholds of around 850 euros per month (about 5 euros per hour), averaging 40% lower than domestic wages (ETI, 2015). In fact, studies report wages between 1.60 and 3 euros per hour over a 12 to 16-hour working day, with which workers have to pay intermediaries for transportation, food, and accommodation (Palmisano & Sagnet, 2016).

The 2011 migrant wave might have further contributed to increase illegal employment in agriculture. In the following, we outline (I) why and (II) how Arab Spring migrants avoid or leave the legal asylum framework, and (III) provide evidence that only Sicily and Apulia are the recipients of the illegal labor supply shock.

(I) Landed migrants want to avoid expulsion and, most often, reach other EU destinations. However, high risks of asylum rejection which implies detention and expulsion, or even immediate repatriation are strong incentives to avoid or leave first aid and reception centers in the landing regions. In 2011, the risk of asylum rejection was very high for most migrants because local administrations considered many countries of origin to be safe (CeSPI, 2012).[10] Also, an immediate repatriation agreement was signed with Tunisia in April 2011(Il Post, 2011).[11] Lastly, migrants' living conditions in overcrowded tempo-

[9]Compared to the overall value of the informal economy (200 billion euros), the value of unreported labor amounts to 77 billion euros (4.8% of GDP), and comes from about 3 million workers (ISTAT, 2017).
[10]Indeed, 65% of political asylum requests were rejected, though, most verdicts were appealed (MPP, 2012; Sasso & Sironi, 2012).
[11]Of the 19,000 Tunisians held in temporary camps for expulsion, only about 4,000 and 2,400 were actually expelled in 2011 and 2012, respectively (ISMU, 2015). This led to revolts and further fleeings.

rary facilities were usually very bad, and law infringements including violations of human rights were often reported (e.g., in ASGI, 2011). Thus, migrants have strong incentives to find alternative ways to make money and to obtain (forged) documents in the landing regions. One common way is to use the intermediation of gangmasters, called *caporali*, who negotiate with farmers and supply workers (Spagnolo, 2017; Flai-Cgil, 2012).

(II) Once landed, migrants can enter illegal labor channels in several ways. Many migrants land on Italian shores undetected (INEA, 2014). Detected migrants obtain first aid in emergency shelters called hotspots. In 2011, of the approximately 64,000 detected landings on southern Italian shores, only around 55,000 arrivals are recorded at the hotspots (SPRAR, 2011). Further 11,700 migrants seem to have disappeared in the initial months of 2011 (Polchi, 2011). Yet, without documents and money, migrants are most often not able to travel to their destination country immediately. Among the landed migrants, those not applying for asylum are sent to detention and expulsion centers. Instead, asylum seekers are hosted in reception camps called Centers for First Assistance (abbreviated as CDA), and Centers for Assistance of Asylum Seekers (CARA). In these camps, authorities decide whether immigrants should be expelled. Migrants stay in such facilities also for years, and, despite surveillance, many cases of migrant fleeing camps, temporarily or permanently, have been reported (Gatti, 2015).

(III) As a consequence of the 2011 migration wave, the number of migrants hosted in CDA and CARA tripled: From around 12,000 and 11,000 in 2009 and 2010, respectively, to around 32,000 in 2011 (SPRAR, 2012; ASGI, 2012). 95% of migrants in CDA and CARA are hosted in Sicily, Apulia, and Calabria (SPRAR, 2012). In particular, in 2011, Sicily's and Apulia's centers hosted around 10,800 and 13,800 migrants, respectively, each hosting around 10,000 migrants more than in 2010, and with a 15 and 10 percentage point increase of the share of hosted migrants, respectively. On the contrary, centers in other Italian regions, specifically, Lazio, Marche, and Friuli Venezia Giulia on average reduced both the number and the share of hosted migrants from around 2,100 in 2010 to 1,600 in 2011, and from around 18% in 2010 to 5% in 2011. Therefore, we may expect that, despite surveillance, only migrants hosted in CARAs and CDAs of Sicily, Apulia, and Calabria may leave camps in large numbers, and work in the informal sector on a regular basis.

Furthermore, a number of asylum seekers, only after completed identification procedures, are relocated in reception centers across the country within the System for Protection of Asylum Seekers and Refugees (SPRAR). Numbers reveal that Italian regions did not experience abnormal arrivals or departures of asylum seekers in their SPRAR centers over the years – which may have caused shocks to the local formal and/or informal agricultural labor market (see Table 7 in the Appendix). Moreover, with funds available for emergency management, the Italian government provided about 22,000 extra places in hotels and apartments to host asylum seekers, which were distributed in each region according to its population (DPC, 2017).[12] As a result, each region received a relatively low number of migrants (around 1,000 on average). Regional numbers as of September 2012 as well as the share of the 17,859 people hosted in each region are reported in Table 1.[13]

Table 1: Number of Arab Spring asylum seekers and refugees relocated across Italian regions in apartments and hotels (source: Sasso & Sironi, 2012).

	n.people	share	pop.*		N.people	Share	Pop.*
North-East				**North-West**			
Friuli V.G.	397	0.02	1.2	Aosta V.	20	0.00	0.1
Veneto	1274	0.07	4.9	Piedmont	1549	0.09	4.4
Trentino A.A.	172	0.01	1.1	Lombardy	2548	0.14	10
Emilia R.	1585	0.09	4.5	Liguria	540	0.03	1.6
Center				**South**			
Tuscany	1141	0.06	3.7	Abruzzo	11	0.00	1.3
Marche	462	0.03	1.5	Molise	116	0.01	0.3
Lazio	1790	0.10	5.9	Campania	2155	0.12	5.8
Umbria	338	0.02	0.9	Basilicata	200	0.01	0.6
Islands				Apulia	1071	0.06	4.1
Sicily	1110	0.06	5.1	Calabria	956	0.05	2.0
Sardinia	424	0.02	1.6	**Total**	17.859		60.5

* total regional population in million people

As in SPRAR facilities, migrants could not take an unjustified daily leave and were not allowed to work.[14] Further, in addition to board and lodging, they also received pocket money and temporary documents to access health care services (ANCI, 2011). Therefore, it does not seem plausible that relocated migrants illegally worked on vineyards in other parts of Italy. In fact, both asylum seekers and refugees had no incentive to put their status at risk by working illegally. Also, most refugees seem to have left Italy after positive asylum decisions (Labanca, 2016). However, as of November 2012, most of the pending and rejected asylum seekers did not leave migrant facilities waiting for asylum decisions and appeals, respectively (Lambruschi, 2012). Thus, the Italian government

[12] I.e., 10,000 migrants for 100,000 inhabitants. Also note that, in 2011, Lombardy's SPRAR hosted less asylum seekers (see Tab. 7), but the highest share of the extra places was assigned to this region.

[13] Note that 22,216 persons were hosted as of November 2011, therefore regional numbers were slightly higher compared to September 2012.

[14] Officially, asylum seekers cannot work for six months from the asylum application, however, practically, work permits are very difficult to obtain also within 12 months from the request (MPP, 2013).

offered an accelerated procedure to regularize asylum applicants through concession of an humanitarian visa (Interior Ministry, 2012). Thereby, from 2013 onwards, rejected asylum seekers, in particular, may have entered illegal labor channels to avoid expulsion (Giangrande, 2017).[15]

1.2 Literature

A vast literature on the impacts of immigration on productivity and labor market outcomes has developed (for overviews see Peri, 2016; Dustmann *et al.*, 2016; Okkerse, 2008). This includes studies on the long-term consequences of immigration as well as on the labor market effects of sudden migration waves. However, the effects on the informal sector have been rarely addressed.

