Clicking on Heaven's Door: The Effect of Immigrant Legalization on Crime*

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October 2014

Abstract

We estimate the effect of legalization on the crime rate of immigrants by exploiting the Italian policy framework as an ideal regression discontinuity design: fixed quotas of residence permits are available each year, applications must be submitted electronically on specific "Click Days", and they are processed on a first-come, first-served basis until the available quotas are exhausted. By matching data on applicants – including the timing of applications in milliseconds – with restricted-use criminal records for the year before and after Click Days, we show that obtaining legal status reduces the probability of being reported for having committed a serious crime by 0.6 percentage points on average, on a baseline crime rate of 1 percent. This effect is driven by applicants that are allegedly employed by other immigrants as domestic workers (but are most likely unemployed) who exhibit the highest crime rate in the year before Click Days as well as the greatest reduction in crime after obtaining legal status. By contrast, applicants sponsored by firms exhibit a very low crime rate both before and after Click Days. We conclude that undocumented immigrants confronting worse labor market opportunities are at a higher risk of committing crimes, while they are also more responsive to legalization.

keywords: legal status, crime, regression discontinuity design
JEL codes: J61, K37, K42

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1 Introduction

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 residents (Hoefer et al., 2012a, Hoefer et al., 2012b). The share of illegals is also high in several European countries, such as Italy, Greece, Portugal, and Spain.[1]

Both in the US and in Europe, illegal immigrants cannot officially be employed or start a new economic activity. Such immigrants can work only in the informal economy and thus receive considerably lower earnings than legal immigrants (Kossoudji and Cobb-Clark, 2002, Kaushal, 2006, Amuedo-Dorantes et al., 2007, Accetturo and Infante, 2010, and Lozano and Sorensen, 2011). According to the Becker-Ehrlich model of crime, a lower income from legitimate activities means a lower opportunity cost of – and thus a higher propensity to participate in – illicit activities.

As a matter of fact, the presence of large illegal populations raises crime concerns in destination countries. According to an annual survey conducted in North American and European countries, approximately two-thirds of the people interviewed were concerned that illegal immigrants increase crime, whereas only one-third holds similar concerns about legal immigrants (Transatlantic Trends, 2009). Moving from perceptions to criminal statistics, illegals constitute between 20% and 30% of all immigrants in Italy, but they represent 80% of those arrested for serious crimes (Italian Ministry of Interior, 2007).

However, these figures may reflect the different composition of the two groups, as opposed to the (causal) effect of legal status. In particular, illegal immigrants in all countries are typically young, single males and are less educated than legal immigrants (Cohn and Passel, 2009, Caponi and Plesca, 2013, and Mastrobuoni and Pinotti, 2014). 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 illegally in the country and to commit crimes. It is thus difficult to identify the causal effect of legal status on the number of crimes committed by immigrants in the destination country.

The institutional framework in Italy provides an ideal Regression Discontinuity (RD) design

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

that allows this effect to be estimated separately from the selection into legal status. The primary method of acquiring legal status in Italy is through work-related residence permits sponsored by the immigrant's (prospective) employer. Fixed quotas of permits are available each year for different groups of applicants defined by the type of employer and occupation, nationality, and Italian province, for a total of 1751 (potential) groups. Applications must be submitted online by employers starting on a few "Click Days" of the year, which vary by category of applicant, and they are processed on a first-come, first-served basis within each group until the exhaustion of the available quota of permits. The rationing of quotas 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.[2]

In this paper, we match all male immigrants sponsored on Click Days 2007 – including the timing of the application in milliseconds – with restricted-use data on all male foreigners reported by the police for having committed a serious crime in Italy and compare the crime rates in the year before and the year after Click Days between those 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 immigrants applying within a narrow bandwidth of the cutoff were unable to *precisely* determine the 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 is equal to 1 percent in both groups (i.e., 1 in 100 individuals reported for serious crimes).

Turning to the year after Click Days, controlling parametrically for a smooth polynomial in the timing of application the crime rate declined by 0.3 percentage points for immigrants who applied before the cutoff whereas no significant change was observed for other applicants. Since the difference in the probability of obtaining legal status between these two groups is

[2]Although applications are materially 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.

about 50 percentage points, the two-stage-least-squares (2SLS) estimated effect of legal status is a 0.6 percentage-point decrease in the crime rate of legalized applicants (the magnitude is larger when restricting non-parametrically to a narrow bandwidth of the cutoff time).

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 – but are nevertheless sponsored through fictitious job offers. Such individuals exhibit a higher crime rate before the 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 (0.6 percent). Based on these findings, we conclude that immigrants in the latter group experience a higher opportunity cost of crime due to the fact they are already (unofficially) employed in the sponsor firm before Click Days. At the opposite, male immigrants sponsored by other immigrants as domestic workers in most cases have neither a job nor a real job offer, so their opportunity cost of committing a crime is very low before the Click Days. At the same time, they seem more responsive to legalization, possibly because legal status partly addresses detachment from the labor market. In particular, obtaining legal status opens up the possibility of searching for a job in the official sector. 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 non-parametric regressions. In particular, the coefficient of interest is largely unaffected when varying the degree of the polynomial in the running variable (i.e., 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 groups of applicants competing for different quotas of permits. This last result suggests that the estimated coefficient of legal status is not biased by heterogeneity in the baseline crime rate between 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 allows us to exclude that the negative coefficient of legal status is explained by differences in the under-reporting of crimes committed by legal and illegal immigrants. If anything, the strategic misreporting of identity, which should be easier for illegal immigrants that do not hold official documents, should entail an opposite bias.

This paper contributes to the growing body of evidence regarding the effect of immigration on crime.[3] Earlier work by Butcher and Piehl (1998) shows no evidence that immigration affected crime across US counties during the 1980s, whereas Spenkuch (2014) reaches a different conclusion for subsequent periods. Borjas et al. (2010) also find that immigration increases crime, although only indirectly (by raising the crime rate of native black males).

As for other countries, Bianchi et al. (2012) show that the causal effect across Italian provinces is not significantly different from zero, while Alonso-Borrego et al. (2012) obtain an opposite result across Spanish provinces. Finally, Bell et al. (2013) distinguish between the effect of two large immigrant waves in the UK, namely, asylum seekers and the post-2004 inflow from EU accession countries. Interestingly, only the former group, which was characterized by limited economic opportunities in the official labor markets, caused a significant increase in (property) crime. This last result suggests that average estimates across geographical areas may mask a significant degree of heterogeneity in the effect of different groups of immigrants. Indeed, the propensity to commit crimes should differ across immigrants depending on individual characteristics, such as age, gender, and education, in the same manner as for natives. However, immigrants differ with respect to another important factor: legal status.

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 illegal immigrants in the US. Using the distance from the ports of entry and 1960 immigrants' enclaves as instruments for the presence of IRCA applicants, Baker (2013) shows that higher legalization rates caused a reduction in crime across US counties. At the same time, the IRCA enforced stronger control over the hiring of illegal immigrants, creating obstacles to the employment of

[3]Bell and Machin (2013) provide a survey of this literature.

those who were not legalized. Freedman et al. (2013) focus on the 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 (2014), 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% to 2.3% 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.

Relative to these previous studies, the present analysis takes advantage of a clean quasi-experimental design. Under the assumption of no manipulation of the assignment variable, in fact, 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. At the same time, the institutional details of Italian migration policy allow 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 3 discusses the empirical strategy and the data. Section 4 presents the main results. Finally, Section 5 concludes the paper with some policy implications as well as some caveats of our analysis.

2 Institutional framework

Italian migration policy, as designed by Laws 40/1998 and 189/2002, links legal residence to (official) employment in the country. Each year, the central government establishes the total number of residence permits available during the following year through the so-called Flows Decree ("Decreto Flussi"). Almost all permits are reserved for employees sponsored by Italian native citizens or legal foreign residents willing to hire immigrant workers *before* they enter Italy.[4] Each applicant can be sponsored only by one employer and only for a specific job.

However, the matching of vacancies with foreign workers living abroad is extremely difficult (if not impossible), so most immigrants enter Italy illegally, begin working unofficially, and wait for their employers to sponsor them at the following Flows Decree. During this period, they are subject to removal by immigration authorities, although such risk is relatively low. Due to the cost of enforcing deportations, most immigrants apprehended by the police for being illegally present in Italy receive just an injunction to leave the country.

Indeed, the rise in the number of official foreign residents in Italy over the last two decades (from 600,000 at the beginning of the 1990s to 4.5 million in 2011) parallels that of unofficial immigrants. Although no official estimates have been produced, the number of applications for amnesty presented during the frequent legalization episodes provides a lower bound for the size of the illegal immigrant population. The number of applications grew from 100,000 to 250,000 between 1986 and the 1990s, reaching a peak of 700,000 in 2002. Combining this information with data on the number of removals actually enforced by the Italian police during the period 2004-2007, Dustmann et al. (2010) estimate the risk of deportation (conditional on being illegally present in Italy) to be approximately 5 percent.

In the event of obtaining a residence permit, immigrants start to work (officially) for their sponsor employers and are allowed to reside in Italy for that purpose. They are *not* allowed, on the other hand, to travel or reside in other European countries (or, for that matter, in any other country). In this respect, immigrants acquiring legal status through a residence permit differ from new EU-member citizens, who enjoy free mobility within the European Union (see Mastrobuoni and Pinotti, 2014).

Importantly, successful applicants have full access to the official labor market in Italy, meaning that they are not stacked with the sponsor employer: they can search for other jobs, start a new business, and so on. If they remain unemployed, they have six months to find another occupation, after which period they become illegal and should leave the country. Again, most of these individuals would remain unofficially in Italy upon the expiration of the permit.

[4]Special permits are allowed for students as well as for the spouses and children of permit-holders.

As for employers, their main incentive in sponsoring applicants on Click Days is avoiding future sanctions, although such sanctions were very mild in 2007.[5] To encourage exit from informality, Click Day applications were never used to track illegal immigrants or their sponsor employers, despite of the fact that a number of anomalies and irregularities frequently emerge, particularly in relation to male immigrants allegedly employed as domestic workers. Before turning to such anomalies, we describe the quota system governing the allocation of permits across different groups of applicants.

2.1 The quota system

Immigrants can be sponsored for two main types of permits: type-A permits for domestic workers employed by individuals and families as housekeepers, caregivers, and babysitters, and type-B permits, for non-domestic workers employed by firms. The latter type of permits is further distinguished into B1 and B2 for workers employed in the construction or non-construction sectors, respectively. Specific national quotas without distinction by type of permit are reserved for immigrants coming from 14 "privileged" countries that subscribe to bilateral agreements to control illegal migration.

Column (1) of Table 1 shows the quotas fixed by the Flow Decree 2007 for year 2008. Of 170,000 permits, slightly less than three quarters were awarded to immigrants of non-privileged nationalities, divided almost equally between type-A and type-B permits. The remaining 44,600 permits were reserved for privileged nationalities, which accounted for more than half of all applications. Contrary to the intentions of the policy-maker, such nationalities thus faced a tighter rationing of residence permits relative to non-privileged nationalities, the ratio of quotas over applications being 13 and 49 percent for the former and the latter group, respectively (see the last column of Table 1).[6]

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). Figure 1 plots the number of permits available in each province compared with the number of applications re-

[5]The discipline was strengthened in later years in response to a European directive of 2009.

