

Clicking on Heaven's Door: The Effect of Immigrant Legalization on Crime[†]

By PAOLO PINOTTI*

We estimate the effect of immigrant legalization on the crime rate of immigrants in Italy by exploiting an ideal regression discontinuity design: fixed quotas of residence permits are available each year, applications must be submitted electronically on specific “click days,” and are processed on a first come, first served basis until the available quotas are exhausted. Matching data on applications with individual-level criminal records, we show that legalization reduces the crime rate of legalized immigrants by 0.6 percentage points on average, on a baseline crime rate of 1.1 percent. (JEL J15, J61, K37, K42)

Recent estimates place the number of undocumented immigrants in the United States at 11.5 million, up from 8.5 million in 2000 and close to exceeding the number of legal permanent immigrants (Rytina 2012; Hoefer, Rytina, and Baker 2012). The share of undocumented immigrants is also high in several European countries, such as Italy, Greece, Portugal, and Spain.¹

Both in the United States and in Europe, undocumented immigrants cannot officially work or start a new economic activity. They can only work in the informal economy, and hence face inferior earnings opportunities relative to their legal counterparts (Kossoudji and Cobb-Clark 2002; Kaushal 2006; Amuedo-Dorantes, Bansak, and Raphael 2007; Accetturo and Infante 2010; and Lozano and Sørensen 2011). According to the Becker-Ehrlich model of crime (Becker 1968; Ehrlich 1973), a lower income from legitimate activities means a lower opportunity cost of, and thus a higher propensity to participate in, illicit activities.

* Bocconi University, Fondazione Rodolfo De Benedetti, and BAFFI-CAREFIN Centre, Via Roentgen 1, 20136 Milan, Italy (e-mail: paolo.pinotti@unibocconi.it). I would like to thank two anonymous referees for very constructive feedback on an earlier version of this paper. I also thank Alberto Abadie, Jerome Adda, Josh Angrist, Erich Battistin, Tito Boeri, Christian Dustmann, Claudio Ferraz, Ray Fisman, Andrea Ichino, Eliana La Ferrara, Steve Machin, Magne Mogstad, Marco Ottaviani, Nicola Pavoni, Michele Pellizzari, Nicola Persico, Luigi Pistaferri, Imran Rasul, and seminar participants at the American Economic Association 2015, NBER Summer Institute 2014 (joint Labor Studies and Crime session), LSE, Berlin Applied Micro Seminar, UCLA, UC-Davis, Free University of Bozen, University of Stockholm, Gothenburg University, America Latina Crime and Policy Network (AL CAPONE) in Sao Paulo, HEC Lausanne, European University Institute, UCL-NHH Crime Conference, IFS Workshop on Advances in Policy Evaluation for useful comments. The Dipartimento Libertà civili e Immigrazione (Immigration Department) and the Direzione Centrale della Polizia Criminale (Criminal Police Department) of the Italian Ministry of Interior kindly provided the data on residence permits and crime. Financial support from Fondazione Rodolfo De Benedetti is gratefully acknowledged.

[†] Go to <https://doi.org/10.1257/aer.20150355> to visit the article page for additional materials and author disclosure statement.

¹ The Italian case is discussed in detail in the rest of the paper, estimates for the other European countries are provided by the EU-funded project CLANDESTINO (www.irregular-migration.net).

Indeed, immigration raises crime concerns in all main destination countries. According to the survey *Transatlantic Trends*, more than half of Europeans and North Americans are concerned that “immigration will increase crime in our society.” A similar share opposes immigration because it “will cause taxes to be raised,” while only one-third of respondents are concerned that “immigrants take jobs away from the native born.”² However, most respondents operate a clear distinction between regular and irregular immigrants: while the irregulars are generally blamed for increasing crime, regular immigrants cause much less concern in all countries.³

In Italy irregular immigrants accounted for 80 percent of all immigrants arrested in 2006 for serious crimes (Italian Ministry of Interior 2007) while their share over total foreigners remained below 20 percent (ISMU Foundation 2015). However, these figures may reflect the different composition of the two groups, as opposed to the (causal) effect of legal status. In particular, the undocumented are typically young, single males, and are less educated than legal immigrants (Cohn and Passel 2009; Caponi and Plesca 2014; and Mastrobuoni and Pinotti 2015). More generally, the two groups could differ along other (possibly unobserved) dimensions that are relevant to criminal behavior. For instance, individuals who are less risk-averse, or those who have a higher propensity to violate laws, would be more likely to reside unofficially in the country and to commit crimes. It is thus difficult to tease out the causal effect of legal status from selection.

The institutional framework in Italy provides an ideal regression discontinuity (RD) design that allows us to identify the causal effect of legal status on the number of crimes committed by immigrants in the destination country. The primary method of acquiring legal status in Italy is through work-related residence permits sponsored by the immigrant’s employer. Typically, immigrants enter Italy irregularly, start working unofficially for an employer, and hope that the employer will subsequently sponsor them for a residence permit. Fixed quotas of permits are available each year for different groups of applicants, as defined by the type of employer, nationality, and Italian province, for a total of 1,751 groups. Applications must be submitted online by employers starting at 8:00 AM on given “click days” of the year and are processed on a first come, first served basis until the exhaustion of the available quota of permits. The rationing of permits and the frequency of applications during the first hours of click days are such that several thousand applicants are denied legal status every year simply because their employers applied a few minutes (or seconds) after the cutoff time.⁴

We obtained restricted-use criminal records on the universe of applicants sponsored on the click days in December 2007 for receiving a permit in 2008. The total number of applicants reached 610,000, just below the 650,000 (estimated) undocumented immigrants present in Italy the year before (ISMU Foundation 2015). Therefore, the greatest majority of undocumented immigrants applies for

²The United States is the only country where natives are more concerned about the labor market impacts of immigration, possibly because greater labor market flexibility increases competition from immigrant workers.

³Detailed results from the 2008 and 2009 rounds of the *Transatlantic Trends* survey are presented in online Appendix Figures A1 and A2. The survey covers a number of countries in Europe and North America, interviewing about 1,000 people in each country. Additional information is available at <http://trends.gmfus.org/immigration/about/>.

⁴Although applications are notionally sent by employers, in what follows we use for convenience the term “applicant” to denote the immigrant sponsored in the application and the term “sponsor” to denote the employer.

a residence permit on click days. Taking advantage of detailed information on the timing of application—available at the millisecond—we compare the probability of being reported for having committed a serious crime in Italy, before and after legalization, across males who applied shortly before and shortly after the cutoff. For most groups of applicants, this cutoff occurred fewer than 30 minutes after the start of the click day. Most importantly, the exact timing of the cutoff for each group was unknown *ex ante*, as it depended on the timing of all applications as well as on how many applications were rejected for being inaccurate, false, or incomplete. These complexities provide a compelling argument for the fundamental identification assumption that applicants within an arbitrarily narrow bandwidth of the cutoff were unable to *precisely* determine their assignment to either side of it. Indeed, density and balance tests cannot reject the hypothesis that residence permits are as good as randomly assigned across immigrants who applied shortly before or shortly after the cutoff. In particular, the crime rate in the year before click days equals 1.1 percent in both groups.

In the year after click days, the crime rate declines to 0.8 percent for immigrants who applied before the cutoff while it remains at 1.1 for those who applied after the cutoff. The difference is statistically significant and it is unaffected when controlling parametrically for a smooth polynomial in the timing of application. Since the difference in the probability of obtaining legal status between these two groups is about 50 percentage points, the two-stage-least-squares (2SLS) estimated effect of legal status is a 0.6 percentage-point reduction in the crime rate—or 55 percent of the baseline. Estimates obtained using nonparametric methods are similar and slightly larger in magnitude.

Such effect depends both on the change in criminal behavior conditional on not being expelled and on the change in the probability of expulsion. In the absence of information on expulsions—or, for that matter, mobility across the border—it is impossible to separately identify these two components.

To gain some insights into the mechanisms behind the relationship between legal status and crime, we parse the data by the type of applicant, offense, and sponsor. The average effect across all applicants is driven by a reduction in the number of economically-motivated crimes committed by applicants sponsored by other immigrants as domestic workers (e.g., housekeepers, elder care givers, and so on) whereas the effect is zero for firm-sponsored employees. In the next section, we preliminarily show that the former category of applicants likely includes a significant number of individuals that would not qualify for a residence permit—male immigrants are typically *not* employed as domestic workers in Italy—but are nevertheless sponsored through fictitious job offers. Such individuals exhibit a higher crime rate before click days (almost 2 percent, twice the average across all applicants), which however declines significantly in the event of obtaining legal status (−1.3 percentage points). Instead, the crime rate for firm-sponsored employees, whose applications are generally backed by real job offers, is very low both before and after click days (0.5–0.7 percent).

A potential reconciliation of these findings is that firm-sponsored applicants are characterized by a higher opportunity cost of crime before click days, due to the fact they are already (unofficially) employed by the sponsor firm. In contrast, undocumented immigrants sponsored by other immigrants as domestic workers

often have neither a job nor a real job offer, so their opportunity cost of committing a crime is very low. At the same time, this group seems very responsive to legalization, likely because obtaining legal status encourages these people to search for a job in the official labor market. This can be particularly important for individuals at the margin between pursuing licit or illicit activities, who are probably over-represented among applicants with worse labor market opportunities.

The empirical results are extremely stable under a variety of specifications of both parametric and nonparametric regressions. In particular, the coefficient of interest is largely unaffected when varying the degree of the polynomial in the timing of application between 0 and 6, when varying the bandwidth around the cutoff time between 1 and 30 minutes, and when including a full set of fixed effects for applicants competing for the same quota of permits. This last result suggests that the estimated coefficient of legal status is not biased by heterogeneity in the baseline crime rate across groups. Based on a placebo exercise in which we permute the cutoff times across groups, we can also exclude that such an estimate is driven to a significant extent by small sample bias within groups. Finally, additional evidence suggests that the drop in crime is explained neither by the bureaucratic procedures imposed on successful applicants—particularly, the need to go back to collect an entry visa from the Italian embassy in the country of origin and re-enter Italy—nor by differences in the under-reporting of crimes committed by regular and irregular immigrants. If anything, the availability of fingerprints for regular immigrants, but not for the irregular ones, should entail an opposite bias.

This paper contributes to the growing body of evidence regarding the relationship between immigration and crime.⁵ Earlier work by Butcher and Piehl (1998) shows no evidence that immigration increased crime across US counties during the 1980s, whereas Spenkuch (2014) reaches an opposite conclusion for subsequent periods. Borjas, Grogger, and Hanson (2010) also find that immigration increases crime, although only indirectly (by raising the crime rate of native black males).

As for other countries, Bianchi, Buonanno, and Pinotti (2012) show that the causal effect across Italian provinces is not significantly different from zero, while Alonso-Borrego, Garoupa, and Vázquez (2012) find that immigration increased crime across Spanish provinces. Finally, Bell, Fasani, and Machin (2013) distinguish between the effect of two large immigrant waves in the United Kingdom, namely, asylum seekers and the post-2004 inflow from EU accession countries. Interestingly, only the former group, which was characterized by limited access to the official labor market, caused a significant increase in (property) crime.

Previous evidence on the effects of legal status is primarily based on the 1986 Immigration Reform and Control Act (IRCA), which granted legal resident status to long-time unofficial immigrants in the United States. Using the distance from the ports of entry and 1960 immigrants' enclaves as instruments for the presence of IRCA applicants, Baker (2015) shows that the legalization caused a reduction in crime across US counties. At the same time, the IRCA enforced stronger control over the hiring of undocumented immigrants, creating obstacles to the employment of those who were not legalized. Freedman, Owens, and Bohn (2013) focus on the

⁵ Bell and Machin (2013) provide a survey of this literature.

implications of these additional IRCA provisions and document an increase in the felony charges filed against Hispanic residents of San Antonio, Texas, after the expiration of the amnesty deadline.

With regard to other countries, in a previous paper with Giovanni Mastrobuoni (Mastrobuoni and Pinotti 2015), we exploit variation in legal status across pardoned prison inmates in Italy after the EU expansion of January 2007. We find that after the EU accession, recidivism declined markedly—from 5.8 percent to 2.3 percent over a six-month period—among inmates from the new EU member countries, whereas no change occurred in a control group of inmates from EU-candidate member countries.

The present analysis estimates the causal effect of legal status taking advantage of a clean quasi-experimental design. Under the assumption of no manipulation of the assignment variable, the RD approach ensures greater internal consistency than other quasi-experimental methods, comparable (at least locally) to that of randomized controlled trials (Lee 2008). With regard to the external validity of our estimates, the present paper examines the effect of changes in legal status that are routinely induced by migration policy, as opposed to exceptional events such as one-time amnesties or the EU expansion. Indeed, the institutional framework considered here is not specific to the Italian context, as immigration policy in many destination countries (e.g., Austria, Canada, and Spain) is based on analogous quota systems. Moreover, the application procedure for residence permits in Italy allows us to estimate the policy effect separately for groups of immigrants characterized by different employment opportunities. This heterogeneity allows for a better understanding of the channels through which legal status affects criminal behavior.

