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Intergenerational mobility in the very long run: Florence 1427-2011

Guglielmo Barone and Sauro Mocetti ♣

Abstract. We examine intergenerational mobility in the very long run, across generations that are six centuries apart. We exploit a unique dataset containing detailed information at the individual level for all people living in the Italian city of Florence in 1427. These individuals have been associated, using their surnames, with their pseudo-descendants living in Florence in 2011. We find that long-run earnings elasticity is about 0.04; we also find an even stronger role for real wealth inheritance and evidence of persistence in belonging to certain elite occupations. Our results are confirmed when we account for the quality of the pseudo-links and when we address the potential selectivity bias behind the matching process. Finally, we frame our results within the existing evidence and argue that the quasi-immobility of preindustrial society and the existence of multigenerational effects might explain the long-lasting effects of ancestors' socioeconomic status.

Keywords: intergenerational mobility, earnings, wealth, occupations, informational content of surnames, Florence.

JEL classification: J62, N33, D31.

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1. Introduction

Almost all of the empirical studies on intergenerational mobility have focused on the correlation in socioeconomic status between two successive generations – parents and their children – and have found that such a correlation is much lower than one. A straightforward implication is that the economic advantages and disadvantages of ancestors should vanish in a few generations.[1] In this paper, we provide further evidence on persistence by empirically documenting that there is not full convergence even after generations that are six centuries apart, thus suggesting that the speed of convergence is much less than it has been expected. This remarkable result is even more surprising if we consider the huge political, demographic, and economic upheavals that have occurred over a so long time span and that were not able to untie the Gordian knot of socioeconomic inheritance.

Linking people belonging to generations that are distant from each other is difficult because of data limitations. In this paper, we overcome this obstacle by exploiting a unique dataset containing the main socioeconomic variables, at the individual level, for people living in the Italian city of Florence in 1427. These individuals (the ancestors) have been connected, using their surnames, to their pseudo-descendants living in Florence in 2011. We use a two-sample two-stage least squares (TS2SLS) approach: first, we use the sample of ancestors and regress the log of earnings on surname dummies (and on age and gender); second, we observe the taxpayers present in the 2011 Florence tax records and regress the log of their earnings on that of their ancestors, as predicted by the coefficient of surname dummies estimated in the first step. The same strategy has been repeated using the log of real wealth instead of the log of earnings as dependent variable.[2]

[1] Earnings persistence has been observed in all countries studied so far, although to varying degrees. See Black and Devereux (2011) and Corak (2013) for recent surveys.

[2] Björklund and Jäntti (1997) were the first to apply the TS2SLS approach to intergenerational mobility estimation. Thenceforth, the same strategy has been adopted for many country studies, typically using occupation, education and sector of activity to predict pseudo-fathers' earnings. On the contrary, Aaronson and Mazumder (2008) used state and year of birth, while Olivetti and Paserman (2015) exploited the information conveyed by first names. Some of these variables,

We find that the elasticity of descendants' earnings with respect to ancestors' earnings is positive and significant, with a point estimate around 0.04. Stated differently, being the descendants of a family at the 90th percentile of earnings distribution in 1427 instead of one at the 10th percentile would entail a 5% increase in earnings among current taxpayers. Wealth elasticity is also statistically significant and the magnitude of the implied effect is even larger: the 10th-90th exercise entails a 12% difference in real wealth today. Looking for non-linearities, we find some evidence of the existence of a glass floor that protects the descendants of the upper class from falling down the economic ladder.

In order to understand the persistence of socioeconomic status in the long run we provide two further pieces of evidence. First, we find evidence of dynasties in certain (elite) occupations: the probability of belonging to such occupations today is higher the more intensely the pseudo-ancestors were employed in the same occupations. This result is consistent with our baseline evidence on the long-run earnings persistence, particularly at the top of the economic ladder. Moreover, it also highlights a potential channel of inheritance, related to the market and non-market mechanisms governing access to elite occupations. Second, using the methodology proposed by Güell et al. (2015), we show that intergenerational mobility in the 15th century was much lower than nowadays. This result might partly explain the long-lasting effects of ancestors' socioeconomic status.