This paper, by proposing a new way to identify unreported labor and quantifying illegal employment causal to an irregular migrant wave, contributes at the intersection of three streams of literature. The first stream aims to identify and quantify irregular migrants already present in the host country, and studies their labor supply (see, e.g., Borjas, 2017; Warren & Passel, 1987; Kelly, 1977). The second evaluates the effects of immigrants working illegally on legal employment in the agricultural sector (see, e.g., Venturini & Villosio, 2008; Vaiou & Hadjimichalis, 1997). Finally, the third analyzes the effects of migrant labor supply shocks on illegal employment and, generally, on the informal labor market using available survey data on informal workers and irregular migrants (see, e.g., Tumen, 2016; Ceritoglu *et al.*, 2015; Del Carpio & Wagner, 2015).

Borjas (2017) represents the latest example within the first stream of literature. In his paper, Borjas extends past methods to identify undocumented migrants at individual level in survey data based on residual calculations and reconstructions of foreign-born persons that cannot be labelled as legal. This allows the analysis of long-term trends of migrant illegal employment, and the comparison with legal migrant as well as native employment trends. Findings show that irregular male migrants participate more in the labor force than natives and legal migrants, a gap found to increase over time. We relate to this literature in scope as we try to "count the uncountable" by estimating the population of illegal workers when an illegal labor supply shock occurs, and by giving an indication of the magnitude of the phenomenon. Also, we contribute by proposing a new way to estimate the relative size of the illegal labor employed when a source of exogenous variation is available but data on incoming as well as incumbent populations are not.

[15]For this reason we do not extend our empirical analysis after 2012.

In the second stream of literature, to the authors' knowledge, Venturini (1999) is the study most closely related to ours. It uses national-level estimates of the number of illegal immigrant workers in Italy to estimate substitution and complement effects with legal workers. Results reveal that the former compete with the latter in the agricultural sector due to the unskilled, homogeneous nature of farm labor, causing legal labor to be displaced. Similarly for Greece, Vaiou & Hadjimichalis (1997) and Lianos *et al.* (1996) find that illegal employment of agricultural labor competes with legal labor, lowers factor costs, and has a dampening effect on wages of legal workers. We contribute to this literature by providing further evidence of the displacement effects of inflows of irregular migrants on legal workers in the agricultural sector for the Arab Spring migrant wave.

Finally, in the third stream of literature, a series of papers analyzes the labor market impacts of the migration waves following the outbreak of the Syrian civil war in 2011. Focusing on Turkey, Ceritoglu *et al.* (2015) estimate negative causal effects on employment of informal native workers using survey data in a DiD framework. Likewise, Del Carpio & Wagner (2015), Tumen (2016), and Balkan & Tumen (2016) conclude that a strong displacement of natives by immigrants occurs especially in the informal sector. This literature concludes that the combination of prevalent informal employment along with a supply shock of undocumented refugees increased the magnitude of the negative effects of the Syrian migrant wave on natives' employment. However, Peri (2016) argues that the estimation of these causal effects could be biased due to potential war spillovers between the neighboring Syria and Turkey not directly related to the migrant influx, preventing potential outcomes caused by forced migration to be disentangled from other labor market adjustments. The question of which type of worker is actually displaced by incoming irregular migrants is very important also in our case, though, it remains open because we lack statistical information to estimate this relation. However, the above conclusions are in line with our findings which suggest that, due to the Arab Spring migrant wave, legal labor was partly displaced by illegal labor on vineyards – a sector strongly characterized by informality, especially in southern Italy.[16]

[16] Another paper closely related to ours estimating the short-term impact of the Arab Spring migrant wave is provided by Labanca (2016). Analyzing selected subgroups of (legal) refugees, and based in particular on Tunisians, results indicate that the migration wave induced natives to shift sectors of employment, specifically from mining, wholesale trade, restaurants and hotels to construction. Interestingly, no effects on the legal agricultural sectors are found.

2 Methodology

2.1 Identification and estimation

We aim at identifying the causal effects of the migration wave on illegal employment by analyzing labor productivity under the assumption that employment of illegal labor leads to underreported labor input and, thus, overreported labor productivity. In the agricultural sector, unskilled labor productivity is fairly constant over time once we control for farm inputs, and production environment, e.g., weather conditions (Jorgenson & Griliches, 1967; Lamouria *et al.*, 1963). This persistence of outcomes may be due to time-invariant, unit-specific unobservables (fixed effects) and due to so-called state dependence. State dependence (Heckman, 1981), i.e., a dynamic outcome process in which past outcomes help predicting future outcomes, can be explained by observed past behavior, but also by time-varying effects of unobserved heterogeneity. If ignored, the latter would cause inconsistent model estimates due to Omitted Variable Bias (OVB).

In particular, in our setting, the estimation of labor productivity would suffer from OVB if regional differences in farmers' propensity to hire illegal labor are not accounted for. This varies among regions and over time, and its determinants are, e.g., organized crime intensity, and farmers' attitudes toward risk and law obedience due to different incentive and monitoring systems at regional level. Further, the propensity to hire illegal labor in a region is likely serially correlated because farmers who have hired illegal labor in the past are more likely to do so also in the future.

To account for these effects, we model labor productivity as a dynamic linear process with limited memory under the assumption of sequential exogeneity. The latter means that the inclusion of a Lagged Dependent Variable (LDV) makes the outcome conditionally independent on past values of observed and unobserved variables. In particular, the inclusion of a LDV controls for state dependence as long as it absorbs time-varying effects of unobserved heterogeneity at regional level. On the contrary, a static model with fixed effects is not able to capture state dependence. While combining both LDV and fixed effects is possible, testing for unobserved time-invariant, unit-specific effects allows to identify the most likely source(s) of this persistence (Breusch & Pagan, 1980; Honda, 1985).

The model to identify the average causal effect of the illegal labor supply shock writes:

$$y_{it} = \rho y_{it-1} + \gamma X_{it} + \delta D_{it} + \mu_i + \varepsilon_{it}, \tag{1}$$

where subscripts i and t indicate units and time, respectively, y_{it} is labor productivity, y_{it-1} is the (observed) LDV capturing (unobserved) time-varying, unit-specific effects on y_{it} through the parameter ρ,[17] X_{it} are other exogenous regressors, μ_i are (unobserved) fixed effects, ε_{it} error terms, and D_{it} is a treatment dummy. Following the potential outcome approach by Rubin (1974), the DiD estimator $\hat{\delta}$ estimates the Average Treatment effect on the Treated (ATT) as the difference of two differences: the average outcome in the treatment group, i.e., Sicily and Apulia, before and after shock, and the average outcome in the control group before and after shock. In particular, the ATT is the state-dependent causal effect of the migration-induced labor supply shock on labor productivity. This is a conditional DiD estimator (Fitzenberger *et al.*, 2009) where the treatment effect is conditional on the reported labor productivity in the previous period $t-1$, including the amount of misreporting already in $t-1$. Thus, by conditioning on the reported labor productivity in the previous year, we account for the short-run dynamics in labor misreporting.

