ILLEGAL MIGRATION AND CONSUMPTION BEHAVIOR OF IMMIGRANT HOUSEHOLDS

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Abstract

We analyze the effect of immigrants' legal status on their consumption behavior using unique survey data that samples both documented and undocumented immigrants. To address the problem of sorting into legal status, we propose two alternative identification strategies as exogenous source of variation for current legal status: First, transitory income shocks in the home country, measured as rainfall shocks at the time of emigration. Second, amnesty quotas that grant legal residence status to undocumented immigrants. Both sources of variation create a strong first stage, and—although very different in nature—lead to similar estimates of the effects of illegal status on consumption, with undocumented immigrants consuming about 40% less than documented immigrants, conditional on background characteristics. Roughly one quarter of this decrease is explained by undocumented immigrants having lower incomes than documented immigrants. Our findings imply that legalization programs may have a potentially important effect on immigrants' consumption behavior, with consequences for both the source and host countries. (JEL classification: F22, D12, K42).

1. Introduction

The consumption behavior of immigrants is not only an important subject in its own right, affecting the welfare of what constitutes now a large part of the population in many developed countries, but it also impacts on evaluations of the effects of immigration. Through affecting aggregate demand, immigrants' consumption may influence prices and wages, leading Borjas (2013) to conclude that "consumption behavior of immigrants is a topic ripe for empirical investigation".[1] Yet, although impressive progress has been made in many areas in the economics of migration, consumption behavior of immigrants and the way it is affected by immigration policies is surprisingly under-researched.

This paper attempts to fill this void. It is, to the best of our knowledge, the first to investigate immigrants' consumption behavior. Our focus is on one aspect that is in our view particularly important: the way immigrants' consumption responds to their legal status. The share of undocumented immigrants in the overall foreign-born population in developed economies has increased over past decades,[2] and policies that regulate immigrants' legal status are at the core of the policy debate in many hosting countries.[3] Legalization gives immigrants access to the regular labor market, as well as to tax benefit and health systems, but it may also affect their consumption decisions—which is what this paper concentrates on.

One problem with the study of immigrants' consumption behavior in relation to their legal status is the availability of data that provides reliable measures of both. Our analysis is based on a unique survey of both documented and undocumented immigrants residing in Italy over the 2004–2007 period. One major advantage of this data is the information it includes on consumption of immigrants, broken down by purpose, which is very rare. A unique feature is the reliable construction of measures of legal status the data contains, which makes it particularly suitable for our analysis.[4]

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1. In line with that, recent work by Dustmann et al. (forthcoming) finds large employment effects of a labor supply shock induced by a commuting policy. One way to reconcile this with findings of smaller employment effects in other works is that consumption induced demand effects are an important component of the immigration impact on local labor markets.

2. This share ranges from about 30% of the overall immigrant population in the United States (or 11.5 million in 2011, see U.S. Department of Homeland Security 2012) to 7.5%, 4.7%, 10.9%, and 15.1% in the UK, Germany, Spain, and Italy, respectively (HWWI 2009; Fasani 2010a). Estimated yearly inflows are also large: Passel and Cohn (2008) estimate that in 2008 alone, the United States received about 500,000 new unauthorized immigrants, whereas Jandl (2004) estimates that a similar number entered the E.U.15 in 2001 alone.

3. A number of theoretical and empirical papers investigate policies aimed at managing illegal entry of immigrants or legalizing illegal immigrants, see, for example, Ethier (1986), Chiswick (1988), Hanson and Spilimbergo (1999), Chau (2001), Hanson et al. (2002), Woodland and Yoshida (2006), Hotchkiss and Quispe-Agnoli (2009), Angelucci (2012), Bohn et al. (2014), Facchini and Testa (2014), and Chassamboulli and Peri (2015). See Hanson (2006) for a review of the literature on illegal migration from Mexico to the United States, and Orrenius and Zavodny (2005) for analysis on the self-selection among undocumented immigrants from Mexico.

4. We define undocumented immigrants as immigrants who do not possess a regular residence permit and are therefore not entitled to legally reside and work in the host country. We use the term "undocumented" as a synonym for "unauthorized" and "illegal."

Besides that, Italy provides an ideal context for studying the effects of immigrants' legal status because its immigrant population has intensely increased over the last two decades and unauthorized inflows of immigrants have played a major role in this expansion. Moreover, immigrants arrive from a large and very diverse set of origin countries, and Italy has a quota based system that allows for legalization—both aspects that we will use for our identification strategy. Italy also deports a significant fraction of its illegal population, and deportation efforts vary over time and across regions in a way that individual immigrants are unlikely to foresee—which is another feature that we will use for identification.

We address two important and relevant questions. First, what is the *difference* in consumption behavior between the populations of immigrants living legally and illegally in the country? Answers to this question are important to assess for instance the impact immigration has on aggregate consumption, or on tax revenue through value added taxes. It should be noted that this comparison *includes* the effect of endogenous sorting on the composition of the two populations. Second, how will an exogenous change in residence status from illegal to legal affect immigrants' consumption behavior? Answers to this question are relevant, for example, the assessment of the economic impact of legalization programs. Although the first question can easily be answered by comparing the two populations in the data, the key methodological problem in addressing the second question is the possible sorting of immigrants into legal status.

We propose two alternative identification strategies to address this issue. Our first strategy is based on the idea that higher levels of rainfall induce a positive and unexpected temporary income shock that allows those who would like to emigrate to cover the cost of an immediate *illegal* migration (rather than applying for the lengthy *legal* pathway). Drawing on earlier evidence that shows that weather conditions affect income in developing countries (see, e.g., Wolpin 1982; Paxson 1992; Miguel et al. 2004; Barrios et al. 2010; Brückner and Ciccone 2011; Bazzi 2014) we use weather shocks at the time of emigration as an instrument for legal status. We illustrate that these shocks have a strong effect on income in the emigration countries we consider, and that higher than average levels of rainfall are strong predictors of immigrants' current legal status. We also show that our instrument is orthogonal to immigrants' other characteristics (suggesting that weather shocks do not affect the composition of the immigrant population), and stronger for countries that are more specialized in agriculture (where rainfall variations are important economic shocks), and in countries where households are more likely to face binding financial constraints.

Our second strategy is based on yearly variation in quotas that allowed legalization of resident illegal immigrants and that were introduced in the late 1990s (see Fasani et al. 2013). We construct for each individual in our data a measure of the additional accumulated "exposure" to amnesty quotas, induced by yearly variation away from the predictable trend. Again this instrument is a strong predictor of current legal status. Based on different sources of variation, these alternative IV strategies provide us with two independent possibilities to identify the effect of illegal status on consumption behavior.

Our results show that legal status has a strong effect on consumption: undocumented immigrants consume about 40% less than documented immigrants, conditional on background characteristics. Our IV estimates are throughout larger than OLS estimates and remarkably similar across the two alternative IV strategies. About one quarter of the difference in consumption results from undocumented immigrants having lower earnings: *conditional* on household income, illegal immigrants consume about 30% less than legal immigrants.

There are different reasons as to why illegal immigrants may consume less (and save more) than legal immigrants, conditional on income. Perhaps most importantly, legal status may reduce future income risk, and therefore the amount of precautionary savings immigrants accumulate. Illegality may also increase the probability that the migration will be terminated prematurely, with migrants facing lower wages and employment probabilities back home, which may lead to intertemporal substitution of leisure and postponement of consumption if leisure and consumption are complements. Further, illegal status may create constraints and costs that prevent individuals from completing certain transactions (e.g., purchasing a registered motor vehicle, sign a contract to rent accommodation, obtain a mortgage) or using formal saving channels (e.g., opening a bank account).

Although we do not attempt to distinguish between these various channels, we provide evidence that is consistent with illegal status imposing restrictions on certain types of consumption. We further suggest an alternative estimation strategy based on variation in deportation risk across spatial areas and over time that supports the hypothesis that precautionary motives are one contributing factor in the lower consumption of illegal immigrants.

The existing literature on the relation between legal status and immigrants' labor market outcomes relies predominantly on variation in legal status induced by the 1986 US Immigration Reform and Control Act (see, e.g., Borjas and Tienda 1993; Kossoudji and Cobb-Clark 2002; Amuedo-Dorantes et al. 2007; Amuedo-Dorantes and Mazzolari 2010; and see Fasani 2015 for a recent review of this literature).[5] Our paper contributes to this literature by suggesting two novel alternative identification strategies that avoid many of the problems in previous papers that seek to identify the effect of illegality on immigrant outcomes.[6] In addition, we provide first analysis of the interplay between consumption behavior and legal status and of the various channels along which consumption differs between the two populations.

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5.   The Immigration Reform and Control Act (IRCA) reformed the United States' immigration law in 1986. It granted amnesty to illegal immigrants who entered the United States before January 1, 1982, and had lived in the United States continuously since that time. To the best of our knowledge, only few other papers (Kaushal 2006; Devillanova et al. 2014; Mastrobuoni and Pinotti 2015; Pinotti 2017) study the relation between legal status and behavior based on designs other than the IRCA reform.

6.   Most of the papers in this literature study the IRCA reform using the same longitudinal survey of amnesty applicants (the Legalized Population Survey, LPS) and all face similar limitations. First, using data on *applicants* for the amnesty only may lead to ignoring endogenous selection into amnesty. Second, there is no obvious control group in the LPS. Third, general equilibrium effects of a program that legalized 2.7 million individuals can confound the estimates. Our strategy is not affected by these issues.

Our paper also adds to the literature on precautionary savings.[7] Considering immigrants' legal status as a measure that is strongly correlated with the income risk households face, our analysis addresses the sorting problem that bedevils that literature (see Browning and Lusardi 1996). Our findings are in line with results of Fuchs-Schündeln and Schündeln (2005), who use a methodological approach similar to ours by exploiting differences in income risk between workers in the public and private sectors using German reunification as an exogenous reassignment of sector of employment.

Finally, we extend the use of weather shocks as a source of exogenous variation, a method used by several other authors in different applications to study weather's impact on savings behavior, remittances, network size, migration, health, economic growth, democracy, and/or conflicts (see, among others, Paxson 1992; Munshi 2003; Miguel et al. 2004; Giles and Yoo 2007; Yang and Choi 2007; Deschênes and Moretti 2009; Maccini and Yang 2009; Barrios et al. 2010; Brückner 2010; Feng et al. 2010; Pugatch and Yang 2010; Brückner and Ciccone 2011; Ciccone 2011; Dell et al. 2012; Bazzi 2014). We add to this literature by demonstrating that temporary weather shocks are also a powerful predictor of immigrants' legal status.

The paper is organized as follows. Section 2 addresses the relation between legal status and immigrant's consumption, discusses endogenous selection into legal status, and explains our empirical strategy and instrumental variable approach. Section 3 provides relevant background information on immigration to Italy and introduces our data. Section 4 reports our empirical results. Finally, Section 5 discusses our findings and concludes the paper.