Our empirical findings may suffer from some potential sources of bias. First, the strength of the pseudo-links may be questioned, as we work with generations that are six centuries apart. However, a rich set of robustness checks is largely reassuring on the quality of the pseudo-links and on their large informational content. Namely, we have run regressions giving more weight to rare surnames, to those that are more Florence-specific, and to those characterized by a lower within-surname variation in 1427, with the implicit assumption that in those cases the pseudo-links are more reliable. In all cases the results are largely confirmed and, if anything, the estimated elasticity is slightly upwardly revised consistently with the attenuation bias related to measurement errors. We also perform placebo regressions where we randomly reassign surnames to the descendants and find that surnames indeed have a large informational content. Second, the likelihood of finding ancestors for current taxpayers (or finding descendants for 1427 taxpayers) may vary with earnings and/or wealth so that our results might suffer from selectivity bias. However, we show that earnings and wealth (and other observables at our disposal) are roughly comparable between matched and

however, are partly endogenous, since they are related to parental characteristics, but they may also directly affect children's outcomes (e.g. parents' education or state of residence), thus leading to an upward bias. Surnames, in contrast, are more exogenous markers.

unmatched surnames, thus minimizing the risk of having a selected sample. Moreover, we directly account for the selectivity bias using surnames' characteristics (e.g., their complexity) as exclusion restriction.

To the best of our knowledge, we are the first to provide evidence on intergenerational mobility over the very long run, linking ancestors and descendants that are six centuries apart. Lindahl et al. (2015) use Swedish data that links individual earnings (and education) for three generations and find that persistence is much stronger across three generations than predicted from simple models for two generations. Other studies find significant association – in terms of education and occupational prestige (Braun and Stuhler, 2018) or social class (Chan and Boliver, 2013) – between grandparents and grandchildren, even after parents' outcomes are taken into account. There is also a growing interest in using surnames to estimate intergenerational mobility. Collado et al. (2012), using data from two Spanish regions, found that socioeconomic status at the end of the 20th century still depends on the socioeconomic status of one's great-great grandparents. Clark and Cummins (2014) used the distribution of rare surnames in England and found significant correlation between the wealth of families that are five generations apart. Finally, Clark (2014) goes beyond the standard definition of intergenerational mobility based on earnings, wealth or occupation and suggests that *status* correlation is much higher and fairly constant across centuries – his book contains estimates of status mobility as early as 1300 for England and 1700 for Sweden.[3] Although controversial, Clark's results increased interest in multigenerational and long-term mobility (Solon, 2018).[4]

Our empirical analysis also has other prominent strengths and elements of novelty with respect to previous literature. First, we consider different socioeconomic outcomes, including earnings, wealth, and belonging to an elite occupation. Indeed, most of the empirical evidence is focused on labor income, though wealth inheritance (Piketty, 2011; Piketty and Zucman, 2015) and intergenerational transmission of occupation (Black and Devereux, 2011) have recently attracted renewed interest.[5] Second, we predicted ancestors'

[3] Güell et al. (2015) use surnames with a different perspective: they also assume that surnames are indicative of familial linkages but they propose a new approach to estimating intergenerational mobility using cross-sectional (instead of panel) data. Namely, they suggest an indicator capturing how much surnames explain of the total variance of individual incomes.

[4] Chetty et al. (2014) argue that the Clark's focus on distinctive surnames effectively identifies the degree of convergence in income between racial or ethnic groups rather than across individuals. Braun and Stuhler (2018) and Vosters (2018) do not find empirical support for some of Clark's predictions, while Solon (2018) discusses a variety of other studies contrasting with them.