If outcome persistence is mainly due to state dependence, the model includes only the dynamic component, and μ_i drops. Excluding μ_i, the dynamic model (1) can be estimated by pooled OLS (DPOLS) under the assumption of exogeneous regressors and sequential exogeneity. If the LDV absorbs residuals' autocorrelation, parameter estimates are unbiased and consistent.[18]

The identification of δ in (1) is based on two assumptions: conditional independence (CIA), and no externalities of treatment aka stable unit treatment value assumption (SUTVA).[19]

The CIA requires that, conditional on explanatory variables, the assignment of the treatment is as good as random. Indeed, the 2011 migration wave was an exogenous shock as it was unexpected and abnormally large, only hitting the southern Italian coasts due to their geographic vicinity. In particular, migrants escaping from wars did not select southern Italy as their preferred destination due to its large agricultural informal sector. Because neither migrants nor the treated regions self selected into treatment and because regressors are chosen to be exogenous, we consider this assumption as fulfilled. In addi-

[17]Any LDV model assumes two (tested) stability conditions on the autoregressive process, i.e., covariance stationarity ($|\rho| < 1$) and weak dependence of the outcome (Hsiao, 2014).

[18]If, instead, autocorrelation would still be present, the model is likely non-stationary even if $|\rho| < 1$, causing inconsistent and biased estimates (Keele & Kelly, 2006), and, in particular, upward bias for $\hat{\rho}$, and downward bias for the other coefficients (Achen, 2000).

[19]Note that parallel trends assumption in the case of a LDV model is generally not required to hold as there is regression to the mean.

tion, political and economic dynamics of the immigrants' countries of origin do not affect southern Italy as these economies are not interconnected and no spillovers occur.

Lastly, SUTVA states that no spillover effects on the control units take place after the shock. Indeed, no treatment effect is expected outside Sicily and Apulia (and excluding Calabria) for the years 2011 and 2012. In particular, migrants either avoiding or leaving first aid and reception camps are irregular and at risk of expulsion; thus, provided that they find informal employment in the landing regions, they have no incentive to leave, travel undocumented across Italy, and run a higher risk of detention and expulsion. In addition, relocated migrants across Italy do not represent a supply shock to other regional informal sectors. The main reason is that asylum seekers and refugees had no incentive to put their status at risk by working illegally (see related discussion in Section 1.1). However, with the end of the state of emergency on January 2013, the Italian government interrupted the extra financing of asylum seeker and refugee support services, which may have caused rejected asylum seekers to stay irregular and to look for an irregular job (Giangrande, 2017). For this reason we do not extend our analysis after 2012.

In any observational study, the choice of the optimal control group is ex ante difficult. In our case, a panel instead of a pure time series regression allows to distinguish the treatment from a year effect such as general weather conditions. However, control regions should be untreated and resemble the treated regions in the relevant observed characteristics. Thus, we use Italian and French regions as the control group, but we cannot extend our sample to regions of other European countries because they are either recipients of large migrant waves, or have very different viticultural traditions, grape sectors, climatic conditions, and regulations of agricultural labor markets. On the contrary, Italian and French regions are comparable in many respects and are therefore suitable as a control group. Specifically, France and Italy share a border and have similar climatic conditions with warm Mediterranean climate in the south and temperate oceanic climate in the north. Further, both countries are EU member states, grape growers are working under mostly identical regulation, and both countries have a long grape growing tradition. Finally, also on French farmlands, migrant seasonal workers are or become often illegal: Most workers are recruited from abroad via foreign recruitment agencies, and are given a temporary work permit for the harvesting season after which some of them are often offered to work illegally (Flai-Cgil, 2016).[20] Also, farmers face limited risks in terms of sanctions due to

[20]In 2002, the increasing magnitude of this phenomenon and the need to document illegal practices led to the creation of the Association for the Defense of Foreign Seasonal Workers in Agriculture aka CODETRAS (see codetras.org).

lack of labor inspectorate effectively monitoring working conditions and paid wages (Meyer & Dumortier, 2014).[21]

Furthermore, since no treatment is expected to occur before the migrant wave, the assumption of joint significance of treatment dummies before treatment, aka in-time placebos, should be tested, and rejected (Autor, 2003).

2.2 Robustness checks

As a robustness check against the residual presence of unobserved fixed effects, μ_i, we implement an Anderson-Hsiao type regression by first differencing the model and instrumenting the endogeneous first-differenced LDV with lagged outcomes (AH, Anderson & Hsiao, 1981). First differencing may, however, induce new problems: First, if the DPOLS errors in (1) are not serially correlated, this induces autocorrelation in the first-differenced errors, weakening the exogeneity of the chosen lagged outcomes as instruments; second, first differencing may induce outcome cross-sectional dependence that was not present in levels.

In addition, we perform a sensitivity analysis on the choice of the control group. Given that some regions are more likely to be similar to the treated regions in terms of pre-treatment characteristics, we use k nearest neighbor matching to identify for each treated region the k best control matches based on covariate balance. Successively, we re-estimate the DiD model for different values of k.

To check for model misspecification and to allow for more flexible functional forms, we use the post-lasso regression method as introduced by Belloni *et al.* (2012, 2013). With this approach, in a first stage, a model is selected using a lasso-type regression applied to a high-dimensional model. In a second stage, the ATT is estimated using the selected model (post-lasso). For this purpose, we first extend our initial model by other farm-related variables and many technical variables from the chosen covariates, such as interaction terms, log specifications, as well as second- and third-order orthogonal polynomials. We obtain a high-dimensional model with many parameters relative to the sample size that can be estimated under approximate sparsity and, as before, conditional sequential exogeneity. Lasso regularizes the regression by the penalized L1-norm to avoid overfitting, thereby selecting the variables with the best explanatory power (for details see, e.g., Chernozhukov *et al.*, 2013; Chernozhukov *et al.*, 2017). In the second stage,

[21] To address this problem, the 2014 National Action Plan against Human Trafficking reformed and extended the mandate of labor inspectors (NAPHT, 2014).

the selected model is used to estimate the ATT. This post-lasso regression undoes the regularization bias caused by the parameter shrinkage.[22] By doing so, we retain those variables that can increase prediction accuracy while reducing variance. However, model specifications selected with a lasso approach lack theoretical justification, and the economic interpretation of parameter estimates is often not straightforward. For this reason, we use post-lasso as a complementary approach to check if our ATT estimates are robust to alternative model specifications.

3 Data

We use data from the Farm Accountancy Data Network (FADN, EC, 2017) and explanatory variables from the Eurostat labor force survey (LFS, EUROSTAT, 2017). The dataset is a balanced panel over the period 1999-2012. Each cross-section is the average farm of each region on the *NUTS 2* level. The sample is representative due to stratified sampling and weighting. The farms in our sample are vineyards, i.e., farms specialized in grape and wine production.[23] The sample consists of 25 regions of which 14 are located in Italy and 11 in France, adding up to in total 350 observations.[24]

We consider the two southern Italian regions Sicily and Apulia as the treated units, for which the treatment, i.e., the illegal labor supply shock, takes place in 2011, and may have effects in 2012. As Figure 1 highlights, a sudden increase in landings of migrants on the southern Italian coast took place in the spring preceding the grape harvest of that year. Further, as previously discussed, the number of landings is likely underestimated (INEA, 2014), as well as a large number of irregular migrants seem to have avoided or fled migrant shelters, and might have been available as labor force from 2011 onwards.