[6]Beginning with the Flows Decree 2009, however, the quotas for the non-privileged nationalities were always set at a very low level.

Table 1: Total quotas fixed by the Flows Decree 2007 and applications for residence permits received

	(1) **quota**	(2) **applications**	(3) **ratio**
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
Morocco	4,500	97,079	0.05
Moldova	6,500	31,286	0.21
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 fixed by the Flows Decree 2007 for different groups of applicants, the number of applications received, and the ratio of quotas to applications.

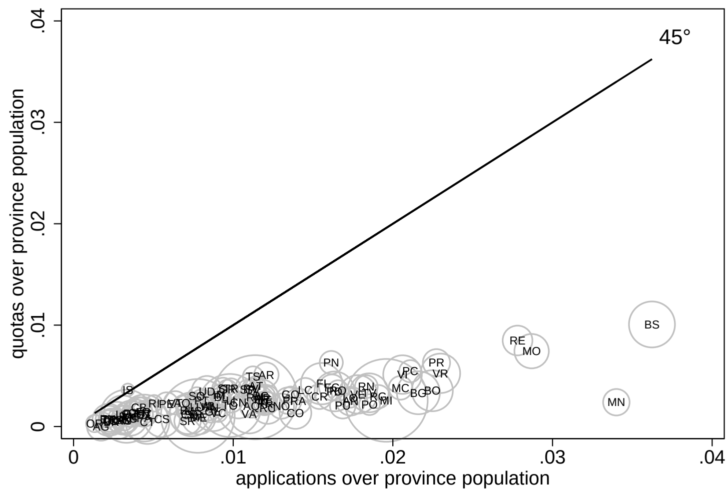
ceived. The markers for all provinces lie below the 45-degree line, indicating that quotas fall short of demand for permits almost everywhere, often by a large number. In Milan, for instance, the number of applicants was 10 times greater than the number of available quotas.

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".

2.2 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 be used to legalize immigrants that would not qualify for a residence permit. The existence of a black market for residence permits has long been recognized

Figure 1: Quotas fixed by the Flows Decree 2007 and applications for residence permits received across Italian provinces



Note: This graph plots the quotas for residence permits allocated to each province by the Flows Decree 2007 and the number of applications received, both divided by province population. The size of markers is proportional to the total province population; the 45-degree line is also included in the graph.

in Italy as well as in many other countries (see Sciortino, 1991, and Wasem, 2008, for an exhaustive account of the Italian and the US cases, respectively). Immigrants that are illegally present in Italy but did not receive a job offer may find individuals or organizations willing to sponsor them against 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 a minimum of 800 euros per month – a very low threshold, indeed.

There is extensive anecdotal evidence of this type of fraud. In 2010, the Corriere della Sera – a leading Italian newspaper – noticed "The Strange Case of Chinese Housekeepers. Where do they work, who hired them, who ever saw them in Italy? Yet, the final data on Click Days uncover 33,000 domestic workers from the People's Republic (...) An anomalous figure indeed: twice as much the number of Ukrainians, who usually work in this occupation (...) A contract as housekeeper is the only way to enter in Italy, it is easier to obtain through family and friends" (March 11, 2011). As for the sponsors, "One Out of Three Chinese People Wants the Housekeeper. The Flows Decree 2010 speaks Chinese. According to the data, 1-in-3 Chinese people – including the under-age! – applied to hire (and, thus, to legalize) a housekeeper"

(February 16, 2011). Similar anomalies are routinely reported by the Italian press after every Click Day.[7]

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 yearly since 2001 by the NGO "Iniziative e Studi sulla Multietnicitá" on cross-sections of about 9,000 individuals per year, including both legal and illegal immigrants. The sampling of illegal immigrants is based on social networks around a number of "aggregation centers," such as 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).[8]

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 when expanding this figure to include illegal immigrants; see the first two rows of Table 2. Compared with these figures, the incidence of type-A applicants on Click Days in the same year is clearly abnormal, amounting to 40.9 percent of all male applicants; see the second row of the table. The difference is not explained by the fact that the ISMU survey covers just Lombardy, as it even increases when we consider only Click Day applications from the same region (the last row of the table).[9]

Figure 2 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).

Finally, male immigrants sponsored in type-A application permits are also anomalous in

[7]Baker (2013) discusses similar evidence for applications by agricultural workers in the United States at the time of the IRCA.

[8]Lombardy is located in the northwest of Italy, and it is the largest of 20 regions in terms of both population and GDP – approximately 16% and 21% of the total, respectively – and the second-richest one in terms of GDP per capita. The region hosts more than 1 million (legal) immigrants, or one-fourth of all immigrants present in Italy. More information on the ISMU survey is available from the website of the Foundation, www.ismu.org.

[9]The incidence of type-A applications among female applicants at Click Days is also abnormal; 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, only Lombardy	0.181	0.025	0.431
ISMU, only Lombardy and illegal immigrants	0.234	0.041	0.522
Click Day, all regions	0.562	0.409	0.829
Click Day, only Lombardy	0.589	0.461	0.844

Note: This table compares the incidence of domestic workers in the ISMU survey and among Click Day applicants in year 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 illegal immigrants in the same survey. The third and fourth rows report the fraction of type-A applicants among all applicants at Click Days and those among the applicants in the region of Lombardy, respectively.

terms of individual characteristics, as they appear to be much younger than domestic workers in the ISMU survey, while the age distribution for females is very similar between the two data sets; see Figure 3.

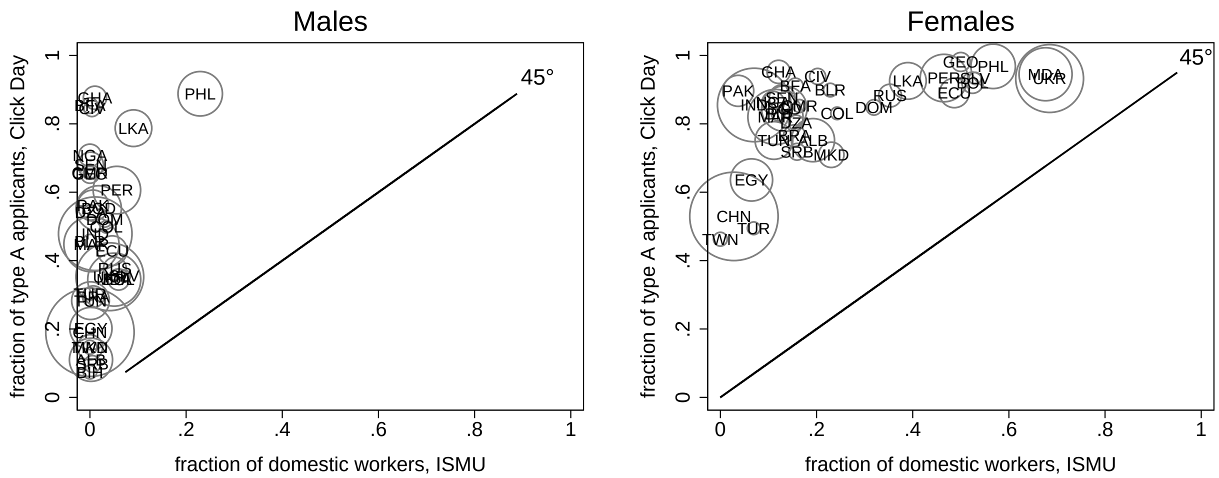
Overall, both anecdotal evidence and statistical evidence suggest that type-A applicants may include a large number of unemployed individuals disguised as domestic workers. This fact will be important for interpreting heterogeneity in the effect of legal status across different lotteries.

2.3 The regression discontinuity design

Although the quota system has been in place since 1998, the application procedure was completely digitized in 2007. Beginning in that year, applications must be sent by employers through the Internet during specific Click Days each year. In particular, 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 on such Click Days, employers are able to log in to the website of the Ministry of the Interior and sponsor one or more immigrants for a residence permit. 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 information about the employer with administrative archives; only for the sponsors of type-B applicants, such archives include the electronic registries of firms and self-employees maintained by UNIONCAMERE. If the application is accurate and complete, the residence permit is issued; however, if some

Figure 2: Fraction of domestic workers among employed immigrants in the ISMU survey and type-A applicants on Click Days by gender and nationality



Note: 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 at 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.

of the information is missing, inaccurate, or manifestly fraudulent, the application is rejected. The process continues until the quota of permits available for that lottery is exhausted.

Although in 2007 the mechanism was surrounded by some uncertainty, due to the fact that it was being adopted for the first time, everybody was aware of the "first-come, first-served" rule. This is evident by looking at the frequency of applications received during Click Days: in all lotteries, most applications were received in the very first minutes of the day.

Figure 4 shows two examples. The top 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% 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 was flooded with hundreds of applications per minute, and by 9:40 am the flow had already decreased to nearly zero.

Turning to the ratio of accepted applications, also shown in the graph, it is initially very high, as only the applications deemed fraudulent or incomplete by immigration officers were rejected. However, the probability of success decreases to zero when the entire quota assigned to this group is exhausted, less than half an hour after the start of the lottery; to be precise,

Figure 3: Age distribution for domestic workers in the ISMU survey and type-A applicants on Click Days, by gender.

Note: The graph shows the age distribution of domestic workers in the ISMU survey and applicants at Click Days, by gender.

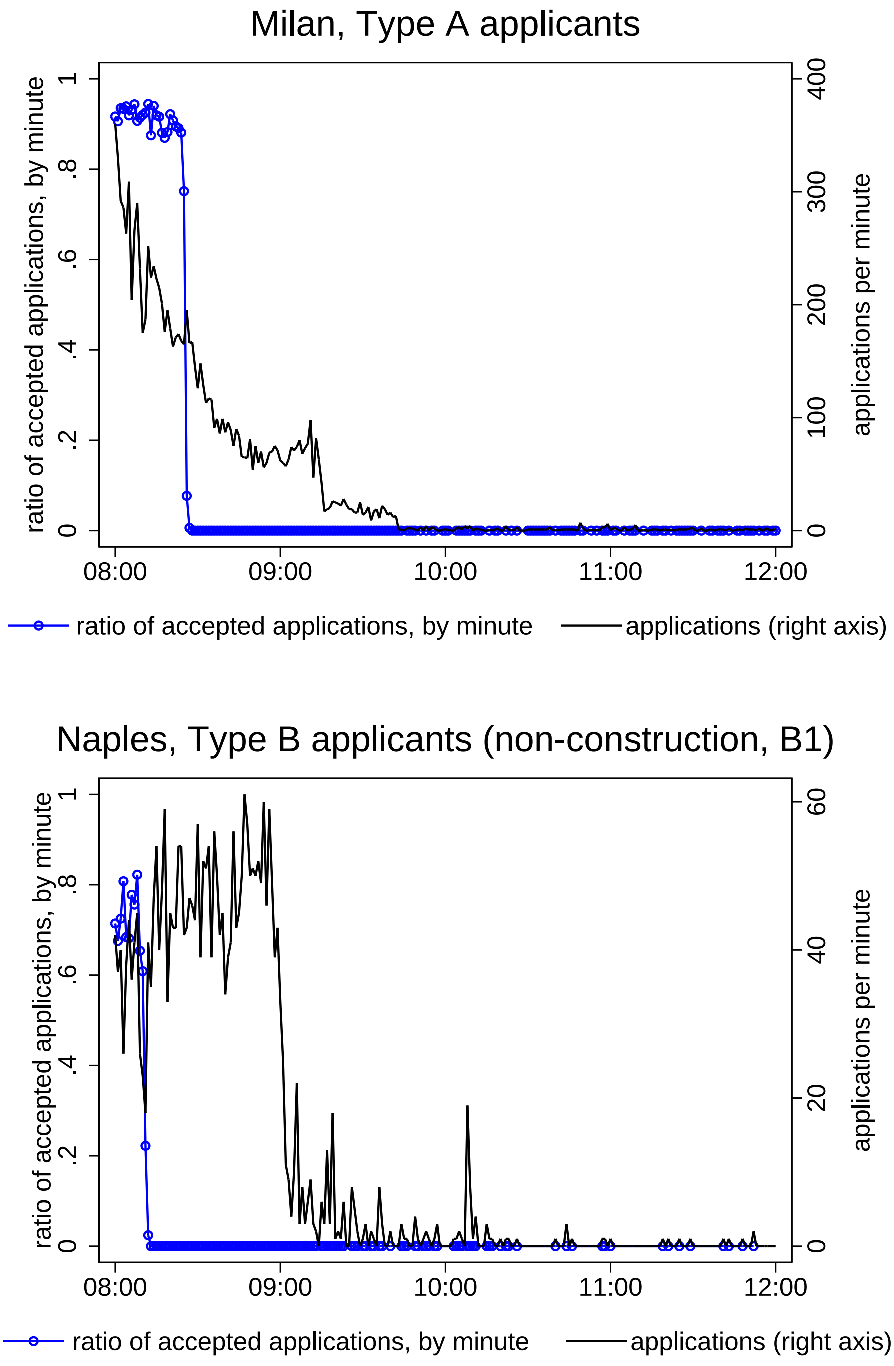
the last accepted application was received at 8:27:04.