2. Consumption and Illegal Status

Our analysis focuses on immigrants' consumption in the host country, and how it varies with legal status. There are several reasons why illegal immigrants may have a different consumption behavior than documented immigrants. Lacking legal status generally implies being exposed to higher uncertainty about current and future earnings and may thus lead to more savings for precautionary motives. Illegal status may also lead to inter-temporal substitution of leisure: As an illegal migration may be terminated early through detection and deportation,[8] and low wages and employment opportunities at home could lead to intertemporal substitution of leisure, so that leisure-consumption complementarities may lead to lower consumption today (as in

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7.   See, for example, Dynan (1993), Hubbard et al. (1995), Gourinchas and Parker (2001), and Fuchs-Schündeln and Schündeln (2005). In the migration literature, Dustmann (1997) develops a model of return migration and precautionary savings, showing that immigrants may have a higher income uncertainty and, therefore, engage in more precautionary savings.

8.   Over the period 2004–2007, the probability of deportation was 5% per year in Italy. In comparison, over the same period this probability was close to zero in the United States (see Fasani 2010a,b for Italy; Goyle and Jaeger 2005 for the United States).

Heckman and MaCurdy 1980). Further, illegal status may impose barriers to, and costs on consumption that prevent individuals from making specific purchases (e.g., a registered motor vehicle or registered housing). All these channels point at lower consumption of illegal immigrants in the host country (conditional on income). In addition, consumption could be indirectly affected through the impact legal status has on employment opportunities and earnings (see, e.g., Borjas and Tienda 1993; Kossoudji and Cobb-Clark 2002; Kaushal 2006; Amuedo-Dorantes et al. 2007). In our analysis we will not attempt to distinguish between these different mechanisms, which is beyond the possibilities of the data we have available. We will however provide further evidence that precautionary motives are likely to be one important reason as to why illegal immigrants consume less than legal immigrants.

We will also refrain from investigating savings and remittances, due to both conceptual issues and data limitations.[9] Total savings consist of savings held in Italy as well as in the home country, and we have no measure of the latter. Dustmann and Mestres (2010) show that failing to take into account savings accumulated in the home country may severely distort any conclusion on immigrants' saving behavior. Remittances, instead, are a composite of different transfers, with an overall ambiguous relationship to illegal status, including moneys used to finance consumption of family members in the home country, to accumulate savings at home, to invest into durable consumption or investment goods, or to support the wider village community (as insurance for a future return and reintegration). Further, our survey asks migrants to report the average amount they send home each month. This measure may systematically mismeasure remittances if transfers are less frequent, and if transfer frequency differs by legal status (e.g., because illegal migrants have no access to official banking channels). Documented migrants may, moreover, travel back home more frequently than undocumented migrants and carry money with them in addition to (or rather than) sending remittances. For all these reasons we do not believe that our measures of savings and remittances can be related to legal status in a meaningful way.

In contrast, consumption in the host country is well measured in our data (see Section 3). It is an outcome that can be directly linked to individuals' optimizing behavior and parameter estimates have therefore a clear interpretation. Furthermore, our consumption measure is related to a precise reference period, as respondents are asked about the average monthly expenditure of their household in Italy for different groups of consumption items.

In our empirical analysis, we therefore estimate the following model:

$$C_{it} = \alpha + \beta I_{it} + X_{it}'\gamma + f_c + d_t + \epsilon_{it}, \tag{1}$$

where i is an index for the individual migrant, c is the country of origin, and t is the year of interview. The dependent variable C_{it} is the log of i's monthly consumption in

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9.   Consumption can be written as the difference between income earned in the host country Y, savings S, and income transferred back to the source country (or remittances) R: $C = Y - S - R$.

TABLE 1. Documented and undocumented immigrants: descriptive statistics.

Variable	Documented immigrants		Undocumented immigrants		Significant difference (5%)
	Mean	Std. Dev.	Mean	Std. Dev.	
Respondent					
Age	33.32	7.64	31.60	8.50	*
Female	0.42	0.49	0.37	0.48	*
Education					
None	0.09	0.28	0.11	0.31	*
Primary/compulsory	0.37	0.48	0.35	0.48	
Secondary	0.40	0.49	0.42	0.49	
Tertiary	0.14	0.35	0.12	0.33	*
Residence in Italy (years)	5.81	2.44	2.68	1.78	*
Unemployed	0.04	0.20	0.08	0.27	*
Single	0.33	0.47	0.55	0.50	*
Children (dummy)	0.58	0.49	0.45	0.50	*
Household					
# Household members in Italy	2.43	1.47	1.45	0.95	*
Spouse abroad (if married)	0.25	0.44	0.59	0.49	*
Children abroad (if children > 0)	0.38	0.49	0.84	0.37	*
# Children abroad (if any)	1.82	0.98	1.92	1.02	*
Children in Italy (if children > 0)	0.68	0.47	0.23	0.42	*
# Children in Italy (if any)	1.76	0.89	1.40	0.64	*
Living in own house in Italy	0.15	0.36	0.01	0.11	*
Area of origin					
Sub-Saharan Africa	0.17	0.38	0.16	0.36	
East Asia (and Pacific)	0.06	0.23	0.03	0.18	*
Eastern Europe and Central Asia	0.27	0.44	0.36	0.48	*
Latin America	0.15	0.35	0.19	0.39	*
Middle East and North Africa	0.23	0.42	0.20	0.40	*
South Asia	0.12	0.33	0.06	0.24	*
Observations	11865		1807		
Total observations			13672		
Undocumented immigrants share			0.13		

Note: * denotes a difference between documented and undocumented immigrants that is significant at least at 5% level. Sample: immigrants with 1–10 years of residence in Italy. ISMU survey years: 2004–2007.

the host country, and I_{it} is a dummy variable equal to 1 if the immigrant has illegal status. The vector X_{it} includes individual controls of the respondent (gender, age, age squared, education level, years since arrival, dummies for province of residence) and household characteristics (number of household members living in Italy, a dummy for spouse living abroad, number of children living in Italy and abroad, dummies for type of accommodation in the destination country). In Table 1, we provide detailed descriptive statistics for these variables. Country of origin fixed effects and year of interview dummies are denoted as f_c and d_t respectively, and ε_{it} is an error term. The parameter of interest is β.

2.1. Identification

Estimation of (1) using OLS generates an estimate β that measures the (conditional) difference in consumption between legal and illegal immigrants who are living in Italy. It combines the causal effect of legal status on consumption, and the effect through sorting of immigrants into legal status. This composite parameter, although not causal, is nevertheless important for policy when determining, for example, the difference in fiscal contributions, or the differences in aggregate demand of the existing populations of legal and illegal immigrants. However, if interest focuses on the *causal* effect on consumption by exogenously changing migrants' legal status (e.g., to inform policy about the effect of implementing legalization programs), we need to eliminate the effect of sorting into legality. One such likely source for sorting may be risk aversion. If immigrants who enter the country illegally are less risk averse than those who enter legally, and if less risk averse individuals save less, then our estimate of β is biased toward zero (see Dynan 1993 for a discussion of this bias).[10] To address this, we suggest two alternative estimation strategies, based on instruments that affect legal status at two different stages: At the point when the migration decision is taken, and after having arrived in Italy as an illegal migrant.[11]

More specifically, consider the current residence status I_{it} of migrant i at time t that is determined by status at entry (legal vs. illegal) in period t_0, I_{it_0}, and opportunities of obtaining legal status after arrival (between t_0 and t), L_{it-t_0}, as well as individual unobserved characteristics μ_i:

$$I_{it} = f\left(I_{it_0}, L_{it-t_0}, \mu_i\right). \tag{2}$$

Our identification strategy relies on plausibly exogenous variation that varies either I_{it_0} or L_{it-t_0}. To vary I_{it_0}, we use shocks to income in the home country *before* emigration as exogenous determinant of the initial decision of migrating legally or illegally. To vary L_{it-t_0}, we employ variation in the opportunities for legalization in the host country that arise for each immigrant *after* emigration and that is induced by accumulated yearly differences in legalization quotas from a predictable trend. We now discuss these two strategies in more detail.

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10.   This case is similar to the problem of selection into self-employment (Skinner 1988; Guiso et al. 2002) or public sector employment (Fuchs-Schündeln and Schündeln 2005): more risk averse workers may select into occupations and sectors that imply lower income uncertainty. In that context, as in our case, occupational/sector choice is endogenous, and failing to control for selection leads to a systematic underestimation of the importance of reducing consumption for precautionary reasons.

11.   Another issue in measuring behavior of immigrants is selective return migration (see Dustmann and Görlach 2016 for discussion). In our data, there is no significant difference in return intentions between documented and undocumented immigrants (results can be provided upon request).

2.2. Rainfall and Income Shocks

Individuals have two possibilities to migrate to Italy: Either legally, through a visa application, or illegally. Although the first option implies long waiting times, and may well be unsuccessful in the end, the second option is immediate, but rather costly, so that credit constrained individuals may be restrained from migrating through this route.[12] A positive temporary income shock may alleviate credit constraints of those who would want to emigrate but would rather avoid the long and potentially unsuccessful legal route in favor for the illegal route,[13] by being able to afford an immediate illegal migration. If the destination country, moreover, offers possibilities to obtain legal status after arrival, opting initially for an illegal migration can be an effective, though more expensive, way of speeding up the process of becoming a legal resident.

We use here rainfall shocks at the time of emigration as a temporary shock to income that may potentially trigger an illegal migration and predict current residence status through the persistence in legal status over time. The rationale for our instrument is that rainfall generates income shocks by affecting agricultural production, thereby temporarily relaxing the liquidity constraints that restrict migration. Most of the source countries included in our sample are highly dependent on agricultural production.[14] Therefore, shocks to that sector are likely to have an important impact on the livelihoods of large parts of the population, either directly (by individuals working in that sector), or indirectly (by affecting sectors related to agriculture, such as retail). Further, as rainfall shocks are transitory, and uncorrelated over time, they should not affect permanent income, and therefore the more fundamental decision whether or not to emigrate. As a consequence, they should not affect the overall population of potential migrants – something we test in our empirical section.

We measure rainfall shocks using data from the NASA Global Precipitation Climatology Project (see Adler et al. 2003), which provides monthly mean rainfall data on a $2.5° \times 2.5°$ latitude–longitude grid from 1979 onward. Based on these data, we first compute the yearly rainfall averages for each country of origin for the immigrants in our sample. We then match each individual with the average yearly

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12.   Illegal migration implies far higher monetary costs as migrants have to compensate smugglers, buy forged documents, pay border officials at home and abroad, and so on. Existing evidence illustrates that the price paid by undocumented migrants is substantially higher than the simple cost of the trip (see, e.g., Friebel and Guriev 2006; Gathmann 2008; The Economist 2012). Recent UNODC data, for instance, show that the price for being smuggled into the United States from Central America in 2009 was about $3,500 for Central Americans, $7,000–7,500 for Africans and Indians and $45,000 for Chinese (UNODC 2012).

