Rule Breaking, Honesty, and Migration

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Abstract

Using census data, we study false birth-date registrations in Italy, a phenomenon well known to demographers, in a setting that allows us to separate honesty from the benefits of cheating and deterrence. By comparing migrants leaving a locality with those who remain in it, we illustrate the tendency of Italians to sort themselves across geographic areas according to their honesty levels. Over time, this tendency has modified the average honesty level in each locality, with relevant consequences for the distribution across geographic areas of outcomes like human capital, productivity, earnings growth, and the quality of local politicians and government.

1. Introduction

Economists think of observed rule breaking as an outcome of a decision-making process in which subjects compare their aversion to breaking rules (honesty) with the gain they derive from breaking them and with the level of public deterrence against such actions. It is therefore surprising that observed rule breaking is often used as an indicator of social capital, because what should enter into such a measure is only intrinsic honesty, not the effects of the benefits of cheating or deterrence.

We are grateful to the Italian National Institute of Statistics (Istat) for giving us access to the individual observations of the restricted census at the protected Adele sites. When the COVID-19 pandemic started, access to the Adele sites was no longer permitted, but Istat allowed us to export group averages of the data so that we could continue our research. We are particularly indebted to colleagues who shared their data: Ethan Ilzetzki and Saverio Simonelli (vote-counting rates); Josh Angrist, Eric Battistin, and Daniela Vuri (school cheating); and Lorenzo Casaburi and Ugo Troiano (property tax evasion). We also benefited from conversations with Vittorio Bassi, Diogo Britto, Adriano De Falco, Alice Dominici, Roberto Galbiati, Diego Gambetta, Giulia Giupponi, Joseph Heath, David Levine, Moti Michaeli, Massimo Morelli, and Yannick Reichlin and from seminar presentations at the Berkeley and Davis campuses of the University of California, Bocconi University, Boston College, Cornell University, Dartmouth University, Princeton University, the Berlin School of Economics Applied Micro Seminar, the University of Florida, and the University of Milan.

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In this paper, we study false birth-date registrations in Italy, a phenomenon well known to demographers, using census data in a setting that allows us to separate honesty from the benefits of cheating and deterrence. We confirm the evidence from vital statistics documented by Livi (1929), Maroi (1954), and Breschi, Gonano, and Ruiu (2018) suggesting that Italians, in some localities more than others, tend to register false birth dates for their children. Starting in early December of each year, the frequency of registered births per day declines substantially, while an abnormally large mass of registered births is concentrated in the first 5 days of the following January.

The demography literature describes the main motives for lying about birth dates as delaying school entry, compulsory military service, marriage, and the age of emancipation. Irrespective of the motive, a parent who registers a false date of birth for a child violates the Italian penal code, which, at least since 1889, carries a punishment of 3–10 years' imprisonment for any false declaration in a public or private legal document (Codice Zanardelli, art. 278, R.D. June 30, 1889, n. 6133).[1] What makes this indicator particularly interesting for our purposes is that, to our knowledge, it is the only cheating measure that can be computed for groups of the Italian population observed in small localities at different times during the 20th century. As in the case of the unpaid parking violations of United Nations diplomats examined in Fisman and Miguel (2007), this setting allows us to compare subjects facing the same degree of local deterrence who derive similar benefits from breaking a rule. Differences in cheating between these groups can therefore capture the different inclinations of their members toward honest behavior.[2]

We then exploit this setting to compare the honesty of families that migrate from and families that remain within small locality-biennium cells and show that it differs across the two groups in a way that illustrates the tendency of Italians to sort themselves across geographic areas according to their honesty levels. We cannot determine the precise reasons for this sorting process. For example, cheaters in a locality could obtain material advantages that improve their economic conditions, which reduces their need to migrate. Honest citizens, instead, may pay a price for respecting rules, which in the long run constitutes a push factor for migration. Or it could be that the honest population prefers to move if too many peers who do not care about rules free ride on public resources (for a discussion of some mechanisms, see Michaeli et al. 2023).

Independent of the reason that drives sorting based on honesty, this phenomenon may have important long-term consequences because it can change the distribution of social capital across geographic areas. Our data allow us to measure the extent to which average honesty in various Italian localities has changed over

[1] Falsification of a date of birth is explicitly punished in the Penal Code of 1930 (Codice Rocco), as discussed in Maroi (1954, p. 414).

[2] While we cannot exclude the possibility that our measure of cheating conditional on location and observable characteristics also captures dimensions aside from intrinsic honesty, for simplicity we use "honesty" to refer to subjects with low levels of cheating.

the 20th century as a result of migration. In this respect, a collateral contribution of our paper is to suggest that sorting based on honesty may be one reason for the unequal distribution of social capital and economic prosperity extensively documented around the world and particularly in Italy (see, for example, Rupasingha, Goetz, and Freshwater 2006; Braeseman and Stephany 2017; Cohn et al. 2019; Fisman and Miguel 2007; Lowes et al. 2017).[3]

We then use our measure of the honesty drain or gain experienced by various southern Italian localities to show that those characterized by a more severe honesty drain have lower indicators of human capital, productivity, and wage growth. Those localities also appear to be governed by politicians characterized by higher rates of birthday cheating, and, interestingly, their city councils are more frequently dismissed by the central government because of corruption or poor functioning. These estimates cannot be interpreted as causal, but, relying on Oster (2019), we show that they are likely to be robust to potential unobserved confounders.

Our paper is organized as follows. Section 2 describes the historical and census evidence of birthday cheating. Section 3 presents evidence for the propensity of Italians to sort themselves across local areas on the basis of honesty. Section 4 measures the honesty drain or gain at the level of localities and shows that it correlates with important economic outcomes. Section 5 concludes.

2. Historical and Census Evidence of the Falsification of Birth Dates in Italy

Using data from the 1991 Italian census for the cohorts born between 1921 and 1954,[4] Figure 1 shows the histograms of births (in thousands) in bins of 5 days for northern and southern Italy.[5] While an almost uniform distribution of birth dates over the year is expected, it is evident that around mid-December the frequency of births declines abnormally in the South and then suddenly increases in the first 5 days of January. In the North the pattern is similar, although less pronounced.[6]

Livi (1929), Maroi (1954), and Breschi, Gonano, and Ruiu (2018) demonstrate

[3] A path-breaking book, Putnam, Leonardi, and Nanetti (1994), systematically explores the heterogeneity of social capital measures, and rule breaking specifically, within and between Italian regions. For more recent studies about Italy, see Ichino and Maggi (2000), Guiso, Sapienza, and Zingales (2004), Buggle and Durante (2020), Bigoni et al. (2016), Buonanno et al. (2015), and Michaeli et al. (2023).

[4] The first year in which publicly available census data contain complete birth dates, cities of birth, and residence is 1991. See Section OA1 in the Online Appendix for a more complete description of the data. The evidence described below can be duplicated with more recent census data, as shown in Figures OA1 and OA2 in the Online Appendix.

[5] The South is defined as the set of localities that between 1816 and 1861 were part of the Kingdom of the Two Sicilies for reasons that will be made clear below. The North is the complementary set.