As another example, the bottom graph in Figure 4 shows the lottery for type-B1 applicants (i.e., non-domestic workers outside construction) in the province of Naples, the most important labor market for immigrants in southern Italy. In this case, the cutoff was even earlier, at 8:10:56. Additionally, the ratio of applications sent on time that were rejected by immigration officers is larger for type-B applicants in Naples than for type-A applicants in Milan. This difference could reflect a higher incidence of fraudulent applications, a more stringent scrutiny by local immigration authorities, or both.

In general, the fraction of applications sent on time varies both across provinces and across types of applications. In particular, rejections are typically more frequent for type-B applicants (as it also evident from Figure 4) because information on sponsor firms is checked against administrative firm registries, which are not available for individuals and families sponsoring type-A applications.

Turning to applications sent after the cutoff time, they are automatically rejected, and these applicants have no right to reside and work in Italy. As already noted, however, the great majority of these individuals are already present in the country, and they remain there (illegally) even after being refused an official permit. Our empirical analysis is based on comparing the

Figure 4: Timing of applications and probability of obtaining a residence permit for two lotteries in Milan and Naples



Note: These graphs show the total number of applications received (black line, right axis) and the fraction of those that were eventually accepted (blue line, left axis) at each minute in time between 8:00 and 12:00 for the case of two lotteries: type-A applicants in Milan and type-B applicants (non-construction, B1) in Naples.

criminal records of immigrants whose applications were received just before and just after the cutoff. These individuals should be similar in all respects except legal status, so any difference in criminal activity over the following period can be attributed to the causal effect legal status (as opposed to selection bias).

3 Empirical strategy and data

This section presents the methods and data employed to estimate the regression discontinuity design described in the previous section. It also discusses potential sources of measurement error and their implications for estimating bias.

3.1 The policy effect

Let L be an indicator variable equal to 1 for applicants that eventually obtain legal status ($L = 0$ otherwise) and $C = 1$ for those who subsequently commit a crime in the destination country ($C = 0$ otherwise). Following Rubin (1974), the observed difference in crime rates between applicants who obtain and do not obtain legal status can be expressed in terms of "potential outcomes" when residing in the country legally and illegally, C_1 and C_0 respectively:

$$E(C|L=1) - E(C|L=0) = \underbrace{E(C_1 - C_0|L=1)}_{\tau} + \underbrace{[E(C_0|L=1) - E(C_0|L=0)]}_{\varepsilon}. \qquad (1)$$

The first term on the right-hand side, $\tau \equiv E(C_1 - C_0|L = 1)$, is the causal effect of legal status on the crime rate in the destination country across legalized immigrants. The parameter τ depends both on the change in criminal behavior conditional on not being expelled and 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. However, that the overall effect τ is of primary interest from a policy perspective, as it reveals the sign and magnitude of the effect of legalization on the number of crimes per applicant committed in the destination country.

The main threat to empirically identifying τ 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 legal status – for instance, labor market ability and risk aversion. However, differences along these other dimensions should tend to zero when we restrict to applicants within a narrow bandwidth (and, yet, on opposite sides) of the cutoff time. Following Hahn et al. (2001), let T be the timing of applications, with $T = 0$ at the cutoff. Under the assumption that both τ and ε vary continuously with T at $T = 0$, the average causal effect for $T \to 0$ is identified by

$$\frac{\lim\limits_{t \to 0^-} E(C|T=t) - \lim\limits_{t \to 0^+} E(C|T=t)}{\lim\limits_{t \to 0^-} E(L|T=t) - \lim\limits_{t \to 0^+} E(L|T=t)}, \tag{2}$$

i.e., the ratio between the "reduced form" effect of applying on time on the probability of committing crimes over the "first stage" effect on the probability of obtaining legal status.

3.2 Estimation

The ratio (2) can be estimated using both parametric and non-parametric Wald-type estimators. In the former case, one can estimate the effect of L on C by two-stage least squares (2SLS) using $Z \equiv 1\{T \leq 0\}$ as an instrument for L, and including on the right-hand side a J-th order polynomial in T and its interaction with Z (Lee and Lemieux, 2010). Specifically, the reduced-form and first-stage estimating equations are

$$C = \theta_0 + \beta Z + \sum_{j=1}^{J} \theta_j T^j + \sum_{j=1}^{J} \vartheta_j T^j \cdot Z + \epsilon \tag{3}$$

$$L = \pi_0 + \alpha Z + \sum_{j=1}^{J} \pi_j T^j + \sum_{j=1}^{J} \varpi_j T^j \cdot Z + \nu, \tag{4}$$

where ϵ and ν are error terms summarizing the effect of other omitted factors. After controlling for any smooth trend in T, the coefficients β and α capture the extent of any discontinuity in C and L, respectively, at the cutoff, and their ratio provides an estimate for (2). Following Gelman and Imbens (2014), we focus on a quadratic polynomial in T in the main specification, although we explore the sensitivity of the results to using any polynomial degree between 0 and 6.

Alternatively, non-parametric estimators restrict the sample to applicants within an arbitrarily small bandwidth of the cutoff $T = 0$, applying kernel weights and conditioning on the running variable to eliminate residual differences between accepted and rejected applicants.

Following Hahn et al. (2001), the reduced-form and first-stage local kernel regressions are

$$\min_{\beta,\gamma'} \quad \sum_i K\left(\frac{T_i}{h}\right) \quad (Y_i - \beta Z - \gamma_0 - \gamma_1 T_i - \gamma_2 T_i \cdot Z_i)^2 \tag{5}$$

$$\min_{\alpha,\delta'} \quad \sum_i K\left(\frac{T_i}{h}\right) \quad (L_i - \alpha Z - \delta_0 - \delta_1 T_i - \delta_2 T_i \cdot Z_i)^2 , \tag{6}$$

where $i = 1, 2, ..., N$ denote the individuals in the sample, and $K\left(\frac{T_i}{h}\right)$ is a triangular kernel attaching positive weights only to observations within a bandwidth h around the cutoff. In this case, the properties of the estimator depend crucially on the choice of h: larger bandwidths increase asymptotic bias as a result of extrapolation away from the cutoff, whereas smaller bandwidths reduce bias at the cost of greater asymptotic variance. We will explore the sensitivity of the results to alternative criteria for choosing the bandwidth, namely, the theory-based rules of Imbens and Kalyanaraman (2012) and Cattaneo et al. (2014) as well as a battery of heuristic bandwidths.

3.3 Data and measurement issues

To implement the empirical strategy described above, we merged the applications presented on the Click Days in 2007 with the criminal records of all offenders reported by the police in the following year. Specifically, the Italian Ministry of Interior provided us with the administrative records of all applications processed by immigration officers. We obtained records on 403,741 applications of a total of approximately 610,000 (because the quotas of available permits totaled 170,000, the last 200,000 applications were not even considered). 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.[10]

Each record includes the country of origin and age of the applicant, the type of permit sponsored in the application, the province in which it was presented, the timing in millisecond, and the outcome (i.e., whether the applicant eventually obtained a residence permit). Columns (1) to (3) of Table 3 report the average characteristics of applicants who obtained and did not obtain the permit, respectively, as well as the difference between the two groups. Both

[10]As it is typically the case, the crime rate of females is very close to zero (at least as far as serious crimes are concerned).

age and the country of origin – the only individual characteristics available in our data set – differ significantly between the two groups, although the magnitude of such differences is generally small. A similar picture emerges when focusing on variation within type-A and type-B applicants (columns 4 to 9).

Table 3: Descriptive statistics

	all applicants			**type-A applicants**			**type-B applicants**		
	$L=1$	$L=0$	diff.	$L=1$	$L=0$	diff.	$L=1$	$L=0$	diff.
Age	33.76	33.92	-0.157***	33.94	34.01	-0.067	33.64	33.88	-0.241***
	(0.04)	(0.03)	(0.050)	(0.06)	(0.05)	(0.080)	(0.05)	(0.04)	(0.064)
Americas	0.100	0.080	0.020***	0.131	0.096	0.036***	0.077	0.072	0.005**
	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)
Africa	0.156	0.171	-0.014***	0.197	0.234	-0.036***	0.126	0.139	-0.013***
	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	(0.003)
Asia	0.522	0.558	-0.036***	0.561	0.570	-0.009*	0.494	0.552	-0.058***
	(0.003)	(0.002)	(0.003)	(0.004)	(0.003)	(0.005)	(0.003)	(0.002)	(0.004)
Europe	0.222	0.191	0.031***	0.111	0.101	0.010***	0.303	0.236	0.066***
	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.004)
Low income country	0.072	0.074	-0.003***	0.121	0.132	-0.011***	0.036	0.045	-0.009***
	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.003)	(0.001)	(0.001)	(0.002)
Lower-middle income	0.496	0.510	-0.014***	0.595	0.630	-0.036***	0.424	0.448	-0.024***
	(0.003)	(0.002)	(0.003)	(0.004)	(0.003)	(0.005)	(0.003)	(0.002)	(0.004)
Upper-middle income	0.427	0.404	0.023***	0.283	0.233	0.050***	0.531	0.490	0.041***
	(0.003)	(0.002)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.002)	(0.004)
High income country	0.005	0.012	-0.007***	0.001	0.004	-0.003***	0.009	0.016	-0.008***
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	38,949	71,388		16,436	24,015		22,513	47,373	

Note: This table shows the average characteristics and the total number of observations of the individuals in our sample, by the type of applicant and outcome of the application. Standard errors are reported in parentheses.