13.   Note that this will not affect potential migrants who already secured a legal entry to the destination country.

14.   Almost all the origin countries of immigrants residing in Italy are low and middle-income nations that are highly dependent on the agricultural sector. According to World Bank data, over the 1995–2007 period, the sample-weighted average share of agricultural employment for the 20 countries in our sample with the largest number of immigrants (which accounts for 81% of our overall sample) is almost 41%, whereas the average share of agriculture on GDP is about 19% (see Online Appendix Table A.1 for details). These numbers are roughly 10 times larger than the OECD average (4% and 2%, respectively) and 6–8 times larger than those for Italy (5% and 3%, respectively).

rainfall in the year of emigration (and in the previous year) in the country of origin. As we condition on country fixed effects, which removes the country-specific mean of rainfall, our rainfall measure is equivalent to using rainfall deviations from the country mean and can be interpreted as a temporary "shock" to precipitation in the home country in the period preceding migration.

We show below that rainfall in the year, and the year previous to emigration is indeed a strong predictor of current legal status. We further demonstrate that our instrument is stronger in countries with a larger agricultural sector and where the households are more likely to be cash-in-advance constrained. Moreover, based on World Bank data on agricultural and total income for a panel of almost 100 developing countries over the period 1979–2012, which we match to the GPCP data on rainfalls, we show that rainfall shocks have a strong impact on per capita (agricultural) income.

2.3. Legalization Quotas

Our second identification strategy relies on the accumulated exposure of immigrants to deviations of yearly quotas to grant legal residence *after* their arrival in Italy.

In the late 1990s, Italy adopted a quota system that was meant to regulate the entry of migrant workers to the Italian labor market. Since its introduction, however, the system has been widely used to grant legal status to undocumented immigrants who reside in Italy (Fasani et al. 2013; Pinotti 2017), as the Italian authorities are unable to discriminate between applicants who apply from abroad, and who live unlawfully in Italy. At the end of each year, the government issues a "flows decree" that establishes the number of immigrants that will be allowed to enter the country in the following year for work reasons. The government should choose the size of the quota—which can also be set to zero—based on forecasts of labor market shortages and demand for foreign workers as well as availability of public and social services at the local level (housing, schools, health services, etc.). Once the quotas are set, a date is announced for employers to start filing applications to sponsor an immigrant. Residence permits are then allocated to valid applications, in order of application receipt and until the quota is reached. Quotas are usually lower than the number of applications filed. This rationing of residence permits, in conjunction with unforeseen differences from year to year in the number of total permits, generates randomness in granting legal status, as is well documented in Pinotti (2017). As Online Appendix Table A.3 shows, quotas started in the late 1990s at about 20,000 working permits per year, increased gradually to almost 90,000 in 2001 and decreased to 80,000 in 2002–2004, only to increase again in 2005, reaching a peak of 550,000 in 2006 and then dropping to 250,000 in 2007.[15] Besides "general" quotas, open to immigrants of any origin, there were also country-specific quotas, allocated only to immigrants from certain origin countries. As Online Appendix Table A.3 shows, over the period considered, three nationalities (Albania,

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15.   In 2006, the Italian government initially set a quota of 170,000 working permits but, once the applications had been submitted, decided to legalize all valid applicants. Such a decision was unexpected and unprecedented and it was never repeated in following years.

Morocco, and Tunisia) were the primary beneficiaries of these country-specific quotas. As for total quotas, these national-reserved quotas experienced substantial fluctuations over the period 1996–2007.

The underlying rationale for the instrument is that immigrants who were "exposed" to larger quotas, and had the possibility to apply for legalization schemes over a longer period, should be more likely to have acquired legal status. The identifying assumption is that quotas set by the Italian government are orthogonal to migrants' individual characteristics and affect their consumption decisions only via the impact on immigrants' legal status. To implement this strategy, we therefore match each migrant in our sample with the total number of residence permits offered through the quota system since arrival in Italy. More specifically, for each immigrant, we compute the overall exposure to general residence permits offered by Italy to which the individual has been exposed since arrival, $Q_{it} = \sum_{t=t_{i0}}^{T-1} q_t$, where t_{i0} is the arrival year of individual i, $T-1$ is year before the interview, and q_t is the quota of residence permits offered in year t.[16] We do not include in the summation the quota offered in the interview year T because it cannot affect the migrant's legal status.[17] Note that the total number of quotas an individual is exposed to until interview differs for individuals who arrived at different dates, as there is variation in the quotas over time. We illustrate that in Online Appendix Figure A.1, which carries the log of total quotas on the vertical axis, and the year of arrival on the horizontal axis, and where points are connected for total exposures to quotas for each of the four interview years. Comparing points vertically in this figure refers to individuals of the same arrival cohort who were interviewed in different years and thus exposed to different quotas. Comparing points along upward sloping diagonals across lines refers to individuals with the same residence in Italy, but who arrived in different years, and were therefore exposed to different quotas. The connected lines decline over time because migrants who arrived later in Italy had fewer opportunities to be legalized.

In our empirical strategy, we address the (unlikely) possibility that individuals are able to predict future quotas in Italy. We assume that any such predictions are "rational" in the sense that they can be expressed as linear projections of the implemented quotas to which an individual is exposed to after arrival. We then use only the accumulated deviations of yearly quotas from these expectations for identification. This amounts to using the residual of the following regressions as an instrument:

$$\ln Q_{it} = a_0 + a_1 \left(t - t_{i0} \right) + d_t + \omega_{it}, \tag{3}$$

16. We discuss only the case of residence permits to which individuals from every country can apply. Extensions to country specific permits are obvious.

17. The ISMU survey interviews take place in the spring of each year, whereas quotas are set and announced at different dates each year. Furthermore, several months elapse between the beginning of the application window and the actual granting of residence permits to successful applicants.

where $\ln Q_{it}$ is the (log of) total numbers of working and residence permits offered by the Italian government since arrival date t_{i0} of migrant i, $t - t_{i0}$ is the total number of years the immigrant has resided in Italy, and d_t are dummies for the interview year. The predicted residual $\hat{\omega}_{it}$ is our instrument. We illustrate the residual variation that we use for identification in Online Appendix Figure A.2.

To test the robustness of this identification strategy we also use two alternative instrumental variable approaches. First, we assume that migrants have "adaptive" expectations, in the sense that they predict future quotas after their arrival based on past quotas observed up to the point of immigration. In particular, we assume that a migrant arriving in Italy in year t_{i0} expects the quota for that year and the following years to be equal to the average quota observed until the year before migration. Our instrument is then the difference between the actual cumulative quotas and the expected cumulative quotas, and captures the deviations in the policy from what the migrant has predicted before migrating, based on observed past realizations of yearly quotas. Second, we exploit the heterogeneity across nationalities in quotas and match citizens of the three main "privileged" countries to their country-specific quotas, whereas all other immigrants are matched to the overall quotas (net of national-reserved quotas).[18] We maintain the assumption of "rational" expectations of future quotas and use as instruments the residuals from separately estimating equation (3) for each of the three privileged countries and for all the remaining countries pooled together. In our estimations, all these strategies lead to similar results. We report therefore results using $\hat{\omega}_{it}$ as our main instrument, and results using the alternative instruments as robustness checks.

2.4. Parameter Interpretation

The two instruments we propose—rainfall shocks in origin countries *before* migration and unexpected legalization opportunities in Italy *after* migration—rely on entirely different variation. They also identify two potentially different local causal parameters. Weather shocks, by relaxing the budget constraint, identify a LATE effect of a population of "compliers" (those who would have liked to emigrate legally but who were induced by a positive income shock to choose the immediate illegal option).[19] The quota instrument, by randomizing illegal immigrants who are already residing in Italy into the pool of legal immigrants, identifies a "local" effect for illegal "compliers" who have been exposed to legalization opportunities measured as the accumulated

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18.   Over the period 1996–2007, the Italian government granted two general amnesties (in 1998 and 2002) that provided opportunities for legalization in addition to the quota system we have just described. We do not exploit this additional source of exogenous variation in legal status because the instrument one could construct using these amnesties (i.e., having arrived in Italy before 1998 or before 2002) is mechanically correlated with the duration of residence in Italy (a control always included in our regressions).

19.   In Online Appendix Section A.2.1, we discuss the potential bias implied by using rainfall shocks as instrument for legal status. In particular, we show that if positive shocks affect the overall pool of potential migrants, the IV estimator will identify a lower bound (in absolute value) of the effect of being undocumented on immigrant consumption.

deviations from the trend of issued residence permits since year of arrival in Italy. The
two estimated parameters can, but do not need to be the same.

2.5. Household Income

Although equation (1) is our main specification, it may also be of interest to determine
how illegal status affects consumption *conditional* on income. Our measure of income
refers to overall household income, which consists of various components, as we
explain below (Section 3). As for consumption, we use this measure by apportioning
to individuals their share of household income using an equivalence scale (see
Section 3.1). One potential concern is that this measure is potentially correlated with
unobservables that affect also consumption. For instance, as household income includes
the household's total hours of work, it may well be that hours of work are correlated
with unobservables that also affect consumption, or that unobserved components that
affect underlying wages are also correlated with consumption. If income and legal
status are correlated, any bias in estimates of the coefficient on income will also affect
the estimate of the impact of legal status on consumption.[20]

To address these potential concerns, we would need an additional instrument for
immigrants' income. Since immigrants' earnings are highly responsive to economic
conditions (Dustmann et al. 2010), one possibility is to exploit the exogenous variation
in labor market conditions across different provinces of residence and over time.
In particular, we match each immigrant to the unemployment rate in the province
of residence in the interview year and use this variable to instrument individual
income (see Section 4.2.2). The exclusion restriction assumes that changes in local
unemployment rates affect immigrants' consumption only through disposable income.

3. Background, Data, and Descriptives

After Italy became a net immigration country in the late 1970s, its immigrant population
initially remained smaller than that of other European countries, with an immigrant
share of 1% still in the late 1980s (Del Boca and Venturini 2003). From the early
1990s onward, however, immigrant inflows increased dramatically, with much of it
being undocumented. Between 1986 and 2002, different Italian governments granted
five general amnesties that legalized almost 1.5 million unauthorized immigrants. As
discussed in Section 2.3, a quota system was adopted in the late 1990s to regulate the
legal entry of foreign-born workers. Over the years, family reunification entries had
also increased greatly. By 2008, the number of legal resident immigrants was about 3.7
million or 6% of the total population. In the same year, the undocumented immigrant
population was around 650,000 or about 15% of the foreign-born population (Fasani
2010a). Many of these illegal immigrants find employment in Italy's large shadow

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20. Mismeasurement of monthly income is another concern.

economy, which in 2003 accounted for about 26% of the official GDP, compared with an average 16% and 9% for the OECD and the United States, respectively (Schneider 2005).