This paper is organized as follows. The next section describes Italian migration policy, the characteristics of different groups of applicants, and the functioning of click days. Section II discusses the empirical strategy and the data. Section III presents the results. Finally, Section IV concludes the paper with some policy implications as well as some caveats to our analysis.

I. Institutional Background

Immigration is a very recent phenomenon in Italy. The number of legal foreign residents increased from 500,000 to 5 million between 1990 and 2015, whereas the native population remained approximately constant at 55 million during the same period. In the face of this tumultuous increase, Italian migration policy, as designed by Laws 40/1998 and 189/2002, maintained a rigid “demand-driven” approach. At the end of each year (usually in November) the central government establishes the quotas of residence permits available the following year for different categories of workers. On given click days, employers in Italy—either natives or legal foreign residents—should apply on behalf of perspective immigrants *before* they enter Italy. Successful applicants obtaining a permit could start working for the sponsor employer but can also search for other jobs, start a new business, and so on. If they remain unemployed, however, they have six months to find another occupation, after which they should leave Italy.

In practice, the system is rarely employed as designed. Most employers in Italy are reluctant to hire complete strangers from abroad—particularly families hiring

domestic and care workers (*colf* and *badanti*, in Italian) and the small enterprises that constitute the bulk of Italian industry.⁶ As a consequence, most immigrants enter Italy unofficially, start working in the unofficial economy, and hope to be sponsored at one of the following click days. Therefore, migration quotas are “used as crypto-to-regularizations” (Cuttitta 2008, p.41) alongside the numerous general amnesties implemented since the late 1980s.

General amnesties allow us to gauge the size of irregular migration, as on these occasions applicants typically obtain residence permits under very mild conditions. Therefore, the number of amnesty applications provides a lower bound to the number of undocumented immigrants. The number of applications increased from 218,000 to 702,000 between the general amnesties of 1990 and 2002, 30 and 34 percent of total immigrants in the same years, respectively. The share of undocumented immigrants declined markedly over the following decade as a consequence of the process of EU enlargement, which granted legal status to immigrants from new EU countries.⁷

The main advantage of obtaining a residence permit, either through a general amnesty or on click days, is the possibility of working in the legal economy, which in turn improves employment and earning perspectives. Also, legal status eliminates the risk of deportation, although such risk is relatively small.⁸ As for employers, their main incentive in sponsoring applicants is avoiding future sanctions for employing irregular workers. To encourage exit from informality, applications were never used to track undocumented immigrants or their sponsor employers, despite recurrent anomalies such as the excessive number of male applicants allegedly sponsored as domestic and care workers.

We next describe the details of the quota system as well as the anomaly just mentioned.

A. The Quota System

Immigrants can be sponsored for two main types of permits: type-A permits for domestic and care workers employed by individuals and families; and type-B permits for firm employees, further distinguished into B1 and B2 permits for construction or nonconstruction workers, respectively. In addition, specific quotas, without distinction by type of permit, are reserved for immigrants coming from 14 “privileged” countries that subscribe to bilateral agreements to control irregular migration.

Column 1 of Table 1 shows the quotas fixed for 2008, the year considered in our analysis. Of 170,000 permits, slightly less than three quarters were awarded to immigrants of non-privileged nationalities, divided almost equally between type-A

⁶Law 40/1998 allowed third country nationals to access and reside in Italy for up to 12 months while searching for a job, however this possibility was dismissed with the reform of 2002.

⁷Online Appendix Figure A3 plots the yearly series of legal foreign residents, residence permit-holders, and (estimated) undocumented immigrants.

⁸Due to the cost of enforcing deportations, most immigrants apprehended by the police for being unofficially present in Italy receive just an injunction to leave the country. Combining estimates of the undocumented population and information on removals enforced by the Italian police during the period 2004–2007, the risk of deportation is approximately 5 percent per year (Dustmann, Fasani, and Speciale forthcoming).

TABLE 1—QUOTAS OF RESIDENCE PERMITS ESTABLISHED FOR 2008
AND APPLICATIONS RECEIVED

	Quota (1)	Applications (2)	Ratio (3)
Domestic workers (type-A permits)	65,000	136,576	0.48
Firm employees (type-B permits)	60,400	120,676	0.50
Privileged nationalities (type A + type B)	44,600	352,987	0.13
Albania	4,500	28,564	0.16
Algeria	1,000	1,904	0.53
Bangladesh	3,000	55,070	0.05
Egypt	8,000	18,833	0.42
Ghana	1,000	12,057	0.08
Moldova	6,500	31,286	0.21
Morocco	4,500	97,079	0.05
Nigeria	1,500	5,889	0.25
Pakistan	1,000	27,530	0.04
Philippines	5,000	21,805	0.23
Senegal	1,000	14,835	0.07
Somalia	100	159	0.63
Sri Lanka	3,500	21,966	0.16
Tunisia	4,000	16,010	0.25
Total	170,000	610,239	0.28

Note: This table reports the quotas established at the end of 2007 for 2008, the number of applications received, and the ratio of quotas to applications.

and type-B permits. The remaining 44,600 permits were reserved for privileged nationalities, which accounted for more than half of all applications.⁹

National quotas are then partitioned across the 103 Italian provinces based on the expected labor demand for foreign workers communicated in each previous year by the Union of Italian Chambers of Commerce, Industry, and Agriculture (UNIONCAMERE). In line with the tight rationing of permits at the national level, quotas fall short of demand for permits in all provinces, often by a large number. In Milan, for instance, the number of applicants was ten times greater than the number of available quotas.¹⁰

The quota system described above partitions applicants into 1,751 distinct groups: 1,442 groups of immigrants from privileged countries (103 provinces \times 14 nationalities) and 309 groups of immigrants from other countries (103 provinces \times 3 types of permits: A, B1, and B2). All applicants within each group compete for permits within the same quota. To ensure brevity and to emphasize the primary source of quasi-random variation at the heart of our identification strategy, we subsequently refer to such groups as “lotteries.”¹¹

⁹Contrary to the intentions of the policymaker, such nationalities thus faced a tighter rationing of residence permits relative to non-privileged nationalities. The ratio of quotas over applications for privileged and non-privileged nationalities was 13 and 49 percent, respectively (see the last column of Table 1). From 2009, however, the quotas for the non-privileged nationalities have also been set at a very low level.

¹⁰Online Appendix Figure A4 plots the number of permits available in each province against the number of applications received.

¹¹The term *lotteria* (Italian for lottery) has been repeatedly used in the public discourse to describe click days (see, e.g., Italian Senate 2011).

B. Type-A and Type-B Applicants

In principle, type-A permits should be used by families and individuals to sponsor domestic workers. However, they may also be fraudulently used to legalize immigrants that would not qualify for a residence permit.¹² Immigrants that are unofficially present in Italy but did not receive a job offer may find individuals or organizations willing to sponsor them for the payment of a few thousand euros. In such cases, the sponsor would pretend to hire the immigrant as a domestic worker in order to avoid a number of administrative procedures and controls that are imposed on firm employees (e.g., labor and health inspections). In fact, the only prerequisite for sponsoring a domestic worker is declaring an income above €800 in the last month before the click day.

There is extensive anecdotal evidence of this type of fraud. In 2010, the *Corriere della Sera*, a leading Italian newspaper, noticed the anomalous number of Chinese workers sponsored as housekeepers. The article attributed such anomaly to the fact that it is very easy to obtain a contract as housekeeper through family and friends.¹³ Also, up to 75 percent of all applications for domestic workers were presented by other immigrants.¹⁴ Similar anomalies are routinely reported by the Italian press after every click day.¹⁵

To go beyond anecdotal evidence, we compare the incidence and characteristics of type-A applicants in the click day files with the results of the ISMU survey of immigrants in the Italian region of Lombardy. The ISMU survey has been conducted annually since 2001 by the NGO “Iniziativa e Studi sulla Multietnicità” on cross sections of about 9,000 individuals per year, including both regular and irregular immigrants. The sampling of irregular immigrants is based on social networks around a number of aggregation centers: train stations, shops, and telephone centers. Sampling weights are provided in order to make the survey representative of the whole foreign population in Lombardy (Blangiardo 2008).¹⁶

According to ISMU, in 2007 the share of domestic workers among the employed, male immigrants in the region of Lombardy was only 2.4 percent, increasing to 4.1 percent among irregular immigrants (see the first two rows of Table 2). In comparison, 40.9 percent of male applicants in Lombardy applied for a type-A permit on click days (last row of the table), which is clearly anomalous.¹⁷

¹²The existence of a black market for residence permits has long been recognized in Italy and in many other countries (see Sciortino 1991 and Wasem 2008, for an exhaustive account of the Italian and the US cases, respectively).

¹³“Lo strano caso delle colf cinesi.” *Corriere della Sera*, March 5, 2011 (http://nuovitaliani.corriere.it/2011/03/05/lo_strano_caso_delle_colf_cine/, accessed October 28, 2016).

¹⁴“Primo click-day per i nuovi flussi: meno domande e molte sospette.” *Il Gazzettino*, February 1, 2011 (<http://www.cisveneto.it/Rassegna-stampa-Veneto/Primo-click-day-per-i-nuovi-flussi-meno-domande-e-molte-sospette>, accessed October 28, 2016).

¹⁵Baker (2015) discusses similar evidence for IRCA applications by agricultural workers in the United States.

¹⁶Lombardy is located in the northwest of Italy, and it is the largest of 20 regions in terms of both population and GDP—approximately 16 percent and 21 percent of the total, respectively. The region hosts more than 1 million (regular) immigrants, or one-fourth of all immigrants present in Italy. More information on the ISMU survey is available at www.ismu.org.

¹⁷The incidence of type-A applications among female applicants on click days is also abnormal, although to a lesser extent; see the last column of Table 2.

TABLE 2—FRACTION OF DOMESTIC WORKERS AMONG EMPLOYED IMMIGRANTS IN THE ISMU SURVEY AND TYPE-A APPLICANTS ON CLICK DAYS, BY GENDER

Employed as a domestic worker	All	Males	Females
ISMU	0.181	0.025	0.431
ISMU, only undocumented	0.234	0.041	0.522
Click day, only Lombardy	0.589	0.461	0.844

Notes: This table compares the incidence of domestic workers among immigrants in Lombardy according to the ISMU survey and to click day applicants in 2007. In particular, the first row of the table shows the fraction of domestic workers among the employed individuals in the ISMU survey, whereas the second row refers to the subsample of undocumented immigrants in the survey. The third row reports the fraction of type-A applicants among click day applicants in Lombardy.

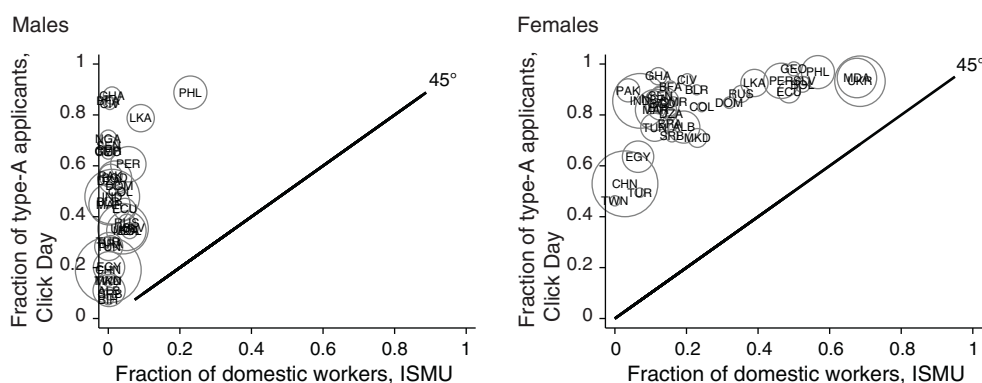


FIGURE 1. FRACTION OF DOMESTIC WORKERS AMONG EMPLOYED IMMIGRANTS IN THE ISMU SURVEY AND TYPE-A APPLICANTS ON CLICK DAYS BY GENDER AND NATIONALITY

Notes: The two graphs compare the fraction of domestic workers among the employed individuals in the ISMU survey (on the horizontal axis) with the incidence of type-A applicants among all applicants on click days (on the vertical axis) distinguished by gender and nationality. The size of the markers is proportional to the size of the national-gender groups among click day applicants; the 45-degree line is also included in the graph.

Figure 1 provides a more detailed comparison by nationality. For the number of type-A applications to be consistent with the observed patterns of employment in domestic services, the plots should be close to the 45-degree line. However, this is clearly not the case. The anomalies in click day applications are particularly evident for males, whose employment in domestic services is traditionally limited to immigrants from Philippines and Sri Lanka, whereas males of all nationalities are sponsored as domestic workers on click days (see the left graph in the figure). Additional anomalies emerge from restricted-use data available to the Italian Ministry of Interior. In particular, sponsors and applicants for type-A permits share the same nationality in 38 percent of applications, the same city of birth in 32 percent of applications, and the same surname in 21 percent of the applications (Italian Ministry of Interior 2009). Overall, both anecdotal evidence and statistical evidence suggest that type-A applicants may include a large number of unemployed (undocumented) immigrants disguised as domestic workers. This fact will be important for interpreting heterogeneity in the effect of legal status across different lotteries.