[6] This is not the only form of birthday cheating that emerges from the census. Italians are also abnormally less likely to be born on the 17th of each month, a form of birthday cheating that is clearly driven by superstition because the number 17 is associated with *la disgrazia* (misfortune) in the traditional Neapolitan game tombola. See Liccardo (2019) and Maroi (1954).

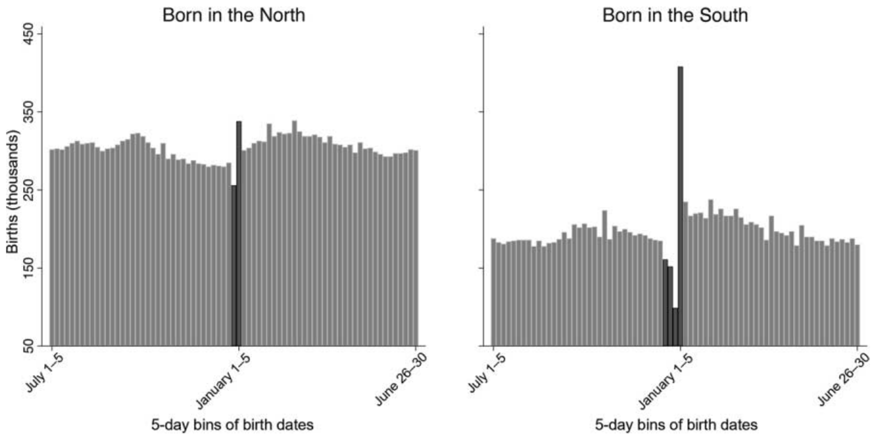

Figure 1. The distribution of birth dates over a calendar year

with vital statistics that this phenomenon is the result of declaring false birth dates, so it can be classified as a form of rule breaking.[7] It is remarkable that the evidence they collected using official birth registries for December and January from 1924–25, 1950–51, and 1951–52 closely matches our census-based evidence, which includes only subjects who survived until 1991. The data are compared in Figure 2, which shows that the spikes in Figure 1 are not due to differential mortality rates for subjects born immediately before or after January 1.

In Figure 2, we use the methodology described in Section OA2 to obtain the share Π_{glt} of birth dates that can be considered falsified in a population group g of a locality l at a time t from the information in Figure 1.[8] Figure 2 shows the shares in Italian provinces that Livi (1929) calculates from daily vital statistics for December 1924 and January 1925 and the same statistics for the entire country constructed using the data of Maroi (1954) for the days around New Year's Eve in 1950 and 1951. It also shows the analogous shares of falsified birth dates we computed using the 1991 census data. In each geographical unit the values are similar, as one would expect in the absence of differences in mortality between cheaters and noncheaters.[9] Interestingly, Π_{glt} is substantially lower in the four northern provinces (Modena, Mantova, Milano, and Alessandria) than in the six south-

[7] Table OA3 in the Online Appendix shows that birthday cheating correlates well with more traditional rule-breaking indicators like cheating on school exams, excessive absenteeism, and property tax evasion.

[8] We assume that births should naturally be distributed according to a uniform distribution around January 1. We thus consider the excess number of recorded births in the first 5 days of January relative to the number predicted by the uniform distribution as the result of false-date declarations.

[9] Considering the greater incentive to falsify birth dates for male children so as to delay military service, we replicate this exercise in Figure OA3 by gender for the provinces considered by Livi (1929).

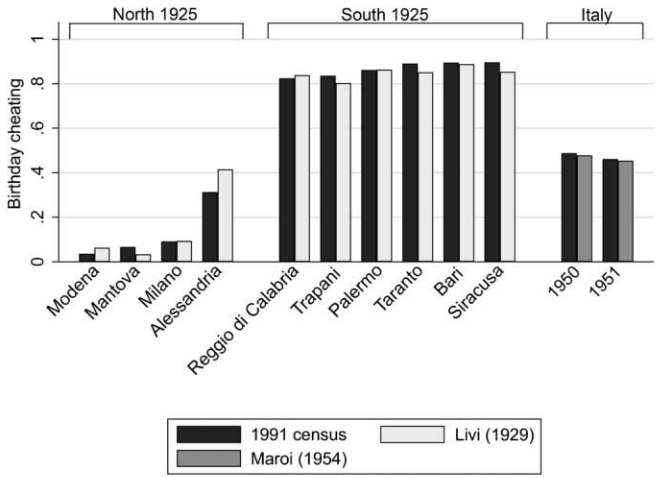

Figure 2. Birthday cheating around January 1925, January 1950, and January 1951

ern ones. Therefore, the difference between the South and the North in birthday cheating emerges similarly from historical vital statistics and recent census data.

Figure 3, based on 1991 census data, shows that birthday cheating is markedly more frequent in the localities that between 1816 and 1861 were part of the Kingdom of the Two Sicilies, whose historical borders are indicated with a heavy line. Particularly striking is the sharp discontinuity in the frequency of false birth dates, which are almost absent in municipalities located just north of the kingdom's border. Interestingly, the border does not correspond to the official boundaries of modern regional administrations (namely, Lazio, Umbria, and Marche, as shown by d'Adda and de Blasio [2017]).

Birthday cheating appears to be related more to institutions of the past than of the present and specifically to state authorities characterized by less efficient administrations and lower levels of deterrence against rule breaking (see, for example, Putnam, Leonardi, and Nanetti 1994; Di Liberto and Sideri 2015; Bosker et al. 2008). For example, birthday cheating is almost absent in the insular region of Sardinia, which is usually included in the standard definition of southern Italy but historically was part of the northern Kingdom of Piedmont and Sardinia, ruled by the Savoy dynasty. Historians typically credit this kingdom with an efficient administration and high levels of deterrence against crime (see Putnam, Leonardi, and Nanetti 1994).[10] For these reasons, in the present paper the South is defined as the set of localities that historically were part of the Kingdom of the Two Sicilies.[11]

[10] The House of Savoy unified Italy in the second half of the 19th century.

[11] More precisely, we follow d'Adda and de Blasio (2017) in defining the South as the set of localities belonging to the modern regions of Sicily, Calabria, Apulia, Campania, Basilicata, Molise, and Abruzzo; the provinces of Frosinone, Latina, and Rieti in Lazio; the province of Ascoli Piceno in Marche; and a few municipalities in Perugia.