Being an unauthorized immigrant in Italy implies daily exposure to substantial uncertainty, which may have effects on precautionary savings, being one factor that explains the lower consumption of illegal immigrants. By law, all citizens are required to carry the official Italian ID with them at all times, whereas immigrants must always carry their passport and the documents proving the legitimacy of their residence in the country. Italy is an ethnically homogenous country where immigrants can be easily recognized by officers who inspect individuals routinely and apply racial profiling in doing so.[21] The lack of a residence permit prevents immigrants from having a legal working contract, accessing the welfare system (apart from emergency care), or signing a house rental contract and confines them to employment in the informal sector. Moreover, the probability of apprehension and removal of undocumented immigrants is high. Over the 2004–2007 period, the time span covered by our data, the average estimated population of undocumented immigrants was around 600,000, with more than 90,000 arrested and 26,400 subsequently deported each year (Fasani 2010a,b). These figures imply an overall probability of apprehension and deportation of 14% and 5% per year, respectively. For comparison, over the same period, the US resident undocumented immigrants faced a 2% probability of being apprehended and a negligible probability of removal (see Goyle and Jaeger 2005).

3.1. Data and Descriptive Evidence

Our analysis is based on a large and representative sample of both documented and undocumented immigrants residing in Italy's Lombardy region. Data are taken from an annual survey run by the Institute for Multiethnic Studies (ISMU). This survey was launched in 2001 and administered to around 8,000 immigrants in each wave. Since 2004, it has included items on household expenditure, savings, and remittances. For this analysis, we pool four survey waves (2004–2007) to obtain a sample of 13,672 observations representing over 100 different nationalities (Online Appendix Table A.1).[22] Of the overall sample, 11,865 individuals are documented immigrants and 1,807 are undocumented (Table 1). The data, which are detailed in

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21.   During the period under study (2004–2007), the Italian police report having checked and identified about 7–9 million individuals each year, a very large number for a country that has about 59 million residents.

22.   We eliminate from our sample all immigrants who are nationals of one of the New Member States (NMS) that joined the European Union in 2004. Although Italy adopted transitional period restrictions that prevented NMS nationals from legally working in the Italian labor market, these immigrants immediately acquired the status of European citizens and the right of legal residency. Further, we restricted the sample to individuals with at most 10 years of residence in Italy to ensure common support between documented and undocumented immigrants (since the latter group has substantially shorter average residence duration than the former; see Table 1). In Online Appendix Table A.7, we show that our findings are very similar when the threshold varies from at most 5 to at most 15 years of residence in Italy (see Section 4.2.1).

Online Appendix Section A.1, contain information on both the interviewee (e.g., gender, age, employment status) and the household (e.g., number of members, accommodation, income, consumption).

The ISMU survey is specifically designed to elicit truthful reporting of legal status.[23] We construct three definitions of undocumented immigrants. The most restrictive definition assigns the label "undocumented" only to those who reported not having a residence permit. A second definition also covers those who reported having applied for amnesty but had not yet received a response (1% of our sample). A third definition includes also all those who reported being currently in the process of renewing their residence permit (5% of our sample), which implies that they may not have had legal status at the time of interview. Based on these alternative definitions, the share of undocumented immigrants in the estimation sample is 12%, 13%, and 18% respectively (see Online Appendix Table A.2). Throughout this paper, unless otherwise noted, we use the second definition; however, we show that our results are not sensitive to the definition adopted (see Section 4).

Table 1 provides summary statistics on individual and household characteristics for the two groups. Undocumented immigrants are slightly younger than documented immigrants, with a mean age of 31.6 (vs. 33.3) years. Both groups have similar levels of education (more than half of each group has received some secondary or tertiary education), and the share of females is comparable (37% vs. 42%). The household structure, however, differs considerably: undocumented immigrants are more likely to be single (55% vs. 33% among documented immigrants) and less likely to have children (45% vs. 58% among documented immigrants). The average size of their household in Italy is smaller (1.4 vs. 2.4 members) and those who are married or have children are more likely to have left their spouse (59% vs. 25%) and children (84% vs. 38%) abroad. The share of unemployed individuals among undocumented immigrants is also twice as high as among documented immigrants (8% vs. 4%). On average, the undocumented immigrants in our sample have been in Italy for about 2.7 years versus 5.8 years for documented immigrants. The last column in Table 1 shows that most of these differences are statistically different at least at a 5% significance level.

In the ISMU survey, each interviewee is asked to report average monthly expenditures of their household in Italy for each of the following broad categories: (a) food, clothing, and other basic needs; (b) housing; (c) other items (e.g., transportation, leisure, etc.). Our measure for consumption in the host country is the sum of these three types of consumption expenditure. Immigrants are also asked about the household's average monthly expenditure for remittances and average monthly savings. Our measure for total household income is then computed as the sum of these five items.

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23. To elicit truthful reporting of legal status, the interviews are anonymous, ask for no sensitive information (e.g., addresses), and are carried out in public spaces by foreign-born interviewers (when possible, from the same country as the interviewees) who emphasize the independence of the ISMU Foundation from any Italian government body. The information on legal status is obtained by asking the immigrants about the type of legal documents they have, starting with the most permanent (being an Italian citizen) and moving down to the option of "no documents". Online Appendix Table A.2 shows how the question is structured.

TABLE 2. Documented and undocumented immigrants: consumption and income.

	Documented immigrants		Undocumented immigrants		Significant
	Mean	Std. Dev.	Mean	Std. Dev.	difference (5%)
Monthly values (euros)					
Consumption					
Total	581.6	*267.2*	424.6	*243.8*	*
Food and Clothes	220.0	*116.9*	177.5	*113.1*	*
Housing	244.4	*156.3*	151.5	*137.1*	*
Other	117.3	*95.5*	95.6	*82.5*	*
Total Income	815.5	376.9	710.4	339.2	*
Share of total income					
Consumption					
Total	0.74	*0.22*	0.65	*0.27*	*
Share of total consumption					
Food and Clothes	0.39	*0.14*	0.44	*0.20*	*
Housing	0.40	*0.17*	0.31	*0.21*	*
Other	0.21	*0.13*	0.25	*0.21*	*
Observations	11,865		1,807		

Note: * denotes a difference between documented and undocumented immigrants that is significant at least at 5% level. Sample: immigrants with 1–10 years of residence in Italy. ISMU survey years: 2004–2007.

Table 2 reports descriptive statistics on consumption and income of documented and undocumented immigrants. As the ISMU survey collects information on total expenditure at the household level, we obtain individual consumption (income) as the ratio between reported household consumption (income) and the number of members of the household residing in Italy (converted into "equalized adults" using the "modified OECD" equivalence scale).[24] As Table 2 shows, total net monthly household income is higher for documented than for undocumented immigrants (€815 vs. €710), as is consumption expenditure (€581 vs. €424) and the share of income attributed to consumption (74% vs. 65%). Decomposing total household consumption into its three subcategories shows that the share of consumption expenditure by documented (undocumented) immigrants is 39% (44%) for food and clothes, 40% (31%) for housing, and 21% (25%) for "other" consumption goods. The last column in Table 2 shows that all these differences are statistically significant at least at the 5% level. These descriptive statistics suggest therefore not only that undocumented immigrants consume less than documented immigrants, but also that the composition

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24. The "modified OECD scale" is the official Eurostat equivalence scale. It assigns 1 to the first adult household member, 0.5 to each additional adult member and 0.3 to each child. An alternative is the "standard OECD scale" that assigns a value of 1 to the first adult household member, 0.7 to any further adult and 0.5 to each child. Throughout our empirical analysis (see Section 4), we primarily use individual measures of consumption and income obtained using the "modified OECD scale", but we show that our estimates are robust to using the other two alternative scales.

of consumption expenditure between the two groups differs, with the largest difference in housing expenditure.

4. Empirical Results

We now present our estimation results. We first discuss first stage estimates for our instrumental variables (Section 4.1). In Section 4.2, we report OLS and IV estimates of our main consumption equation.

4.1. First Stage Estimates

4.1.1. Rainfall Shocks.   Table 3 reports the results of Linear Probability Models of current illegal status on log rainfall in the country of origin at the time of migration (with rainfall normalized by the average within-country standard deviation in the sample).[25] All regressions include a set of "baseline controls" (interview year dummies, country of origin dummies and years since migration) and cluster the standard errors by country of origin. Because we always condition on country of origin dummies, rainfall levels can be interpreted as deviations from the country means. Columns (1)–(3) show that rainfall shocks at the time of emigration T and in the year before migration $T - 1$ are strong predictors of illegal status, whereas shocks in period $T - 2$ are not significant once we condition on shocks in T and $T - 1$. Further lags of rainfall shocks are even smaller in magnitude and not significant. In column (4), we include individual controls and we regress current illegal status on the mean of rainfall shocks in the immigration year (T) and previous year ($T - 1$), as well as on rainfall shocks in the origin country at the time of the interview ("current log rainfall"). Although the former variable is strongly significant, the latter has no predictive power. Estimates hardly change if we add further household controls and log income in columns (5) and (6) of Table 3. Columns (7) and (8) show that our results are not sensitive to the particular definition of undocumented immigrants (see Online Appendix Table A.2).

These results suggest that rainfall-induced income shocks in the year of emigration and the year previous to that, but not in years further back or in the current year, affect illegal status in Italy. According to the estimates in Table 3, a one standard deviation increase in the precipitation level in the year of migration (the year before emigration) leads to a 3.2 (1.8) percentage point increase in the probability of being an illegal resident in Italy.[26] These effects are interesting in their own right because they suggest the importance of weather conditions in explaining illegal migration flows.

Not only is the first stage strong, with an *F*-statistic of about 40, but the estimated coefficient on past rainfall shocks barely changes with the addition of further

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25.   Probit regressions provide very similar results.

26.   The standard deviation is computed as the average within country standard deviation across all countries of origin in our sample over the period 1979–2007.

TABLE 3. Illegal residence status and rainfall shocks: first stage regressions.