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/or rape – in year 2007 and/or 2008 (i.e., the year before and the year after the Click Days 2007). We did not extend the series to later years (from 2009 onwards) because the applicants that did not fall within the quotas for 2008 were given priority one year later. To clear the backlog accumulated in the previous year, no new applications were in fact allowed in 2008. Rather, the first 150,000 excluded in 2007 were re-processed upon renewal of the applications (to be sent between December 15, 2008, and January 3, 2009). Therefore, applicants to the right of the cutoff, who constitute our control group, also had the possibility of obtaining legal status in 2009.

The final data are potentially subject to measurement error from two primary sources. First, reported crimes always underestimate the true number of committed crimes (MacDonald, 2002). Second, errors can arise in the matching between Click Day applications and criminal records. In particular, because the two archives were matched by name, surname, nationality, and year of birth, any mistake in the coding of these variables in either of the two archives (or both) could generate Type-I errors, namely, whenever an applicant has been reported for having committed a crime but the algorithm fails to detect the match, or Type-II errors, namely, whenever an applicant was never reported but the algorithm still finds a match.

To measure the implications of such errors for estimating bias, let $\tilde{C} = 1$ for immigrants reported by the police ($\tilde{C} = 0$ otherwise). If the probability of being reported conditional on having committed a crime is the same for legal and illegal immigrants, $E(\tilde{C}|C = 1) = \mu \leq 1$, then the differences in reported crimes would have the same sign and would provide a lower bound for the magnitude of differences in the actual crime rates between the two groups:

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

This is the case when the extent of under-reporting is symmetric between legal and illegal immigrants. The use of police charges (as opposed to incarceration rates) eliminates distortions resulting from the differing treatment of suspected offenders by the judicial system (e.g., only legal immigrants can ask for alternative measures to incarceration, such as home detention). Moreover, violations of migration law do not constitute a serious crime, so differences in the observed crime rates between legal and illegal immigrants do not depend on the fact that the latter can be reported for being illegally present in the country.

Turning to the match between applications and criminal records, errors that occur at random would also bias the estimated effect of legal status toward zero. However, individuals who are arrested by the police may *intentionally* misreport their identity to maintain a clean criminal record for their true identity or to avoid the application of aggravating circumstances (if they are recidivists). Clearly, illegal immigrants have a better probability of hiding their true identities, as they do not carry official documents. Indeed, this phenomenon is well known in Italy. For instance, Barbagli (2008) reports the case of an illegal immigrant who was arrested in Bologna and who subsequently confessed to have used 15 aliases in previous years (see Table A1 in the Web Appendix).

For all of these reasons, the matched data set would overestimate the crime rate of legal immigrants relative to that of illegal immigrants, $E(\tilde{C}|C=1, L=1) > E(\tilde{C}|C=1, L=0)$, thus biasing the estimates toward finding a positive effect of legal status on crime:

$$\begin{aligned} E(\tilde{C}|L=1) - E(\tilde{C}|L=0) &= E(\tilde{C}|C, L=1)E(C|L=1) - E(\tilde{C}|C, L=0)E(C|L=0) \\ &> E(C|L=1) - E(C|L=0) \end{aligned}$$

In summary, we can interpret a negative estimated effect of legal status as a lower bound (in terms of magnitude) of the true effect, whereas a positive estimate could reflect either measurement error or a positive effect of legal status or both. Therefore, our empirical strategy only allows us to reject the null hypothesis of no effect of legal status against the alternative of a negative effect but not of a positive effect.

3.4 Implementation

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 2.1) because 68 lotteries received no applications and because the 103 provincial lotteries for Sri Lankan immigrants were corrupted by a computer bug.[11]

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 one or 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 et al., 2013 and Dahl et al., 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).[12] We will also

[11]Specifically, the system blocked most applications from Sri Lankan immigrants due to the unusual length of their names. The 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 illegal permits, discussed in Section 2.2 above.

[12]This particular form of omitted variable bias is sometimes called the "Yule-Simpson Paradox," see e.g.,

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 raises the issue of how to define this cutoff.

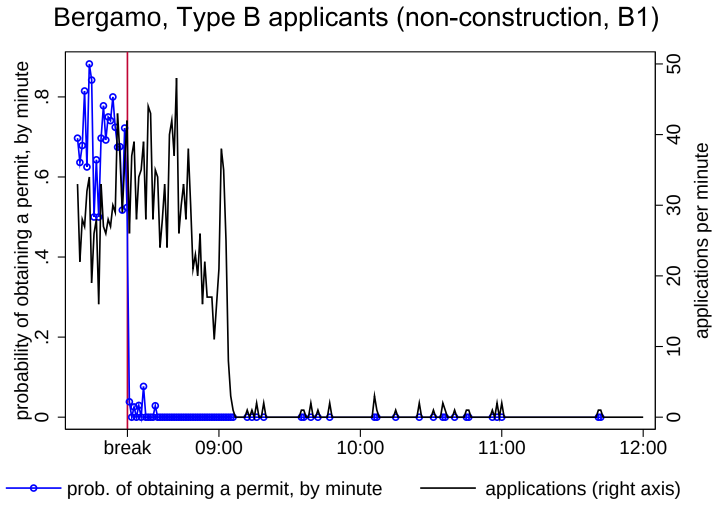
When the probability of obtaining a residence permit immediately drops to zero, as in Figure 4, the cutoff would coincide with the timing of the last accepted application. This situation actually occurs for most lotteries. In a few cases, however, the acceptance rate falls to a low but still positive level for several minutes. The lottery for B1 permits in the province of Bergamo (a city near Milan) provides an example. The probability of acceptance declines from 70% to 5% at approximately 8:22 am, but it reaches zero only a few minutes later (see Figure 5). 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 non-random subversions – for instance, because of voluntarily manipulation by the immigration officials – can be easily accommodated in the fuzzy RD design, as the Wald-type estimators in (3)-(4) and (5)-(6) exploit the variation in legal status that depends only on the initial ordering of applications near the cutoff (as opposed to later subversions of such ordering).

The only complication that arises when the discontinuity is fuzzy on both sides is that the timing of the last accepted application may no longer be an adequate measure of the cutoff, as the probability of obtaining legal status could have already changed several minutes earlier (as in Figure 5). Confronted with the same problem (i.e., estimating an unknown cutoff point in

Chen et al. (2009).

Figure 5: The timing of applications and the probability of obtaining a residence permit for a lottery in the province of Bergamo



Note: This graph shows the total number of applications received (black line, right axis) and the fraction of those that were eventually accepted (blue line, left axis) at each minute between 8:00 and 12:00 for the lottery of type-B applicants (non-construction, B1) in the province of Bergamo. The vertical line shows the timing of the structural breakpoint according to the Andrews (1993) test.

a fuzzy RD design), Chay et al. (2005) and Bertrand et al. (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 on the probability of success at each second in time. The test identifies the "most likely break point" and allows us to test for the significance of the structural break. Such a test is important in our context because in a small number of lotteries the permits were not rationed.

In the example in Figure 5, the estimated structural break point is represented by the vertical line. In general, for the great majority of lotteries, the test rejects the null hypothesis that there is no structural break. Indeed, these lotteries constitute 98% of the applicants in our sample, the median cutoff time is 8:39:06 and the majority of quotas were exhausted before 9:00.[13]

[13]The distribution of cutoff points across applicants is reported in Figure A2 of the Web Appendix.

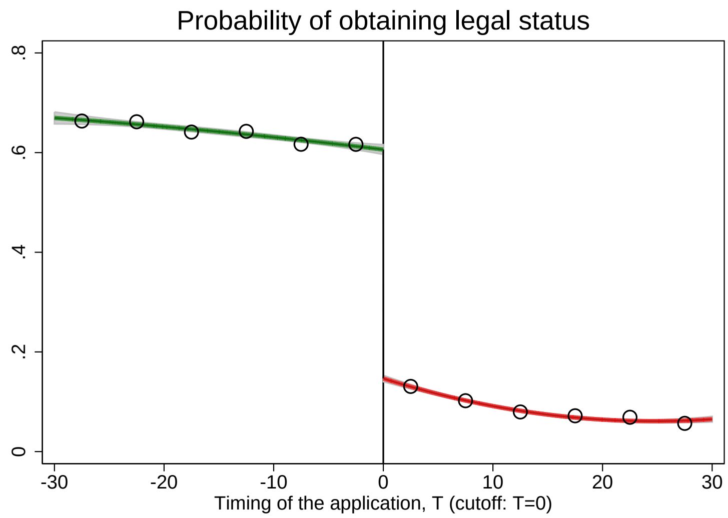
4 Results

The RD approach allows for a graphical representation of treatment effects that is both intuitive and transparent. We thus begin with this analysis and then move to parametric and non-parametric regression methods.

4.1 Baseline estimates

Figure 6 plots, respectively, the average probability of obtaining legal status conditional on the timing of the application T. The circles are average probabilities across five-minutes bins of T 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 quadratic polynomial regression (i.e., equation 4 with $J = 2$). Based on this regression, the probability of obtaining legal status decreased by about 50 percentage points at the cutoff $T = 0$.

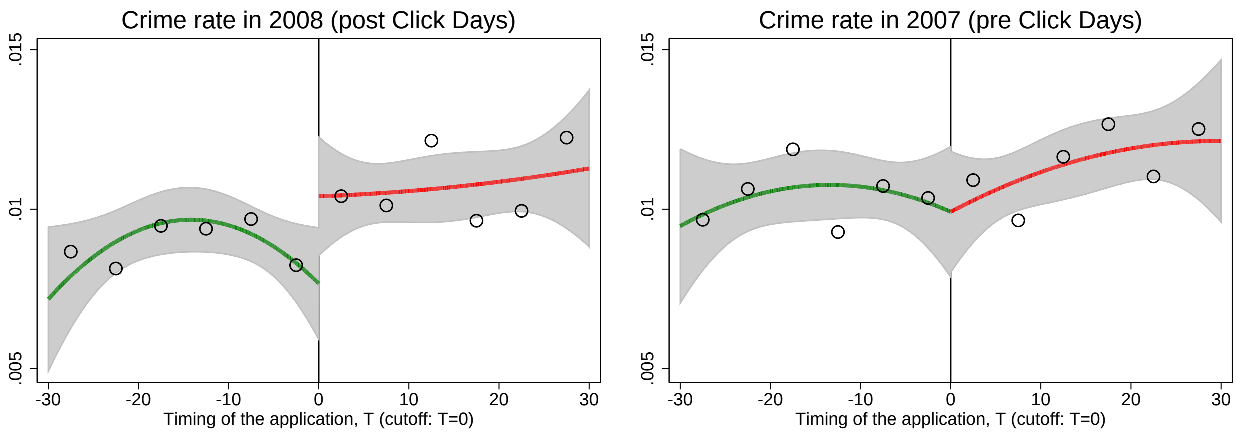
Figure 6: Probability of obtaining legal status conditional on the timing of the application



Note: The figure shows the probability of obtaining legal status conditional on the timing of application. The scatterplots are averages within 5-minute bins, and the solid lines and shaded areas are the predicted outcomes and associated confidence intervals, respectively, based on a quadratic polynomial regression.