C. The Application Procedure

Each applicant can be sponsored only by one employer, although the same employer can sponsor more than one applicant. Until 2006, applications for residence permits were submitted on paper and processed on a rolling basis. This system, described at length in Cuttitta (2008), imposed tremendous delays in the processing of applications, because the manual data entry of paper forms required several months. To remedy this situation, in 2007 the application procedure was completely digitized, and applications are now submitted by employers via the Internet on specific click days each year. For instance, in 2007 the click day for privileged nationalities was December 15, whereas applications for type-A and type-B permits for other (non-privileged) immigrants had to be sent on December 18 and December 21, respectively.

Starting at 8:00 AM on click days, employers can log in to the website of the Ministry of the Interior and sponsor one or more immigrants for a residence permit.¹⁸ Applications are then processed on a first come, first served basis within each lottery according to the order in which they are received by the electronic system. The processing of each application involves cross-checking the criminal record of the sponsor and, only for type-B permits, the electronic registries of firms and self-employees maintained by UNIONCAMERE. If there are severe legal impediments—e.g., if the sponsor has been previously convicted for human smuggling—or if part of the information provided in the application is missing, inaccurate, or manifestly fraudulent, the application is rejected. If there are no impediments, instead, the sponsor is notified that the application was successful (*nihil obstat*) and an entry visa allowing the applicant to enter Italy is sent to the Italian consulate in his/her country of origin. This is because applicants should, in principle, enter Italy *after* having obtained a residence permit. As discussed before, however, most of them are already (unofficially) in Italy at the time of click days. Nevertheless, to comply with formal rules they need to go back to their country of origin first and then re-enter Italy, pretending they have never been there before.¹⁹

When the number of accepted applications fills the quota established for a given lottery, no further applications are accepted. This mechanism generates a discontinuity in the probability of obtaining a residence permit between those who applied just before and just after the moment in which the quota is exhausted.

Figure 2 shows two examples. The left graph refers to the lottery for type-A applicants of non-privileged nationalities in the province of Milan, the largest city in northern Italy, in which immigrants represent almost 15 percent of the resident population. The black line shows the total number of applications received at each minute in time. Immediately after 8:00 AM, the system receives hundreds of applications per minute, and by 9:40 AM the flow has already decreased to nearly zero. It is evident from this pattern that, although the system was adopted in 2007 for the first time, applicants were well-aware of the “first come, first served” rule.

¹⁸Online Appendix Figure A5 shows a screenshot of the dedicated page on the website of the Ministry of Interior.

¹⁹We discuss the implications of this provision for our empirical strategy in Section IIIB.

Panel A. Milan: type-A permits
(Dec. 18, 2007)

Panel B. Bergamo: type-B permits,
nonconstruction sectors (Dec. 21, 2007)

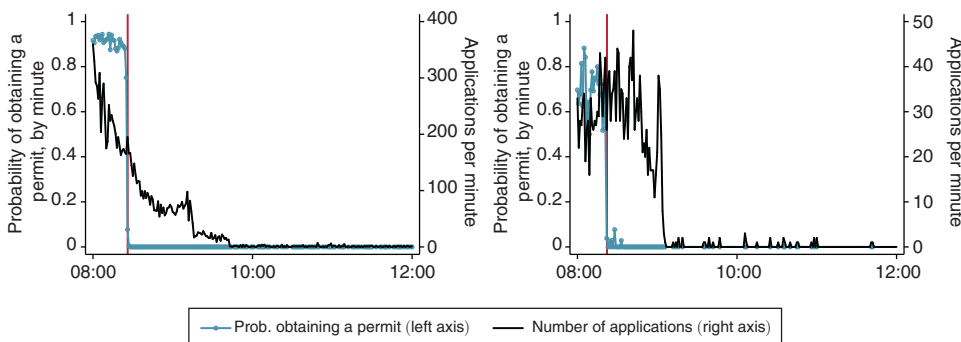


FIGURE 2. TIMING OF APPLICATIONS AND PROBABILITY OF OBTAINING A RESIDENCE PERMIT FOR TWO LOTTERIES IN MILAN AND BERGAMO

Notes: These graphs show the total number of applications received (black line, right axis) and the fraction of those that were eventually accepted (gray line, left axis) at each minute in time between 8:00 AM and 12:00 PM for the case of two lotteries: type-A applicants in Milan and type-B applicants (nonconstruction, B1) in Bergamo. The vertical lines in each graph show the timing of the structural breakpoint in the probability of acceptance, according to the Andrews (1993) test.

The ratio of accepted applications (also shown in the graph) is initially very high, as only fraudulent or incomplete applications were rejected, and it decreases to zero when the entire quota assigned to this group is exhausted. This occurred half an hour after the start of the lottery; to be precise, the last accepted application was received at 8:27:04 AM. Applications sent after the cutoff time are rejected, and these applicants have no right to reside and work in Italy. As already noted, however, the great majority of applicants were already present in the country before click days, and they would typically remain (unofficially) even after being refused an official permit. Our empirical analysis will compare the crime rate of immigrants whose application was received just before or just after the cutoff.

As another example, the right graph in Figure 2 shows the lottery for type-B1 applicants (i.e., nondomestic workers outside construction) in the province of Bergamo, a city near Milan. The ratio of applications sent on time that are rejected by immigration officers is larger for type-B applicants in Bergamo than for type-A applicants in Milan. This difference could reflect a higher incidence of fraudulent applications, more stringent scrutiny by local immigration authorities, or both. In general, rejections of applications sent before the cutoff are more frequent for type-B applicants, because information on sponsor firms is checked against administrative registries that are not available for individuals and families sponsoring type-A applications.

Also notice that, in the case of Bergamo, the probability of acceptance declines from 70 percent to 5 percent at 8:22:34 AM, but it reaches zero only a few minutes later. This situation occurs because when an application has been rejected because of missing, inaccurate, or false information or when the applicant did not collect the permit that had been authorized, the ordering in which the next applicants were re-contacted sometimes subverted the initial ordering. As long as the subversion results from idiosyncratic implementation errors, it would not cause any systematic

bias in the comparison of immigrants obtaining or not obtaining a residence permit (in fact, it could make an even stronger case for random treatment assignment). Moreover, even nonrandom subversion—for instance, because of manipulation by the immigration officials—can be easily accommodated for within the “fuzzy” RD framework presented in the next section.

II. Empirical Strategy and Data

Let L be a treatment indicator equal to 1 for applicants who obtained legal status on click days 2007 ($L = 0$ otherwise), and let $C = 1$ for applicants committing a crime in Italy during the following year ($C = 0$ otherwise). For each individual, we can also define the potential outcomes C^1 and C^0 when $L = 1$ and $L = 0$, respectively (Rubin 1974). The difference between the average crime rate of regular and irregular immigrants equals

$$(1) \quad E(C|L = 1) - E(C|L = 0) = \underbrace{E(C^1 - C^0|L = 1)}_{\tau} + \underbrace{[E(C^0|L = 1) - E(C^0|L = 0)]}_{\varepsilon}.$$

The first term on the right-hand side, $\tau \equiv E(C^1 - C^0|L = 1)$, is the average treatment-on-the-treated effect of legal status on the crime rate of click day applicants in the year after click days. Such effect depends both on the change in criminal behavior conditional on not being expelled and on the change in the probability of expulsion. In the absence of information on expulsions—or, for that matter, mobility across the border—it is impossible to separately identify these two components.

Notice that the same limitation applies generally to empirical studies of crime, as the crime rate is typically measured by the number of crimes committed in a given period (e.g., one year) over the beginning-of-period stock of potential offenders (e.g., the population in a given area). Insofar as there is population mobility across areas during the period of interest, the estimated effect of any crime determinant captures the combined effect on the stock of potential offenders and on the crime probability of those who did not change location. Although this problem is admittedly more relevant for immigrants, we should stress that obtaining a residence permit in Italy does *not* allow for free mobility in the rest of the European Union—or, for that matter, in any other country—so there is no reason to expect greater mobility across the border by legalized immigrants. If anything, the opposite might be true, as some of the rejected applicants could leave the country (or be expelled) in the months after click days. In this case, the change in the probability of committing crimes conditional on not being expelled would over-estimate the reduction in crime achieved through legalization. For this reason, the unconditional change—i.e., the treatment-on-the-treated effect τ in equation (1)—is arguably the relevant parameter from a policy perspective. In the end, when weighing the advantages and disadvantages of legalization policies, we wish to know the change in the number of crimes caused by the concession of legal status to the group of successful applicants. Our estimates specifically identify this parameter.

A. The RD Design

The main threat to empirically identifying τ in (1) is the selection bias $\varepsilon \equiv E(C^0|L = 1) - E(C^0|L = 0)$, which summarizes the effect of other determinants of criminal behavior that are potentially correlated with the timing of the application and, thus, with the probability of obtaining legal status. For instance, sponsors of high-ability workers may have a greater incentive to send their applications sooner, and labor market opportunities could also influence criminal behavior. However, such differences should tend to zero when we restrict our sample to applicants close enough to the cutoff, while the incidence of treated applicants still changes (discontinuously) at the cutoff. Therefore, comparing applicants within a sufficiently narrow bandwidth of the cutoff, but on opposite sides of it, identifies the treatment effect of legal status.

Formally, let X denote the timing of applications with $X = 0$ at the cutoff, so $Z \equiv 1\{X \leq 0\}$ is a “treatment assignment” dummy equal to 1 for immigrants applying before the cutoff.

Then, the difference in limits $\lim_{\Delta \rightarrow 0} [E(C|X < -\Delta) - E(C|X > \Delta)]$ identifies the reduced form effect of Z on crime across applicants near the cutoff. The fundamental identifying assumption is that Z is as-good-as-randomly assigned within an arbitrarily narrow bandwidth of $X = 0$. In practice, applicants close to the cutoff must be unable to *precisely* determine whether their application is received immediately before or immediately after the cutoff. This assumption is particularly plausible in this context, given that the cutoff time is revealed only after click days and it depends on the timing of all applications. For instance, in the example of Figure 2 an application at 8:25 AM would be received before the cutoff in Milan but after the cutoff in Bergamo, and this is unknown at the time of the application.

Given the fuzzy nature of our RD design, we rescale the reduced form effect by the first stage difference in the treatment probability at the cutoff,

$$(2) \quad \lim_{\Delta \rightarrow 0} \frac{E(C|X < -\Delta) - E(C|X > \Delta)}{E(L|X < -\Delta) - E(L|X > \Delta)}.$$

Under the additional monotonicity assumption that applying before the cutoff does *not* decrease the probability of obtaining legal status for any applicant (which seems plausible) the ratio in (2) is the local average treatment effect (LATE) of legal status on crime across the subset of *compliers* near the cutoff (see, e.g., Hahn, Todd, and Van der Klaauw 2001). These are applicants whose legal status is actually affected by whether the application is received before or after the cutoff, and they are thus most relevant from a policy perspective. Also, conditional independence near the cutoff allows for endogenous noncompliance with treatment assignment. In particular, rejection (acceptance) of applications received before (after) the cutoff may depend on applicant characteristics that are also correlated with the propensity to commit crimes.²⁰

²⁰Lee and Lemieux (2010) draw a useful analogy between fuzzy RD designs and randomized experiments with imperfect compliance.

In practice, we estimate the ratio in (2) by 2SLS using Z as an instrument for L , controlling parametrically for a flexible polynomial in X or restricting nonparametrically the sample to observations within a narrow bandwidth of the cutoff.²¹ The asymptotic properties of parametric and nonparametric estimators depend, respectively, on the polynomial order and the size of the bandwidth. In particular, there is a trade off between the bias and variance of estimates: higher-order polynomials and smaller bandwidths reduce bias but increase variance. We will explore the sensitivity of the results to a battery of choices regarding the polynomial order and the bandwidth.

B. Data

To implement the empirical strategy described above, the Immigration Department and the Criminal Police Department of the Italian Ministry of Interior agreed to match the administrative data on applicants at the 2007 click days with restricted-use data on the universe of criminal offenders reported by the police.

The data on applications include the country of origin, age, and gender of the applicant, the type of permit sponsored in the application, the province in which it was presented, the timing in milliseconds, and the outcome of the application (i.e., whether the applicant eventually obtained a residence permit). To avoid extrapolation bias in the global polynomial regressions, we restrict the sample to applications received within a one-hour symmetric time window around the cutoff, which leaves us with a total of 186,608 observations. Limiting ourselves to males, the final sample includes 110,337 applicants.²²

The data on applications were matched at the individual level with the criminal records of all offenders reported by the police for having committed (at least) one serious crime—robbery, theft, drug trafficking, smuggling, extortion, kidnapping, murder, and rape—in 2007 or 2008 (i.e., the year before and the year after the 2007 click days). The data include the type of crime, the month in which it was committed, and whether the offender was arrested *in flagrante* (i.e., red-handed). Violations of migration law do not constitute a serious crime, so differences in the observed crime rates between regular and irregular immigrants do not depend on the fact that the latter can be reported for being unofficially in the country.²³

The information reported on each application (specifically, the country of origin, the province of destination, and the type of permit) allows us to identify the lottery in which each applicant participated. In particular, click days in 2007 generated 1,580 out of 1,751 potential lotteries (see Section IA) because 68 lotteries received

²¹ The reduced form and first stage estimating equations are presented in the online Appendix.