	Alternative definitions of Illegal residence status								Demographics characteristics		
	Illegal						Illegal2	Illegal3	ln (age)	Female	Higher education
	1	2	3	4	5	6	7	8	9	10	11
ln (Rainfall (T))	0.032*** [0.005]	0.030*** [0.005]	0.031*** [0.005]								
ln (Rainfall (T − 1))		0.018*** [0.005]	0.017*** [0.005]								
ln (Rainfall (T − 2))			0.006 [0.004]								
ln (Rainfall (T,T − 1))				0.046*** [0.007]	0.046*** [0.007]	0.045*** [0.007]	0.049*** [0.008]	0.039*** [0.007]	0.002 [0.003]	−0.004 [0.005]	0.007 [0.006]
ln (Current Rainfall))				−0.002 [0.006]	−0.002 [0.006]	−0.003 [0.007]	0.002 [0.006]	0.005 [0.008]			
Baseline controls	X	X	X	X	X	X	X	X	X	X	X
Individual controls				X	X	X	X	X			
HH controls					X	X	X	X			
ln (income)						X					
Observations	13,672	13,672	13,672	13,672	13,672	13,672	13,672	13,672	13,672	13,672	13,672
R-squared	0.211	0.214	0.214	0.234	0.240	0.254	0.237	0.205	0.133	0.182	0.078
IV: F-stat	34.46	20.93	15.18	40.15	39.49	35.55	38.51	29.32	0.31	0.45	1.40
IV: p-value F-stat	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.58	0.50	0.24

Note: This table reports LPM estimates of the "*Illegal residence status*" dummy (columns (1)–(8)) and of individual characteristics (log age, gender, education; columns (9)–(11)) on rainfall and other controls. The dummy *Illegal residence status* equals one if the respondent lacks legal status (alternative definitions of illegal residence status are used in columns (7) and (8)). The variables *ln (Rainfall)* are the logarithm of rainfall in the country of origin (normalized by the average within-country standard deviation) in the year of emigration (*T*), 1 year before emigration (*T* − 1), 2 years before emigration (*T* − 2), and averaged over the year of migration and the year before (*T, T* − 1). The variable *ln (Current Rainfall)* is the logarithm of rainfall in the country of origin (normalized by the average within-country standard deviation) in the year of the interview. *Baseline controls* include: origin country dummies, year dummies (2004–2007) and number of years of residence in Italy of the respondent. *Individual controls* include: gender, age, age squared, dummies for education level (none, primary, secondary, tertiary) and dummies for Italian province of residence. *HH controls* include: number of members in the household living in Italy, a dummy for spouse living abroad, number of children living in Italy and abroad, dummy for home ownership. *ln (income)* is log monthly income. For each column, the table reports the *F*-statistic (*IV: F-stat*) and the *p*-value (*IV: p-value F-stat*) from a joint significance test of the excluded instrument(s). Sample: immigrants with 1–10 years of residence in Italy. ISMU survey years: 2004–2007. Standard errors: robust and clustered by country of origin (103 clusters); * significant at 10%; ** significant at 5%; *** significant at 1%.

controls, suggesting that observable individual and household characteristics are hardly correlated with rainfall shocks in T and $T–1$. This suggests that rainfall shocks do not change the composition of the migrant population—something we investigate in more detail in columns (9)–(11), where we report results from a regression of predetermined personal characteristics at emigration (log age, gender, and education) on rainfall shocks at the time of emigration, years since migration and a set of dummies for interview year and country of origin (standard errors are clustered by country of origin). Estimates suggest no significant relation between weather shocks at the time of emigration and age, gender, or education.[27] This is in line with the hypothesis that these shocks, while affecting the probability of illegal status, do not change the overall composition of the pool of immigrants, at least not with respect to some key observables that are typically correlated with risk aversion.[28]

The underlying rationale for our instrument is that rainfall provokes income shocks by affecting agricultural production, thereby temporarily relaxing the liquidity constraints that restrict migration.[29] Our instrument should thus be stronger in countries with a larger agricultural sector and where the households are more likely to be cash-in-advance constrained. To investigate this conjecture, we rerun our first stage regressions but interact the instrument (log rainfall in time T and $T – 1$) with dummies that identify countries whose GDP (or employment) share in agriculture is, respectively, above the 75th percentile or below the 25th percentile of the distribution of countries in our sample (columns (2) and (4) of Online Appendix Table A.4). Estimates show that the effect of rainfall in countries with GDP (employment) shares in agriculture above the 75th percentile is about 2 times larger than in the rest of the countries, although the effect of rainfall remains important also in countries with a smaller agricultural sector; results remain unchanged even when we control for log GDP per capita in

27.   Similarly, for the United States Munshi (2003) finds no significant relation between gender, age and education of Mexican migrants and rainfall levels at the time of migration (see his footnote 33). There is evidence that risk aversion is a strong correlate of some observable characteristics: women are found to be more risk averse than men (see, e.g., Borghans et al. 2009), risk aversion is higher among the least educated (Guiso and Paiella 2008) and nonmonotonically related to age (see Barsky et al. 1997).

28.   Although these estimates suggest that rainfall shocks do not affect the pool of potential immigrants, in Online Appendix Section A2.1 we discuss the consequences for the IV estimates when this condition is violated. In particular, we show that if rainfall shocks select low risk averse individuals into the pool of potential migrants, the IV estimates will be upward biased (similarly to OLS), and identify a lower bound (in absolute value) of the effect of being illegal on consumption. The second assumption for our identification strategy is that, conditional on observable characteristics and legal status, rainfall shocks at emigration have no effect on immigrants' current economic decisions either through weather shock persistence or through effects on later behavior. If there is persistence in rainfall shocks, past rainfall shocks would be correlated with current shocks, which in turn would affect behavior. In unreported regressions, however, we find no evidence for any persistence in rainfall shocks.

29.   Other papers that establish a positive relationship between income shocks and migration include Bryan et al. (2014) and Angelucci (2015). Dustmann and Okatenko (2014) show that the likelihood of an intention to migrate increases with wealth in both Africa and Asia. McKenzie and Rapoport (2007) find that the probability of migration is mostly increasing in household resources for households in communities with less developed migration networks, likely to be the case in Italy due to its relatively recent immigration experience.

the country of origin at the time of migration (columns (3) and (5)). In columns (6) and (7) of Online-Appendix Table A.4, we investigate more directly the role played by credit markets. If credit markets are sufficiently developed, potential migrants (or their relatives) should be able to borrow against their future earnings in the destination country (and/or some collateral in the origin country) in order to finance the migration cost. We therefore expect the first stage of our IV estimates to work better in countries with low levels of financial development. Proxying financial market development by using the World Bank's World Development Indicator that estimates the credit provided by the domestic banking sector as a percentage of GDP,[30] we find that our instrument is stronger in countries that are financially less developed—a finding that further supports our hypothesis that rainfall shocks relax budget constraints that prevent individuals from migrating.

We further test the robustness of our first stage estimates to the exclusion of large countries where our instrument is potentially less precise. After ranking the countries in our sample according to their total land area, we have rerun our regressions and progressively excluded the 10, 20, and 30 largest countries (columns (8)–(10) of Online Appendix Table A.4). Estimates remain unaffected. In further robustness checks, we have tested for potential heterogeneity in the effect of rainfall on legal status across different geographical areas, showing that our instrument is relevant in all areas included in our sample (see Online Appendix Table A.6).[31] As we discuss in Section 4.2.1, our estimates are also not sensitive to the exclusion of major rainfall shocks.

To test the assumption that rainfall shocks generate indeed temporary income shocks in the sending countries, we use World Bank data on agricultural and total income for a panel of almost 100 developing countries over the period 1979–2012 that correspond to the countries included in our main estimation sample (see Online Appendix Table A.1), and match to them the GPCP data on rainfalls. In columns (1) and (2) of Online Appendix Table A.5, we report regressions of agricultural income and total income per capita on the log of yearly rainfall, conditioning on country dummies, year dummies and country-specific time trends, and we cluster standard errors at the country level. In both regressions, the estimated coefficient on log rainfall is positive and strongly significant, with a rainfall elasticity of agricultural income and total income of about 0.2 and 0.03, respectively. Further, to determine whether rainfall

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30.   Giuliano and Ruiz-Arranz (2009) use this variable as a proxy for financial development. The banking sector includes monetary authorities and deposit money banks, as well as other banking institutions (savings and mortgage loan institutions and building and loan associations) for which data are available. We match each individual in our sample with the average size of the agricultural sector over the period 1995–2007 in their country of origin (irrespective of their year of migration), and with the size of the banking sector in the year of emigration.

31.   In Online Appendix Table A.6, we have estimated the same regression excluding each geographic area of origin at a time (i.e., Sub-Saharan Africa, Eastern Europe, Latin America, Middle East, and North Africa, and Asia), with very similar estimates across specifications (see columns (1)–(6)). In column (7), we estimated our first stage regression interacting (log) rainfall with dummies identifying each region in our sample. The estimated coefficients on the interaction terms are all positive, similar in magnitude and statistically significant (with the exception of the interaction for Latin America, which is in the same range than the other estimates, but imprecisely estimated).

affects income through its effect on agricultural output, we regress the log of total income per capita on the log of agriculture output, instrumenting agricultural output with log rainfall. We report OLS and IV estimates in columns (3) and (4), and the first stage estimate in the second panel in column (4). The agricultural output elasticity of income is about 0.24, whereas the first-stage rainfall elasticity of agricultural output is about 0.13, with all coefficients being statistically significant at the 1% level. These estimates therefore support our hypothesis that weather-induced positive income shocks may relax credit constraints.[32]

4.1.2. Legalization Quotas. In Table 4, we report first stage estimates of the quota instrument. In Panel A, we use our main quota instrument, regressing illegal residence status on the residuals of the log of total quotas obtained from estimating equation (3) (see Section 2.3). Columns (1)–(4) gradually add controls, where all specifications condition on both years since arrival in Italy and interview year dummies. Since we eliminate the systematic relationship between time spent in Italy and accumulated quotas, the coefficient on the quota IV captures the variation in accumulated residence permits off the accumulated linear trend. The estimated coefficient on the quota variable is strongly significant, hardly changes across specifications (which suggests that it is orthogonal to individual characteristics), and has the expected negative sign: being "exposed" to larger accumulated deviations of quotas from their linear trend since arrival in Italy reduces the probability of being illegal at interview. In particular, a 1% increase in the cumulative number of residence permits offered through the quota system since arrival in Italy–corresponding to almost 6,000 more places with respect to the mean value in the sample—would imply a 0.3 percentage point lower probability of being undocumented.[33] Overall, estimates in Panel A of Table 4 show that accumulated residence permits off the accumulated linear trend are a strong predictor of current legal status, with an *F*-statistics of around 60.

In Panel B of Table 4, we report first stage estimates from the alternative quota instruments we describe in Section 2.3. We first report results using the difference between actual and "expected" quotas, predicted under the assumption that immigrants have "adaptive" expectations on future quotas. We then report results where we maintain the assumption of "rational" expectations, but we split the instrument between country-reserved quotas and general quotas (see Section 2.3). For the three major sending countries with reserved quotas—Albania, Morocco, and Tunisia (see Online Appendix Table A.3)—we match each migrant with the residuals of the cumulative number of available slots in the national-specific quotas ("country-reserved quotas") since arrival in Italy, whereas we match migrants from all other countries to the residuals

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32. Other papers find evidence of a positive effect of rainfall on income. For instance, using data from Sub-Saharan African countries, Brückner and Ciccone (2011) show a rainfall elasticity of total income of about 0.07. See also Miguel et al. (2004) and Barrios et al. (2010) for qualitatively similar results.

33. Our instrument varies by years since arrival in Italy (1–10) and interview year (2004–2007) and we cluster the standard errors accordingly (i.e., 40 clusters). Clustering by country or origin—as in Table 3— leads to slightly smaller standard errors.

TABLE 4. Illegal residence status and quotas: first stage regressions.