[5] Clark and Cummins (2014) use wealth drawn from estate data, thus ignoring inter-vivos transfers and are referred to a selected sample of the population. Our data, on the contrary, are more

socioeconomic status using surnames at the city level, thus generating more precise links across generations with respect to other studies that use names or surnames at the national level. Moreover, the huge heterogeneity and "localism" of Italian surnames further strengthen the quality of the pseudo-links and represent an ideal setting for analyses that exploit the informational content of surnames. Third, we estimate intergenerational mobility out of rare surnames, precisely for the reasons just mentioned (i.e. the focus on a unique city rather than a whole country and the high variability of surnames in Italy). Therefore, our estimates are more immune to selectivity issues and more generalizable to population-wide measures of intergenerational mobility while the results by Clark and his coauthors – which explicitly refer to elite and underclass surnames – mostly reflect historical advantages/disadvantages of specific groups (Chetty et al., 2014).[6] Fourth, the Italian cities offer a unique background to trace family dynasties and investigate the transmission of inequalities across the centuries. In the 15th century, Florence, unanimously recognized as the cradle of the Renaissance, was already an advanced and complex society, characterized by a high level of economic development, a rich variety of occupations and significant occupational stratification. Finally, we are the first to provide a measure of intergenerational elasticity (between two successive generations) in a preindustrial society.

The rest of the paper is structured as follows. Section 2 presents the empirical strategy. Section 3 provides background information and describes the data. Section 4 shows the main empirical results, while Section 5 examines potential biases due to the quality of the pseudo-links and to selectivity issues. Section 6 contains further results. Section 7 concludes.

2. Empirical strategy

The main requirement when analyzing socioeconomic mobility is an appropriate data set that spans over generations. Unfortunately, such a suitable dataset is not easily available, and this is even more true if we consider generations that are centuries apart. To overcome the problem, we adopt the TS2SLS approach that combines information from two separate samples.[7]

representative and have the advantage of being available when an individual is adult. Moreover, we can control for the evolution of the outcome variable in the life cycle by adding age as control.

[6] Indeed, elasticity estimates based on a highly selected population might largely differ from those drawn from population-wide studies. Björklund et al. (2012) find an elasticity around 0.9 at the extreme top of the distribution in Sweden, a country known for having high generational mobility.

[7] The two-sample estimation method was introduced by Angrist and Krueger (1992) and Arellano and Meghir (1992). See also Ridder and Moffit (2007) and Inoue and Solon (2010) for a review of the approach and for a discussion of its properties.

In the first sample, we have information about the socioeconomic outcomes (log of earnings or log of real wealth) of the ancestors (superscript a), their surnames, and some other covariates, and we run the following regression:

$$y_{i,k}^{a} = \delta' S + \rho' X_{i,k}^{a} + u_{i,k}^{a} \tag{1}$$

where $y_{i,k}^{a}$ is the outcome of ancestor i with surname k living in Florence in the 15th century; S is vector of surname-dummy variables with $S_k = 1$ if ancestor i has surname k and $S_k = 0$ otherwise; $X_{i,k}^{a}$ is a vector of individual controls, including age, age squared and gender, and $\mu_{i,k}^{a}$ is the error term.

In the second sample, we have information about pseudo-descendants (superscript d), i.e. taxpayers currently living in Florence. For reasons of data availability, the data are aggregated at the surname level. The regression of interest is:

$$y_{k}^{d} = \beta\big(\hat{\delta}' S\big) + \gamma' X_{k}^{d} + u_{k}^{d} \tag{2}$$

where y_{k}^{d} is the average (log) outcome of taxpayers with surname k currently living in Florence; X_{k}^{d} is, as above, a vector of controls for age, age squared and gender, where all these variables considered are averages at the surname level; $\hat{\delta}' S$ is the log of ancestors' outcomes imputed using the coefficients for surname-fixed effects estimated in equation (1) and μ_{k}^{d} is the residual; the parameter β is the TS2SLS estimate of the intergenerational elasticity. To replicate the original population, the regressions are weighted by the frequency of the surnames in 2011. The standard errors have been bootstrapped with 1,000 replications in order to take into account the fact that the key regressor is generated.