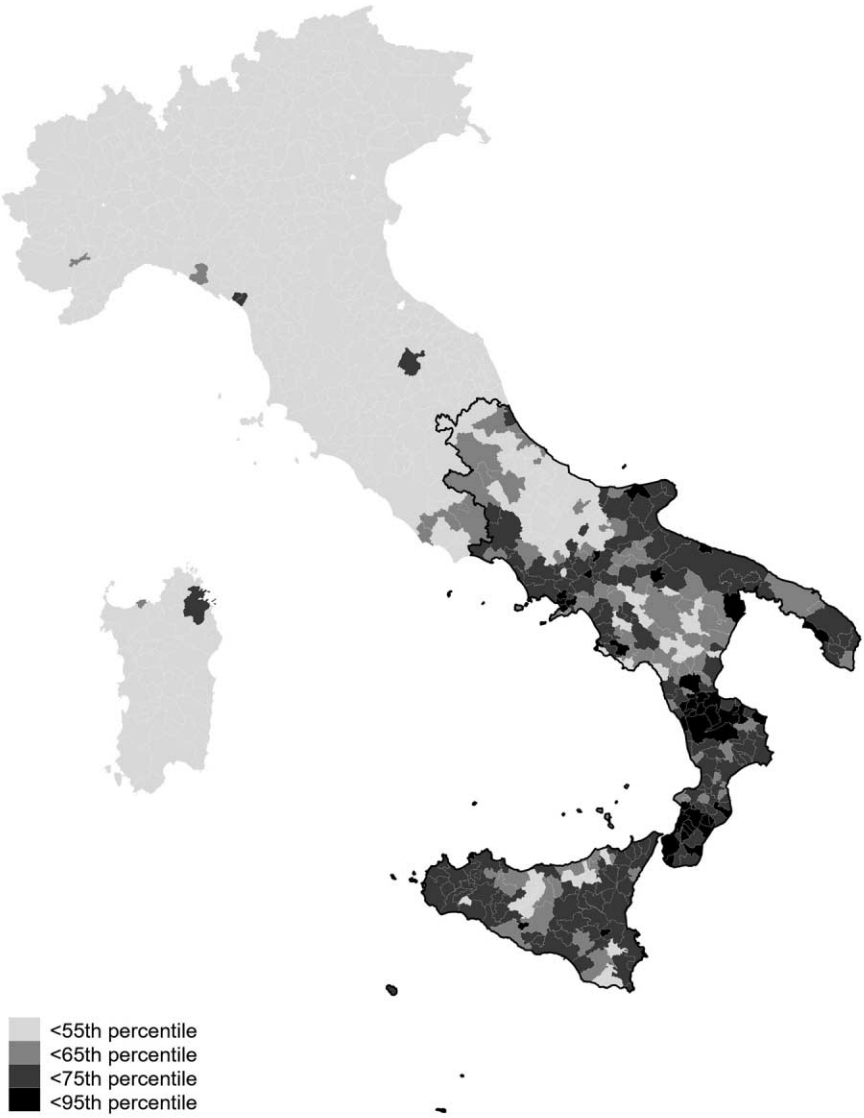

Figure 3. Birthday cheating in the Kingdom of the Two Sicilies

A well-defined set of motives may induce parents to register false birth dates for their children, as suggested by Livi (1929), Maroi (1954), and Breschi, Gonano, and Ruiu (2018). The likely most important motive involves the fact that children typically participate in activities with their birth cohort (defined as individuals born in the same calendar year). A child born in December is always among the youngest in the group with whom she competes. If the same child is instead registered as being born in early January, she will be the oldest in her cohort. This is particularly relevant for school activities, sports competitions, and army enrollment, which was compulsory in Italy for children born before 1985.[12]

Another relevant motive for shifting the birth date of a child born in December to early January is that it keeps the child home longer, which postpones military service or allows more time to find a spouse. As shown in Figure OA4, birthday cheating is observed for both females and males, although it is more pronounced for the latter. Cheating is also observed for children who reach higher levels of education (more than high school) and those who do not go beyond compulsory education or are dropouts (Figure OA5). Therefore, birthday cheating does not seem to be specifically related to educational attainment. This is not surprising because motives like being older in a cohort, delaying military service, or having more time to find a spouse are largely unrelated to education.

Independent of the motive, for the purposes of this study a major advantage of birthday cheating as a measure of rule breaking is that it can be estimated, using census information, for small population groups in given localities and at different times during the 20th century. As in Fisman and Miguel (2007), who compare parking violations of United Nations diplomats of different nationalities in New York City in 2 periods characterized by different levels of deterrence, we can compare the birthday cheating of different Italian groups who live in the same city at the same time and hence arguably face the same deterrence and derive similar benefits. Therefore, conditioning on a locality and time period, differences in birthday cheating may reflect differences in the proclivity to cheat in general and thus in the honesty of the groups. Moreover, we can compare them using census data for an entire country over about a century.[13]

Two groups in particular can be studied in such a context: migrants between localities and remainers in each locality. These two groups are interesting because rule breaking has been shown to be unequally distributed across nearby geographic areas in different parts of the world and in Italy in particular (see Putnam, Leonardi, and Nanetti 1994; Ichino and Maggi 2000; Guiso, Sapienza, and Zingales 2004; Buggle and Durante 2020; Bigoni et al. 2016; Buonanno et al. 2015; Michaeli et al. 2023). Comparing migrants and remainers in terms of their honesty, as revealed by differences in birthday cheating for given deterrence and benefits to cheating, allows us to explore the hypothesis that migration explains at least part of this heterogeneity across localities.

Migrants and remainers may differ in terms of honesty for many reasons, and we cannot tell them apart with our data. For example, cheaters could obtain material advantages that improve their economic conditions, which reduces their need to migrate. Honest citizens may pay a price for respecting rules, which in the long run constitutes a push factor for migration. In the case of birthday cheat-

[12] Practices aimed at shifting the activities of children to later-born cohorts are called redshirting in the United States.

[13] According to Breschi, Gonano, and Ruiu (2018), birthday cheating is common in other countries.

ing, a child registered as being born in January instead of December will start school later and become one of the oldest in his or her cohort. Fenoll, Campaniello, and Monzón (2019), among others, show that children who start school later have better test scores. If this educational achievement translates into higher earnings, the probability of migration is likely to decline. Alternatively, Michaeli et al. (2023) suggest that members of a community who, ceteris paribus, dislike breaking rules may prefer to move elsewhere if too many peers who do not care about rules engage in free riding.[14]

Whether migration generates a drain or a gain of honesty for a given locality is an important empirical question that we address in Section 3. Answering this question is also important because honesty drains or gains may have, in the long run, relevant economic consequences, as we show in Section 4.

Before proceeding with the analysis, however, we must highlight one caveat of our measure of rule breaking. Cheating in registering the birth date of a newborn child is the parents' decision, while the later decision to migrate may be the parents' decision, if the entire family moves, or the child's decision when she becomes an adult and decides to migrate alone. Although we observe the place of birth and the place of residence in 1991, we have no information about when migration took place. However, given the extensive evidence of intergenerational transmission of ethical values,[15] an agent in our analysis should be thought of as a family, which may or may not cheat and may or may not migrate. For brevity, we use "migrants" and "remainers" to refer, respectively, to migrant families and remainer families.

3. Geographic Sorting by Honesty

Our observations are aggregated from census data for the entire Italian population and represent groups of migrants (from South to North or vice versa) and remainers (in the corresponding macroregion) born in late December or early January in a local labor market (LLM, a narrowly defined locality by the Italian National Institute of Statistics [Istat]) during one of the 17 bienniums in the 1921–54 period.[16] As explained in Section OA3, we restrict the analysis to localities and bienniums in which a minimum number of migrants and remainers are born (at least six of each type). This constraint is particularly binding for the North because migration flows from North to South are rare and small. For the South we end up with 294 localities of the 327 defined by Istat; for those 294

[14] At the same time, dishonest subjects may prefer to leave a community if it becomes poor because of excessive free riding. In the model in Michaeli et al. (2023), subjects with high or low levels of honesty are characterized by their attitudes toward risk and beliefs about deterrence in the place of origin versus the place of destination.