In Figure 7, we turn to the main outcome of interest, namely, the crime rate of applicants in the year after and the year before the Click Days (left and right graphs, respectively). The average probability of committing a serious crime in the year after the Click Days increases by about 0.3 percentage points at the cutoff, whereas there is no discontinuity in the crime rate during the year before the Click Days. This supports the fundamental identification assumption that after controlling for a smooth polynomial in the timing of application, early and late applicants are as good as randomly assigned to either side of the cutoff.

Figure 7: Number of crimes per applicant before and after the Click Days, conditional on the timing of application



Note: The left and right graphs show the average number of crimes per applicant committed during the years after and before the Click Days, respectively, 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 magnitude of these effects is reported in column (1) of Table 4. In particular, the first two panels of the table report the 2SLS-estimated effect of legal status on the crime rate in the years after and before the Click Days, respectively, obtained as a ratio of the reduced-form effects in Figure 7 and the first-stage effect in Figure 6; the first-stage coefficient and the F-statistic for the excluded instrument are also reported in the bottom panel of Table 4. The causal effect of legal status amounts to a 0.6 percentage-point reduction in the crime rate after the Click Days on a baseline crime rate of 0.9 percent.

Importantly, the difference between legal immigrants and illegal immigrants after Click Days is determined by a decrease in the crime rate of applicants obtaining legal status as opposed to an increase in the crime rate of rejected applicants. Indeed, the baseline crime rate, as captured by the intercept of the polynomial regression, is basically identical in 2007 and 2008 (see also the graphs in Figure 7).

In column (2), we include lottery-fixed effects and cluster standard errors by lottery to account for interactions in crime among groups of immigrants who apply for the same lottery; we also control for a quadratic polynomial in age, the only individual characteristic included

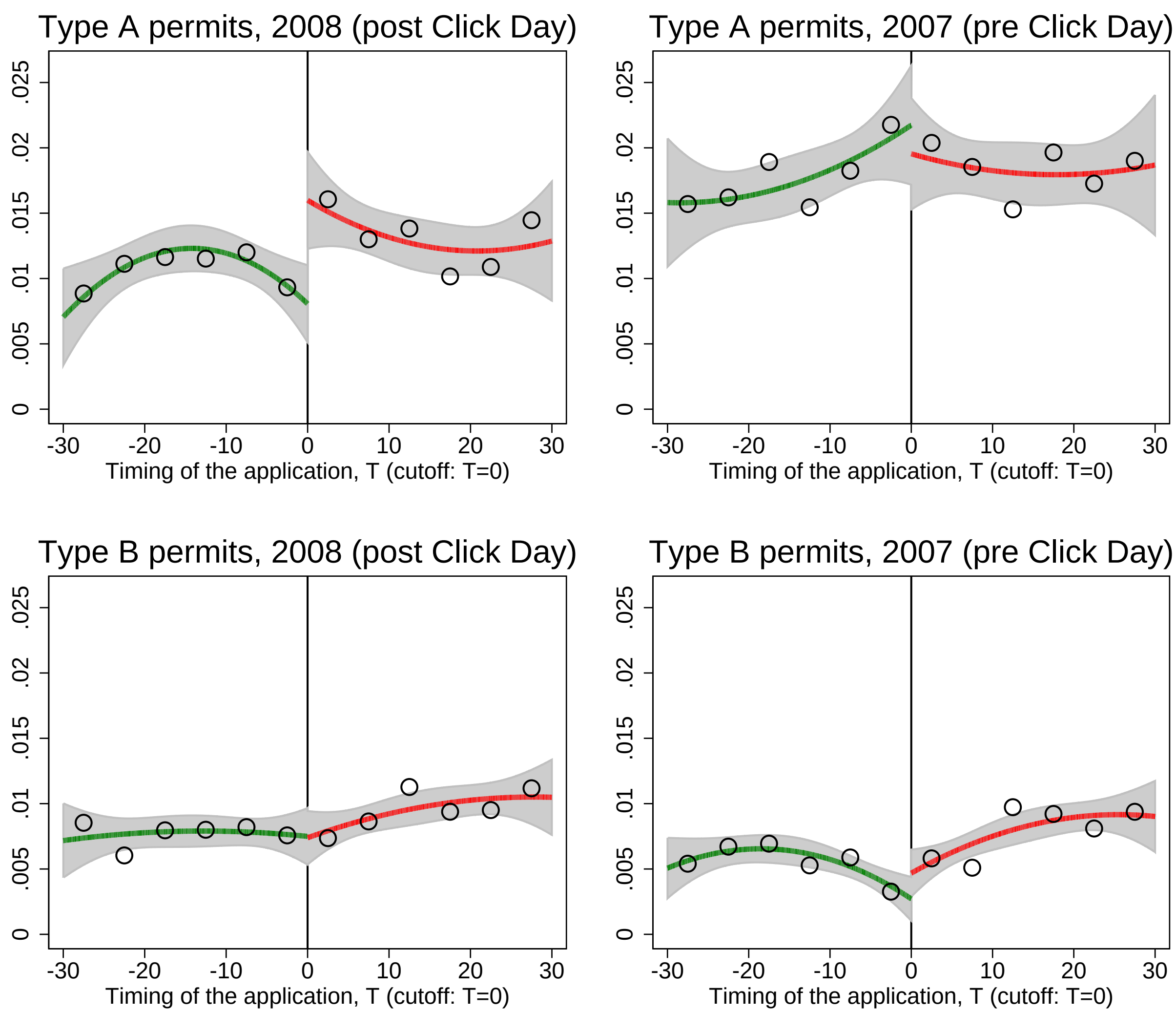
Table 4: The effect of legal status on the number of crimes per applicant, global polynomial regression

	(1)	(2)	(3)	(4)	(5)	(6)
	all applicants		**type-A applicants**		**type-B applicants**	
SECOND-STAGE RESULTS FOR YEAR 2008 (POST-CLICK DAYS):						
Legal status	-0.006*	-0.006	-0.013***	-0.013**	0.000	0.001
	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Constant	0.009***		0.015***		0.006***	
	(0.001)		(0.003)		(0.002)	
SECOND-STAGE RESULTS FOR YEAR 2007 (PRE-CLICK DAYS):						
Legal status	0.000	-0.001	0.004	0.004	-0.005	-0.005
	(0.004)	(0.004)	(0.006)	(0.007)	(0.004)	(0.005)
Constant	0.010***		0.019***		0.006***	
	(0.002)		(0.003)		(0.002)	
FIRST-STAGE RESULTS FOR THE PROBABILITY OF OBTAINING LEGAL STATUS:						
$Z(T \leq 0)$	0.459***	0.463***	0.610***	0.621***	0.372***	0.374***
	(0.007)	(0.029)	(0.010)	(0.032)	(0.009)	(0.041)
First stage F-stat	4303.3	248.7	3488.8	369.4	1645.3	83.2
Observations	110,337	110,317	40,451	40,386	69,886	69,840
Lottery FE & age	NO	YES	NO	YES	NO	YES
clustered s.e.	NO	YES	NO	YES	NO	YES

Note: This table reports the parametric 2SLS estimates of the effect of legal status on the crime rate of Click Day applicants in the year after and before the Click Days. The dependent variable is a dummy $C = 1$ for individuals that committed at least one serious offense in 2008 and 2007, respectively, the explanatory variable of interest is a dummy $L = 1$ for those obtaining legal status, and the first-stage instrument is a dummy $Z = 1$ for having applied before the cutoff time. The first-stage results are also reported in the table, the first-stage F-statistic refers only to the excluded instrument Z. 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 (2), (4), and (6) also include lottery-fixed effects and a quadratic polynomial in age. Separate estimates are presented for the subsamples of type-A and type-B applicants (columns 3-4 and 5-6, respectively). Robust standard errors are reported in parentheses, and they are clustered by lottery in columns (2), (4), and (6). *, **, and *** denote statistical significance at the 90%, 95%, and 99% confidence levels, respectively.

in the our data.[14] Controlling for lottery-fixed effects is particularly important to exclude the possibility that our results are driven by aggregation bias across lotteries (see Section 3.4). Such a possibility does not appear to be a concern, however, as the estimated coefficient of legal status remains identical to the specification without fixed effects, only becoming marginally non-significant when clustering standard errors (p-value equal to 0.110).

Figure 8: Number of crimes per applicant by the type of applicant, conditional on the timing of application



Note: The left and right graphs show the average number of crimes per applicant committed during the years after and before the Click Days, respectively, conditional on the timing of application, distinguishing between type-A applicants (top graphs) and type-B applicants (bottom graphs). The scatterplots are averages within five-minute bins, and the solid lines and shaded areas are the predicted outcomes and associated confidence intervals, respectively, based on quadratic polynomial regressions.

In Figure 8 and columns (3)-(6) of Table 4, we present separate results for type-A and type-B applicants. Important differences emerge between the two types of applicants. On

[14] Differences in the countries of origin and province of destination are absorbed by lottery-fixed effects.

the one hand, type-A applicants exhibit both a higher crime rate before the Click Days (1.9 percent) and a greater decline in the event of obtaining legal status (-1.3 percentage points). By contrast, type-B applicants are characterized by a lower crime rate before the Click Days (0.6 percent), which is not affected by legal status.

The stability of coefficients when including lottery-fixed effects and the balance in crime rates before the Click Days seem to exclude that differences between legal and illegal immigrants after the Click Days depend on compositional effects. This finding is further confirmed in Figure 9, which plots the relationship between application times and average individual characteristics across type-A applicants – the subsample for which we find a significant discontinuity in crime rates at the cutoff. Although there are clear trends in the timing of application, individuals applying just before and just after the cutoff are on average identical.

The empirical density of the running variable presented in Figure 10 provides additional support for the assumption that legal status is as good as randomly assigned near the cutoff. If immigrants near the cutoff were able to select on either side of it, and if they did so in a monotonic way (i.e., all individuals manipulate the running variable in the same direction), then the density of the running variable would exhibit a discontinuity at the cutoff. Consistent with the visual evidence, the McCrary (2008) test does not reject the null hypothesis of no discontinuity in the density of application times.