²² The crime rate of females is very close to zero, as is typically the case for serious crimes.

²³ We do not extend the series to later years (from 2009 onward) because applicants that did not fall within the quotas for 2008 were prioritized one year later. To clear the backlog accumulated after the click days of December 2007, no new applications were in fact allowed on December 2008. Rather, the first 150,000 applicants excluded in 2007 were reprocessed upon renewal of the application by the sponsor employer (to be sent between December 15, 2008, and January 3, 2009). Therefore, applicants to the right of the cutoff also had the possibility of obtaining legal status in 2009. This procedure was exceptional and it was publicized only at the beginning of December 2008—a few days before the time window for renewing the application—so it should have not affected the behavior of this group. In any case, anticipating the possibility of obtaining a residence permit in 2009 should attenuate any difference in behavior between legalized and nonlegalized applicants, so our estimates still provide a lower bound to the effect of legal status in the ideal experiment in which only one of the two groups obtains legal status.

no applications and because the 103 provincial lotteries for Sri Lankan immigrants were corrupted by a computer bug.²⁴

The largest lotteries were those for type-A applicants of non-privileged nationalities in Rome and Milan, with 9,974 and 12,207 (processed) applications, respectively; meanwhile, dozens of lotteries for privileged nationalities in smaller provinces received only a few applications. The mean and median number of applications processed across all lotteries are 246 and 57, respectively, and there were 311 lotteries receiving fewer than 10 applications. Following common practice in RD studies (see, for instance, Fredriksson, Öckert, and Oosterbeek 2013 and Dahl, Loken, and Mogstad 2014), we pool the observations from all lotteries and include lottery-fixed effects to prevent heterogeneity in the baseline crime rate across lotteries (the intercept of the regression) from biasing the estimate of the effect of legal status (the slope parameter).²⁵ We will also perform a permutation exercise to exclude the possibility that our estimates average a small sample bias across lotteries.

The running variable is the delay of each application relative to its lottery's cutoff. In contrast to most RD designs in which treatment assignment depends on a predetermined threshold rule, the cutoff in this case is unknown *ex ante*, depending on the realized timing and outcome of all applications submitted in the same lottery. On the one hand, this feature reinforces the fundamental identifying assumption that applications are as good as randomly assigned near the cutoff, on the other it complicates the definition of the cutoff. When there is fuzziness on both sides of the cutoff, as in the right graph in Figure 2, the probability of obtaining legal status may drop significantly several minutes before the last successful application is received. Confronted with the same problem (i.e., estimating an unknown cutoff point in a fuzzy RD design), Chay, McEwan, and Urquiola (2005) and Bertrand, Hanna, and Mullainathan (2010) run a battery of regressions of treatment assignment on a dummy that equals 1 after each possible cutoff point and choose the one that maximizes the R^2 of the regression. Following the same idea, we conduct a formal Andrews (1993) test for the existence (in each lottery) of a structural break with an unknown breakpoint. The test identifies the "most likely break point" and allows us to test for the significance of the structural break.

The vertical lines in the graphs in Figure 2 show the estimated structural break points in the case of Milan and Bergamo. For the overwhelming majority of lotteries (corresponding to 98 percent of the applicants in our sample) the test rejects the null hypothesis that there is no structural break. The median (estimated) cutoff time across lotteries is 8:39:06 AM and the majority of quotas were exhausted before 9:00 AM.²⁶

²⁴The issue was brought up also in Sri Lankan news, see <http://www.lankanewspapers.com/news/2007/12/22962.html>. Notice that the article also makes reference to the black market for residence permits, discussed in Section IB above.

²⁵This particular form of omitted variable bias is sometimes called the "Yule-Simpson Paradox," see, e.g., Chen, Bengtsson, and Ho (2009).

²⁶The distribution of cutoff points is reported in online Appendix Figure A6. Card, Mas, and Rothstein (2008) argue that estimates of unknown cutoff points over-reject the null hypothesis that the discontinuity is not different from zero. This is an issue when the discontinuity in the running variable is the outcome of main interest, which however is not the case in the present analysis. The main implication of over-rejecting the null, in the present context, is that we would include in the sample some lotteries with no significant discontinuity in the probability of obtaining legal status. This would bias the estimated effect of legal status toward zero.

III. Results

The RD approach allows for a transparent graphical representation of the effect of interest. We thus begin with this analysis and then move to parametric and nonparametric regression methods.

A. Baseline Estimates

Figure 3 plots the crime rate of click day applicants in the years before and after click days, conditional on the timing of the application X , for different types of applicants. The circles are averages across five-minute bins of X to the left and right of the cutoff, whereas the solid line and shaded area are the predicted outcomes and associated confidence intervals, respectively, based on a polynomial regression. Following Gelman and Imbens (2014), in the baseline specification we consider a quadratic polynomial in X ; later in this section we experiment with all polynomial orders between 0 and 6. The discontinuity in the predicted crime rate at the cutoff equals the reduced form effect of applying before the cutoff. Such coefficient is reported in Table 3 together with the first stage and 2SLS coefficients and the baseline crime rate in 2007.

In the year before click days (2007), the crime rate is identical—1.1 every 100 applicants reported for having committed a serious crime—for immigrants applying before and after the cutoff, respectively. This provides strong evidence that, after controlling for a smooth polynomial in the timing of application, applicants are as good as randomly assigned to either side of the cutoff.

In the year after click days (2008), the crime rate declines to 0.8 for immigrants applying on time, while it remains at 1.1 for other applicants. Therefore, the reduced form effect (−0.3 percentage points, significant at the 90 percent confidence level) reflects a decrease in the crime rate of applicants to the left of the cutoff, rather than an increase in the crime rate of late applicants. The 2SLS estimated effect of legal status, obtained as the ratio between the reduced form and first stage coefficient, amounts to a 0.6 percentage point reduction in the crime rate.²⁷

As discussed in Section IIA, the 2SLS coefficient identifies the LATE on compliers with treatment assignment. The average treatment-on-the-treated effect τ in equation (1) averages such effect with the effect on the “always-takers,” applicants who would obtain legal status independently of whether they applied before or after the cutoff (for instance, through family reunification). Although it is not possible to single out compliers and always-takers in the data—the treatment status under the counterfactual treatment assignment is not revealed—we can nevertheless describe the average characteristics of compliers as

$$(3) \quad E(g(K) | \text{Compliers}) = \lim_{\Delta \rightarrow 0} \frac{E(g(K)L | X < -\Delta) - E(g(K)L | X > \Delta)}{E(L | X < -\Delta) - E(L | X > \Delta)},$$

²⁷The first stage regression is plotted in online Appendix Figure A7.

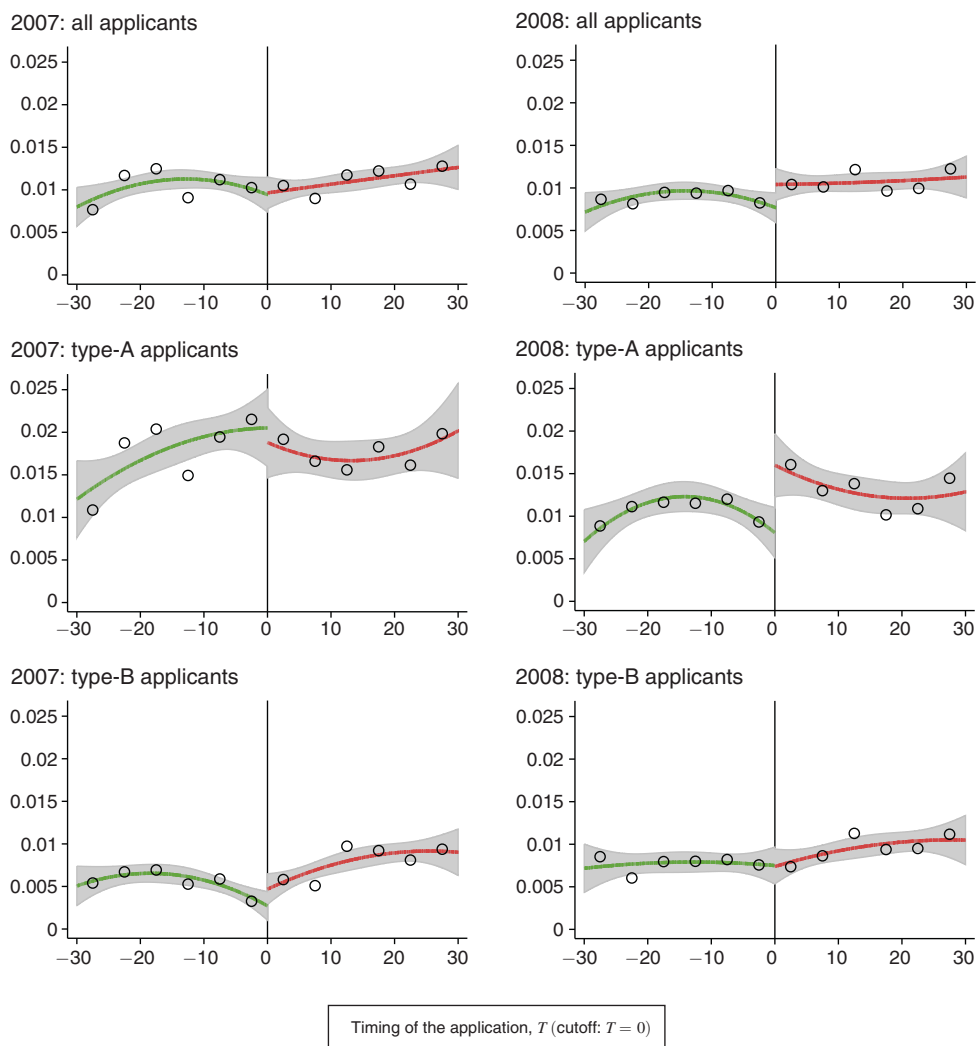


FIGURE 3. NUMBER OF CRIMES PER APPLICANT BEFORE AND AFTER CLICK DAYS, CONDITIONAL ON THE TIMING OF APPLICATION

Notes: The graphs show the average number of crimes per applicant committed before and after click days, conditional on the timing of application. The scatterplots are averages within five-minute bins, and the solid line and shaded areas are the predicted outcomes and associated confidence intervals, respectively, based on quadratic polynomial regressions. The graphs in the right and left columns show the crime rates in the year before and after click days (2007 and 2008, respectively). The top, middle, and bottom rows show the results for all applicants, the subsample of type-A applicants, and the subsample of type-B applicants, respectively.

where $g(K)$ is the distribution of an individual characteristic K (see, e.g., Angrist et al. 2016). The right-hand side of equation (3) is easily estimated by the 2SLS regression of $g(K)L$ on L using Z as an instrument and controlling parametrically or nonparametrically for differences in X .