[15] A large literature on parenting styles suggests that this is the case. See, for example, Tabellini (2008), Algan and Cahuc (2010), Houser et al. (2016), Lowes et al. (2017), and Doepke and Zilibotti (2017).

[16] In 1954, a major reform of civil registries made doctors and obstetricians responsible for the registration of birth dates, and after that year the practice of falsifying birth dates rapidly disappeared. See Breschi, Gonano, and Ruiu (2018).

Figure 4. Local labor markets in the analysis

localities the minimum number of migrants and remainers are present in 6,432 cells defined by locality-biennium-migration status of the 9,996 (294 × 17 × 2) theoretically possible cells. The corresponding figures for the North are 17 localities of 454 and 206 cells of 578. Figure 4 shows the localities.[17]

If within the same locality and biennium migrants and remainers face the same deterrence and the same benefits to cheating, the frequency of birth-date cheating in the two groups is informative about their honesty levels. Figure 5 uses aggre-

[17] Black areas in Figure 4 indicate local labor markets (LLMs) with at least six migrants and six remainers. White areas are LLMs with insufficient emigration and are therefore excluded from the analysis. The white line indicates the border of the Kingdom of the Two Sicilies.

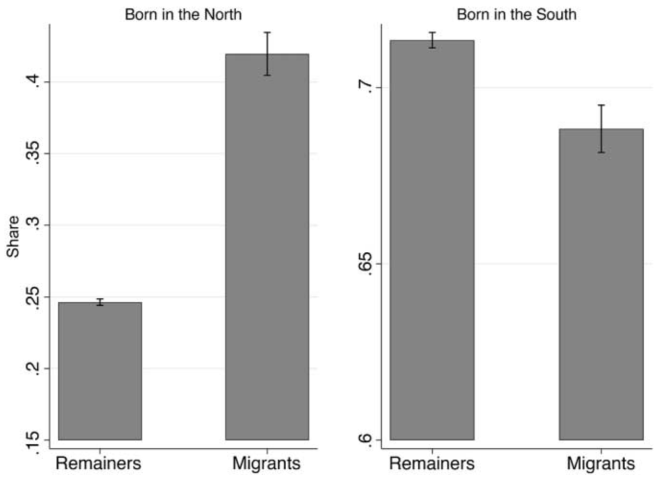

Figure 5. Geographic sorting based on honesty

gated data over the 1921–54 period to show preliminary descriptive evidence of migration sorting by honesty. For migrants and remainers born in the 17 northern localities (where this comparison is possible), the share of cheaters is 42 percent among migrants from the North and 17 percentage points lower among remainers, and the difference is statistically significant. In contrast, in the 294 southern localities for which the comparison is possible, the share of cheaters is higher among remainers in the South than among migrants born in the South. The difference is relatively small (2.51 percentage points) but statistically significant. This descriptive evidence suggests that migrants from South to North and vice versa were not randomly selected with respect to honesty.

Given the large migration rates from South to North (29 percent on average in the data set), this nonrandom selection may have induced a drain of families with high levels of honesty from the South even if the average difference between the probability of cheating for migrant and remainer families is small in this macroregion. We quantify precisely the size of the drain in Section 4. Migration rates form North to South were significantly less intense (7 percent on average in the data set), but the migrant families' probability of cheating is substantially higher than that of remainer families, which suggests a localized loss of low-honesty families from the North in particular geographical contexts.[18]

To dig more deeply into the sorting pattern displayed in Figure 5, Table 1 reports controlled estimates of the shares of cheating families in the four groups defined by migration status (migrant or remainer) and macroregion of birth (North or South). An observation in this analysis is a cell of migrants or remainers (g), in a locality (l) and a biennium (t), weighted by the population the cell represents

[18] It is possible that the North-to-South migrants are return migrants, but we cannot assess that possibility with our data.

Table 1

Birthday Cheating of Migrant and Remainer Families

	(1)	(2)	(3)	(4)
Migrant × South (β_4)	−.024**	−.013**	−.015**	−.015**
	(.009)	(.004)	(.004)	(.005)
Migrant × North (β_3)	.181*	.071**	.058*	.104**
	(.074)	(.023)	(.026)	(.030)
Remainer South (β_2)	.466**			
	(.063)			
Remainer North (β_1)	.248**			
	(.062)			
R^2	.411	.921	.923	.923
LLM-biennium fixed effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Controls interacted	No	No	No	Yes
Oster δ for $\beta_4 = 0$		9.642	15.13	13.25
Oster δ for $\beta_3 = 0$		3.750	2.807	4.007
F-test for controls (p-value)			0	0

Note. Results are ordinary least squares estimates from 1991 census data covering 311 local labor markets between 1921 and 1954. The F-test is for the joint significance of all included controls. $N = 6{,}638$ group-locality-biennium observations.
* $p < .05$.
** $p < .01$.

(namely, births around New Year's Eve), to give more weight to cells in which our measure of cheating is more precise.[19] The estimates in Table 1 are obtained with the following regression:

$$\begin{aligned}\Pi_{glt} = {} & \beta_1 + \beta_2 \text{South}_{glt} + \beta_3 \text{Migrant}_{glt} \times \text{North}_{glt} + \beta_4 \text{Migrant}_{glt} \times \text{South}_{glt} \\ & + \gamma \boldsymbol{X}_{glt} + \psi_{lt} + \varepsilon_{glt}.\end{aligned} \tag{1}$$

We use the sample of 6,638 group-locality-biennium cells (6,432 in the South and 206 in the North) and cluster standard errors at the locality level.[20] The term ψ_{lt} is the interaction of locality and biennium fixed effects,[21] $\boldsymbol{X}_{glt}$ is the vector of average characteristics of migrants or remainers in the cells (share of females, average year of birth, share with primary education, and share with tertiary education),[22] and ε_{glt} contains unobservable characteristics of groups, localities, and periods. Note that, in accordance with the discussion in Section 2, gender, education, and

[19] These weights are hence also a proxy for the population size in each locality.

[20] This specification is equivalent to running the regression on individual data points with clustering at the cell level and attributing to each subject the birthday cheating of its cell (Angrist and Pischke 2009). If the regression had been run at the individual level, the number of observations would have been 354,817, which corresponds to the total Italian population in the 1991 census born around New Year's Eve in the localities we consider for 1921–54.

[21] When this interaction is included in equation (1), the values of β_1 and β_2 are of course not identified.

[22] Secondary education, corresponding to a junior high level (8 years) or a high school diploma (13 years), is the omitted category. Primary education consists of 5 years of elementary school. Tertiary education indicates a high school diploma.

year of birth are likely to capture the most important determinants of the benefits of birthday cheating, while an interaction of LLM and biennium fixed effects should control almost perfectly for the level of deterrence.