In Section 6, we complement the evidence on the long-run elasticities with an empirical exercise aimed at testing the persistence in belonging to the following occupations: lawyers, bankers, medical doctors and pharmacists, and goldsmiths. We restrict the analysis to them because they are affluent occupations already existing in 1427 and for which data are currently publicly available. By merging information drawn from the surname distribution in the province of Florence with the public registers containing the surnames of the above-mentioned occupations, we built a dataset at the individual level where, for each taxpayer, we are able to define a dummy variable indicating whether or not she belongs to a given occupation. Finally, for each occupation, we regress this dummy variable on the share of ancestors in the same occupation. Namely, for each occupation p (p = lawyers, bankers, medical doctors and pharmacists, and goldsmiths), we estimate a probit model whose estimating equation reads as:

$$Pr\{d_{i,k,p} = 1\} = \Phi(\ \theta z_{k,p}) \tag{3}$$

where $d_{i,k,p}$ is a dummy variable that equals 1 if individual i with surname k belongs to occupation p in 2005 and 0 otherwise, $z_{k,p}$ is the share of ancestors with surname k belonging to occupation p, and $\Phi(.)$ is the cumulative distribution function of the standard normal distribution. Since the estimation combines individual-level data for the dependent variable and surname-level data for the covariate, the standard errors are clustered at the surname level (Moulton, 1990).

3. Data and descriptive analysis

3.1 Data sources

Florence was politically, economically, and culturally one of the most important cities in the world from the 14th to 16th centuries.[8] In 1427, in the midst of a fiscal crisis provoked by the protracted wars with Milan, the Priors of the Republic decreed an entirely new tax survey that applied to the citizens of Florence and to the inhabitants of the Florentine districts (1427 Census, henceforth). The assessments, entrusted to a commission of ten officials and their staff, were largely complete within a few months, although revisions continued during 1428 and 1429. It has been acknowledged as one of the most comprehensive tax surveys to be conducted in pre-modern Western Europe. The documentary sources are fully described in Herlihy and Klapisch-Zuber (1985).

The 1427 Census represents our first sample, containing information on the socioeconomic status of the ancestors. Indeed, the dataset reports, for each household, among other variables, the name and the surname of the head of the household, his/her occupation at a two-digit level, assets (i.e. the value of real property and of private and public investments), age and gender. The data were enriched with estimates of the earnings attributed to the household head on the basis of the occupations and the associated skill group.

The Florence 2011 tax records represent our second sample, containing information on the socioeconomic status of the pseudo-descendants. We consider only Italian taxpayers among potential descendants. From the tax records, we draw information on incomes and the main demographic characteristics (age and gender). The income items reported on personal tax returns include salaries and

[8] See the online Appendix A.1 for more details.

pensions, self-employment income, real estate income, and other smaller income items. In order to comply with the privacy protection rules, the variables have been collapsed at the surname level, and only surnames with more than five occurrences have been included. Because of the same privacy-related restrictions, we only have information for surnames that can be matched with those of the 1427 Census. We define as earnings the total income net of real estate income, while real wealth has been estimated from real estate income.[9]

Examining persistence across centuries in certain occupations, as in equation (3), requires additional datasets, because the tax records do not contain information on occupations. We proceed as follows. First, we have individual-level data on the universe of taxpayers in the province of Florence in 2005, for which we observe only surnames, drawn from the Italian Internal Revenue Service. After selecting only Italian surnames, we merge this dataset with the public registers containing the surnames of lawyers, bankers, medical doctors and pharmacists, and goldsmiths. For example, suppose that there are n taxpayers with a certain surname and that we know that there are p_1 lawyers and p_2 bankers with the same surname. We assume, without loss of generality, that the first p_1 individuals are lawyers and the second p_2 are bankers (obviously, with $\sum_i p_i < n$).