The fraction of previous offenders—arguably the best proxy for the average propensity to commit crimes—is nearly identical between compliers and the total sample (1.2 and 1.1 percent, respectively). The same is true for the age distribution, whereas compliers are more likely to come from poorer countries relative to the rest

TABLE 3—EFFECT OF LEGAL STATUS ON THE NUMBER OF CRIMES PER APPLICANT, GLOBAL POLYNOMIAL REGRESSION

	Baseline estimates			Lottery FE, clustered SE		
	All (1)	Type A (2)	Type B (3)	All (4)	Type A (5)	Type B (6)
<i>Panel A. Year 2008</i>						
Reduced form	−0.003 (0.002)	−0.008 (0.003)	0.000 (0.002)	−0.003 (0.002)	−0.008 (0.003)	0.000 (0.002)
2SLS effect of legal status	−0.006 (0.003)	−0.013 (0.005)	0.000 (0.005)	−0.006 (0.004)	−0.013 (0.005)	0.001 (0.005)
First stage coeff. of Z	0.459 (0.007)	0.610 (0.010)	0.372 (0.009)	0.463 (0.029)	0.621 (0.032)	0.374 (0.041)
First stage F -stat. for Z	4,304.08	3,492.26	1,645.23	248.73	369.39	83.15
<i>Panel B. Year 2007</i>						
Reduced form	−0.000 (0.002)	0.002 (0.004)	−0.002 (0.001)	−0.000 (0.002)	0.002 (0.004)	−0.002 (0.002)
$E(C Z = 0)$	[0.011]	[0.018]	[0.008]	[0.011]	[0.018]	[0.008]
Observations	110,337	40,451	69,886	110,337	40,451	69,886

Notes: This table shows parametric estimates of the effect of legal status on the crime rate of click day applicants. Panel A reports reduced form and 2SLS regressions of a dummy $C = 1$ for applicants that committed at least one serious offense in 2008 on a dummy $L = 1$ for applicants obtaining legal status on click days. The first stage instrument is a dummy $Z = 1$ for having applied before the cutoff time, the first stage coefficient of Z and the F -statistic for the excluded instrument (adjusted for heteroskedastic and clustered standard errors) are also reported. Panel B of the table reports the reduced form regression of a dummy for committing at least one serious offense in the year before click days (2007) on the instrument Z and (in square brackets) the baseline crime rate in such year, $E(C|Z = 0)$. All regressions control for a quadratic polynomial in the time elapsed since the cutoff (by the millisecond) and its interaction with Z , and the specifications in columns 4–6 also include lottery-fixed effects and a quadratic polynomial in age. Robust standard errors are reported in parentheses, and they are clustered by lottery in columns 4–6.

of the sample. Overall, the average characteristics of compliers are very similar to those of other applicants.²⁸

Distinguishing between different types of applicants, the decrease in crime rate is entirely driven by type-A applicants. These applicants start from a higher crime rate before click days (1.8 percent) but exhibit the greatest decrease when legalized (−1.3 percentage points), see the middle row of Figure 3 and column 2 of Table 3. By contrast, type-B applicants are characterized by a lower crime rate before click days (0.5 percent), which remains largely unaffected during the following year, see the bottom row of Figure 3 and column 3 of Table 3.²⁹

In columns 4–6 we include lottery fixed effects, a quadratic polynomial in age (the only additional individual characteristic included in our data), and we cluster standard errors by lottery to account for interactions in crime among groups of immigrants who apply for the same lottery. All coefficients are unaffected by the inclusion of fixed effects, suggesting that results are driven neither by aggregation bias across lotteries (discussed in Section IIB) nor by compositional effects.³⁰ Figure 4

²⁸Online Appendix Table A1 and Figure A8 present the detailed comparison between compliers and the total sample of applicants.

²⁹The difference in coefficients between the two groups is statistically significant, see the interaction coefficients reported in online Appendix Table A2.

³⁰Lottery fixed effects implicitly absorb province fixed effects interacted with indicators for the 14 privileged nationalities, and with a dummy for the type of permit for applicants of non-privileged nationalities. Online

confirms indeed that applicants close to the cutoff are on average identical and the empirical density of the running variable in Figure 5 provides additional support for the assumption that legal status is as good as randomly assigned near the cutoff. Consistent with the visual evidence, the McCrary (2008) test does not reject the null hypothesis of no discontinuity in the density of applications. Therefore, neither manipulation of the running variable nor differences in average characteristics can explain the discontinuity in the probability of committing crimes at the cutoff. This is consistent with the fact that the timing of the cutoff was unknown to applicants during the submission process.

The results in Table 3 are confirmed when using nonparametric methods. Table 4 shows the results of 2SLS kernel local linear regressions. To choose the bandwidth, we follow the theory-based criteria proposed by Imbens and Kalyanaraman (2012)—henceforth, IK2012—and Calonico, Cattaneo, and Titiunik (2014)—henceforth, CCT2014. The criterion used in each specification, the size of the bandwidth, and the number of observations included within the bandwidth are reported in the table. The size of the bandwidth remains very similar when applying the IK2012 and the CCT2014 criteria. The 2SLS estimated effect in year 2008 is around -1.0 percentage point, larger in magnitude than the parametric estimate and statistically significant at the 95 percent confidence level (columns 1 and 4). When distinguishing between different groups of applicants, the crime rate declines significantly for type-A applicants obtaining legal status (columns 2 and 5), and the magnitude of the effect is virtually identical to the estimate obtained using parametric methods (Table 3). The absence of any effect on the crime rate of type-B applicants is also confirmed (columns 3 and 6).

The consistency of parametric and nonparametric estimates requires, respectively, that controlling for a flexible polynomial in X , or restricting the sample to observations in a neighborhood of the cutoff, removes the effect of other factors that vary (smoothly) around the cutoff. Both methods involve a trade-off between bias and efficiency: in general, higher-order polynomials and smaller bandwidths reduce the bias at the cost of greater asymptotic variance. To examine the sensitivity of our results to different choices regarding these trade-offs, the graphs in Figure 6 plot the estimated 2SLS coefficient of legal status and the associated confidence intervals for different specifications of parametric and nonparametric regressions. In particular, the first two graphs plot the results for type-A and type-B applicants, respectively, of the global polynomial regression when varying the order of the polynomial between 0 and 6; the last two graphs plot the results of local kernel regressions for each possible bandwidth between 1 and 30 minutes (the IK2012 and CCT2014 bandwidths are also indicated on the horizontal axis). The estimates are remarkably similar when adopting different specifications of parametric and nonparametric methods.³¹

Figure 7 compares such estimates with a distribution of 1,000 placebos. Each placebo estimate is obtained by permuting the cutoff points at random across lotteries, computing a “fake” running variable \tilde{X} as the difference between the timing

Appendix Table A3 shows that results are unaffected when we use either province fixed effects or province fixed effects interacted with a full set of nationality fixed effects, and we cluster standard errors at the corresponding level of aggregation.

³¹ The same is true when looking at the reduced form effect in 2007, see online Appendix Figure A9.

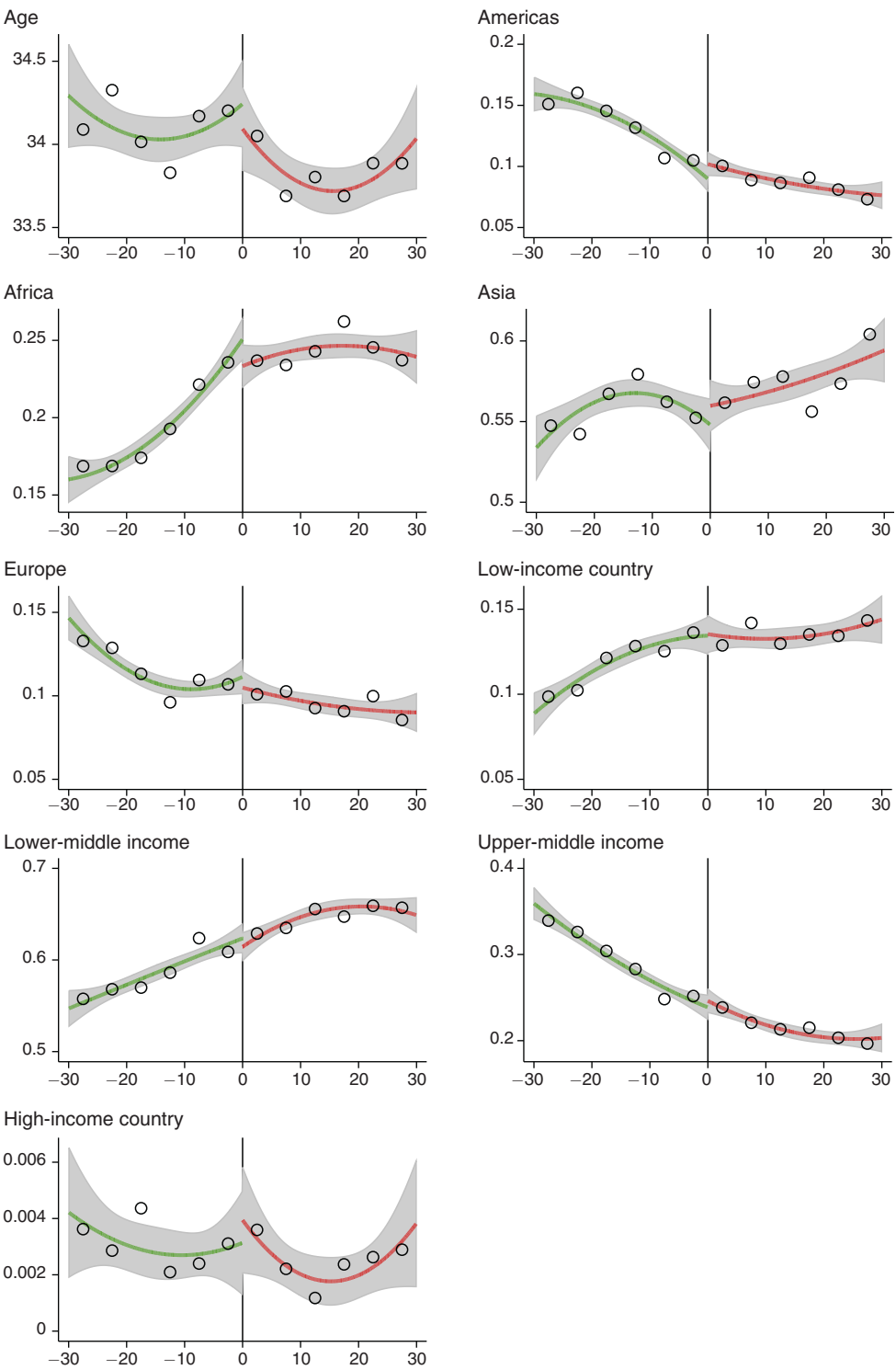


FIGURE 4. INDIVIDUAL CHARACTERISTICS, CONDITIONAL ON THE TIMING OF APPLICATION

Notes: These graphs show the average individual characteristics of type-A applicants, conditional on the timing of application. The scatterplots are averages within five-minute bins, and the solid lines and shaded areas are the predicted values and associated confidence intervals, respectively, based on a quadratic polynomial regression.

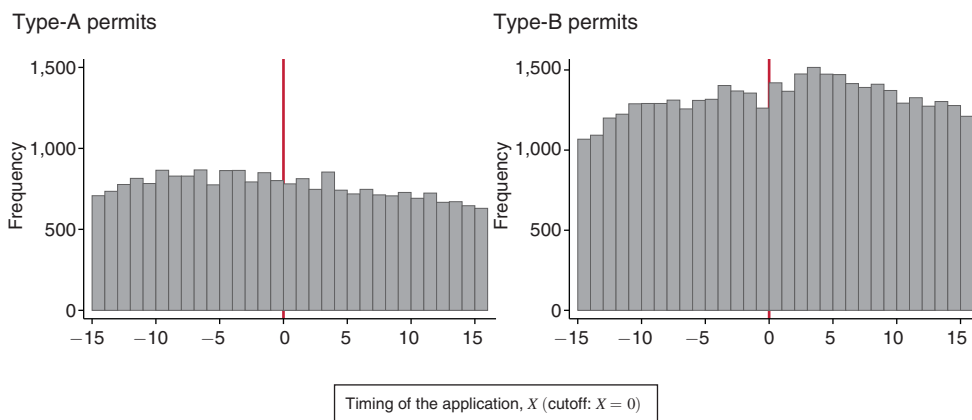


FIGURE 5. FREQUENCY OF APPLICATIONS NEAR THE CUTOFF

Note: The graph shows the density of applications received by type-A and type-B applicants at each minute in time before and after the cutoff.

TABLE 4—EFFECT OF LEGAL STATUS ON THE NUMBER OF CRIMES PER APPLICANT, KERNEL LOCAL LINEAR REGRESSION

	Bandwidth: IK2012			Bandwidth: CCT2014		
	All (1)	Type A (2)	Type B (3)	All (4)	Type A (5)	Type B (6)
<i>Panel A. Year 2008</i>						
Reduced form	−0.004 (0.002)	−0.008 (0.003)	−0.001 (0.002)	−0.005 (0.002)	−0.009 (0.003)	−0.002 (0.002)
2SLS effect of legal status	−0.008 (0.004)	−0.013 (0.005)	−0.003 (0.006)	−0.010 (0.005)	−0.014 (0.006)	−0.005 (0.007)
Size of bandwidth	9:26	10:52	9:55	6:37	8:17	7:57
Observations inside bandwidth	41,743	17,378	27,801	29,805	13,454	22,532
<i>Panel B. Year 2007</i>						
Reduced form	0.001 (0.002)	0.003 (0.003)	−0.002 (0.002)	0.001 (0.002)	0.004 (0.004)	−0.001 (0.002)
Size of bandwidth	10:20	25:43	11:15	7:27	9:51	7:31
Observations inside bandwidth	45,472	36,362	31,160	33,310	15,905	21,371
Observations	110,337	40,451	69,886	110,337	40,451	69,886

Notes: This table shows nonparametric estimates of the effect of legal status on the crime rate of click day applicants. Panel A reports reduced form and 2SLS regressions of a dummy $C = 1$ for applicants that committed at least one serious offense in 2008 on a dummy $L = 1$ for applicants obtaining legal status on click days. The first stage instrument is a dummy $Z = 1$ for having applied before the cutoff time. Panel B of the table reports the reduced form regression of a dummy for committing at least one serious offense in the year before click days (2007) on the instrument Z . All coefficients are estimated using a kernel local linear regression in a symmetric bandwidth around the cutoff. The table reports the bandwidth selection rule, IK2012 or CCT2014, the size of the bandwidth (expressed in minutes:seconds), and the number of observations included in the bandwidth. Robust standard errors are reported in parentheses.

of each application and the placebo cutoff, and estimating the discontinuity in the probability of committing crimes at $\tilde{X} = 0$.³² The distributions of the parametric

³²We consider only the reduced-form coefficients because the placebo cutoffs have, by construction, no predictive power in the first stage.