Our benchmark specification includes no controls.[23] Among remainers in the North, the share of cheaters is 24.8 percent, while it is 46.6 percent for remainers in the South. Migrants born in the North have a probability of cheating that is 18.1 percentage points higher than for remainers born in the same region, while migrants born in the South have a lower probability of cheating (by 2.4 percentage points) with respect to remainers in the same region. These differences are statistically significant at conventional levels.

The specifications in Table 1 progressively add more stringent controls to purge confounders related to differences in benefits and deterrence.[24] Including the interaction of LLM and biennium fixed effects (ψ_{lt}) is possible given that observations are weighted by the number of subjects in each cell. The $\boldsymbol{X}_{glt}$ controls are added linearly; the fully saturated specification includes interactions of the $\boldsymbol{X}_{glt}$ controls.[25] Even in these more demanding comparisons, families migrating from North to South have a probability of cheating that is 6–10 percentage points higher than that of remainer families in the North, while for migrants to the North the analogous probability is 1.3–1.5 percentage points lower than remainers in the South.

These controlled estimates give further support to the conclusion that sorting based on honesty has occurred in Italy. Depending on the intensity of migration flows out of the various LLMs, this sorting may have induced a local drain of honest families from South to North and a drain of low-honesty families in the opposite direction. This conclusion rests, however, on the identifying assumption that, within a given LLM-biennium cell and controlling for what we can observe, the distribution of benefits and the level of deterrence are similar for migrants and remainers. The δ-statistics proposed by Oster (2019) and reported for the parameters β_3 and β_4 in Table 1 provide evidence in favor of this assumption. To interpret this parameter in our context, note that the R^2-values indicate that the locality and biennium fixed effects together with the observed characteristics of migrants and remainers in each LLM-biennium cell explain more than 50 additional percentage points of the variability in the probability of cheating, on top of the 41 percent explained by the uncontrolled benchmark specification. Therefore, the controls must capture a good part of the variability in deterrence and benefits to cheating. However, the estimated coefficients on β_3 and β_4, which in-

[23] Therefore, as in Figure 5, these results are still not informative about underlying differences in honesty because deterrence and the distribution of benefits are not controlled for in this specification. Note also that these results are not numerically equal to those of Figure 5, which uses observations aggregated over the entire 1921–54 period, while in Table 1 they are disaggregated by LLM-biennium cells weighted by births.

[24] The coefficients on β_1 and β_2 in columns 2, 3, and 4 are not reported because they do not have a meaningful interpretation given the inclusion of the interaction of LLM and biennium fixed effects.

[25] Table OA4 reports coefficients for all controls.

dicate sorting related to honesty, remain relatively stable when the controls are included.

In light of this evidence, the δ-statistic proposed by Oster (2019) measures by how many times the remaining unobserved characteristics of localities (namely, the unobservable determinants of deterrence and benefits) should be correlated with migration status in each macroregion of birth to reduce to 0 the coefficients on β_3 and β_4, given that these unobservables can explain only the small remaining variability in the probability of cheating. For example, in the fully saturated specification, the value 13.25 for β_4 means that if the unobservable characteristics (which explain less than 8 percent of the variability in the outcome) could be included, they would have to be about 13.25 times more correlated with migration status than the observed ones to conclude that migrants and remainers born in the South have the same probability of cheating. As for β_3, the analogous correlation would have to be four times higher to conclude that there has been no drain of low-honesty families from the North. Such high correlations between unobservable characteristics and migration status are arguably implausible in a context in which they explain only a relatively small part of the variability in the outcome.

4. Long-Term Consequences of an Honesty Drain

Having established that migrants between the South and the North of Italy are nonrandomly selected with respect to their honesty, our next goal is to measure the honesty drain (or gain) that localities experienced because of internal migration. Such measurements reflect not only the difference in honesty of migrant and remainer families but also the size of migration flows. A small outflow of very different migrants and a big outflow of randomly selected migrants would not generate a relevant drain of honesty. We also want to assess to what extent this phenomenon is heterogeneous across localities and whether it is quantitatively large enough to have detectable economic consequences.

To this end, we restrict the analysis to the 294 LLMs in the South, where migration to the North has significantly characterized the entire macroregion.[26] Moreover, we abstract from time differences during the 20th century, and for each LLM we collapse bienniums to a single period from 1921 to 1954 (and thus we omit the time subscript t in the remainder of the paper); we retain the first biennium (1921–22) to measure baseline characteristics of each locality and use recent years (after 1954) to measure outcomes.

4.1. A Measure of Honesty Drain or Gain

Consider a locality l in the South and two overlapping sets of agents: all those born in l, denoted b, and a subset containing those who are born and remain in l

[26] For completeness, Table OA5 also has results that include the 17 localities in the North for which a drain can be computed. Results are similar, both in terms of point estimates and significance.

(the remainers, denoted r). The complement of the set r in the b set are migrants to the North, denoted m. Consider the quantity

$$\theta_l = \Pi_{rl} - \Pi_{bl}, \tag{2}$$

which measures the difference in the probability of falsifying a birth date by remainers in l versus those born in l. If $\theta_l = 0$, the probability of cheating by remainer and migrant families must be identical, and both groups are random samples of the population of families giving birth in l with respect to birthday cheating. Therefore, even in the presence of a large migration outflow, there would be no drain or gain of honesty in this case. If instead $\theta_l > 0$, remainer families cheat more frequently than migrant families and therefore more frequently than the average family's children born in l. In this case, θ_l captures the honesty drain suffered by locality l because it measures how average honesty declined in the remaining population as a result of the South-to-North migration between 1921 and 1954. Similarly, $\theta_l < 0$ indicates that locality l experienced a gain in honesty for the opposite reason.

Note that θ_l does not consider other types of migration in and out of a locality l. The focus on South-to-North migration is justified by three empirical observations. First, until the late 1970s migration from abroad was essentially absent in Italy (see Del Boca and Venturini 2005). Second, migration flows from North to South were so small as to be practically irrelevant (see Online Appendix OA3). Third, migration within the South was practically irrelevant as well.[27,28]

Figure 6 shows the honesty drain θ_l ($\theta_l > 0$) or gain ($\theta_l < 0$) for the 294 southern LLMs for which we can compute it. Table 2 provides the descriptive statistics for θ_l, weighted by the size of each locality (number of births around New Year's Eve between 1921 and 1954).

Figure 6 shows that θ_l is highly heterogeneous across southern LLMs. The distribution of θ_l ranges between -21.4 and 22.2 percentage points, with the 10th and 90th percentiles respectively equal to -1.2 and 1.9 percentage points. On average, the drain is small (the mean is .3 of a percentage point), but its variability across localities is substantial, and our goal is to exploit it to understand whether it may have had relevant consequences.

To this end, we estimate variants of this equation:

$$Y_l = a + b\theta_l + c\Pi_{l21} + gX_l + \psi_l + \varepsilon_l, \tag{3}$$

where Y_l is an outcome for locality l, θ_l is the standardized honesty drain measured across bienniums between 1921 and 1954,[29] X_l is a set of locality controls, and ψ_l is a set of fixed effects for the seven current administrative regions that partition the South. Ideally, we would like to regress the change in outcomes Y_l on

[27] See Bonifazi (2009), who shows that South-to-North migration rates are more than four times larger than within-South rates. Using Istat migration matrices (Istituto Centrale di Statistica 1965, p. 684, table 11.VII), South-to-North migration rates are 3.4 times larger.