However, despite data availability for later years, we restrict the analysis to 2011 and 2012. The reasons are twofold. First, the effect of the treatment could be confounded by the additional landings registered in 2013 (see Figure 1 and FRONTEX, 2016). Second, potential spillover effects from 2013 onwards may violate the assumption on the untreated status of the control regions (for a discussion, see Section 2.1).

[22]Post-lasso is shown to perform at least as well as lasso in terms of the rate of convergence, and has the advantage of a smaller regularization bias (Belloni & Chernozhukov, 2013).

[23]Additional farm income from other agricultural activities plays a minor role amounting to only 3.5% on average, with very low variation across regions and time.

[24]This selection excludes non-grape growing regions. For six regions single missing data points until 2003 are imputed using Multivariate Imputation by Chained Equations (MICE, see Buuren & Groothuis-Oudshoorn, 2011). Few extreme observations are also handled with transformation.

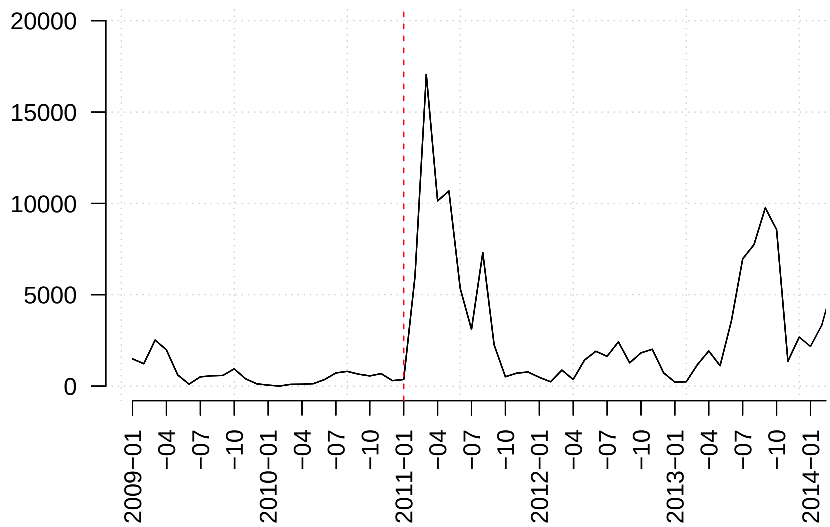

Figure 1: Detected illegal border crossings on the Central Mediterranean route (own illustration, source: FRONTEX, 2016)

To analyze labor productivity, we define our logged dependent variable *LabProd* as the total output from crops in euros divided by total hours worked, which is the sum of all paid and unpaid hours worked and includes family labor. While this measure controls effectively for seasonal/unseasonal jobs and working regimes, it should be noted that it may vary due to quantity and price variations, with the latter being the main source of concern. However, we are confident in ruling out such effects for several reasons. First, price effects could be sizable only for high-quality wines, i.e., those with Protected Designation of Origin (PDO). In 2011, in Sicily (Apulia) PDOs account for only 4% (15%) of the regional wine production, and for only 1% (6%) of total Italian PDO production (Baccaglio, 2016). Thus, price fluctuations can have only limited impact. Second, no sizable price shocks among Sicilian and Apulian PDOs have been registered (ISMEA, 2018). Third, average PDO wine prices follow similar trends in all regions of Italy and France,[25] decreasing in 2009 due to a lower international demand, and evolving fairly stable until 2012 when they increase due to scarce harvests (FranceAgriMer, 2014; ISMEA, 2018, and see Figure 3 in the Appendix).[26]

[25] The assumption of parallel trends across regions is not rejected (F-test p-value = 0.7).

[26] In fact, in 2012, both France and Italy had similar growing conditions with wet spring and hot summer (WineSpectator, 2012). Extreme cases are 2012 hail storms in Burgundy and Beaujolais that caused particularly low yields and high prices, and Veneto's Prosecco boom between 2009 and 2012.

Our explanatory variables are chosen to be exogenous, and contain measures of capital, land, labor, as well as other farm and labor market characteristics. Regarding capital, we include capital intensity in terms of book values of machinery over total vineyard hectares (in logs, *lnMachinery*) as well as the lagged investment rate computed as gross investment over total fixed assets (*invRate*). In fact, the degree of capital intensity determines both workers' day-long fatigue level and the efficiency of grower management practices, two main drivers of labor productivity in grape harvesting and grapevine pruning (Lamouria *et al.*, 1963). Further, these variables control for the more intense use of grape harvesting machines in some regions. Land input is included as total vineyard hectares (in logs, *lnLand*) to capture potential returns to scale. To account for different degrees of vineyard specialization at regional level, we use the share of vineyard hectares over total utilized agricultural area (*vineSpec*). Furthermore, we include two variables to control for potential competition on the labor market: the deviation of the regional unemployment rate from its long-term mean (*unempRate*), and the share of population above the age of 15 with less than primary or secondary education (ISCED11) (*unskilledLab*) as an indicator of unskilled workforce available for the harvesting. To account for weather effects that may impact both potential output and harvested output through workers' fatigue, we include the average of the daily minimum temperatures measured at the major regional airports (*minTemp*).[27] Finally, three additional control variables are included: A fixed effect for France to account for unobserved systematic differences in labor productivity at country-level (*france*), a linear time trend (*trend*), and a time dummy to absorb the effects on both vineyard output and hours worked of 2002 anomalous weather events including heavy precipitations that destroyed considerable shares of the harvest in both Italy and France ($weather_{2002}$). The treatment dummy, D_{it}, used to estimate the treatment effect, δ, is the interaction term of $treated \times post$, where *treated* is a unit dummy equal to one for Sicily and Apulia, and *post* is a time dummy equal to one for the post-treatment periods.

Table 2 reports data descriptive statistics separately for the treated and control units. In particular, the table shows that variable values for Sicily and Apulia are within the range of the control regions, and mostly around the sample mean. This can be also visualized from the boxplots in the Appendix (Figure 4) displaying the distribution of data. The figure shows overlap of support for individual variable values for Sicily and Apulia, which is especially important when estimating the average treatment effect for the two regions.[28] In particular, the dependent variable for the treated units lies between the first and the

[27]This data is scraped from www.wunderground.com.

[28]Only the maximum value of *minTemp* for Sicily exceeds the range slightly.

second quartile of the sample.