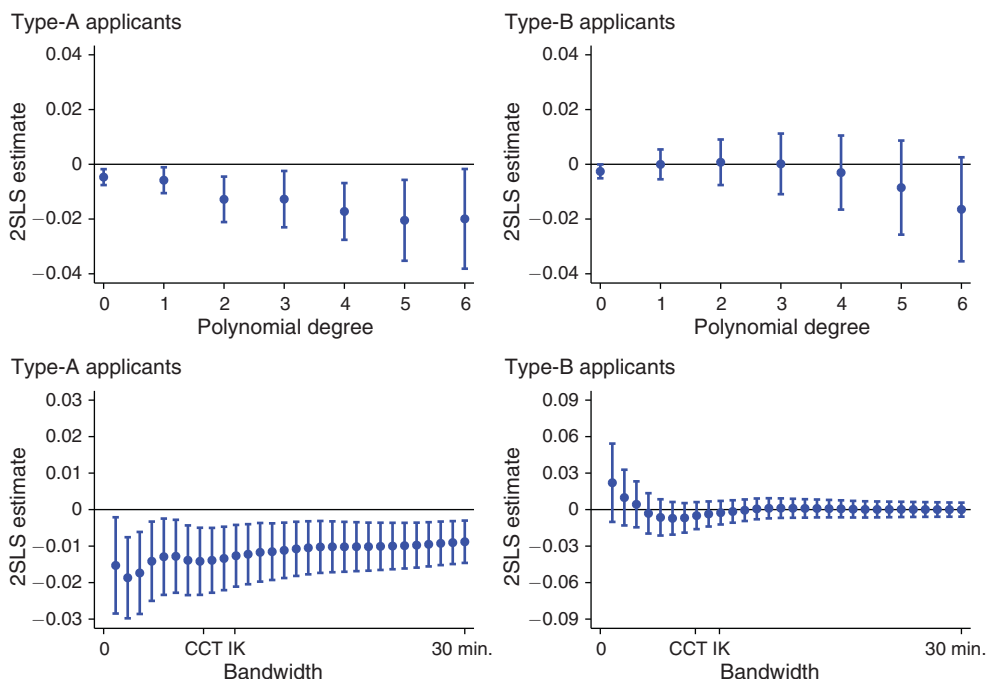


FIGURE 6. SENSITIVITY ANALYSIS

Notes: The graphs show the 2SLS estimated coefficients of legal status for different specifications of parametric and nonparametric regressions, distinguishing between type-A and type-B applicants. In particular, the two graphs on the top show the point estimates and associated confidence intervals when varying the degree of the parametric polynomial regression between 0 and 6. The two graphs on the bottom show the point estimates and confidence intervals when varying the bandwidth of the nonparametric regressions between 1 and 30 minutes; the bandwidths selected according to the IK2012 and CCT2014 criteria are also reported on the horizontal axis.

and nonparametric placebos are centered at zero, and the probability of obtaining values below the estimates at the true cutoff for type-A applicants (i.e., the estimates reported in Table 3) is as small as 0.003. These p -values can be interpreted as the probability that, under the null hypothesis of no effect of legal status, the estimating bias is sufficiently large to account for the magnitude of the estimated coefficient. In particular, these results appear to exclude the possibility that the baseline estimates only average a small sample bias across lotteries.

B. Threats to the Identification

The results presented so far suggest that obtaining legal status lowers the crime rate of type-A applicants in the year after click days. This effect is sizable, statistically significant, and robust to alternative estimation methods. We next discuss a few identification challenges implied, respectively, by the fact that we cannot determine the presence of applicants in Italy at each point time and that there is measurement error in crime data.

Presence in Italy before Click Days.—In line with the previous discussion of the institutional framework (Section I), so far we maintained that most applicants are

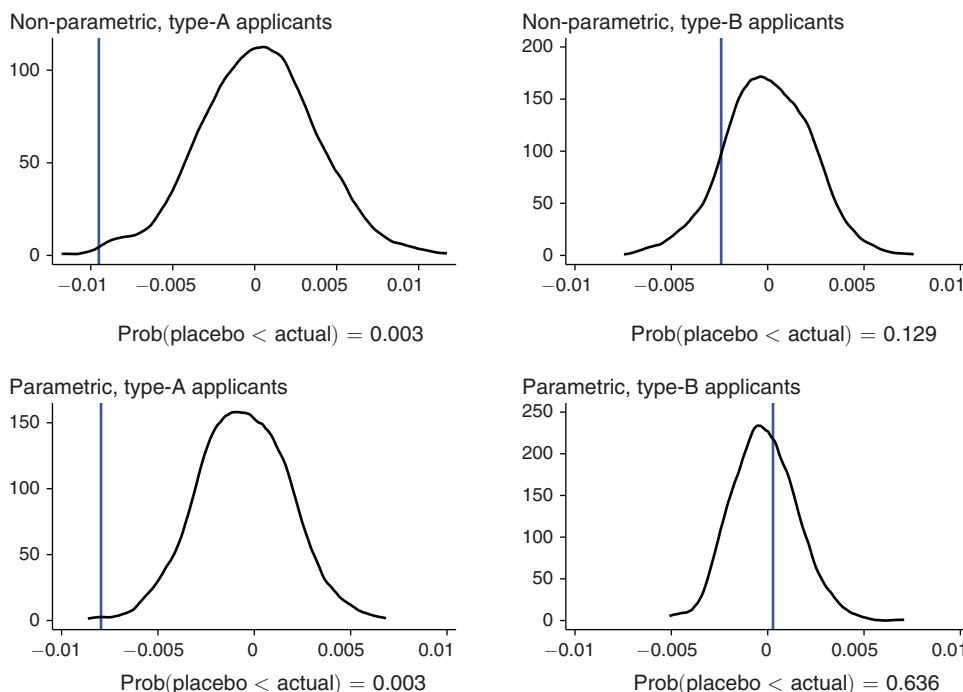


FIGURE 7. REDUCED-FORM EFFECT OF LEGAL STATUS ON THE NUMBER OF CRIMES PER APPLICANT, PLACEBO ESTIMATES

Notes: The graphs show the distribution of the reduced-form estimates obtained for 1,000 random permutations of the cutoff points across lotteries using parametric and nonparametric methods and distinguishing between type-A and type-B applicants. The vertical lines represent the estimates obtained at the true cutoff point (i.e., the estimates reported in Tables 3 and 4). The fraction of placebo estimates lower than the actual estimate is also reported at the bottom of each graph.

present in Italy before click days, despite formal rules allowing them to enter only after obtaining a residence permit. We now consider the possibility that a (potentially large) fraction of applicants comply with the formal rules of admission, so they are abroad at the time of the application. Clearly, immigrants abroad at the time of the application could not commit any crime in Italy before click days. After click days, it seems reasonable that applicants obtaining a residence permit are more likely to enter Italy than applicants who did not obtain a permit. In this case, the number of crimes committed in Italy—the numerator of the crime rate—would increase more for the legalized relative to the nonlegalized. At the same time, the number of applicants—the denominator of the crime rate—would not change for either group. Therefore, differential entry after click days amongst the two groups should induce a greater increase in the crime rate of the legalized relative to the nonlegalized. Since the observed crime rate of the legalized *decreases* relative to that of the nonlegalized, our estimate is a lower bound (in terms of magnitude) to the effect that we would observe if all the applicants were already present in Italy at the time of click days. Notice that this is true even when applicants abroad have a lower propensity to commit crimes than applicants who are already in Italy before click days. This is because the number of crimes committed can only increase as more immigrants enter Italy.

Conceptually, the possibility that a share of the applicants were abroad before click days may thus bias our estimates against finding a decrease in the crime rate of legalized applicants relative to the nonlegalized in the year after click days. Empirically, the evidence in Figure 3 is consistent with the common wisdom that the overwhelming majority of applicants were already present in Italy before click days. The crime rate for the year before click days is not lower—and it is indeed slightly higher—than that observed the following year for applicants who did not obtain a residence permit. This fact is hard to reconcile with a large fraction of applicants being in their country of origin at the time of the application.

Collecting the Entry Visa in the Country of Origin.—Successful applicants must collect a visa, allowing them to enter Italy, from the Italian embassy in their country of origin. Although the overwhelming majority of applicants are already in Italy at the time of click days, they must nevertheless comply with this procedure in order to finally obtain a residence permit (see Section IC). Therefore, in the weeks after click days, successful applicants must travel to their country of origin, thus spending less time in Italy. In principle, this fact could partly explain the decrease in crime observed among legalized immigrants in the year after click days. In practice, the traveling time required to account for the estimated effect seems implausibly large. If the propensity to commit crime does not significantly vary over the year, legalized applicants would need to spend more than half a year outside Italy for their crime rate to decline by 0.6 percentage points (on a baseline of 1.1 percent). Following the same logic as before, legalized type-A applicants in this group would need to spend 8.7 months outside Italy to collect the visa.

In any event, we first replicate our baseline analysis separately for applicants coming from countries that are closer and farther away from Italy (e.g., Albania and Philippines, respectively). Applicants in the latter group would spend more time in their country of origin (e.g., to visit family and friends) as traveling back home is more costly for them. If results are driven by time spent outside Italy, we should then observe a larger effect among successful applicants from more distant countries. However, this is not the case. Columns 2 and 3 of Table 5 show the estimate obtained for applicants coming from countries above and below the median in terms of distance from Italy, respectively. Both coefficients remain statistically significant and very similar in magnitude to the baseline estimate, reported in column 1 of the same table (we cannot reject the null hypothesis that the three coefficients are identical).

However, the two samples in columns 2 and 3 may differ along several dimensions. For this reason, we next compare the effect of legal status on crimes committed in different periods of the year after click days. If the reduction in the crime rate of legalized applicants is driven by the fact that they leave Italy to collect the visa in the weeks after click days, there should be no difference between the crime rate of legalized and nonlegalized applicants in later periods of the year. Table 5 shows, instead, that the coefficient of legal status remains statistically significant when we progressively exclude the first quarter of 2008 (column 4), the first two quarters (column 5) and, finally, when we include only crimes committed in the last quarter (column 6). The annualized effect—obtained multiplying the 2SLS coefficient by 12 over the number of months included in the estimation—remains very similar to the baseline estimate for all the periods (such effect is reported in the table in square

TABLE 5—EFFECT OF LEGAL STATUS ON THE NUMBER OF CRIMES PER APPLICANT, ROBUSTNESS

	Baseline (1)	Distance from home		By period of year 2008			Red-hand (7)
		Low (2)	High (3)	Apr–Dec (4)	Jun–Dec (5)	Oct–Dec (6)	
Reduced form	−0.009 (0.003)	−0.008 (0.004)	−0.010 (0.005)	−0.005 (0.003)	−0.004 (0.002)	−0.002 (0.001)	−0.009 (0.003)
2SLS effect	−0.014 (0.006)	−0.014 (0.006)	−0.016 (0.008)	−0.009 (0.004)	−0.006 (0.004)	−0.004 (0.002)	−0.012 (0.005)
Annualized effect				[0.012]	[0.012]	[0.016]	
Size of bandwidth	8:17	5:01	10:39	8:39	8:44	7:53	7:32
Observations inside bandwidth	13,454	3,163	10,832	14,000	14,094	12,788	12,247

Notes: This table replicates the nonparametric estimates in Table 4 for different subsamples of type-A applicants and different definitions of crime events. Columns 2 and 3 distinguish between applicants from countries above and below the median in terms of distance from Italy, respectively. Columns 4 to 6 report the estimated effect when we progressively exclude crimes committed in the first trimesters of the year after click days. The dependent variable is thus a dummy equal to 1 for applicants that committed a serious crime during the period indicated on top of each column. Finally, column 7 reports the estimated effect when considering only those criminal cases in which the offender was arrested *in flagrante*. The specification replicates that in Table 4, the bandwidth is computed according to the CCT2014 criterion. Robust standard errors are reported in parentheses.

brackets). As an alternative way of displaying the stability of the effect, Figure 8 shows the coefficients and confidence intervals for each semester of 2007 and 2008. Overall, the evidence in Table 5 for different subsamples of applicants and different periods of the year seems to exclude the possibility that our results are driven by time spent outside Italy.

Measurement Error.—Our data are subject to potential systematic measurement error from two sources. First, charged crimes always underestimate committed crimes, as a number of crimes are not reported (MacDonald 2002). Second, offenders recorded by the police were matched with the data on click day applicants by name, surname, nationality, and year of birth, so any difficulty in identifying individuals subsequently recorded in either of the two archives could generate systematic errors.