[28] This period was also characterized by migration to other countries. However, our census data do not contain information about individuals who were living abroad by 1991.

[29] One standard deviation of θ_l corresponds to 1.7 percentage points.

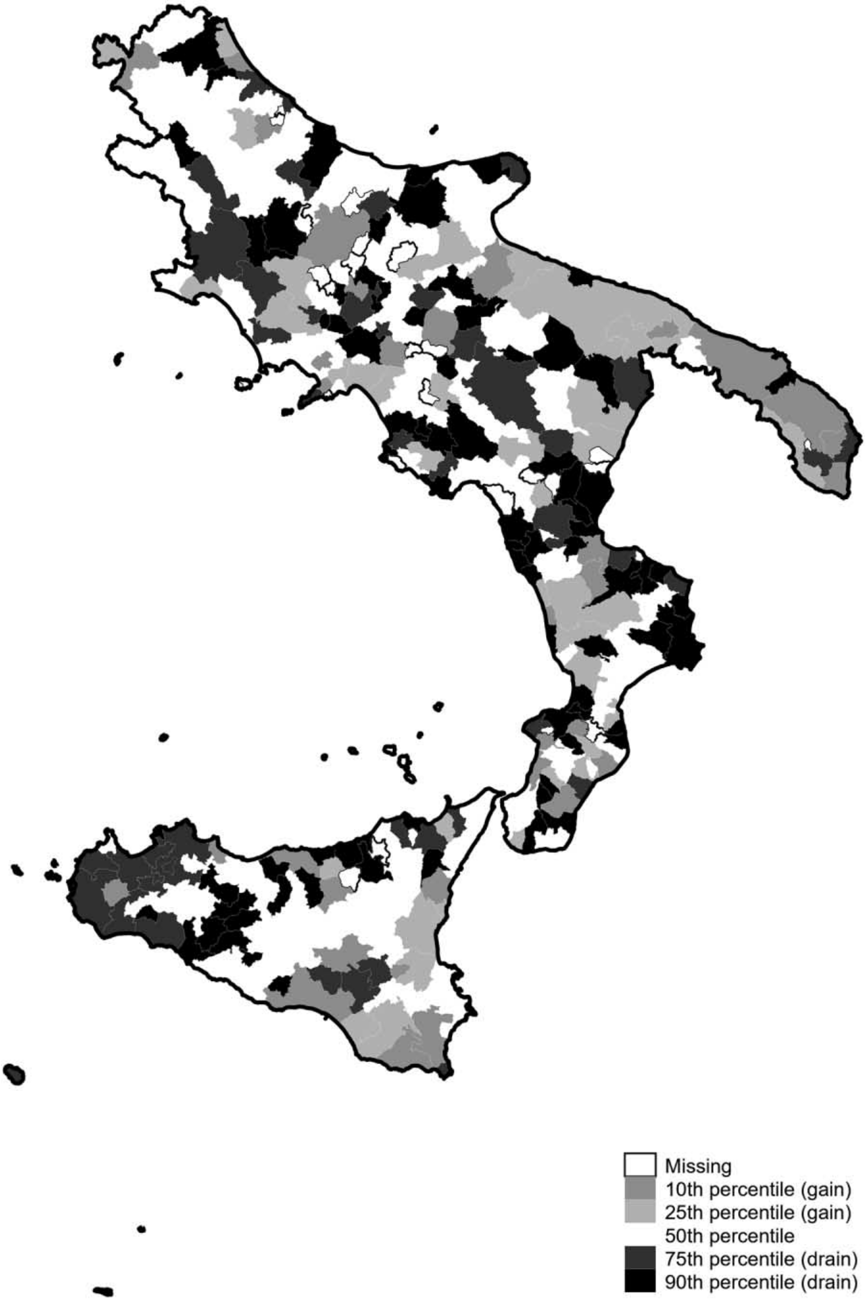

Figure 6. Honesty drains and gains during the 20th century

Table 2
Honesty Drain in Southern Local Labor Markets

θ_l	Value
Mean	.003
SD	.017
Min	−.214
10th percentile	−.012
Median	.0037
90th percentile	.0194
Max	.22
Skewness	−.252
Share with $\theta_l < 0$	.452

Note. $N = 294$.

honesty drain θ_l, but most information about the outcomes for 1921 is not available. We address this limitation by including the probability of birthday cheating in l measured in the biennium 1921–22 (Π_{l21})[30] to capture differences in initial birthday-cheating conditions.[31] As in the other regressions, we weight observations by the population size (number of births in each locality during 1921–54).

The main parameter of interest is b, which in general does not have a causal interpretation and offers only a suggestive controlled correlation. However, again using the δ-statistic of Oster (2019), we can determine whether it is plausible that the unobservable confounders are sufficiently correlated with θ_l, relative to how the observable controls X_l are correlated, to reduce to 0 the estimate of b in equation (3).

4.2. Economic Outcomes

Table 3 reports ordinary least squares estimates of equation (3) based on data for LLMs in the South. The first outcome is a measure of human capital: the log of the average math test score of high school students of locality l in the standardized national exams, averaged over the 2012–21 period.[32]

In the specification without controls, a 1-standard-deviation increase in the honesty drain reduces math test scores by 2.5 percent, and this estimate is significantly different from 0 at conventional values. Column 2 includes the full set of controls: the probability of birthday cheating for the same locality in the cohort born in the 1921–22 biennium, to control for initial conditions of honesty; a set

[30] The coefficient for the initial condition of cheating is presented in Table OA6 together with coefficients for the controls.

[31] Since $\theta_l = \Pi_{rl} - \Pi_{bl}$, a more flexible specification would allow for estimation of different coefficients for Π_r and Π_b. As discussed below, when we estimate this more flexible specification, we reject the hypothesis that the coefficients of Π_r and Π_b differ.

[32] The math test is implemented by Invalsi, the Italian agency that evaluates the school system (see Falzetti 2021). High schools for which the average math test score can be constructed occur in only 245 of the 294 southern LLMs in the sample. Estimates based on the literacy test score in the Invalsi data set are qualitatively similar to those for the math test score.