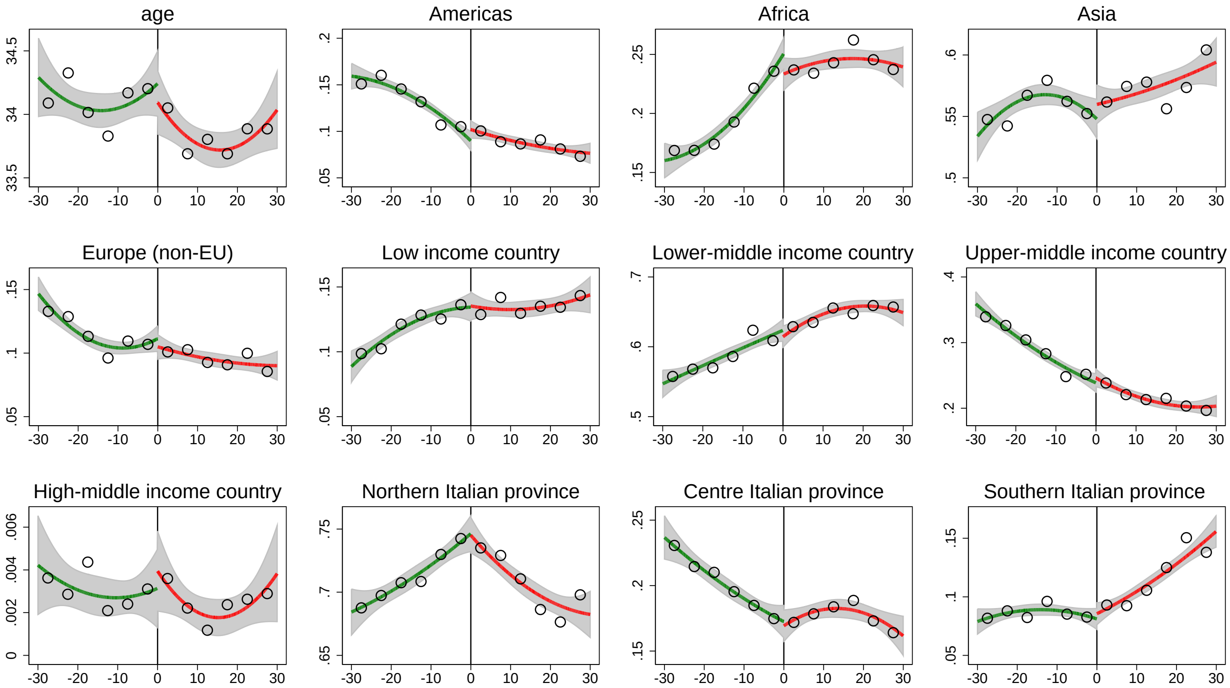
This evidence suggests that neither the manipulation of the running variable nor differences in average characteristics can explain the discontinuity in the probability of committing crimes at the cutoff.

4.2 Robustness

We next examine the sensitivity of our main results to using non-parametric methods as well as alternative specifications of both parametric and non-parametric regressions, we perform a permutation test to detect the presence of any systematic bias in our estimates, and we further discuss the issue of measurement error.

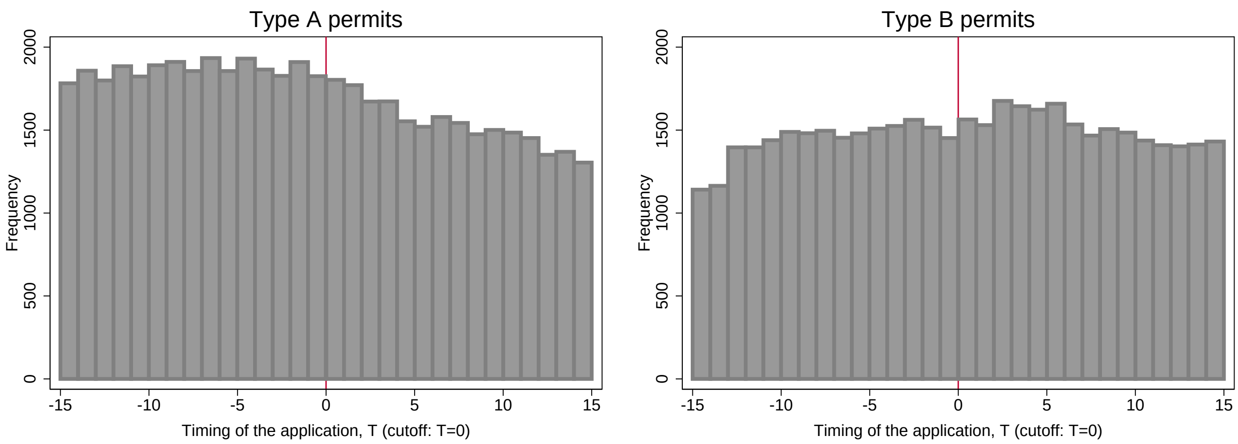
Starting with the first issue, Table 5 shows the results of kernel local linear regressions on equations (5) and (6). In the baseline specifications, we select the bandwidth according to the theory-based criteria proposed by Imbens and Kalyanaraman (2012, IK2012 henceforth)

Figure 9: Individual characteristics, conditional on the timing of application



Note: 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 10: 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.

and Cattaneo et al. (2014, CCT2014 henceforth). 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 different criteria – between six and one half and nine and one half minutes when considering the total sample of applicants, including between 30 and 40 thousand observations. The average coefficient estimated across this sample varies between -0.8 and -1.2 percentage points, larger in magnitude than in the global polynomial regression and statistically significant at the 95% confidence level (columns 1 and 2).

When distinguishing between different groups of applicants, the crime rate declines significantly for type-A applicants obtaining legal status (columns 3 and 4), and the magnitude of the effect – between 1.3 and 1.5 percentage points – is virtually identical to the estimate obtained using parametric methods (Table 4). The absence of any effect on the crime rate of type-B applicants is also confirmed (columns 5 and 6). Finally, the crime rate before the Click Days is always balanced between applicants to the left and to the right of the cutoff inside each bandwidth; see the bottom panel of the table.

The consistency of the estimates in Tables 4 and 5 requires, respectively, that controlling parametrically for a flexible polynomial in T or restricting the sample nonparametrically 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.

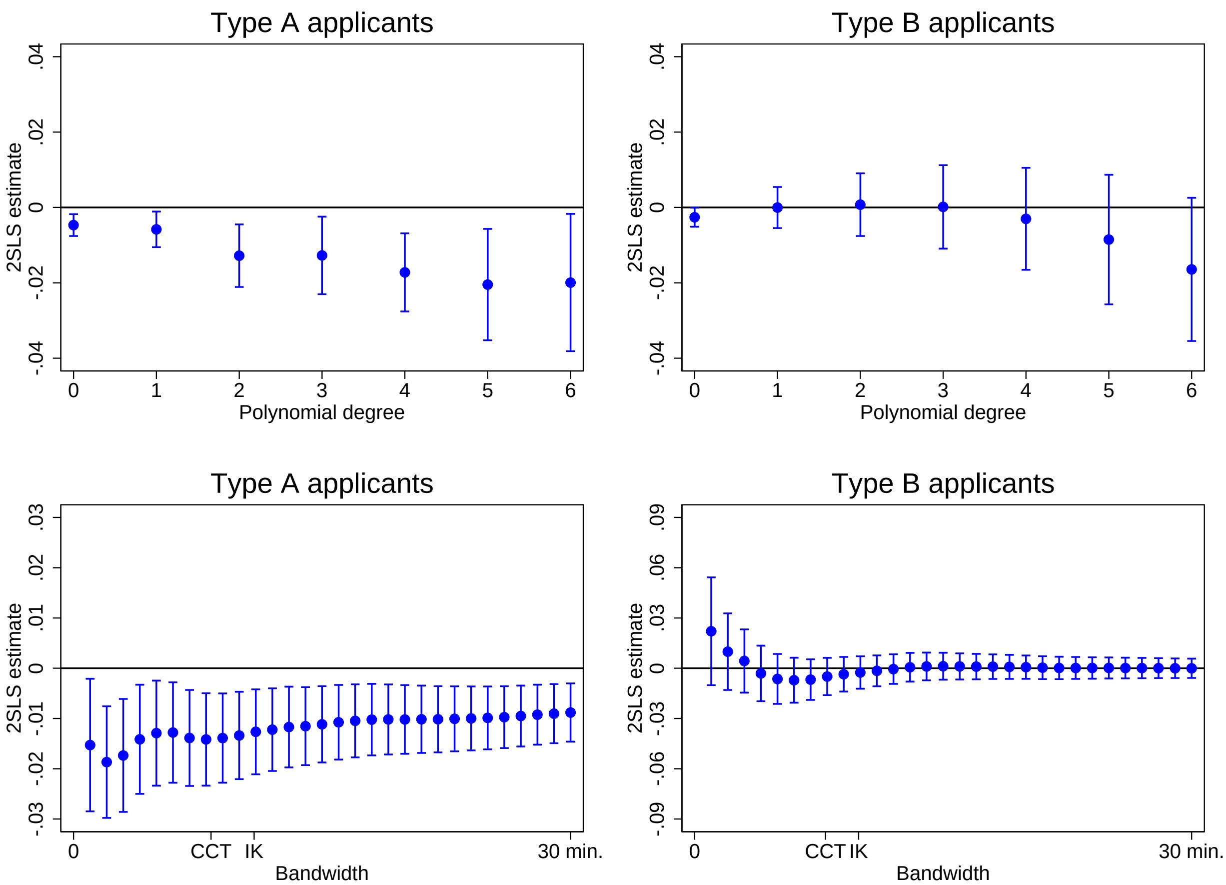
To examine the sensitivity of our results to different choices regarding these trade-offs, the graphs in Figure 11 plot the estimated coefficient of legal status and the associated confidence intervals for different specifications of parametric and non-parametric regressions. In particular, the first 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, J, 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 non-parametric methods.

Table 5: The effect of legal status on the number of crimes per applicant, kernel local linear regression

	(1)	(2)	(3)	(4)	(5)	(6)
	all applicants		**type-A applicants**		**type-B applicants**	
RESULTS FOR YEAR 2008 (POST-CLICK DAYS):						
Legal status (2nd stage)	-0.008**	-0.012**	-0.013**	-0.015**	-0.003	-0.007
	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.008)
First stage	0.454***	0.441***	0.618***	0.621***	0.356***	0.345***
	(0.009)	(0.012)	(0.012)	(0.016)	(0.012)	(0.015)
BW size	9:26	6:37	10:53	8:17	9:55	7:55
obs. inside BW	41,732	29,808	17,397	13,461	27,801	22,400
RESULTS FOR YEAR 2007 (PRE-CLICK DAYS):						
Legal status (2nd stage)	0.002	0.001	0.007	0.007	-0.005	0.003
	(0.004)	(0.005)	(0.007)	(0.007)	(0.005)	(0.006)
First stage	0.454***	0.451***	0.615***	0.617***	0.358***	0.345***
	(0.008)	(0.009)	(0.011)	(0.012)	(0.011)	(0.013)
BW size	13:38	8:25	13:03	11:19	11:14	7:30
obs. inside BW	58,646	37,475	20,658	18,134	31,160	21,311
Observations	110,337	110,317	40,451	40,386	69,886	69,840
criterion for BW	IK2012	CCT2014	IK2012	CCT2014	IK2012	CCT2014

Note: This table reports non-parametric 2SLS estimates of the effect of legal status on the crime rate of Click Day applicants in the year after and before Click Days. The dependent variable is a dummy $C = 1$ for individuals committing at least one serious offense in 2008 and 2007, respectively, the explanatory variable of interest is a dummy $L = 1$ for those obtaining legal status, and the first-stage instrument is a dummy $Z = 1$ for having applied before the cutoff time. Both the second-stage coefficient of legal status and the first-stage coefficient for the excluded instrument are reported. 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, on the bottom of each column; the size of the bandwidth in minutes and seconds, [mm:ss]; and the number of observations included in the bandwidth. Robust standard errors are reported in parentheses. *, **, and *** denote statistical significance at the 90%, 95%, and 99% confidence levels, respectively.

Figure 11: Sensitivity analysis



Note: The graphs show the 2SLS estimated coefficients of legal status for different specifications of parametric and non-parametric 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 non-parametric regressions between 1 and 30 minutes; the bandwidths selected according to the IK2012 and CCT2014 criteria are also reported on the horizontal axis.