	1	2	3	4
Panel A: Main quota IV				
ln (Total Quotas)	− 0.309***	− 0.301***	− 0.306***	− 0.294***
	[0.039]	[0.039]	[0.039]	[0.038]
IV: *F*-stat	61.32	59.72	61.69	58.59
IV: *p*-value *F*-stat	0.00	0.00	0.00	0.00
Panel B: Alternative quota IVs				
ln (Total Quotas)-"adaptive" expect.	− 0.118***	− 0.114***	− 0.116***	− 0.111***
	[0.042]	[0.041]	[0.041]	[0.039]
IV: *F*-stat	7.88	7.54	7.75	8.01
IV: *p*-value *F*-stat	0.01	0.01	0.01	0.01
ln (Country-Reserved Quotas)	− 0.324***	− 0.314***	− 0.318***	− 0.308***
	[0.049]	[0.048]	[0.048]	[0.047]
ln (Non–Country-Reserved Quotas)	− 0.149***	− 0.146***	− 0.149***	− 0.140***
	[0.027]	[0.026]	[0.026]	[0.026]
IV: *F*-stat	49.60	48.07	50.67	46.68
IV: *p*-value *F*-stat	0.00	0.00	0.00	0.00
Baseline controls	X	X	X	X
Individual controls		X	X	X
HH controls			X	X
ln (income)				X
Observations	13,672	13,672	13,672	13,672

Note: This table reports LPM estimates of the "*Illegal residence status*" dummy on the quota system instruments. The dummy *Illegal residence status* equals one if the respondent lacks legal status. In Panel A, we use our main quota instrument, *ln (Total Quotas),* that is, the residuals of log of total quotas since arrival in Italy after estimating equation (3) (see Section 2.3). In panel B, we use two alternative quota instruments. First, we use the instrument *ln (Total Quotas)-"adaptive" expect* (as defined in Section 2.3) and we then jointly use the residuals of the total number of residence permits offered through the quota system to citizens of Albania, Morocco and Tunisia (*ln (Country-Reserved Quotas)*) and of the total number of residence permits offered to immigrants of all other nationalities (*ln (Non–Country-Reserved Quotas)*). *Baseline, individual* and *HH controls* and *ln (income)* are defined as in the note to Table 3. For each regression, the table reports the *F*-statistic (*IV: F-stat*) and the *p*-value (*IV: p-value F-stat*) from a joint significance test of the excluded instrument(s). Sample: immigrants with 1–10 years of residence in Italy. ISMU survey years: 2004–2007. Standard errors: robust and clustered by years since arrival in Italy and year of interview (40 clusters); *significant at 10%; **significant at 5% ***significant at 1%.

of the cumulative nonrestricted number of quotas ("Non–Country-Reserved Quotas"). In both cases, the instruments exhibit negative and strongly significant coefficients, confirming the findings in Panel A. Overall, estimates in Table 4 show that the quota system is a strong predictor of legal status, irrespectively of how we construct the instrument.

4.2. Consumption and Illegal Residence Status

4.2.1. Total Consumption. OLS estimates when regressing the log of individual monthly expenditure for consumption in Italy on illegal residence status and baseline individual controls and household controls are reported in the first two columns of Table 5. Estimates of the illegal status variable are negative and strongly significant (at

TABLE 5. Monthly Consumption: OLS and IV estimates.

	OLS estimates		IV estimates									
			ln (Rainfall (T,T − 1))		ln (Total Quotas)		ln (Rainfall (T,T − 1)) and ln (Total Quotas)		Alternative quota IVs			
									ln (Total Quotas)-"adaptive" expect.		ln (Country-Reserved Quotas) and ln (Non–Country-Reserved Quotas)	
	1	2	3	4	5	6	7	8	9	10	11	12
Illegal residence status	−0.303***	−0.287***	−0.578***	−0.571***	−0.510***	−0.533***	−0.519***	−0.537***	−0.557***	−0.591***	−0.523***	−0.542***
	[0.026]	[0.025]	[0.200]	[0.185]	[0.060]	[0.058]	[0.067]	[0.064]	[0.106]	[0.100]	[0.067]	[0.064]
Baseline controls	X	X	X	X	X	X	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X	X	X	X	X	X	X
HH controls		X		X		X		X		X		X
Observations	13,672	13,672	13,672	13,672	13,672	13,672	13,672	13,672	13,672	13,672	13,672	13,672
IV: F-stat	–	–	39.12	38.52	59.72	61.69	54.33	55.71	7.543	7.754	48.07	50.67
IV: p-value F-stat	–	–	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.01	0.00	0.00
Durbin–Wu–Hausman test: p-value	–	–	0.13	0.09	0.00	0.00	0.00	0.00	0.02	0.00	0.00	0.00
Hansen overidentification test: p-value	–	–	–	–	–	–	0.71	0.82	–	–	0.44	0.54

Note: This table reports OLS (columns (1) and (2)) and IV estimates (columns (3)–(12)) of log monthly consumption on illegal residence status and other controls. The dummy *Illegal residence status* equals one if the respondent lacks legal status. In columns (3)–(12) we treat the variable *Illegal residence status* as endogenous and instrument it with alternative sets of instrumental variables: (a) *ln (Rainfall (T, T−1))* (columns (3) and (4)); (b) *ln (Total Quotas)* (columns (5) and (6)); (c) *ln (Rainfall (T, T − 1)* and *ln (Total Quotas)* (columns (7) and (8)); (d) *ln (Total Quotas)-"adaptive" expect.* (columns (9) and (10)); (e) *ln (Country-Reserved Quotas)* and *ln (Non–Country-Reserved Quotas)* (columns (11) and (12)). Odd columns condition on baseline and individual controls, whereas even columns condition also on household controls. *Baseline, individual,* and *HH controls* are defined as in the note to Table 3. For each column, the table reports the *F*-statistic (*IV: F-stat*) and the *p*-value (*IV: p-value F-stat*) from a joint significance test of the excluded instrument(s) and the *p*-value from a regression-based version of the Durbin–Wu–Hausman test for endogeneity (*Durbin–Wu–Hausman test: p-value*). When illegal residence status is instrumented with more than one instrument (columns (7) and (8) and (11) and (12)) the *p*-value from the Hansen overidentification test is reported. Sample: immigrants with 1–10 years of residence in Italy. ISMU survey years: 2004–2007. Standard errors: robust and clustered by country of origin (103 clusters); *significant at 10%; **significant at 5% ***significant at 1%.

the 1% level), suggesting that undocumented immigrants' total consumption is about 25% lower than that of documented immigrants.

IV estimates that address endogenous selection into legal status are reported in columns (3)–(12) of Table 5, where columns (3)–(4) use the rainfall instrument, columns (5) and (6) use the quota instrument, and columns (7) and (8) use both rainfall and total quotas instruments. In columns (9)–(12), we use the alternative quota instruments we discussed before (see Section 2.3). For each set of instruments, odd columns condition only on baseline and individual controls, whereas even columns add household controls. IV estimates of the coefficient on illegal residence status are all negative, strongly significant and larger in size (about 1.5–2 times larger) than the corresponding OLS estimates reported in columns (1) and (2) of Table 5. The estimates are remarkably stable across specifications, despite using two very different identification strategies and alternative definitions of the instruments. The estimated coefficient on illegal residence status is between -0.51 and -0.57, suggesting a 40%–45% lower total consumption of illegal immigrants. One reason for why IV estimates are larger than OLS estimates is that less risk averse individuals tend to select into illegal migration (see Online Appendix Section A.2.1).

The next to last row of Table 5 reports the p-values from the Durbin–Wu–Hausman test, which tests the null hypothesis that the OLS estimator is consistent and efficient against the alternative that it is inconsistent, for each of the specifications we report. At the 10% level we reject the null hypothesis for all but one specification. Further, when illegal residence status is instrumented with more than one instrument (column (7) and (8), and (11) and (12)), the last row of Table 5 reports the p-value from the Hansen overidentification test. In all cases, we fail to reject the null hypothesis that the instruments are valid (i.e., uncorrelated with the error term).

We report alternative specifications and robustness checks in Table 6, where we condition on baseline and individual controls.[34] We report OLS estimates in Panel A, and IV estimates in Panel B. Each row of Panel B refers to a different instrument set. As a reference point, column (1) reports the estimates in the odd columns of Table 5.

We first test the robustness of our results to alternative measures of consumption. In column (2), we use the standard OECD equivalence scale to calculate individual consumption from household consumption, whereas in column (3) we simply divide household consumption by the unweighted number of household members in Italy. Our estimates are similar across these different measures of individual consumption. Next, we use the two alternative definitions of undocumented immigrants described in Section 3: columns (4) and (5) report estimates for the more restrictive (Illegal2) and less restrictive (Illegal3) definition, respectively. Estimates are again very similar to those of our main specification.

We show in Table 1 that 15% of the households of legal immigrants in our sample live in their own house, whereas this is the case for only 1% of undocumented

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34. Conditioning on additional household controls delivers very similar estimates (as shown in Table 5).

immigrants.[35] Our measure of total consumption includes housing expenditure and we always include among the controls a dummy for home ownership. As a robustness check, we rerun our regressions but with all homeowners excluded from the sample, which leaves us with 11,879 observations. As column (6) in Table 6 shows, the estimated coefficients on the illegal residence status are similar to those in Table 5. Finally, we restrict the sample to immigrants who report being the only member in their household. For this subsample, consumption and income at the household and at the individual level are the same. Although this restriction more than halves the sample, size and significance of the estimated coefficients are again very similar to those in previous specifications (see column (7) of Table 6).

In Online Appendix Table A.7, we report OLS (Panel A) and IV (Panel B) estimates of our main consumption equation, varying the residence threshold from at most 5 to at most 15 years. The estimates are remarkably stable across different thresholds. In Online Appendix Table A.8 we test the robustness of our estimates to the exclusion of major rainfall shocks. Again, OLS and IV estimates are hardly affected by imposing these restrictions on our sample, whereas the rainfall instrument becomes even stronger when extreme rainfall shocks are excluded.

The estimates presented so far suggest that undocumented immigrants consume substantially less than documented immigrants, and that OLS estimation underestimates the difference in consumption between the two groups. This is compatible with illegal immigrants being less risk averse, leading to a bias toward zero in OLS regressions. Although the previous results refer to the overall impact of legal status on consumption behavior, one possible channel may be that illegal immigrants have lower incomes, which in turn leads to lower consumption. In fact, Table 2 points at such income differences between the two groups. We now turn to analysis that investigates how much of the difference in consumption can be accounted for by differences in income.

4.2.2. Consumption and Income.   As we discuss in Section 2.5, one problem with income in a consumption equation is that the same unobservables may affect both variables alike, which may lead to biased estimates. This problem is exacerbated in our case, as we observe income only at the household level, and we do not have a separate measure on wages and hours worked. If legal status and income are correlated then this may lead to biased estimates in the legal status variable if income is included in the consumption equation. To address this problem we use an IV strategy (outlined in Section 2.5), using local unemployment rates in the province of residence as instrument for immigrants' income. As income itself may depend on legal status, the first stage equation will include also the two instruments for illegal status.