To explore the implications of measurement error from these two sources, let $\tilde{C} = 1$ for immigrants reported by the police ($\tilde{C} = 0$ otherwise). Following the same notation of Section II, if measurement error is symmetric between regular and irregular immigrants, then $E(\tilde{C}|C = 1, L = 1) = E(\tilde{C}|C = 1, L = 0) = \mu \leq 1$. In this case, differences in reported crimes have the same sign of differences in the actual crime rates between the two groups, and bound their magnitude from below:

$$(4) \quad E(\tilde{C}|L = 1) - E(\tilde{C}|L = 0) = \mu[E(C|L = 1) - E(C|L = 0)].$$

Therefore, we should be concerned only with errors that affect differentially the measurement of the crime rate of regular and irregular immigrants.

The use of police charges (as opposed to incarceration rates) should eliminate concerns that our results reflect heterogeneous treatment of suspects by the judicial system—e.g., only legal immigrants can ask for alternative measures to incarceration, such as home detention. Despite this, police forces could still disproportionately prosecute irregular immigrants. If this was the case, our estimates would capture a

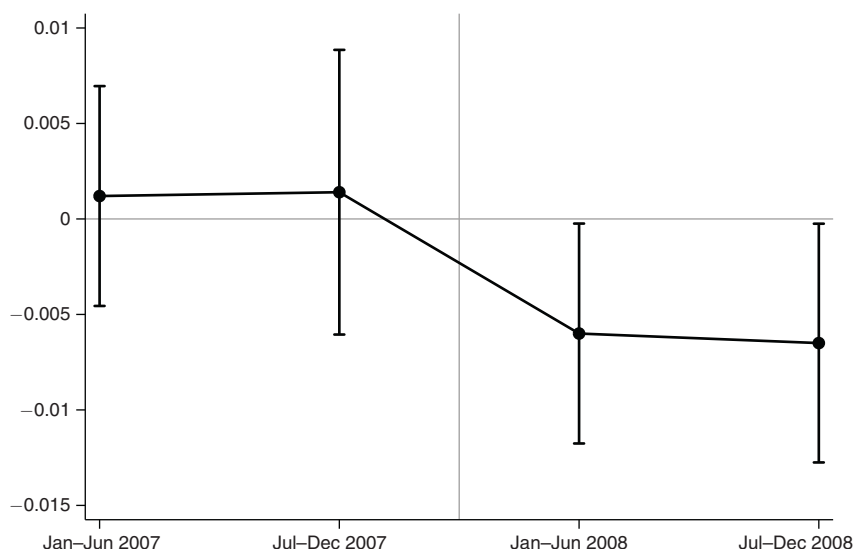


FIGURE 8. EFFECT OF LEGAL STATUS ON THE NUMBER OF CRIMES PER APPLICANT, BY PERIOD

Notes: The graph shows estimated coefficients and standard errors of 2SLS regressions for type-A applicants in each semester of the year before and after click days (2007 and 2008, respectively). The specification replicates that in Table 4, the bandwidth is computed according to the CCT2014 criterion.

change in the probability of being charged conditional on having been apprehended for a crime, as opposed to a change in the underlying crime rate.

To address this concern, we focus on offenders arrested *in flagrante*. As is typically the case for serious crimes, these comprise a very high share of the arrested offenders in our sample (82 percent). The decision to charge these individuals involved less discretion by the police—including considerations about their legal status—as they were caught in the act of committing the crime. When we restrict to this subset of crimes, the results are very similar to our baseline estimates (last column of Table 5).

Turning to the match between click day applications and criminal records, arrested individuals may *intentionally* misreport their identity in order to maintain a clean criminal record for their true identity, or to avoid the application of aggravating circumstances (if they are recidivists). However, according to Article 5 of Law 189/2002, immigrants must provide fingerprints and pictures when collecting the residence permit.³³ This greatly reduces the risk of false negatives for legalized immigrants. By contrast, no biometric information is available for irregular immigrants (unless they were previously incarcerated in Italy). Therefore, the probability of being reported conditional on having committed a crime should be systematically higher for the legalized than for the nonlegalized.³⁴

³³The text of the law is available, in Italian, at <http://www.camera.it/parlam/leggi/021891.htm>.

³⁴The Italian Ministry of Interior (2007, p. 359, my own translation) explicitly states that biometric information “makes the reliable identification of legal immigrants possible, even in cases where they had an interest in concealing their identity, or denying their legal status, for example by hiding or destroying documents, or by providing a false identity. In this sense, the law has made it more difficult to conceal their identity, with the effect of reducing the number of irregular immigrants over the total number reported and arrested.”

For all of these reasons, our data likely over-estimate the crime rate of legalized immigrants relative to that of irregular immigrants. Therefore, we can interpret the estimated negative coefficient of legal status as a lower bound to the magnitude of the true effect.

C. Channels

Two main conclusions can be drawn from the results obtained so far. First, legal status decreased the crime rate of immigrants between 2007 and 2008. Second, the average coefficient hides an extreme heterogeneity amongst applicants. In particular, the effect is high in magnitude and very precisely estimated for type-A applicants, whereas there is no effect on the crime rate of type-B applicants. All estimates are extremely stable when using different methods and specifications. As discussed in Section IB, the former type of applicants are characterized by worse labor market opportunities, with a large number of them being unemployed, whereas the latter are likely employed, although unofficially, by sponsor firms before click days.

A potential explanation for the absence of any effect on the crime rate of type-B applicants is thus that they have a very high opportunity cost of crime also before legalization. This cost would include the salary paid by the firm as well as the possibility of reapplying in the future in case the application in 2007 was not successful. By contrast, type-A applicants confront meager employment opportunities before click days. Nevertheless, legal status triggers a significant change in the behavior of type-A individuals, possibly because improved opportunities following the concession of legal status induce those that were previously detached from the labor market to search for a job. Put differently, the group of type-A applicants may include a large number of individuals at the margin between pursuing a criminal career and entering the labor market.

To further investigate the importance of labor market opportunities, we exploit additional heterogeneity in the type of offense and sponsor.³⁵ In particular, if legal status affects criminal behavior by changing the relative returns on legitimate activities and, moreover, violent crimes depend to a lesser extent on economic considerations, we would expect that the average effect is driven by economically motivated crimes (Machin and Meghir 2004). This is exactly the picture that emerges from Table 6. Distinguishing between economically motivated crimes (thefts, robberies, drug-trafficking, extortion, and smuggling) and purely violent crimes (murders and sex offenses), legal status only affects the former type of offenses.³⁶ This last result is consistent with previous evidence in Baker (2015); Freedman, Owens, and Bohn (2013); and Mastrobuoni and Pinotti (2015).

In Table 6 we also distinguish between applicants sponsored by foreign and native employers. As discussed in Section IB, fictitious job offers aimed at legalizing unemployed immigrants who otherwise would not qualify for a residence permit

³⁵ The estimates reported in this section are obtained using the parametric specification in column 6 of Table 3 (i.e., second-order polynomial regression controlling for lottery fixed effects and clustering standard errors by lottery). As was the case for the baseline estimates, the results obtained by nonparametric methods are very similar and are available upon request.

³⁶ We excluded two types of crime—kidnappings and illegal carrying of arms—that cannot be exclusively classified as either economically motivated or violent.

TABLE 6—HETEROGENEOUS EFFECTS OF LEGAL STATUS BY TYPE OF OFFENSE AND NATIONALITY OF THE SPONSOR

	Type-A applicants				Type-B applicants			
	Type of crime		Sponsor		Type of crime		Sponsor	
	Econ. (1)	Viol. (2)	Foreign (3)	Native (4)	Econ. (5)	Viol. (6)	Foreign (7)	Native (8)
2SLS effect of legal status	−0.009 (0.004)	−0.002 (0.002)	−0.015 (0.007)	−0.008 (0.007)	−0.000 (0.004)	−0.001 (0.002)	−0.003 (0.006)	0.002 (0.008)
Observations	40,451	40,386	23,390	16,859	69,886	69,840	30,441	39,308

Notes: This table reports parametric 2SLS estimates of the effect of legal status on the crime rate of click day applicants in the year after click days by type of applicant, offense, and nationality of the sponsor. The dependent variable is a dummy $C = 1$ for individuals committing at least one serious offense in 2008. The explanatory variable of interest is a dummy $L = 1$ for those obtaining legal status in 2008, and the first-stage instrument is a dummy $Z = 1$ for having applied before the cutoff time. All regressions control for a quadratic polynomial in the time elapsed since the cutoff (by the millisecond) and its interaction with Z , lottery-fixed effects, and a quadratic polynomial in age. Robust standard errors clustered by lottery are reported in parentheses.

typically come through type-A applications sponsored by other immigrants. Indeed, the negative coefficient estimated across type-A applicants is entirely driven by this subsample of individuals.³⁷

IV. Conclusions

Irregularity imposes a heavy toll on foreign immigrants in terms of poorer employment opportunities, lower incomes, and lower access to social services. These conditions lower the opportunity cost of crime, potentially increasing the propensity of irregular immigrants to engage in crime. The present paper documents this effect for immigrants applying for a residence permit in Italy.

We believe that our results can inform the current debate on immigration policy. Immigration amnesty is near top of the agenda both in the United States and in Europe due to the large presence of undocumented immigrants. The present paper highlights a potentially important payoff from amnesty programs, namely, a reduction in the number of crimes committed in a destination country. The heterogeneity in the effect across different types of applicants also sheds light on which groups would be most responsive to legalization—those with worse employment opportunities when irregular.

Of course, these conclusions are subject to a number of caveats. First of all, the results obtained for Italy cannot be immediately generalized to countries characterized by a very different institutional context. In particular, undocumented immigrants in the United States confront a higher risk of being expelled from the country conditional upon committing an offense, which may lower their propensity to engage in crime. Indeed, Butcher and Piehl (2007) argue that selective deportations are the main reason why immigrants exhibit *lower* incarceration rates than

³⁷In a companion paper, Pinotti (2015), we examine an additional dimension of heterogeneity, namely how the effect of legalization varies with the enforcement of migration restrictions. To this purpose, we compare the effect between applicants coming from countries that signed and did not sign a bilateral agreement with Italy for the enforcement of immigration restrictions.

natives in the United States, contrary to what happens in most other countries. In particular, in all European countries immigrants are generally characterized by a high crime rate relative to natives, limited access to employment opportunities, and a very low risk of expulsion.

Second, we emphasize the role of improvements in employment opportunities and wages as a likely driver of changes in criminal behavior by newly legalized immigrants—in light of previous evidence on the effects of legal status along these dimensions. In principle, legal status could affect the opportunity cost of crime through other channels, notably access to social welfare. In practice, however, undocumented immigrants in Italy have access (with the same conditions as legal residents) to essential services such as basic healthcare and schooling; at the same time, no universal income support scheme exists in Italy (not even for legal residents). For both reasons, legal status should have rather limited effects on this additional dimension.

Finally, each legalization episode potentially generates expectations of further amnesties in the future and thus may lead to higher migration pressures in subsequent years. Although previous studies have not found clear evidence of such effects following amnesty programs in the United States (Donato, Durand, and Massey 1992; Orrenius and Zavodny 2003), a complete evaluation of legalization policies would need to take this possibility into account. We leave this issue to further research.

REFERENCES

- Accetturo, Antonio, and Luigi Infante. 2010. "Immigrant Earnings in the Italian Labour Market." *Giornale degli Economisti e Annali di Economia* 69 (1): 1–28.
- Alonso-Borrego, Cesar, Nuno Garoupa, and Pablo Vázquez. 2012. "Does Immigration Cause Crime? Evidence from Spain." *American Law and Economics Review* 14 (1): 165–91.
- Amuedo-Dorantes, Catalina, Cynthia Bansak, and Steven Raphael. 2007. "Gender Differences in the Labor Market: Impact of IRCA's Amnesty Provisions." *American Economic Review* 97 (2): 412–16.
- Andrews, Donald W. K. 1993. "Tests for Parameter Instability and Structural Change with Unknown Change Point." *Econometrica* 61 (4): 821–56.
- Angrist, Joshua D., Sarah R. Cohodes, Susan M. Dynarski, Parag A. Pathak, and Christopher R. Walters. 2016. "Stand and Deliver: Effects of Boston's Charter High Schools on College Preparation, Entry, and Choice." *Journal of Labor Economics* 34 (2): 275–318.
- Baker, Scott R. 2015. "Effects of Immigrant Legalization on Crime." *American Economic Review* 105 (5): 210–13.
- Becker, G. S. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy* 76: 169–217.
- Bell, Brian, Francesco Fasani, and Stephen Machin. 2013. "Crime and Immigration: Evidence from Large Immigrant Waves." *Review of Economics and Statistics* 95 (4): 1278–90.
- Bell, Brian, and Stephen Machin. 2013. "Crime and Investigation: What Do We Know?" In *Lessons from the Economics of Crime: What Reduces Offending?*, edited by Philip J. Cook, Steve Machin, Olivier Marie, and Giovanni Mastrobuoni, 149–74. Cambridge, MA: MIT Press.
- Bertrand, Marianne, Rema Hanna, and Sendhil Mullainathan. 2010. "Affirmative Action in Education: Evidence from Engineering College Admissions in India." *Journal of Public Economics* 94 (1–2): 16–29.
- Bianchi, Milo, Paolo Buonanno, and Paolo Pinotti. 2012. "Do Immigrants Cause Crime?" *Journal of the European Economic Association* 10 (6): 1318–47.
- Blangiardo, Giancarlo. 2008. "The Centre Sampling Technique in Surveys on Foreign Migrants: The Balance of a Multi-year Experience." United Nations Statistical Commission and EUROSTAT Working Paper 12.
- Borjas, George J., Jeffrey Grogger, and Gordon H. Hanson. 2010. "Immigration and the Economic Status of African-American Men." *Economica* 77 (306): 255–82.