The public archives for these occupations are the following: bankers are taken from an archive, managed by the Bank of Italy, which contains registry information on the members of the governing bodies of the banks (we restrict the analysis to Tuscan banks, as Tuscany is the Italian region where Florence is located); lawyers, doctors and pharmacists come from the archives of the provincial professional organizations, to which they are required to be registered; finally, the National Business Register database contains registry information on the members of the governing bodies of goldsmith firms and shops (again, we focus on surnames in province of Florence).[10]

3.2 The origin and distribution of surnames

Pseudo-links between ancestors and their descendants are generated exploiting the informational content of surnames and (implicitly) the geographical localization, as we consider only people living in Florence in both samples.

[9] Specifically, from the biannual Survey of Household Income and Wealth carried out by the Bank of Italy (we used the waves from 2000 to 2012), we selected people living in the province of Florence, we regressed the log of real assets on age, gender, and incomes from the building (actual and imputed rents), and we stored the coefficients. Then, we imputed real wealth for the individuals included in the tax records using age, gender, real estate incomes, and the coefficients estimated and stored above.

[10] See the online Appendix A.2 for more details on the data sources.

Italians surnames have some interesting peculiarities. They are inherited from one generation to the next through the patriline, and most Italians began to assume hereditary surnames in the 15th century. Some surnames derived from one's father's name (patronymics) through the use of the Latin genitive (e.g., Mattei means son of Matteo) or formed by the preposition of *di*/*de* followed by the name (e.g., Di Matteo or De Matteo, meaning the son of Matteo). The large number of Italian surnames ending in *-i* is also due to the medieval habit of identifying families by the names of ancestors in the plural (which have an *-i* suffix in Italian). The origin or residence of the family gave rise to many surnames such as the habitat (e.g., Della Valle, "of the valley"), specific places (e.g., Romano, "Roman"), and nearby landmarks (e.g., Piazza, "square"). The occupations (or utensils associated with the occupation) were also a widespread source of surnames, such as Medici ("medical doctors"), Carradori ("carters"), and Forni ("ovens"). Finally, nicknames, typically referring to physical attributes, also gave rise to some family names, e.g., Basso ("short") and Grasso ("fat"). The huge variety of surnames was amplified by the extraordinary linguistic diversity of Italy. Many surnames' endings are region-specific, such as *-n* in Veneto (e.g., Benetton), *-iello* in Campania (e.g., Borriello), *-u* and *-s* in Sardinia (e.g., Soru and Marras), and *-ai* and *-ucci* in Tuscany (e.g., Bollai and Balducci).

To our aim, the context we analyzed has two striking features. First, in Italy, there are a large number of surnames, likely one of the largest collections of surnames of any ethnicity in the world. This is associated with a high fractionalization: for example, the first 10 most frequent surnames only account for about 1% of the overall population, while the same figures for other European countries (e.g., England, France, Germany and Sweden) are much higher. Second, and unsurprisingly, the surnames present in our data are highly Florence-specific: on average, the ratio between the surname share in Florence and the corresponding figure at the national level, which measures a specialization index centered on 1, is nearly 6. Therefore, the informational content of the surname is presumably much higher than elsewhere, supporting our empirical strategy in the identification of the pseudo-links.

3.3 Descriptive analysis

In the 1427 Census, there are about 10,000 families (1,900 surnames), corresponding to nearly 40,000 individuals. The descriptive statistics are reported in Table 1 (Panel A) and refer to household heads. The earnings and real wealth were equal, on average, to 38 and 414 Florins, respectively.

Members of the guilds were at the top of the economic ladder and held influential positions in society and politics. The most powerful guilds were those involved in the manufacture or trade of wool and silk, and moneychangers. Indeed, many Florentine families were successful bankers (e.g., Bardi, Medici and Peruzzi), and they were known throughout Europe as well, for they established banking houses in other cities such as London, Geneva, and Bruges. In terms of size, the artisans were the most relevant occupational group. Moreover, the vibrant economic activity favored the development of lettered bureaucrats and professionals such as lawyers, judges, medical doctors, and pharmacists (the oldest pharmacy in Europe was set up in Florence). At the bottom of the occupational ladder, there were unskilled workers, such as people beating, cleaning, and washing the raw wool, urban laborers, and the servants of private families.[11]

For slightly less than half of the surnames (and two-thirds of the households) listed in the 1427 Census, we found pseudo-descendants in the 2011 tax records.[12] They correspond to about 800 surnames and 52,000 current taxpayers (about one fourth of Italian taxpayers living in Florence today). On average, they earn about 24,000 Euros per year, and the real wealth is estimated to be larger than 160,000 Euros (Table 1, Panel B).