Table 2: Descriptive Statistics

	Sicily & Apulia				Control units			
	Mean	Min	Max	SD	Mean	Min	Max	SD
LabProd	2.441	2.170	2.840	0.179	3.089	1.574	4.395	0.599
lnLand	1.534	1.092	1.887	0.195	2.035	0.350	3.301	0.751
vineSpec	0.671	0.472	0.821	0.099	0.640	0.260	0.961	0.153
lnMachinery	8.027	7.396	8.530	0.366	8.372	6.326	9.539	0.660
minTemp	10.596	8.333	13.143	1.456	6.134	1	12	2.552
invRate	0.010	0.000	0.075	0.017	0.068	−0.044	0.255	0.064
unempRate	0	−0.046	0.068	0.032	0	−0.059	0.063	0.018
unskilledLab	0.200	0.160	0.230	0.023	0.171	0.086	0.270	0.044

4 Results

We estimate the causal effects of the 2011 migration wave on vineyard labor productivity using a dynamic regression model as outlined in Section 2. After assessing the validity of our dynamic model specification, we estimate the DPOLS model in equation 1 with and without common time effects. Results from these models show that the 2011 migration wave led to a statistically significant increase in labor productivity of, on average, 11% in 2011 and 2012. Further, robustness checks by the Anderson-Hsiao type regression, and the post-lasso approach confirm the results outlined above.[29]

Table 3 reports the results of the DPOLS model. Models M1, M2 refer to DPOLS without time effects, with time effects (unit-demeaned), respectively. The DPOLS models with/without time effects show that the average causal effect estimate over 2011-2012, i.e., the ATT calculated as $\exp(\hat{\delta}) - 1$, ranges between 10.7% and 11.7%, and it is statistically significant at 1%. Considering the average causal estimate from these two models as its lower- and upper-bound, respectively, this means that the 2011 migration wave led to an abnormal average increase in *LabProd* of about 11.2%.[30]

The other explanatory variables show the expected signs. *lnLand* and *lnMachinery* indicate that more land and capital assets increase *LabProd*, pointing toward economies of

[29]For the analysis, we use the software R, and, particularly, the packages plm (Croissant & Millo, 2008), MatchIt (Ho *et al.*, 2011), and hdm (Chernozhukov *et al.*, 2016).

[30]Static fixed effect and simple pooling regressions estimate relatively larger δ of 0.256 and 0.227 with standard errors equal to 0.116 and 0.132 (5% and 10% statistical significance), respectively. This is expected as fixed effects and LDV models bracket the treatment effect between an upper and a lower bound, respectively (Angrist & Pischke, 2010).

Table 3: DPOLS model without time effects (M1), with time effects (M2)

	(M1)		(M2)	
ρ	0.714***	(0.040)	0.723***	(0.041)
lnLand	0.149***	(0.030)	0.136***	(0.029)
vineSpec	0.018	(0.064)	−0.011	(0.059)
lnMachinery	0.119***	(0.028)	0.115***	(0.027)
minTemp	−0.008	(0.005)	−0.006	(0.005)
invRate	−0.191	(0.227)	−0.226	(0.237)
unempRate	−0.924*	(0.546)	−0.278	(0.663)
unskilledLab	1.508**	(0.604)	1.242**	(0.604)
france	0.278***	(0.072)	0.272***	(0.072)
$weath_{2002}$	0.133***	(0.036)		
trend	0.009**	(0.004)		
treated	0.017	(0.037)	0.008	(0.036)
post	0.074**	(0.037)		
δ	0.102**	(0.040)	0.111***	(0.038)
constant	−0.916***	(0.342)		
Obs.	350		350	
Time effects	No		Yes	
F Statistic	450.2***		554.5***	
Adjusted R^2	0.918		0.914	
Notes:	***p=.01; **p=.05; *p=.1			

scale. On the contrary, larger deviations from the long-term unemployment rate, *unemp*, are negatively related to *LabProd*. Given the systematic higher unemployment rate in the south of both France and Italy, as well as the relatively low variance of such variable over time, this partial effect may simply reflect the north-south gap in labor productivities. A higher availability of unskilled labor positively correlates with *Labprod*, likely indicating the higher labor productivity of more developed regional low-skilled labor markets. Lastly, *france* indicates that *Labprod* is on average higher in France than in Italy. The parameter for the 2002 extreme weather events also has the expected sign and positively affects *Labprod*, mainly due to the drop in labor force needed on vineyards in that year. Lastly, the autoregressive parameter ρ is statistically significant and amounts to about 0.7, confirming the presence of a well-behaved autoregressive process with a relatively high degree of state dependence.[31]

To provide evidence supporting the validity of the models and results outlined above, we conduct a series of tests. First, we test a static fixed effect (within) model for the

[31] The key stability conditions of the autoregressive outcome process are initially tested, and are fulfilled, i.e., the outcome covariance structure and unit-root tests indicate weak dependence and covariance stationarity, respectively (Choi, 2001; Hadri, 2000).

residual presence of unobserved unit-specific effects by means of LM tests (Breusch & Pagan, 1980; Honda, 1985). Results indicate a significant, large residual variance across units (all p-values < 0.01), i.e., the presence of leftover unobserved unit-specific effects also after time-demeaning the model. Moreover, tests show that serial correlation of residuals is present (Breusch, 1978; Godfrey, 1978). Second, we perform the same tests on the dynamic model (equation 1). In this case, the lagged dependent variable absorbs these unobserved unit-specific effects (p-values > 0.1) and residuals' first-order serial correlation. Thus, parameter estimates of the DPOLS model are consistent. Moreover, no cross-sectional dependence is detected (p-values > 0.06, Pesaran, 2004), and Newey-West corrected standard errors (Newey & West, 1994) are used as a safeguard also with rejected serial correlation (as suggested by Wooldridge, 2013, ch. 12.5).

We also test our results for robustness against different choices of the control group. Based on covariate balance, we select the k best control matches (nearest neighbors) for each treated region, and we re-estimate the DiD model for different values of k. We find that the DiD parameter is generally of similar magnitude and statistically significant (see Table 8 in the Appendix).[32]

Next, we transform our estimates on labor productivity into estimates of the unreported (illegal) hours worked at Sicily's and Apulia's vineyards. To do so, we perform simple back of the envelope calculations in the following way. Total production $Output$ is a function of labor productivity $LabProd$ and labor input L such that $Output = LabProd^{l} * L^{l} + LabProd^{il} * L^{il}$ with superscripts l and il denoting legal and illegal labor input, respectively. Solving for L^{il} delivers the illegal input as a function of the observed values of output and legal labor input, while the values of true labor productivity of legal and illegal input are not observed. An estimate of the true labor productivity of legal input is calculated from the reported labor productivity $LP^{reported}$ using the estimated average treatment effect: $Lab\hat{P}rod^{l} = e^{-\hat{\delta}} * LabProd^{reported}$. Thereby, we assume homogenous treatment effects for the treated regions. Further, we parameterize labor productivity of illegal input as a function of legal inputs as $LabProd^{il} = \theta LabProd^{l}$. This delivers:

$$\hat{L^{il}} = \frac{L^{l} \times (LabProd^{reported} - Lab\hat{P}rod^{l})}{\theta Lab\hat{P}rod^{l}} = \frac{L^{l}(LabProd^{reported}[1 - e^{-\hat{\delta}}])}{\theta e^{-\hat{\delta}} * LabProd^{reported}} \tag{2}$$

The rationale behind this parameterization is that the productivity of illegal labor might differ from that of legal labor, e.g., due to fatigue from long working hours (Palmisano

[32]Full results for in-time placebos, further dynamic and static models are available upon request.

& Sagnet, 2016; Lamouria *et al.*, 1963). Therefore, θ relates the two labor productivities such that illegal labor and legal labor are identically productive for $\theta = 1$, and for example $\theta = 1.2$ (0.8) indicates 20% higher (lower) labor productivity for illegal labor. Further, not only is the relationship between $\hat{L^{il}}$ and θ non-linear, but $\hat{L^{il}}$ decreases faster for low productivity levels than for higher levels ($\frac{\partial^2 \hat{L^{il}}}{\partial \theta^2} > 0$).