Table 3
Honesty Drain and Economic Outcomes

	Math Score		Firm Value Added		Vote-Counting Rate		Earnings Growth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Honesty drain	−.025*	−.026**	−.029**	−.025**	−.064*	−.045*	−.002*	−.002*
	(.010)	(.009)	(.010)	(.009)	(.026)	(.023)	(.001)	(.001)
N	245	245	186,507	186,507	294	294	294	294
R^2	.022	.338	.002	.281	.017	.332	.017	.529
Controls:								
Region fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Initial period	No	Yes	No	Yes	No	Yes	No	Yes
Employment and geography	No	Yes	No	Yes	No	Yes	No	Yes
Drain mean	.003	.003	.004	.004	.003	.003	.003	.003
Drain SD	.015	.015	.053	.053	.017	.017	.017	.017
Outcome mean	181.638	181.638	22.954	22.954	186.439	186.439	.018	.018
Outcome SD	9.088	9.088	14.791	14.791	31.404	31.404	.006	.006
Oster δ		−806.1		−16.78		6.465		7.783

Note. The dependent variable Math Score is the log of the Invalsi math test score in grade 13 (high school) for 245 local labor markets (LLMs) in the South weighted by births in each locality, with standard errors robust for heteroskedasticity. The dependent variable Firm Value Added is the logarithm of firm value added per employee weighted by the employment share of each firm in a locality. Standard errors are clustered at the LLM level. The dependent variable Vote-Counting Rate is the logarithm of the average vote-counting rate per hour in a locality for the 2016 constitutional referendum. The dependent variable Earnings Growth is the log of per capita yearly labor earnings growth in a locality, with standard errors robust for heteroskedasticity.

* $p < .05$.

** $p < .01$.

of predetermined labor force and geographic characteristics of localities;[33] and fixed effects for the seven modern administrative regions in our definition of the South. The estimated coefficient is slightly larger in absolute size and significance. To exclude the possibility that our measure of honesty drain captures some form of brain drain, we include the share of secondary and tertiary graduates among remainers versus those born in a locality. A proxy for the initial condition of this outcome, measured by the share of illiterate residents from the 1921 census, is also included. As already mentioned, we cannot control for the initial condition of the other outcomes, but it is important to observe that our conclusions are qualitatively similar with or without controlling for the initial conditions of this outcome.[34]

The stability of the honesty drain coefficient for math scores in Table 3 is remarkable given the increase in the R^2-value from .022 to .338. As a result of this stability, the estimates of the Oster (2019) δ-parameter support the claim that the characteristics of localities that we do not observe would not reduce to 0 the coefficient on honesty drain if they were observed.

The second outcome is a measure of labor productivity averaged over 2009–18. This conventional indicator of firm-level value added is constructed with Bureau van Dijk data.[35] Since for this outcome an observation is a firm, denoted f, we estimate the following modified version of equation (3):

$$Y_{lf} = a + b\theta_l + c\Pi_{l21} + gX_{lf} + \psi_l + \varepsilon_{lf}. \tag{4}$$

In this specification, as in Ilzetzki and Simonelli (2017), standard errors are clustered at the locality level, and observations are weighted by the employment share of each firm in locality l. In this way more weight is given to larger firms, and estimates are informative at the locality level.

Labor productivity shows a substantial loss of at least 2.9 percent induced by 1 additional standard deviation in the honesty drain when no controls are included. This loss declines only slightly in absolute value (2.5 percent) with controls and industry fixed effects, the log of physical capital per employee, and a measure of human capital in the LLM. Following Ilzetzki and Simonelli (2017), the last two controls are meant to isolate the effect of the honesty drain on labor productivity. To give a sense of the economic relevance of these estimates, the overall North-to-South gap in labor productivity (34 percent) would decrease by about 7.4 per-

[33] These are employment rate, share of employment in agriculture, share of employment in manufacturing from the 1936 census (share of employment in the service industry is the omitted category), total population in the LLM and population density from the 1921 census, and dummies for coastal land, lowlands, low mountains, high mountains, flood risk, and rock slide risk.

[34] Table OA6 reports the coefficients and standard errors for all covariates.

[35] In the construction of this indicator, we follow Ilzetzki and Simonelli (2017). Data are from the Orbis database from Bureau van Dijk, which provides accounting information for the universe of companies required to register their balance sheets at chambers of commerce (all companies except sole-proprietorship enterprises and partnerships). After selecting the subsample of firms with nonmissing employment and information about value added and capital, the data set covers 656,518 firms, which corresponds to roughly 40 percent of the firms in Italy. Labor productivity is calculated as the value added per employee, averaged over 2009–18 and measured in 2019 euros.

cent with a decrease in 1 standard deviation in the honesty drain. The stability of the value-added estimates of the honesty drain coefficient is remarkable given the increase in the R^2-value from .002 to .281 and that the estimates of the Oster (2019) δ-parameter reach a high absolute value.

Next we consider a less conventional but novel and informative measure of pure labor productivity proposed by Ilzetzki and Simonelli (2017): the per-hour vote-counting rate (VCR) in an election. Ballot counting in Italian elections is a labor-intensive task that must be performed in the same way across the country and does not require any substantial physical capital. The technology and tools used by vote counters are the same in all polling stations, and citizens who perform the job receive monetary compensation independent of the time spent to complete it. Moreover, they are allowed to take paid leave from their workplace to complete the vote count, so the opportunity cost is also controlled for. According to Ilzetzki and Simonelli (2017), this measure of pure labor productivity accounts for about half of the North-to-South gap in the firm-level value added.

While ballots may differ across localities because of different numbers of candidates, this problem does not exist for national referenda, for which voters choose either yes or no. For this reason we use the VCR measure that Ilzetzki and Simonelli (2017) provide for the 2016 Italian referendum on constitutional amendments proposed by the Renzi government.[36]

The VCR is an informative indicator not only because it is a pure measure of labor productivity but also because it is likely to partly capture reciprocal trust. Consider two persons with opposite preferences for the outcome of the referendum who have to count votes in a context in which each is sure that the other respects rules and does not cheat. In this case, they could speed up vote counting by splitting the ballots between them without any double-checking or joint checking. If instead they do not trust each other, they will want to double-check and jointly assess each ballot. This extreme example makes it clear why we can expect to observe a negative correlation between the honesty drain θ_l and the VCR in the same locality for given initial conditions.

The estimates for this less conventional but interesting outcome are reported in Table 3. A 1-standard-deviation increase in the honesty drain reduces the VCR by 6.4 percent when no controls are included, and the effect is statistically significant. When all controls are included, the estimated loss declines slightly to 4.5 percent but remains statistically significant. The Oster (2019) δ-parameter reaches the reassuring value of 6.5.

Finally, Table 3 reports estimates for which the outcome is a measure of yearly labor earnings growth based on tax return data.[37] With information about total labor earnings and the number of workers with positive earnings for each local-

[36] This referendum attracted a lot of attention, created an intense debate, and generated a very large turnout (65.5 percent). It ended up becoming a referendum on Matteo Renzi, who lost, and his quick political decline started immediately afterward. Vote counting in the referendum was perceived to be extremely important by all citizens and political parties.

[37] See Italian Ministry of Finance, Tax Data and Statistics, Tax Returns [in Italian] (https://www1.finanze.gov.it/finanze/pagina_dichiarazioni/public/dichiarazioni.php).

ity, we can construct a measure of per capita yearly earning growth in a locality, averaged over 2005–19. A 1-standard-deviation increase in honesty drain reduces the average earnings growth by .2 percent, and this estimate remains unchanged when controls are included. Once again, the Oster (2019) δ-parameter reaches a very high level.