Table 2 combines the two datasets and provides a first explorative assessment of persistence: we report for the top 5 and bottom 5 earners among current taxpayers (at the surname level) the modal value of the occupation and the percentile in the earnings and wealth distribution in the 15th century (the surnames are replaced by capital letters for confidentiality reasons). The top earners among the current taxpayers were already at the top of the socioeconomic ladder six centuries ago: they were lawyers or members of the wool, silk, and shoemaker guilds; their earnings and wealth were always above the median. On the contrary, the poorest surnames had less prestigious occupations, and their earnings and wealth were below the median in most cases.

4. Main results

As shown in equation (1), in the first stage we regress the log of the ancestors' earnings or the log of the ancestors' real wealth on the surname

[11] The online Appendix A.3 provides further details.

[12] The creation of the pseudo-links between the two samples through surnames has been pursued with some degree of flexibility to account for slight modifications in the surnames across the centuries. For example, for all surnames derived from one's father's name (e.g., Francesco), we apply the traditional Italian patronymic rules and, therefore, current taxpayers with surnames such as (say) Franceschi, De Francesco, or Di Francesco are all considered descendants of Francesco. For fathers' names beginning with a vowel (e.g., Antonio), we also include the use of the apostrophe as a variant to define their descendants (i.e. D'Antonio).

dummies (and, in some specifications, the controls included in the vector X_i^a) using the 1427 Census data. After controlling for age and gender, we find that the surnames account for about 10% of the total variation in the log of earnings and 17% of the total variation in the log of wealth. The p-value of the F-test, testing whether the coefficients of the surnames are jointly different from zero, is far below 0.01. This result supports the hypothesis that the surnames carry information about socioeconomic status. The coefficients for the surnames estimated in the first stage are then used to predict the ancestors' earnings and real wealth for the taxpayers included in the 2011 tax records.

In Table 3 (Panel A) we present our TS2SLS estimates of the intergenerational earnings elasticity, as shown in equation (2).[13] We consider three different empirical specifications, with the first one including only the predicted ancestors' earnings, the second and the third ones adding gender, and gender, age and its square, respectively. The controls in the first-stage regressions are adjusted accordingly. The earnings elasticity is fairly stable across specification, with a magnitude around 0.04, and is statistically significant at the 5% level. We also report the standardized beta coefficient and the rank-rank coefficient.[14] According to the former, in the third column a one-standard-deviation increase in the pseudo-ancestors' log earnings increases the pseudo-descendants' log earnings by 8% of its standard deviation. Put differently, being the descendants of a family at the 90th percentile of earnings distribution in 1427, instead of a family at the 10th percentile of the same distribution, would entail a 5% increase in earnings among the current taxpayers. Therefore, the effect, besides being significant, is also non-negligible from an economic point of view.

In Table 3 (Panel B) we replicate the estimation with respect to the real wealth elasticity. The parameter ranges from 0.02 to 0.03, and is, again, highly significant. The standardized beta coefficient equals 9% and is slightly higher than in the earnings case. The 10th-90th exercise entails a 12% difference in real wealth today. Stronger wealth persistence is confirmed if we restrict the estimation of the intergenerational earnings elasticity to the same sample of families. The larger inertia in the real wealth case is somewhat expected, as real wealth is accumulated through income (net of consumption) over the life cycle, but can also be directly passed down to subsequent generations through bequests or inter-vivos transfers.