Table 4 reports the estimates of unreported work hours for the average farm, the two different model specifications, and for different values of θ arbitrarily chosen to vary between 0.8 and 1.2. Estimates are calculated under the assumption of homogeneous treatment effects, i.e., δ is constant across the treated units. Estimates slightly vary between years and regions. We find generally slightly higher values for Apulia than for Sicily, although differences are small. For identical productivity of legal and illegal workforce ($\theta = 1$), unreported labor input is estimated to vary between 205 and 278 hours. These estimates correspond to 11% (205h/1800h) and 15% (278h/1800h) of an annual work unit (AWU) – which is the agricultural equivalent of a full-time employee as defined by the EU (EC, 2017) – or one person working around 5 to 7 40-hour weeks.[33] This corresponds to the length of the harvest for one grape variety (about 30 days), thus about one full-time grape picker might not be reported by the average farm during this time.

While these estimates seem to be rather low, it should be noted that the sector is characterized by a strong fragmentation with a large number of fairly small vineyards. To estimate the overall effect, we transform our estimates at a regional level by multiplication with the number of farms within a region. Table 4 summarizes the results. For $\theta = 1$, total unreported labor input ranges between 9 and 10.3 million hours in 2011, and 10 and 12.2 million hours in 2012. Annual estimates range between 12.2 and 13.6 million hours for $\theta = 0.8$, and between 8 and 9 million hours for $\theta = 1.2$. In terms of AWU (assumed to work 1800 hours a year), 10 million unreported hours correspond to around 5,500 illegally employed AWUs, or 5,500 full-time employees.

However, this calculation is sensitive to various factors: First, hours worked per day might be considerably higher. Second, estimates vary strongly with θ. Thirdly, the estimated average treatment effect, $\hat{\delta}$, identifies only the increase in labor productivity due the migration wave. If labor productivity is already overestimated before this shock due to unreported labor, the estimated unreported hours are only a lower bound of the actual numbers, which is likely the case due to the historical presence of illegal employment of labor (Flai-Cgil, 2016).

[33] However, for illegal labor input, and in particular for labor input employed through caporalato, working conditions are usually tougher, and 40-hour weeks are likely an underestimate (INEA, 2012).

Table 4: Estimated unreported hours worked for the average farm by region (and total per region in thousand)

	$\theta = 0.8$	$\theta = 1$	$\theta = 1.2$
M1 ($\hat{\delta} = 0.102$)			
Sicily 2011	268 $(5,825)$	214 $(4,660)$	178 $(3,884)$
Apulia 2011	286 $(6,064)$	229 $(4,851)$	190 $(4,043)$
Sicily 2012	249 $(6,490)$	199 $(5,192)$	166 $(4,327)$
Apulia 2012	285 $(6,061)$	228 $(4,848)$	190 $(4,040)$
M2 ($\hat{\delta} = 0.111$)			
Sicily 2011	293 $(6,368)$	234 $(5,095)$	195 $(4,246)$
Apulia 2011	312 $(6,629)$	250 $(5,304)$	208 $(4,420)$
Sicily 2012	272 $(7,095)$	217 $(5,676)$	181 $(4,730)$
Apulia 2012	312 $(6,626)$	250 $(5,300)$	208 $(4,417)$

4.1 Robustness checks

4.1.1 In-time placebo DiD

Since treatment effects on the outcomes of the treated regions before the migrant wave are not expected, thus, should not occur, we introduce placebo treatment dummies for the five years before the shock. The left graph of Figure 2 displays the evolution of the placebo treatment effects over time (x-axis), and the respective parameter estimates as well as confidence intervals (y-axis). For 2011 and 2012, the estimated average causal effects result to be the largest in magnitude and with the smallest confidence levels. Joint (as well as individual) statistical insignificance is not rejected with a p-value = 0.99. Instead, yearly treatment effects for 2011 and 2012 are statistically significant at 5%. On the right, instead, parameter estimates (x-axis) are plotted against their respective standard errors (y-axis), and the shaded area indicates that there are no causal effects more extreme than those for 2011 and 2012.

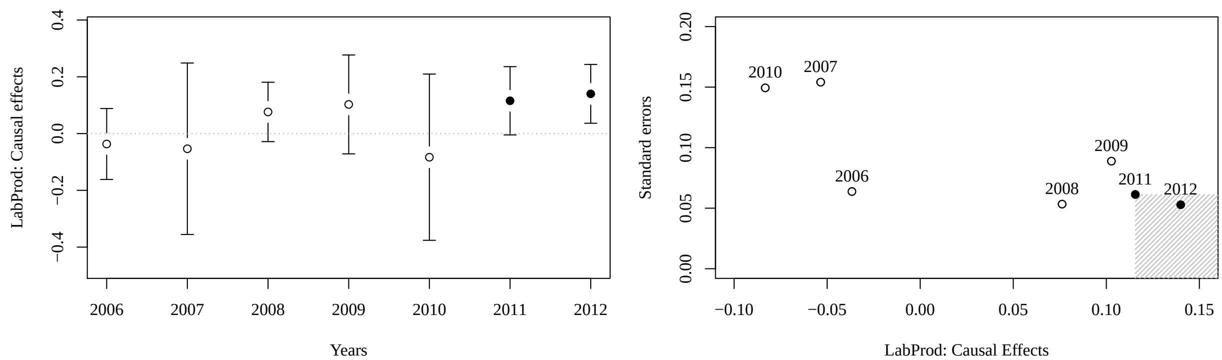

Figure 2: DiD in-time placebos and dynamic causal effects

4.1.2 Anderson-Hsiao type regression

To further check for the robustness of our results against unobserved fixed effects, μ_i, we use the AH estimator on the first-differenced dynamic model (Anderson & Hsiao, 1981). First differencing swaps away unit-specific fixed effects; however, it induces correlation between the first-differenced LDV and error term, thus, endogeneity. We choose the second and third lagged outcomes to instrument the first-differenced lagged outcome, both in levels and in first differences. Table 5 reports the results of the two-stage least square estimation using first-differenced instruments (column 1 and 2) and level instruments (column 3 and 4), respectively, with and without time effects. All four models show a positive causal effect of the illegal labor supply shock on measured labor productivity. Parameter estimates of these causal effects are generally of higher magnitude than those obtained with the DPOLS specification (cp. Table 3), ranging between 15.4 and 17.9%. Despite the overall lower efficiency of the AH estimator, these estimates are statistically significant at the 1% level. The instruments are relevant according to robust F-tests on the first-stage regressions (p-values < 0.01). Further, auxiliary-regression based Sargan tests with heteroskedasticity and autocorrelation corrected residuals indicate that the instruments are exogenous, i.e., uncorrelated with the AH model's residuals (p-value > 0.3). However, it should be noted that first differencing the model induces serially correlated residuals, as well as unobserved unit-specific heterogeneity with a large residual variance across regions (p-values < 0.005).[34] For this reason, both the exogeneity of the first-differenced lagged instruments and the model specification may be problematic. Therefore, we consider robustness of AH model estimates only as an indicator of robustness against alternative

[34]In this case, we follow Driscoll & Kraay (1994) and use standard errors corrected for both potential autocorrelation and cross-sectional dependence in the residuals.

model specifications.