- Butcher, Kristin F., and Anne M. Piehl.** 1998. "Cross-City Evidence on the Relationship between Immigration and Crime." *Journal of Policy Analysis and Management* 17 (3): 457–93.
- Butcher, Kristin F., and Anne Morrison Piehl.** 2007. "Why Are Immigrants' Incarceration Rates so Low? Evidence on Selective Immigration, Deterrence, and Deportation." National Bureau of Economic Research Working Paper 13229.
- Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik.** 2014. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica* 82 (6): 2295–326.
- Caponi, Vincenzo, and Miana Plesca.** 2014. "Empirical Characteristics of Legal and Illegal Immigrants in the USA." *Journal of Population Economics* 27 (4): 923–60.
- Card, David, Alexandre Mas, and Jesse Rothstein.** 2008. "Tipping and the Dynamics of Segregation." *Quarterly Journal of Economics* 123 (1): 177–218.
- Chay, Kenneth Y., Patrick J. McEwan, and Miguel Urquiola.** 2005. "The Central Role of Noise in Evaluating Interventions that Use Test Scores to Rank Schools." *American Economic Review* 95 (4): 1237–58.
- Chen, Aiyou, Thomas Bengtsson, and Tin Kam Ho.** 2009. "A Regression Paradox for Linear Models: Sufficient Conditions and Relation to Simpson's Paradox." *American Statistician* 63 (3): 218–25.
- Cohn, Jeffrey, and Jeffrey Passel.** 2009. *A Portrait of Unauthorized Immigrants in the United States*. Washington, DC: Pew Hispanic Center.
- Cuttitta, Paolo.** 2008. "Yearly Quotas and Country-Reserved Shares in Italian Immigration Policy." *Migration Letters* 5 (1): 41–51.
- Dahl, Gordon B., Katrine V. Loken, and Magne Mogstad.** 2014. "Peer Effects in Program Participation." *American Economic Review* 104 (7): 2049–74.
- Donato, Katharine M., Jorge Durand, and Douglas S. Massey.** 1992. "Stemming the Tide? Assessing the Deterrent Effects of the Immigration Reform and Control Act." *Demography* 29 (2): 139–57.
- Dustmann, Christian, Francesco Fasani, and Biagio Speciale.** Forthcoming. "Illegal Migration and Consumption Behavior of Immigrant Households." *Journal of the European Economic Association*.
- Ehrlich, Isaac.** 1973. "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation." *Journal of Political Economy* 81 (3): 521–65.
- Fredriksson, Peter, Bjorn Öckert, and Hessel Oosterbeek.** 2013. "Long-Term Effects of Class Size." *Quarterly Journal of Economics* 128 (1): 249–85.
- Freedman, Matthew, Emily Owens, and Sarah Bohn.** 2013. "Immigration, Employment Opportunities and Criminal Behavior." <http://www.law.umich.edu/centersandprograms/lawandeconomics/workshops/Documents/Paper%209.Owens.Immigration,%20Employment%20Opportunities,%20and%20Criminal%20Behavior.pdf> (accessed June 10, 2014).
- Gelman, Andrew, and Guido Imbens.** 2014. "Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs." National Bureau of Economic Research Working Paper 20405.
- Hahn, Jinyong, Petra Todd, and Wilbert Van der Klaauw.** 2001. "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." *Econometrica* 69 (1): 201–09.
- Hofer, Michael, Nancy Rytina, and Bryan C. Baker.** 2012. *Estimates of the Unauthorized Immigrant Population Residing in the United States: January 2011*. Washington, DC: US Department of Homeland Security.
- Imbens, Guido, and Karthik Kalyanaraman.** 2012. "Optimal Bandwidth Choice for the Regression Discontinuity Estimator." *Review of Economic Studies* 79 (3): 933–59.
- ISMU Foundation.** 2015. "Stima presenze irregolari—Anni 1991–2013." ISMU Foundation.
- Italian Ministry of Interior.** 2007. "Rapporto sulla criminalità in Italia. Analisi, Prevenzione, Contrasto." http://www1.interno.gov.it/mininterno/export/sites/default/it/assets/files/14/0900_rapporto_criminalita.pdf.
- Italian Ministry of Interior.** 2009. "Secondo rapporto sull'Attività dei Consigli Territoriali per l'Immigrazione." http://www.cnel.it/application/xmanager/projects/cnel/attachments/shadow_documentazioni_attachment/file_allegatos/000/142/180/0994_Consigli_immigrazione_rapporto_2.pdf.
- Italian Senate.** 2011. "Informativa del Ministro dell'Interno sulla Regolamentazione di Lavoratori Extracomunitari e sui Flussi di Ingresso." <http://www.senato.it/service/PDF/PDFServer/BGT/00522262.pdf>.
- Kaushal, Neeraj.** 2006. "Amnesty Programs and the Labor Market Outcomes of Undocumented Workers." *Journal of Human Resources* 41 (3): 631–47.
- Kossoudji, Sherrie A., and Deborah A. Cobb-Clark.** 2002. "Coming Out of the Shadows: Learning about Legal Status and Wages from the Legalized Population." *Journal of Labor Economics* 20 (3): 598–628.
- Lee, David S.** 2008. "Randomized Experiments from Non-random Selection in U.S. House Elections." *Journal of Econometrics* 142 (2): 675–97.

- Lee, David S., and Thomas Lemieux.** 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48 (2): 281–355.
- Lozano, Fernando A., and Todd A. Sørensen.** 2011. "The Labor Market Value to Legal Status." Institute for the Study of Labor Discussion Paper 5492.
- MacDonald, Ziggy.** 2002. "Official Crime Statistics: Their Use and Interpretation." *Economic Journal* 112 (477): F85–106.
- Machin, Stephen, and Costas Meghir.** 2004. "Crime and Economic Incentives." *Journal of Human Resources* 39 (4): 958–79.
- Mastrobuoni, Giovanni, and Paolo Pinotti.** 2015. "Legal Status and the Criminal Activity of Immigrants." *American Economic Journal: Applied Economics* 7 (2): 175–206.
- McCrary, Justin.** 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics* 142 (2): 698–714.
- Orrenius, Pia M., and Madeline Zavodny.** 2003. "Do Amnesty Programs Reduce Undocumented Immigration? Evidence from IRCA." *Demography* 40 (3): 437–50.
- Pinotti, Paolo.** 2015. "Immigration Enforcement and Crime." *American Economic Review* 105 (5): 205–09.
- Pinotti, Paolo.** 2017. "Clicking on Heaven's Door: The Effect of Immigrant Legalization on Crime: Dataset." *American Economic Review*. <https://doi.org/10.1257/aer.20150355>.
- Rubin, Donald B.** 1974. "Estimating Causal Effects of Treatments in Randomized and Nonrandomized Studies." *Journal of Educational Psychology* 66 (5): 688–701.
- Rytina, Nancy.** 2012. *Estimates of the Legal Permanent Resident Population in 2011*. Washington, DC: US Department of Homeland Security.
- Sciortino, Giuseppe.** 1991. "Immigration into Europe and Public Policy: Do Stops Really Work?" *Journal of Ethnic and Migration Studies* 18 (1): 89–99.
- Spenkuch, Jorg L.** 2014. "Understanding the Impact of Immigration on Crime." *American Law and Economics Review* 16 (1): 177–219.
- Wasem, Ruth Ellen.** 2008. *Immigration Fraud: Policies, Investigations, and Issues*. Washington, DC: Congressional Research Service.

This article has been cited by:

1. Christian Gunadi. 2020. Does immigrant legalization affect crime? Evidence from deferred action for childhood arrivals in the United States. *Journal of Economic Behavior & Organization* **178**, 327-353. [[Crossref](#)]
2. Minghui Fu, Chuanjiang Liu, Mian Yang. 2020. Effects of public health policies on the health status and medical service utilization of Chinese internal migrants. *China Economic Review* **62**, 101464. [[Crossref](#)]
3. Sandra V. Rozo, Therese Anders, Steven Raphael. 2020. Deportation, crime, and victimization. *Journal of Population Economics* **14**. . [[Crossref](#)]
4. Moritz Marbach, Dominik Hangartner. 2020. Profiling Compliers and Noncompliers for Instrumental-Variable Analysis. *Political Analysis* **28**:3, 435-444. [[Crossref](#)]
5. Paolo Pinotti. 2020. The Credibility Revolution in the Empirical Analysis of Crime. *Italian Economic Journal* **6**:2, 207-220. [[Crossref](#)]
6. Stefano Comino, Giovanni Mastrobuoni, Antonio Nicolò. 2020. Silence of the Innocents: Undocumented Immigrants' Underreporting of Crime and their Victimization. *Journal of Policy Analysis and Management* **32**. . [[Crossref](#)]
7. Oana Borcan. 2020. The Illicit Benefits of Local Party Alignment in National Elections. *The Journal of Law, Economics, and Organization* **64**. . [[Crossref](#)]
8. Aaron Chalfin, Monica Deza. 2020. Immigration enforcement, crime, and demography. *Criminology & Public Policy* **19**:2, 515-562. [[Crossref](#)]
9. Oded Stark, Lukasz Byra. 2020. Can a deportation policy backfire?. *Public Choice* **183**:1-2, 29-41. [[Crossref](#)]
10. Natalia Guerrero, Oswaldo Molina, Diego Winkelried. 2020. Conditional cash transfers, spillovers, and informal health care: Evidence from Peru. *Health Economics* **29**:2, 111-122. [[Crossref](#)]
11. Catalina Amuedo-Dorantes, Cynthia Bansak, Susan Pozo. 2020. Refugee Admissions and Public Safety: Are Refugee Settlement Areas More Prone to Crime?. *International Migration Review* 019791832092019. [[Crossref](#)]
12. Yang Zhou, Rong Ma, Yun Su, Libo Wu. 2019. Too big to change: How heterogeneous firms respond to time-of-use electricity price. *China Economic Review* **58**, 101342. [[Crossref](#)]
13. Christian Gunadi. 2019. On the association between undocumented immigration and crime in the United States. *Oxford Economic Papers* **113**. . [[Crossref](#)]
14. Francesco Fasani, Giovanni Mastrobuoni, Emily G. Owens, Paolo Pinotti. Does Immigration Increase Crime? **21**, . [[Crossref](#)]
15. Alexia Lochmann, Hillel Rapoport, Biagio Speciale. 2019. The effect of language training on immigrants' economic integration: Empirical evidence from France. *European Economic Review* **113**, 265-296. [[Crossref](#)]
16. Elizabeth U. Cascio, Ethan G. Lewis. 2019. Distributing the Green (Cards): Permanent residency and personal income taxes after the Immigration Reform and Control Act of 1986. *Journal of Public Economics* **172**, 135-150. [[Crossref](#)]
17. José R. Bucheli, Matias Fontenla, Benjamin James Waddell. 2019. Return migration and violence. *World Development* **116**, 113-124. [[Crossref](#)]
18. Sergei Guriev, Biagio Speciale, Michele Tuccio. 2019. How do Regulated and Unregulated Labor Markets Respond to Shocks? Evidence from Immigrants During the Great Recession. *The Journal of Law, Economics, and Organization* **35**:1, 37-76. [[Crossref](#)]

19. Moritz Marbach, Dominik Hangartner. 2019. Profiling Compliers and Non-compliers for Instrumental Variable Analysis. *SSRN Electronic Journal* . [[Crossref](#)]
20. Zachary Liscow, William Gui Woolston. 2018. Does Legal Status Matter for Educational Choices? Evidence from Immigrant Teenagers. *American Law and Economics Review* **114**. . [[Crossref](#)]
21. . Longer-Term Dynamics: Immigrant Economic Adjustment and Native Responses 189-231. [[Crossref](#)]
22. Francesco Fasani. 2018. Immigrant crime and legal status: evidence from repeated amnesty programs. *Journal of Economic Geography* **105**. . [[Crossref](#)]
23. Matthew Freedman, Emily Owens, Sarah Bohn. 2018. Immigration, Employment Opportunities, and Criminal Behavior. *American Economic Journal: Economic Policy* **10**:2, 117-151. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
24. Matteo Gamalerio, Mario Luca, Max Viskanic. 2018. Finding the Warmth of Other Suns? Refugee Reception, Extreme Votes and Hate Crimes. *SSRN Electronic Journal* . [[Crossref](#)]