[13] We regress current outcomes measured in Euros on past averages measured in Florins. However, this is not an issue because the log-log regression coefficients are unit-less elasticities.

[14] In the rank-rank regression, we rank current taxpayers (and their pseudo-ancestors) based on their earnings or wealth relative to other taxpayers (pseudo-ancestors). The slope of this rank-rank relationship identifies the correlation between ancestors' and descendants' positions in the earnings or wealth distribution. As these measures are not sensitive to differences in the variation of the underlying distributions, they are more suitable for comparisons.

Intergenerational elasticities are useful summary measures, but they may conceal interesting details about intergenerational mobility at different points of the distribution. Researchers have used different techniques to relax the linearity assumption, including spline, higher-order terms, or quantile regressions. Unfortunately, the sample size at our disposal prevents us from applying these techniques, and we rely on more traditional and simpler transition matrices, dividing ancestors' and descendants' economic outcomes into three classes, according to terciles (lower, middle, and upper classes). In Table 4, we report the transition matrix referred to earnings and wealth. For those originating from the lower class in terms of earnings, there are fairly similar opportunities to belong to one of the three destination classes. For those coming from the upper class, in contrast, the probability of falling down to the bottom of the economic ladder is relatively lower. A similar "glass floor" is observed for the wealth although, in this case, we also observe a "sticky floor": more than two fifths of descendants from the lower class remain there after centuries.

The results discussed so far suggest that the persistence of socioeconomic status in the long run is significant both from an economic and a statistical point of view. They are even more striking given the huge political, demographic, and economic upheavals that have occurred in the city across the centuries.

5. Robustness

In this Section, we provide some robustness checks related to the imputation of pseudo-links (Subsection 5.1) and potential selection bias (Subsection 5.2).[15]

5.1 Robustness of pseudo-links

Our empirical strategy relies on the assumption that the probability that one taxpayer (randomly) taken from the 2011 tax record is a descendant of one taxpayer (randomly) selected from the 1427 Census is strictly higher if the two share the same surname. In what follows we corroborate this assumption and show that our results are robust to the lineage tracing procedure.

We start by noting that our pseudo-links are more reliable with respect to those adopted in previous studies, as they are generated by surnames living in the same city. For example, if the same data were available for all Italian cities, our strategy would entail the prediction of the ancestors' socioeconomic status using

[15] In the online Appendix A.4 we provide further robustness checks. They deal with potential measurement errors regarding the dependent variables and with the role of outliers. Results are fully confirmed.

the interaction between surnames and cities. This is arguably a more demanding and more precise approach to creating links across generations than the one adopted in previous studies (i.e. surnames at the national level). Moreover, the huge heterogeneity and "localism" of Italian surnames further strengthens the quality of the pseudo-links. Nevertheless, we propose three tests aimed at showing the robustness of our findings to the lineage imputation procedure. Results are reported in Table 5 for both earnings (Panel A) and wealth (Panel B).

The first test is based on the idea that the more common a surname is, the less informative sharing the surname is likely to be about actual kinship. The assumption that rare surnames are more indicative of familial linkages is not new in the literature (Clark and Cummins, 2014 and Güell et al., 2015). Therefore, we re-estimate equation (3) by weighting observations with the inverse of the relative frequency in 1427, thus giving more weight to rare surnames (column 1).

The second test exploits within surnames variation in earnings (wealth). The idea is that mismeasurement of the link between ancestors and descendants is less problematic if ancestors had similar earnings (wealth). Let's consider, for example, two surnames having the same frequency in the population and the same average earnings (wealth). However, ancestors with the first surnames had similar earnings (wealth) while ancestors with the second surnames were characterized by an earnings (wealth) distribution with large variability. Incorrect (within surnames) links between ancestors and descendants would be innocuous in the first case, while they might bias the estimation of the elasticity in the second one. To address this point, we re-estimate equation (3) by weighting observations with the inverse of the coefficient of variation of earnings (wealth) for each surname in 1427, thus giving more weight to surnames characterized by less variability in earnings (wealth) levels (column 2).