Table 5: AH model estimates with different IV strategies and with/without time effects

	First-difference IVs				Level IVs			
	(AH1)		(AH2)		(AH3)		(AH4)	
ρ	0.217	(0.189)	0.034	(0.132)	0.317	(0.270)	0.146	(0.154)
lnLand	0.535***	(0.039)	0.517***	(0.048)	0.533***	(0.038)	0.517***	(0.046)
vineSpec	0.116	(0.241)	0.003	(0.275)	0.138	(0.246)	0.017	(0.300)
lnMachinery	0.065*	(0.039)	0.064*	(0.036)	0.057	(0.043)	0.059	(0.037)
minTemp	0.004	(0.010)	0.013**	(0.006)	0.003	(0.011)	0.013**	(0.007)
invRate	0.249	(0.223)	0.200	(0.201)	0.217	(0.242)	0.171	(0.218)
unempRate	−0.336	(1.297)	−0.300	(2.041)	−0.319	(1.295)	−0.257	(2.117)
unskilledLab	−0.008	(0.845)	0.116	(0.995)	0.048	(0.915)	0.120	(1.075)
post	0.026*	(0.014)			0.025*	(0.015)		
$weather_{2002}$	0.134***	(0.017)			0.127***	(0.023)		
δ	0.151***	(0.039)	0.143***	(0.029)	0.165***	(0.045)	0.161***	(0.032)
Obs.	325		325		325		325	
Time effects	No		Yes		No		Yes	
F Statistic	9.199***		5.394***		8.346***		4.882***	
Adjusted R2	0.151		0.190		0.122		0.099	
Notes:	***p=.01; **p=.05; *p=.1							

4.1.3 Post-lasso approach

To check for possible model misspecification and to allow for a more flexible functional form, we enlarge the set of covariates with additional farm-related characteristics, as well as high-dimensional variables, in particular, interaction terms, log specifications, and second and third order orthogonal polynomials. We perform, first, model selection by lasso and, then, post-selection DiD estimation of the ATT as proposed by Belloni *et al.* (2012, 2013). Main results from the post-lasso regression are the following: (I) The model specification is dynamic (ρ is not shrunk to zero); (II) rigorous lasso selects 17 variables, including some of our original covariates, some of their interaction terms, and some newly included farm-related variables; (III) after model selection, post-lasso DPOLS estimation reports plausible signs, and additionally selects the rent paid for farm land and buildings and rental charges (in logs); and (IV) the treatment dummy is highly statistically significant (p-value = 0.015), amounting to 9.3% (for details, see Table 9 in the Appendix).

Table 6 presents a comparison of the ATT estimates obtained by the previous methods, and the post-lasso approach. Although smaller in magnitude, the ATT computed by post-lasso confirms the presence of a statistically significant average causal effect of the illegal labor supply shock on vineyard labor productivity in Sicily and Apulia, indicating the presence of illegal labor employment.

Table 6: Comparison of ATT estimates

Model	Estimate	Std. Error
DPOLS: M1	0.102	0.040
DPOLS: M2	0.111	0.038
AH2 (lower bound)	0.143	0.029
AH3 (upper bound)	0.165	0.045
Post-lasso	0.093	0.037

5 Conclusions

This paper aims to identify and quantify illegal employment causal to an irregular migrant wave, and shows (I) a new way to identify unreported labor, and (II) a lower-bound estimate of the amount of unreported labor caused by the migration-induced supply shock. To identify changes in illegal employment we use the exogenous variation of the 2011 Arab Spring migrant landings on southern Italian shores, and we consider labor productivity on vineyards in the landing regions. Identification relies on abnormal increases in reported labor productivity coming from underreported labor hours. Based on farm-level data aggregated at regional level and using a dynamic panel model, labor productivity is estimated to abnormally increase by about 11% on average for 2011 and 2012 on vineyards in the landing regions. We show that this effect corresponds to around 10 million hours irregularly worked in the treated regions in each year – or around 5,500 agricultural work units. These workers can be undocumented migrants from the 2011 migration wave, and/or other workers available to work illegally, including former legally employed workers. Thereby, our results suggest that illegal workforce displaced legal workforce, leading to underreported labor input and overreported labor productivity.

These results are in line with the literature which finds low-skilled jobs (see, e.g., Dustmann *et al.*, 2016; Peri, 2016) and informal native employment (see, e.g., Tumen, 2016; Del Carpio & Wagner, 2015) the most vulnerable to migrant labor supply shocks. Indeed, this is the case for vineyard labor: The seasonal nature and the low skill requirements of field picker jobs limit workers' bargaining power and makes them substitutable.

Our results also underline several flaws in existing policies. First, the lack of regulation and inspection on farmlands prevents the effectiveness of existing laws against caporalato and illegal employment because employers face very low probabilities of being caught. Second, in this respect, European laws that assign temporary residence permits to irregular migrants who denounce severe exploitation - partially introduced in Italy only in 2012 - could be fully applied to guarantee workers' protection, and limit employers' exploitment incentives. Third, more efficient evaluations of asylum requests would avoid lengthy, complicated, and often unclear procedures that encourage migrants to stay irregular. Finally, future research should analyze alternatives to the existing voucher systems and other type of contracts designed by European governments that aim to facilitate matching agricultural labor demand and supply.

Also, several questions related to our study remain open and should be addressed in future research. Generally, the impacts of the Arab Spring migration crisis on European labor markets needs further investigation. In particular, in addition to employment effects, the impact of the supply shock on wages of both legal and illegal labor should be analyzed. Further, the current analysis should be extended to the whole agribusiness as it is the sector that absorbs most of the illegal workforce. Finally, long-term effects on labor markets need to be evaluated taking into account the current EU immigration policy and the recent regulatory efforts against labor exploitation.

6 Appendix

Table 7: Share of asylum seekers hosted in SPRAR reception centers in each Italian region over 2010-2012 (SPRAR, 2010, 2011, 2012)

	2010	2011	2012		2010	2011	2012
North-East				**North-West**			
Friuli V.G.	4.8	4.6	4.2	Aosta V.	0.0	0.0	0.0
Veneto	4.7	5.8	4.0	Piedmont	4.6	5.3	4.5
Trentino A.A.	0.6	0.6	0.4	Lombardy	16.5	5.7	16.8
Emilia R.	6.2	7.8	6.8	Liguria	2.7	3.0	2.2
Center				**South**			
Tuscany	4.4	4.5	4.6	Abruzzo	0.5	0.5	0.6
Marche	4.2	4.5	3.5	Molise	0.5	0.6	0.6
Lazio	22.4	26.2	21.2	Campania	2.9	3.2	2.0
Umbria	2.0	2.7	2.0	Basilicata	0.7	0.6	0.7
Islands				Apulia	7.1	8.0	6.2
Sicily	11.4	11.3	14.6	Calabria	3.5	4.7	4.9
Sardinia	0.4	0.4	0.3	**Total**	7056	7598	7823