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35.   In Italy, undocumented immigrants cannot legally buy a house. Some of the undocumented immigrants may however live in a house owned by another member of the household who is a legal resident in Italy.

TABLE 6. Monthly consumption—robustness checks: OLS and IV estimates.

	Alternative measures of consumption			Alternative definitions of Illegal residence status		Subsamples	
	"Modified" OECD equivalence scale	OECD equivalence scale	Unweighted normalization	Illegal2	Illegal3	Excluding home owners	Only one-member HHs
	1	2	3	4	5	6	7
	Panel A: OLS estimates						
Illegal residence status	−0.303***	−0.262***	−0.203***	−0.316***	−0.238***	−0.285***	−0.275***
	[0.026]	[0.027]	[0.029]	[0.027]	[0.025]	[0.027]	[0.025]
	Panel B: IV estimates						
	IV: ln (Rainfall ($T,T-1$))						
Illegal residence status	−0.578***	−0.633***	−0.712***	−0.546***	−0.680***	−0.571***	−0.534***
	[0.200]	[0.201]	[0.208]	[0.193]	[0.240]	[0.180]	[0.131]
	IV: ln (Total Quotas)						
Illegal residence status	−0.510***	−0.531***	−0.570***	−0.511***	−0.568***	−0.505***	−0.543***
	[0.060]	[0.062]	[0.073]	[0.065]	[0.065]	[0.065]	[0.084]
	IV: ln (Rainfall ($T,T-1$)) and ln (Total Quotas)						
Illegal residence status	−0.519***	−0.544***	−0.587***	−0.516***	−0.580***	−0.514***	−0.541***
	[0.067]	[0.068]	[0.076]	[0.072]	[0.072]	[0.068]	[0.077]
Baseline controls	X	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X	X
Observations	13,672	13,672	13,672	13,672	13,672	11,879	6,069

Note: This table reports OLS (Panel A) and IV estimates (Panel B) of log monthly consumption on illegal residence status and other controls. The dummy *Illegal residence status* equals one if the respondent lacks legal status. Each row in Panel B reports IV estimates obtained using a different instrument (set of instruments): (a) *ln (Rainfall (T, T − 1))*; (b) *ln (Total Quotas)*; (c) *ln (Rainfall (T, T − 1))* and *ln (Total Quotas)*. We perform the following robustness checks: (a) we use three alternative definitions of individual consumption, as defined in Section 3.1 (columns (1)–(3)); (b) we use two alternative definitions of illegal status, as defined in Online Appendix Table A.2 (columns (4) and (5)); (c) we drop from the main sample all respondents who report to be living in their own house (column (6)); (d) we restrict the estimating sample to households with only one member (column (7)). *Baseline* and *individual controls* are defined as in the note to Table 3. Sample: immigrants with 1–10 years of residence in Italy. ISMU survey years: 2004–2007. Standard errors: robust and clustered by country of origin (103 clusters); *significant at 10%; **significant at 5% ***significant at 1%.

We report the first stage estimates from regressing log monthly income (obtained by dividing household income by the number of "equalized" adults using the modified OECD scale) on provincial unemployment rates and other controls in Online Appendix Table A.9.[36] We condition on province and year dummies, and gradually add individual and household controls. We also include (the log of) provincial GDP per capita to keep other macro conditions constant. In columns (4)–(9) of Online Appendix Table A.9, we include in addition the rainfall and quota instruments (individually and jointly) that we use to instrument illegal residence status. These specifications are precisely the first stage estimates when both residence status and income are treated as endogenous variables in the consumption equation (see Table 7). Estimates in Online Appendix Table A.9 show that the unemployment rate in the province of residence is a strong predictor of immigrants' income, significant at the 1% level with an *F*-statistics of around 10. In terms of magnitude, a one-percentage point increase in unemployment leads to a 5%–6% reduction in immigrants' income. As a benchmark, the unemployment rate was about 3.7% in the Lombardy region between 2004 and 2007.[37]

We report estimates of the effect of illegal status *conditional on income* on monthly total consumption in Table 7. When conditioning on income, the OLS estimate decreases (in absolute value) but remains strongly significant (column (1)), suggesting that about half of the difference in consumption between legal and illegal immigrants results from illegal immigrants having lower income. The implied income elasticity of consumption is about 0.77.

IV estimates are reported in columns (2)–(7) of Table 7. In all these regressions, we treat illegal residence status as endogenous and present estimates using rainfall shocks (columns (2) and (3)), total quotas (columns (4) and (5)), and both (columns (6) and (7)) as instruments. In columns (3), (5), and (7), we further instrument (log) income. When instrumenting only legal status (columns (2), (4), and (6)), the estimated coefficient on this latter variable increases (in absolute value) with respect to the OLS coefficient reported in column (1), suggesting that undocumented immigrants consume about 20%–25% less than documented immigrants, conditional on income. However, when instrumenting income as well, the coefficient on illegal status increases, implying now that illegal immigrants' consumption is about 28%–32% lower than that of legal immigrants. Further, the estimated elasticity of consumption to household income drops from 0.7 to 0.4 when the latter variable is instrumented. It is straightforward to show that these estimates are consistent with a situation where more

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36.   Our measure of income captures individual current income. In addition, all our regressions always include individual controls of the respondents (such as age, gender and education), which are potentially better measures of permanent income.

37.   Standard errors are clustered at the province (11) and interview year (2004–2007) level (i.e., 44 clusters), corresponding to the variation of the instrument.

TABLE 7. Monthly consumption—conditioning on income: OLS and IV estimates.

| | OLS | IV estimates | | | | | |
| | | ln (Rainfall $(T,T-1)$) | | ln (Total Quotas) | | ln (Rainfall $(T,T-1)$) and ln (Total Quotas) | |
	1	2	3	4	5	6	7
Illegal residence status	−0.132***	−0.296**	−0.394**	−0.182*	−0.323**	−0.196**	−0.338***
	[0.025]	[0.132]	[0.161]	[0.107]	[0.141]	[0.097]	[0.128]
ln (income)	0.773***	0.757***	0.487***	0.768***	0.458**	0.766***	0.446**
	[0.014]	[0.016]	[0.177]	[0.016]	[0.179]	[0.016]	[0.182]
				Variables instrumented:			
Illegal residence status	–	X	X	X	X	X	X
ln (income)	–	–	X	–	X	–	X
All controls	X	X	X	X	X	X	X
Observations	13,672	13,672	13,672	13,672	13,672	13,672	13,672

Note: This table reports OLS (column (1)) and IV estimates (columns (2)–(7)) of log monthly consumption on illegal residence status, log monthly income and other controls. The dummy *Illegal residence status* equals one if the respondent lacks legal status. In columns (2)–(7), we treat the variable *Illegal residence status* as endogenous and instrument it with: (a) *ln (Rainfall (T, T − 1))* (columns (2) and (3)); (b) *ln (Total Quotas)* (columns (4) and (5)); (c) *ln (Rainfall (T, T − 1))* and *ln (Total Quotas)* (columns (6) and (7)). In columns (3), (5), and (7) we instrument *ln (income)* with the unemployment rate in the province of residence. *All controls* include *baseline, individual* and *HH controls* as defined in the note to Table 3. Sample: immigrants with 1–10 years of residence in Italy. ISMU survey years: 2004–2007. Standard errors: robust and clustered by country of origin (103 clusters). * Significant at 10%; ** significant at 5%; *** significant at 1%.

risk averse individuals consume less and also earn less (as they are less entrepreneurial), and income is negatively correlated with illegal status.[38]

To summarize, unconditional on income, IV estimates suggest that undocumented immigrants consume about 40%–42% less than documented immigrants. Part of this reduction in consumption is due to illegal immigrants having lower incomes than legal immigrants. Conditioning on income, and instrumenting both income and legal status, results in an estimated 29% lower consumption of illegal immigrants.

4.2.3. Consumption Categories.   Our data allow us to disaggregate total consumption into three categories: expenditure for food and clothes; housing expenditure; and other expenditure, such as transportation and leisure (see Section 3). To gain further insight into immigrants' consumption choices, we now use this information to estimate the effect of illegal status on consumption in each of these categories. Results are reported in Table 8, where columns (1)–(3) refer to food and clothes, columns (4)–(6) to housing, and columns (7)–(9) to other consumption. We report estimates conditioning on individual and household controls.

Panel A reports OLS results where the dependent variable is the (log of) monthly consumption for each category. As in previous tables, IV estimates in Panel B of Table 8 refer to different sets of instrumental variables. Overall, IV coefficients are larger in size than OLS coefficients and precisely estimated, showing that undocumented immigrants reduce each type of consumption. However, there is substantial variation across categories. Although estimates suggest a 25% lower expenditure for food and clothing and for "other" consumption goods (which is the residual category), the largest reduction is for housing expenditure (minus 55%).

The large reduction in housing consumption is interesting, and may have several explanations. First, given the low income of illegal immigrants, which places them below the poverty line in Italy, housing may be the main margin where adjustments can be made.[39] As illustrated in Online Appendix Table A.10, a larger share of undocumented immigrants tends to use free accommodation such as homeless shelter (11% vs. 5%), sleeps in accommodation provided by the employer (15.3% vs. 5.9%), and shares accommodation with immigrants who are not relatives. Only a small proportion (1.3%) of undocumented immigrants reports living in an owned property compared to 14.9% of documented immigrants. The large difference in housing expenditure between documented and undocumented immigrants may also reflect constraints on housing consumption, as undocumented immigrants cannot sign a legal rental contract, obtain a mortgage or purchase a flat. Obtaining legal residence status

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38. We have also constructed a measure of predicted provincial wages as an alternative instrument for income. Results (available upon request) are very similar to those reported where we use local unemployment as an instrument.

39. Over the period considered (2004–2007), the absolute poverty line for an individual residing in urban areas in the north of Italy was about €710, whereas the relative poverty line was €570. The average undocumented immigrant in our sample falls below both thresholds, whereas the average documented immigrant is just above the relative poverty line.

TABLE 8. Monthly consumption—consumption categories: OLS and IV estimates.