Figure 12 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 $\tilde{T}$ as the difference between the timing of each application and the placebo cutoff, and estimating the discontinuity in the probability of committing crimes at $\tilde{T} = 0$.[15]

The distributions of the parametric and non-parametric placebos are centered at zero, and the probability of obtaining values as extreme as the estimates at the true cutoff for type-A applicants (i.e., the estimates reported in Table 4) 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.

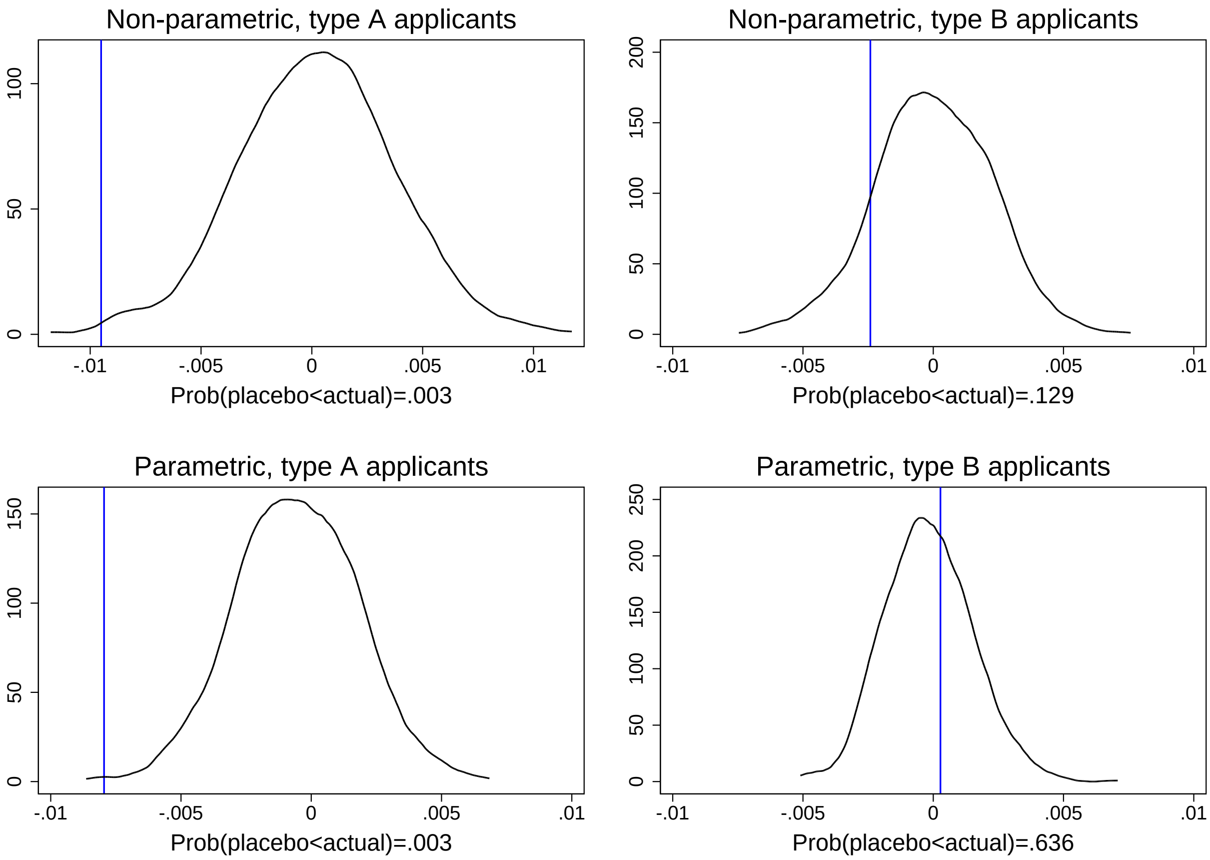
Finally, we consider a further source of bias, namely, errors in the match between permit applications and criminal records. As discussed in Section 3.3, random matching errors and the intentional misrepresentation of identity by illegal immigrants would bias the estimated coefficient toward zero. The possibility of an opposite bias arises if illegal status increases the probability of being reported for serious offenses – conditional on the same level of criminal activity. For instance, immigrants stopped by the police and found without documents could undergo a closer inspection, which could reveal evidence of additional offenses. If this situation occurred, then we would expect a non-significant coefficient when restricting the analysis to immigrants who were *not* reported for illegal status.

On the contrary, the estimated coefficient of legal status remains negative and statistically significant even after we exclude immigrants who were (also) reported for violating the migration law. Replicating the specifications in Tables 4 and 5, the magnitude of the estimated effect decreases to just below one percentage point (from 1.3-1.5 in the complete sample).[16] Note that the estimate on the reduced sample is likely biased toward zero by the exclusion of illegal immigrants who were reported for illegal status *after* having been arrested for another (serious) offense.

[15]We consider only the reduced-form coefficients because the placebo cutoffs have, by construction, no predictive power for the probability of obtaining a residence permit, so the first coefficients of the placebo regressions would be zero.

[16]The full results are presented in Table A2 of the Web Appendix.

Figure 12: Reduced-form effect of legal status on the number of crimes per applicant, placebo estimates



Note: 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 non-parametric 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 4 and 5). The fraction of placebo estimates lower than the actual estimate is also reported at the bottom of each graph.

4.3 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 by the type of applicant. In particular, the effect is high in magnitude (between -1.3 and -1.5 percentage points) 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 2.2, the former type of applicants are characterized by worse labor market opportunities, with a large number of them being actually unemployed, whereas the latter are likely employed, although unofficially, by sponsor firms before the 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 when illegal (i.e., before the Click Days). This cost would include the salary paid by the firm as well as the possibility of re-applying in the future in case the application in 2007 was not successful. By contrast, type-A applicants confront meager employment opportunities before the Click Days. At the same time, our results suggest that legal status may trigger a significant change in the behavior of such individuals, for instance because improved opportunities following the concession of legal status could induce immigrants that were previously detached from the labor market to return to searching for a job. Put differently, the group of type-A applicants may include a larger number of individuals at the margin between pursuing a criminal career and entering the labor market.

To investigate further the importance of labor market opportunities, we exploit additional heterogeneity in the effect of legal status by the type of offense and sponsor.[17] Starting with the former dimension of heterogeneity, 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 should 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-

[17]The estimates reported in this section are obtained for the same parametric specification as in Table 4. As it was the case for the baseline estimates, the results obtained by non-parametric methods are very similar and are available upon request.

Table 6: The effect of legal status on economic and violent crimes

	(1)	(2)	(3)	(4)	(5)	(6)
	all applicants		**type-A applicants**		**type-B applicants**	
	econ	**viol**	**econ**	**viol**	**econ**	**viol**
SECOND-STAGE RESULTS FOR YEAR 2008 (POST CLICK-DAY):						
Legal status	-0.005	-0.002	-0.009**	-0.002	-0.000	-0.001
	(0.003)	(0.001)	(0.004)	(0.002)	(0.004)	(0.002)
SECOND-STAGE RESULTS FOR YEAR 2007 (PRE CLICK-DAY):						
Legal status	0.000	-0.002	0.004	-0.003	-0.004	-0.001
	(0.004)	(0.003)	(0.007)	(0.003)	(0.004)	(0.004)
Observations	110,337	110,317	40,451	40,386	69,886	69,840

Note: This table reports parametric 2SLS estimates of the effect of legal status on the crime rate of Click Day applicants in the year after and before Click Days, distinguishing between economically motivated offenses (columns 1, 3, and 5) and violent crimes (columns 2, 4, and 6). The dependent variable is a dummy $C = 1$ for individuals committing at least one serious offense in 2008 and 2007, respectively. The explanatory variable of interest is a dummy $L = 1$ for those obtaining legal status, and the first-stage instrument is a dummy $Z = 1$ for having applied before the cutoff time. The first-stage results are available in Table 4, so they are not reported here. 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. Separate estimates are presented for the subsamples of type-A and type-B applicants (columns 3-4 and 5-6, respectively). Robust standard errors clustered by lottery are reported in parentheses. *, **, and *** denote statistical significance at the 90%, 95%, and 99% confidence levels, respectively.

trafficking, extortion, and smuggling) and purely violent crimes (murders and sex offenses), legal status significantly affects only the former type of offenses.[18] This last result is consistent with previous evidence in Baker (2013), Freedman et al. (2013), and Mastrobuoni and Pinotti (2014).

Turning to the second dimension of heterogeneity, in Table 7, we distinguish between immigrants sponsored by foreign and native employers, respectively. As discussed in Section 2.2, fictitious job offers aimed at legalizing unemployed immigrants who otherwise would not qualify for a residence permit typically come through type-A applications sponsored by other foreign immigrants. Consistent with this interpretation, the type-A applicants sponsored by foreign employers exhibit a higher baseline crime rate than both type-A applicants sponsored by a native employer and type-B applicants sponsored by any employer. The effect of legal status is also higher within the sub-sample of type-A applicants sponsored by other immigrants. Indeed, the negative coefficient estimated across type-A applicants is entirely driven by this sub-sample of individuals – approximately 23.5 thousand of a total of 40.5 thousand type-A applicants.

[18] We excluded two types of crime – kidnappings and illegal carrying of arms – that cannot be exclusively classified as either economically motivated or violent.

Table 7: The effect of legal status on crimes committed by applicants sponsored by foreign and native sponsors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	type-A applicants				**type-B applicants**			
	foreign sponsor		**native sponsors**		**foreign sponsor**		**native sponsors**	
SECOND-STAGE RESULTS FOR YEAR 2008 (POST-CLICK DAY):								
Legal status	-0.017***	-0.015**	-0.007	-0.008	-0.002	-0.003	0.001	0.002
	(0.006)	(0.007)	(0.006)	(0.007)	(0.005)	(0.006)	(0.008)	(0.008)
Constant	0.024***		0.007**		0.004*		0.010***	
	(0.004)		(0.003)		(0.002)		(0.003)	
SECOND-STAGE RESULTS FOR YEAR 2007 (PRE-CLICK DAY):								
Legal status	-0.002	-0.001	0.015	0.013	0.001	0.000	-0.011*	-0.011*
	(0.008)	(0.008)	(0.010)	(0.010)	(0.004)	(0.004)	(0.006)	(0.006)
Constant	0.026***		0.008**		0.002		0.009***	
	(0.004)		(0.004)		(0.002)		(0.003)	
FIRST-STAGE RESULTS FOR THE PROBABILITY OF OBTAINING LEGAL STATUS:								
$Z(T \leq 0)$	0.658***	0.667***	0.539***	0.549***	0.387***	0.382***	0.360***	0.365***
	(0.013)	(0.032)	(0.017)	(0.041)	(0.014)	(0.046)	(0.012)	(0.041)
First stage F-stat	2601.9	2930.6	999.8	1104.2	785.7	807.4	857.25	918.77
Observations	23,483	23,390	16,968	16,859	30,518	30,441	39,368	39,308
Lottery FE & age	NO	YES	NO	YES	NO	YES	NO	YES
clustered s.e.	NO	YES	NO	YES	NO	YES	NO	YES

Note: This table reports parametric 2SLS estimates of the effect of legal status on the crime rate of Click Day applicants in the year after and before Click Days, distinguishing between type-A and type-B applicants and between foreign and native sponsors. The dependent variable is a dummy $C = 1$ for individuals that committed at least one serious offense in 2008 and 2007. The explanatory variable of interest is a dummy $L = 1$ for those obtaining legal status, and the first-stage instrument is a dummy $Z = 1$ for having applied before the cutoff time. First-stage results are also reported in the table, and the first-stage F-statistic refers only to the excluded instrument Z. 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 (2), (4), (6), and (8) 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 (2), (4), (6), and (8). *, **, and *** denote statistical significance at the 90%, 95%, and 99% confidence levels, respectively.