Table OA7 tests a more flexible specification that includes cheating by the remainers in a locality Π_{rl} and cheating by all those born in it Π_{bl}, instead of $\theta_l = \Pi_{rl} - \Pi_{bl}$. This specification allows the coefficients of Π_{rl} and Π_{bl} to be different and thus to capture the effect of the postmigration level of cheating separate from the effect of the premigration level. Results in Table OA7 show that we cannot reject the null hypothesis that the coefficients of Π_{rl} and Π_{bl} have the same value and that this value is the coefficient b of θ_l in baseline specification (3). Although none of these estimates can be considered causal, they are jointly compatible with the possibility that the more severe honesty drain experienced by some localities during the 20th century has induced lower levels of human capital, labor productivity, and wage growth in recent times.

4.3. *Quality of Politicians and Political Outcomes*

Depending on how close an electoral system is to being proportional, elected politicians should, to some extent, reflect the characteristics of the population that elects them. It is then natural to wonder whether, among localities with similar initial conditions, those that experienced a more severe honesty drain between 1921 and 1954, and thus had a less honest electorate of remainers after that period, also elected less-honest local politicians and therefore had worse political outcomes. An advantage of measuring honesty with birthday cheating Π_{gl} in a locality is that it can be measured in the same way for both the electorate and elected politicians.[38]

Table 4 first reports estimates of equation (3) in which the outcome is birthday cheating Π_{rl} of the electorate of remainers in 282 southern LLMs.[39] As expected, given the definition of the honesty drain in Section 4.1, regardless of whether the usual set of controls is excluded or included, 1 standard deviation of the honesty drain increases the birthday cheating of the electorate of remainers by about .35 of a standard deviation, and the coefficient is statistically significant.

The next estimate in Table 4 is more surprising. In this specification, the outcome is the standardized birthday cheating Π_{pl} of local politicians ($g = p$) born before 1954 and elected between 1985 and 2019 in the 282 southern localities for which this analysis is possible.[40] We employ administrative data provided by the Italian Ministry of Interiors that contain dates of birth for all Italian local govern-

[38] We are grateful to a referee for alerting us to this kind of analysis.

[39] Twelve southern localities were dropped because the number of elected politicians born around New Year's Eve was not large enough to conduct the analysis.

[40] To facilitate the comparability of the estimates, the standardization is computed by subtracting the average birthday cheating of politicians and then dividing by the standard deviation of birthday cheating among the population of remainers.

Table 4
Honesty Drain and Political Outcomes

	Birthday Cheating				City Council Dismissals	
	Remaining Population		Politicians			
	(1)	(2)	(3)	(4)	(5)	(6)
Honesty drain	.356**	.350**				
	(.119)	(.068)				
Birthday cheating			.951**	.687**		
			(.147)	(.208)		
Politicians' birthday cheating					12.760+	2.170**
					(7.559)	(.689)
R^2	.029	.748	.238	.372	.108	.967
Controls:						
Region fixed effects	No	Yes	No	Yes	No	Yes
Initial period	No	Yes	No	Yes	No	Yes
Employment and geography	No	Yes	No	Yes	No	Yes
Drain mean	−.026	−.026	.714	.714	.821	.821
Drain SD	.300	.300	.115	.115	.224	.224
Outcome mean	.714	.714	.821	.821	37.654	37.654
Outcome SD	.115	.115	.224	.224	47.277	47.277
Oster δ		−231.7		1.233		6.082

Note. Birthday cheating among local elected politicians is for those in office between 1985 and 2019 and born between 1921 and 1954. City council dismissals are for serious malfunctioning or corruption in a local labor market between 1990 and 2015. Observations are weighted by births in each locality, and standard errors, in parentheses, are robust for heteroskedasticity. $N = 282$.
$^{+}p < .1$.
$^{**}p < .01$.

ment officials over the 1985–2019 period. The local politicians are mayors (*sindaci*), members of local councils (*consiglieri*), and members of the local executive governing bodies (*giunta*, the municipality board composed of the deputy mayor and executive members called *assessori*). This outcome is regressed on birthday cheating among remainers Π_{rl} without controls.

It is remarkable that the estimated coefficient in this regression is very close to 1 (.951) and statistically significant, as one would expect in an almost perfectly proportional electoral system. However, that electoral system does not prevail in local elections, where the coalition that receives more votes obtains a majority premium on the elected council and selects the mayor and members of the executive governing body. The inclusion of controls reduces the coefficient to .687 without reducing its statistical significance. These estimates confirm that the average honesty of Italian local politicians reflects to a large extent the average honesty of their electorate, inasmuch as honesty is measured by birthday cheating.[41]

Finally, Table 4 shows that the honesty of local politicians correlates with an important political outcome: the frequency of dismissals of city councils in an

[41] Table OA8 shows the robustness of these results to the inclusion of the 17 LLMs in northern Italy. Table OA9 reports the coefficients for the controls.

LLM, which are decided by the central government before a normal electoral term (Anelli and Peri 2017). A city council may be dismissed if there is severe malfunctioning or its members are investigated for corruption or crimes, including organized crime such as affiliation with the Mafia. The dependent variable for these specifications is the number of city council dismissals in 282 southern LLMs over the 1990–2015 period. When controls are not included, an increase in birthday cheating of politicians equal to a standard deviation of the birthday cheating of remainers increases the number of city council dismissals by almost 13, although the estimate is borderline significant. Note that in the average LLM about seven cities are observed for 25 years; the average number of council dismissals (weighted by births in the LLM) is about 37. When controls are included, the estimated coefficient reduces to about 2 but becomes strongly significant.

As in the case of the economic outcomes examined in Section 4, the estimates reported in this section cannot be considered causal. But in all cases the Oster (2019) δ-parameter reaches reassuring values, which suggests that unobservable confounders, if they could be included, should not excessively distort these estimates. Together the results support the plausibility of the hypothesis that in localities experiencing a more severe honesty drain during the 20th century, recently elected politicians were less honest, and city administrators performed less well.

5. Conclusion

We have studied the historical tendency of Italian parents to register false birth dates for children born near the end of a year, shifting the date to early January. This phenomenon is well known to demographers and is a form of rule breaking.

With respect to other rule-breaking indicators used in the literature, birthday cheating has an advantage: census data can be used to construct birthday cheating data for migrants and remainers born in narrowly defined localities and at different times during the 20th century. In our detailed space-time cells, the two groups are likely to share the benefits of cheating and deterrence, so observed rule breaking can be considered an indicator of their average intrinsic honesty.

On the basis of this information, we show that migrants between Italian regions during the 20th century were nonrandomly selected according to their intrinsic honesty. This is particularly clear for migration flows from South to North, which are significantly larger than those in the opposite direction. As a result, some localities in the South experienced a reduction in the average honesty level of their remaining population. We find a large heterogeneity in the honesty drain across localities in the South, where some gained and some lost honest families.

Finally, we measure the size of the honesty drain or gain experienced by each locality and correlate this measure with a set of economic and political outcomes. Results are suggestive that a nonrandom selection of migrants based on honesty may have had important negative consequences in localities that experienced a more intense honesty drain.

We believe that our results are of general relevance beyond Italy because they warn that some localities may be caught in a vicious cycle of increasing diffusion of cheating attitudes and emigration of families more averse to breaking rules. Finding ways to cut this cycle is crucial in some Italian localities and maybe elsewhere.

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