	Food and clothes			Housing			Other		
	1	2	3	4	5	6	7	8	9
Panel A: OLS estimates									
Illegal residence status	−0.292*** [0.049]	−0.274*** [0.044]	−0.130** [0.053]	−0.821*** [0.085]	−0.748*** [0.079]	−0.471*** [0.072]	−0.308*** [0.045]	−0.314*** [0.044]	−0.126*** [0.036]
Panel B: IV estimates									
IV: ln (Rainfall (T,T−1))									
Illegal residence status	−0.721*** [0.239]	−0.718*** [0.233]	−0.470** [0.191]	−1.610*** [0.507]	−1.660*** [0.467]	−1.183*** [0.450]	−0.612 [0.387]	−0.567 [0.380]	−0.228 [0.282]
IV: ln (Total Quotas)									
Illegal residence status	−0.554*** [0.098]	−0.573*** [0.099]	−0.252** [0.124]	−1.227*** [0.333]	−1.375*** [0.315]	−0.756*** [0.291]	−0.777*** [0.150]	−0.736*** [0.145]	−0.313*** [0.119]
IV: ln (Rainfall (T,T−1)) and ln (Total Quotas)									
Illegal residence status	−0.574*** [0.098]	−0.590*** [0.100]	−0.278** [0.117]	−1.273*** [0.318]	−1.409*** [0.302]	−0.808*** [0.272]	−0.757*** [0.149]	−0.715*** [0.144]	−0.303*** [0.110]
Baseline controls	X	X	X	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X	X	X	X
HH controls		X	X		X	X		X	X
ln (income)			X			X			X
Observations	13,672	13,672	13,672	13,672	13,672	13,672	13,672	13,672	13,672

Note: This table reports OLS (Panel A) and IV estimates (Panel B) of log monthly expenditure for three different categories of consumption (food and clothes, columns (1)–(3); housing, columns (4)–(6); other, column (7)–(9)) on illegal residence status and other controls. The dummy *Illegal residence status* equals one if the respondent lacks legal status. Each row in Panel B reports IV estimates obtained using a different instrument (set of instruments): (a) *ln (Rainfall (T, T − 1))*; (b) *ln (Total Quotas)*; (c) *ln (Rainfall (T, T − 1))* and *ln (Total Quotas)*. All these instruments are defined as in the notes to Tables 3 and 4. *Baseline, individual, and HH controls* and *ln (income)* are defined as in the note to Table 3. Sample: immigrants with 1–10 years of residence in Italy. ISMU survey years: 2004–2007. Standard errors: robust and clustered by country of origin (103 clusters); ***Significant at 5%; *significant at 10%; **significant at 5% ***significant at 1%.

would therefore correspond to relaxing these constraints, resulting in an increase in the estimated parameters.

4.2.4. Does Consumption Respond to Increased Uncertainty?

One interpretation of the findings presented in the previous sections is that documented and undocumented immigrants adjust their consumption behavior to the exposure to different degrees of income risk, meaning that the difference in consumption may be partly due to differences in precautionary savings. To investigate this further, and to provide additional evidence that precautionary motives are at least one channel for the reduction in consumption of illegal immigrants, we now use a different identification strategy, based on varying uncertainty *within* the group of illegal immigrants. In particular, we use variation in deportation risk across municipalities and over time and investigate whether consumption behavior of illegal (but not of legal) immigrants responds to these changes.

That deportation risks are well recognized by immigrants is illustrated by Amuedo-Dorantes et al. (2013) who show in the US context that about 50% of a sample of undocumented returnees to Mexico feared being deported while residing in the United States, and that such fears were higher when immigrants lived in states that enacted more punitive measures against illegal residents. In Italy, the enforcement intensity in each Italian province is decided at the local level where instructions from the central government are balanced with local necessities and priorities (see Fasani 2010b). Hence, the number of undocumented immigrants deported each year varies not only across, but also within provinces.[40] Given the unsystematic way in which these changes are imposed, it is unlikely that undocumented immigrants can predict future levels of enforcement across areas to adjust their province of residence ex ante. Deportation risk, however, becomes observable once stricter controls are imposed. Immigrants can react by moving to another province, which however is costly as it means leaving job/accommodation/contacts they have in the province of residence. Although the most risk averse migrants may move away, others will stay and adjust their consumption accordingly. Hence, if there was self-selection into provinces according to deportation risk, we would expect the more risk averse to move to areas with lower risks of deportation. This would mean that the estimates we report below can be interpreted as a lower bound. It is important to note that, although changes in deportation risk are temporary, they can produce long-lasting effects on future income, as a deportation leads to a substantial decrease in lifetime income.[41]

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40. Between 2004 and 2006, in the Lombardy region, the average probability of deportation in each year was about 2.6% (with a standard deviation of 1.68 and a within-province standard deviation of 1.15), with a minimum value of 0.04% and a maximum value of 6.76% (see Table 9). Further, over the period considered here, the probability of undocumented immigrants being arrested and being detained for up to 60 days is about 5 times larger than the probability of actually being deported.

41. In addition to financial losses, migrants deported from a country of the Schengen area—which includes all E.U. countries except Great Britain and Ireland plus Iceland, Liechtenstein, Norway, and Switzerland—are banned from legal access to any Schengen country for a minimum of five and a maximum of 10 years.

To make progress, we construct for each province of immigrant residence a measure of deportation risk for the 2004–2006 period.[42] We then run the same regressions on consumption as in the previous sections but include the probability of deportation in the province of residence as an additional control variable, together with time and province fixed effects and other background characteristics. Therefore, our Diff-in-Diff design uses only the within-province variation in deportation probabilities for identification.[43]

We report our estimates for illegal immigrants in columns (1)–(4) of Table 9, whereas columns (5)–(8) report those for legal immigrants, who should be unresponsive to these variations. For both groups, we first control only for time and province fixed effects, and then add additional individual and household controls. Estimates show that an increase in deportation risk within provinces is significantly associated with lower consumption for undocumented immigrants, with a one (within province) standard deviation increase in the probability of deportation (about 44% with respect to the mean) implying a 13% reduction in consumption (columns (1)–(4)). In contrast, and consistent with our hypothesis, we find no significant effects of deportation probabilities on consumption for legal immigrants (columns (5)–(8)), with point estimates being close to zero. Moreover, estimated coefficients on the deportation probabilities hardly change when additional controls are included, which indicates that deportation risk is uncorrelated with immigrant characteristics, pointing at there being no selection into provinces based on deportation risk.

These findings speak in favor of income risk being an important determinant of illegal immigrants' consumption behavior, and support the hypothesis that precautionary motives are an important reason for illegal immigrants consuming less than legal immigrants.

5. Discussion and Conclusions

This paper is—to the best of our knowledge—the first analysis of consumption behavior of immigrants. Drawing on unique data for a country that experienced large scale immigration over the past decades, much of it undocumented, we illustrate stark differences in consumption pattern between documented and undocumented immigrants. Using two novel instrumental variable strategies to address the sorting of immigrants into legal status, we speak to an important issue in the policy debate on

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42.   The Lombardy region is divided into eleven administrative provinces, and the Italian Ministry of Interior reports the number of undocumented immigrants removed every year from each province (see Fasani 2010b for details). Based on the ISMU survey, we compute the stock of undocumented migrants residing in each county in each year. The ratio of these two measures provides a proxy for the annual probability of being deported. Italian provinces do not simply represent administrative entities, but also functional local labor markets. For instance, the highest level of geographical disaggregation of Labor Force Survey statistics provided by the Italian National Institute of Statistics (ISTAT) is at the provincial level (NUT3).

43.   We use the same estimating sample as the one of our main specification, but we omit observations for 2007 because deportation risk measures are missing for that year.

TABLE 9. Probability of deportation and consumption.

	Undocumented immigrants				Documented immigrants			
	1	2	3	4	5	6	7	8
Prob. of deportation	-0.157***	-0.115***	-0.117***	-0.116***	-0.012	-0.012	-0.012	-0.010
	[0.036]	[0.041]	[0.039]	[0.032]	[0.015]	[0.014]	[0.014]	[0.006]
Province and year dummies	X	X	X	X	X	X	X	X
Individual controls		X	X	X		X	X	X
HH controls			X	X			X	X
ln (income)				X				X
Observations	1,299	1,299	1,299	1,299	8,413	8,413	8,413	8,413
R-squared	0.100	0.317	0.325	0.569	0.141	0.169	0.190	0.588
			Mean	Std Dev	Within Std Dev	Min − Max		
Probability of deportation (%)			2.59	1.68	1.15	0.04 − 6.76		

Note: This table reports OLS regression estimates of log of monthly consumption on the probability of deportation in the province of residence and other controls. *Province and year dummies* include: dummies for Italian province of residence and year dummies (2004–2006). *Individual controls* include: gender, age, age squared, dummies for education level (none, primary, secondary, tertiary), origin country dummies, number of years of residence in Italy of the respondent. *HH controls* include: number of members in the household living in Italy, a dummy for spouse living abroad, number of children living in Italy and abroad, dummy for home ownership. Sample: immigrants with 1–10 years of residence in Italy. ISMU survey years: 2004–2006. Standard errors (in brackets): robust and clustered by Italian province of residence (11 provinces) and year of the interview (3 years); * significant at 10%; ** significant at 5% *** significant at 1%.

illegal immigration—the effect of legalization programs on immigrants' consumption behavior.

Our results imply that legalization programs may lead to an increased level of immigrant consumption in the host country, with potentially beneficial effects for both the host country's economy and the immigrants' degree of socioeconomic integration. Indeed, our estimates could be cautiously used to provide back-of-the-envelope calculations of how an amnesty that regularizes all undocumented immigrants could increase consumption in Italy. To do so, we would need to assume that our estimated coefficient is an average treatment effect and that a generalized amnesty would not produce general equilibrium effects that could affect that coefficient. In that case, after amnesty, the regularized immigrants would raise their monthly per capita expenditure by about €230,[44] meaning that, based on the latest estimate of the number of undocumented immigrants (560,000 in 2010; see Fasani et al. 2013), a legalization of all immigrants in Italy would translate into a total increase in consumption of about €1.5 billion, or about 0.1% of the 2010 Italian GDP.

If we assume that our estimates also apply to the United States, the per capita increase in monthly consumption after regularization would be around US$430, based on the 2012 Consumption Expenditure Survey (CEX) data on the expenditures of individuals of Latino or Hispanic origin.[45] Given an estimate of 11.5 million undocumented immigrants, an amnesty regularizing the legal status of all those immigrants would imply a higher level of expenditures in the United States of about US$60 billion; that is, approximately 0.4% of the nation's 2012 GDP. These calculations suggest that—again, with the caveat that we ignore general equilibrium effects—the expected increase in expenditures after regularization may be substantial, with clear distributional consequences in favor of the host countries.

Our findings are also important for analysis on the effects of immigration on the host economy more generally. Borjas (2013) highlights the importance of understanding the balance between the impact of immigration on the size of the consumer base for assessment of permanent wage effects. Our analysis suggests that undocumented immigrants lead to a lower expansion of consumption than legal immigrants, which may have important implications for how they impact on the labor market, wages, employment, as well as tax revenue.

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44.   We compute per capita expenditures using the information on average monthly consumption and average household size of documented immigrants in Tables 1 and 2.

45.   To compute the per capita increase in monthly consumption, we use information from the 2012 CEX on average annual expenditure (US$42,268) and size (3.3) of Hispanic or Latino origin households. We compute an average monthly individual expenditure of about US$1,070 and assume this is the level of consumption of documented immigrants in the U.S. Note that if the uncertainty associated with illegal status is lower in the United States than in Italy (see Section 3) we would expect a smaller effect in the US context.

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Supplementary Data

Supplementary data are available at *JEEA* online.