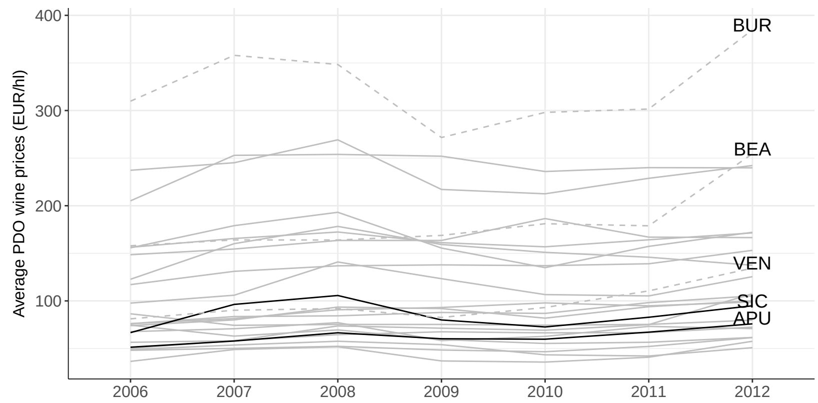

Figure 3: Average high-quality (PDO) wine prices for the treated regions Sicily (SIC) and Apulia (APU) (black) and control regions (grey), highlighting the discussed cases of Burgundy (BUR), Beaujolais (BEA), and Veneto (VEN) (dashed) (own illustration, source: ISMEA, 2018; FranceAgriMer, 2014).

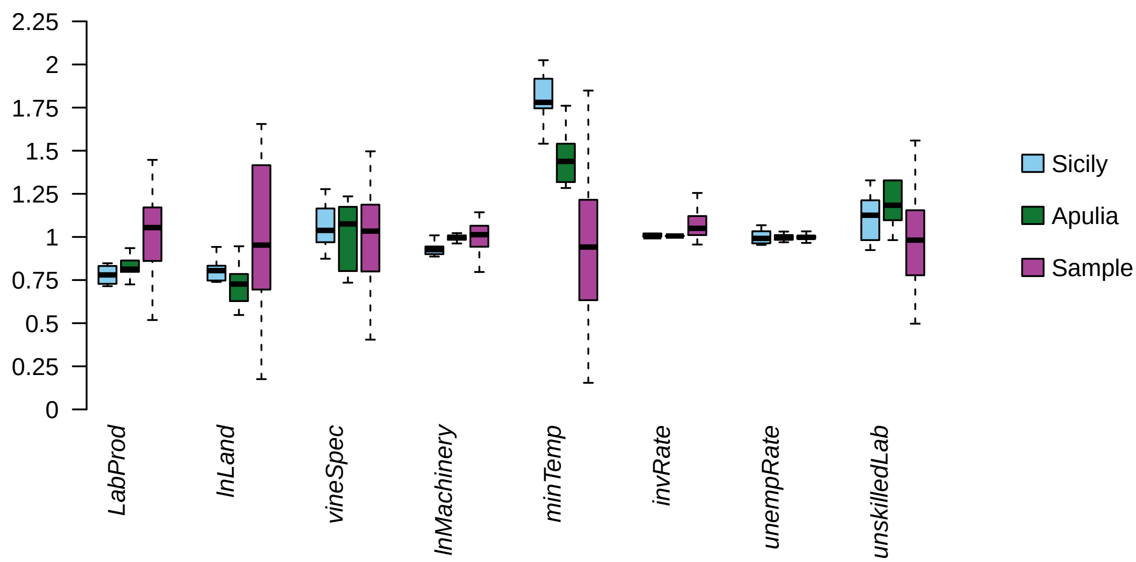

Figure 4: Descriptive statistics: Boxplots of mean-corrected variables (for comparability and visualization reasons, each variable is normalized by dividing it by its mean)

Table 8: DPOLS model estimates with time fixed effects (M2) for different choices of the control group based on k nearest neighbor matching (1999-2012). For each treated region, k nearest neighbors are selected resulting in sample sizes of $2(k+1)T$, and the full control group is selected for $k = 12$.

	$(k = 6)$	$(k = 7)$	$(k = 8)$	$(k = 9)$	$(k = 10)$	$(k = 11)$
ρ	0.665***	0.634***	0.641***	0.682***	0.673***	0.719***
	(0.059)	(0.066)	(0.050)	(0.051)	(0.043)	(0.042)
lnLand	0.121**	0.170***	0.187***	0.202***	0.203***	0.141***
	(0.053)	(0.049)	(0.036)	(0.040)	(0.030)	(0.029)
vineSpec	−0.006	−0.097	0.040	0.046	0.096	0.009
	(0.137)	(0.084)	(0.077)	(0.068)	(0.068)	(0.059)
lnMachinery	0.135***	0.158***	0.106***	0.116***	0.112***	0.119***
	(0.036)	(0.040)	(0.035)	(0.029)	(0.027)	(0.027)
minTemp	−0.008	−0.006	−0.015**	−0.009	−0.011**	−0.007
	(0.008)	(0.006)	(0.007)	(0.005)	(0.006)	(0.005)
invRate	−0.414	−0.273	0.267	0.157	0.196	−0.233
	(0.427)	(0.278)	(0.352)	(0.286)	(0.301)	(0.244)
unempRate	−0.059	0.420	−0.250	−0.326	−0.523	−0.364
	(0.852)	(0.735)	(0.729)	(0.691)	(0.693)	(0.669)
unskilledLab	0.310	−0.058	0.045	1.197*	1.320**	1.394**
	(0.943)	(0.677)	(0.762)	(0.666)	(0.653)	(0.607)
france	0.274***	0.277***	0.072	0.154*	0.158**	0.277***
	(0.102)	(0.083)	(0.089)	(0.080)	(0.077)	(0.074)
treated	−0.024	0.004	−0.018	0.008	0.010	0.011
	(0.042)	(0.038)	(0.040)	(0.035)	(0.036)	(0.036)
δ	0.135**	0.105**	0.115***	0.093**	0.104***	0.118***
	(0.053)	(0.051)	(0.043)	(0.040)	(0.039)	(0.038)
Obs.	196	224	252	280	308	336
F Statistic	175.8***	272.4***	347.2***	358.7***	504.6***	545.6***
Adjusted R^2	0.905	0.892	0.912	0.897	0.906	0.915
Notes:	***p=.01; **p=.05; *p=.1					

Table 9: Post-lasso model estimates

	Post-lasso	Std. Error
ρ	0.630***	(0.049)
*lnLand**ρ	−0.004	(0.007)
*vineSpec**ρ	−0.014	(0.022)
*invRate*unempRate*	−10.115*	(5.235)
$weather_{2002}$	0.109***	(0.035)
france	−0.024	(0.063)
$dummy_{2000}$	−0.073**	(0.031)
$dummy_{2012}$	0.041	(0.030)
trend	0.003	(0.004)
lnRent	0.040**	(0.019)
lnDepreciation	0.057	(0.049)
lnTaxes	0.050*	(0.026)
lnOtherInputs	0.011	(0.028)
lnMachineryCosts	0.052*	(0.027)
$otherInputs^2$	0.053	(0.125)
$otherInputs^3$	−0.110	(0.130)
post	0.012	(0.037)
treated	0.024	(0.037)
δ	0.089**	(0.037)
constant	−0.552**	(0.273)
Obs.	350	
F Statistic	482.8***	
Adjusted R^2	0.922	
Notes:	***p=.01; **p=.05; *p=.1	

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