5 Conclusions

Illegality imposes a heavy toll on foreign immigrants in terms of poorer employment opportunities, lower incomes, and lower access to social services, all of which lower the opportunity cost of engaging in crime, thus increasing the number of crimes committed by immigrants. In principle, such an effect could be offset by an opposite one: since a fraction of illegal immigrants that are expelled no longer commit crimes in the destination country, the halting of expulsions could increase the crime rate of legalized immigrants. Therefore, the overall change in crime would remain ambiguous (whereas the crime rate *conditional on not being expelled* unambiguously decreases).

The results of the present paper suggest that the former effect prevailed among immigrants applying for a residence permit in Italy, leading to a reduction in their crime rate during the year following legalization, although we cannot estimate the two effects separately because we cannot observe the mobility of immigrants across the border (because of expulsion or other reasons). Notice that the same limitation applies to empirical studies of crime in general, 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.[19]

Most important, the overall effect of legalization on crime in the destination country is arguably the relevant parameter from a policy perspective, as opposed to the (unobserved) change in criminal behavior conditional on not being expelled. The latter parameter would, in fact, over-estimate the reduction in crime achieved through legalization. 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 potential pool of

[19]If anything, the opposite might be true, as some immigrants could decide to leave the country upon having the application rejected.

applicants; our estimates specifically identify this parameter.

Indeed, we believe that our results can inform the current debate on immigration policy. Immigration amnesty is near the top of the agenda in the United States as well as in many other countries due to the large presence of illegal immigrants. The present paper highlights a potentially important payoff of amnesty programs, namely, a reduction in the number of crimes committed by legalized immigrants 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, i.e., those with worse employment opportunities when illegal.

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, illegal 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 natives in the United States, contrary to trends in most other countries. Indeed, other European countries seem to exhibit similar trends as Italy, as 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, even within Italy, the results of our analysis cannot be generalized to the whole population of immigrants, as in principle, some of them may decide not to apply for a residence permit. In practice, however, the number of applications received on Click Days is very close to the estimated number of immigrants illegally present in the Italian territory. For instance, the "high-quality" estimate of the illegal population provided by the Clandestino Project for 2006 was 650 thousand, just above the 610 thousand applications for residence permits received the following year (see Clandestino Project, 2009, and Table 1). Moreover, those actually applying for legal status may be the most interesting population from a policy perspective, as legalization programs typically presume the initiative and active involvement of to-be-legalized immigrants as well as their employers.

Third, in light of previous evidence on the effects of legal status, 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 principle, legal status could affect the opportunity cost of crime through other channels, notably, the access to social welfare. In practice, however, illegal 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 increases migration pressures at the border. Although previous studies have not found clear evidence of such effects following amnesty programs in the United States (Donato et al., 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.

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Web Appendix

Figure A1: Screenshot of an application sent through the website of the Ministry of the Interior during a Click Day

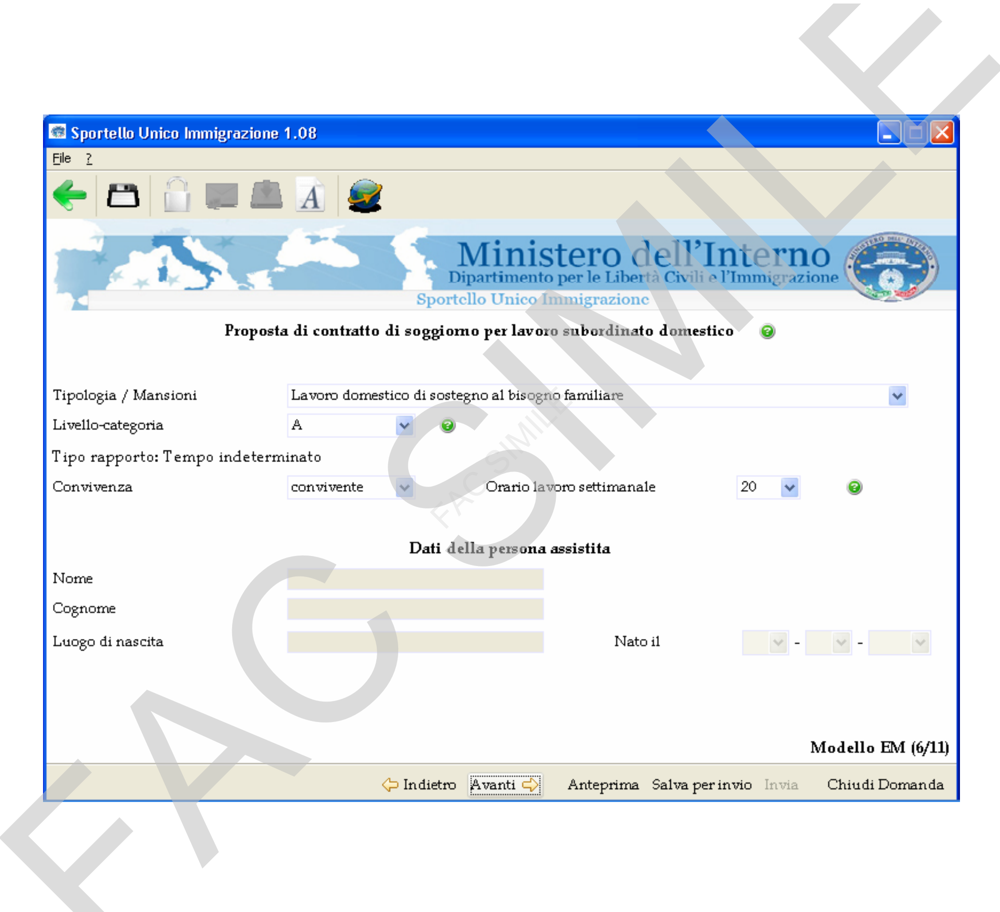

Table A1: Example of the misreporting of identity by an illegal immigrant

NAME	SURNAME	NATIONALITY	BIRTHDATE
Samri	Jamli	Algeria	23.7.1965
Abdi	Aldhi	Algeria	23.7.1970
Abdila	Aldhi	Algeria	23.7.1970
Abdihdi	Abdila	Algeria	23.7.1970
Samir	Karim	Marocco	23.7.1970
Ali	Nasire	Marocco	24.3.1974
Neighebouti	Razki	Algeria	23.7.1967
Abdlhah	Abdlhdi	Marocco	23.7.1970
Sadaci	Mohamed	Marocco	23.8.1975
Galesi	Manim	Per	23.7.1970
Saadi	Karimi	Marocco	23.7.1973
Sissmr	Yamlih	Albania	23.7.1970
Sarim	Karim	Albania	23.7.1970
Chebouti	Akzki	Marocco	23.7.1970
Samri	Yamlih	Algeria	23.7.1966
Sadeik	Sakkipei	Sarajevo	23.7.1970

Note: The table presents the list of alias used over several years by an illegal immigrant arrested in the city of Bologna. This judicial case is described at length by Barbagli (2008).

Figure A2: Density of cutoff points across applicants

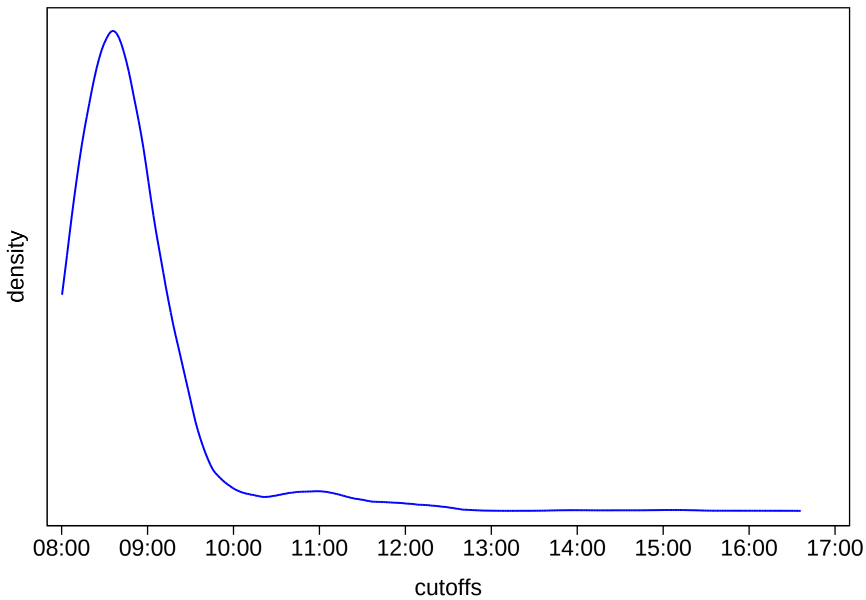

Table A2: The effect of legal status on the number of crimes per applicant, global polynomial regression (excluding immigrants reported *also* for illegal status)

	(1)	(2)	(1)	(2)	(3)	(4)
	all applicants		**type-A applicants**		**type-B applicants**	
SECOND-STAGE RESULTS FOR YEAR 2008 (POST-CLICK DAYS):						
Legal status	-0.004	-0.004	-0.008**	-0.009*	0.000	0.000
	(0.003)	(0.003)	(0.004)	(0.005)	(0.004)	(0.005)
Constant	0.009***		0.012***		0.006***	
	(0.001)		(0.002)		(0.002)	
FIRST-STAGE RESULTS FOR THE PROBABILITY OF OBTAINING LEGAL STATUS:						
$Z(T \leq 0)$	0.463***	0.466***	0.611***	0.623***	0.377***	0.378***
	(0.007)	(0.029)	(0.011)	(0.033)	(0.009)	(0.041)
First stage F-stat	4228.6	251.2	3382.7	360.7	1638.9	84.9
Observations	106,471	106,452	38,725	38,662	67,746	67,700
Lottery FE & age	NO	YES	NO	YES	NO	YES
clustered s.e.	NO	YES	NO	YES	NO	YES

Note: This table reports parametric 2SLS estimates of the effect of legal status on the crime rate of Click Day applicants in the year after and before Click Days, excluding from the sample immigrants that were reported by the police for being illegally present in Italy. The dependent variable is a dummy $C = 1$ for individuals committing at least one serious offense in 2008 and 2007, respectively. The explanatory variable of interest is a dummy $L = 1$ for those obtaining legal status, and the first-stage instrument is a dummy $Z = 1$ for having applied before the cutoff time. First-stage results are also reported in the table, the first-stage F-statistic refers only to the excluded instrument Z. 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 (2), (4), and (6) also include lottery-fixed effects and a quadratic polynomial in age. Separate estimates are presented for the subsamples of type-A and type-B applicants (columns 3-4 and 5-6, respectively). Robust standard errors are reported in parentheses, and they are clustered by lottery in columns (2), (4), and (6). *, **, and *** denote statistical significance at the 90%, 95%, and 99% confidence levels, respectively.