The third test is related to the fact that the city of Florence is not a closed system. For instance, it may well happen that an immigrant having the same surname as those living in Florence in 1427 settled in Florence from outside in the following centuries. Our methodology erroneously treats the latter as a pseudo-descendant of the former. To address this further point, we exploit the extent to which a surname is Florence-specific (specificity is measured as the ratio between the surname share in Florence and the corresponding figure at the national level). The idea is that the more typically Florentine a surname is, the less the same surname is likely to be contaminated by migration patterns. Therefore, as above, we re-estimate equation (3), giving more weight to surnames that are more Florence-specific (column 3). Moreover, we also follow Güell et al. (2015) and Güell et al. (2018a) and replicate baseline estimates including Florence-specificity as control (column 4) as a solution to the threat that surname dummies might capture localism rather than mobility.

In all cases, the results are reassuring as our baseline estimates are confirmed and, if anything, they are upwardly revised, consistent with the fact that mismeasurement of the family links should lead to an attenuation bias.

The exercises discussed above *indirectly* test the robustness of the pseudo-links. We complement them with a *direct* test that goes as follows. We randomly reassigned surnames to taxpayers in 2011 and re-estimated the TS2SLS intergenerational elasticities. If the positive correlations we detected were not related to the lineage (whose measurement might be affected by error), but would emerge by chance, we should find that our estimates are not statistically different from those stemming from a random reshuffling of surnames. Figure 1 shows the distribution of the estimated earnings and wealth elasticity for 1 million replications. The two dashed vertical lines are the 95th and the 99th percentiles, while the red line indicates our estimate based on the observed surnames. These results provide a clear graphical representation of the informational content of the surnames and the statistical goodness of the pseudo-links: the simulated *p*-value in this exercise is lower than 1% for both earnings and wealth.

5.2 Selectivity bias

Our exercise is based on the intersection at the surname-level of two datasets: current and past Florentine taxpayers. As such, we face three potential sources of selection bias. First, not all taxpayers currently living in Florence are matched with their pseudo-ancestors: selection with respect to the 2011 population is addressed in Subsection 5.2.2. Second, not all pseudo-ancestors living in Florence in 1427 are matched with their descendants: selection with respect to the 1427 population is addressed in Subsection 5.2.3. Stated differently, we are addressing selection issues related to different reference populations and, therefore, to somewhat different research question: in the first case, the earnings elasticity of the population currently living in Florence in 2011 and in the second case, the earnings elasticity of the population of descendants of those living in Florence in 1427. Third, even for matched surnames, some form of selection *within* surnames may be at work if, for example, individuals who leave (or keep) a surname are endogenously selected: selection within surnames is addressed in Subsection 5.2.4. Before discussing these issues in more depth, we note that one important explanation for the first two types of selection is purely mechanical and relates to the limitations imposed by the privacy rules. Hence, we preliminarily analyze the implications of privacy rules (Subsection 5.2.1).

The overall message of this Subsection is that due to data limitations, which are intrinsically related to the very long run we analyze, we can not fully rule out the possibility that our results are plagued by some selection biases. However, all the tests we run are quite reassuring and suggest that selection does not play a critical role in explaining our key findings.

5.2.1 Privacy rules

We are allowed to observe, for privacy reasons, only surnames with more than five occurrences in the 2011 tax records. How large is the sample selection implied by the privacy rules? And to what extent does it affect our estimates?

According to our elaborations based on the overall distribution of the surnames in Florence (excluding foreigners) taxpayers whose surname has up to five occurrences (and are thus selected out) are about one-fifth of the whole sample. Since we know what these less frequent surnames are, we have reconstructed the dataset we would have obtained if privacy constraints had not existed. We find about 400 additional surnames that would have been matched to their pseudo-ancestors in 1427, corresponding to nearly 1,000 current taxpayers. The absence of the confidentiality rule, therefore, would have led to a 50% increase of our sample in terms of surnames but only 2% in terms of the number of taxpayers. Moreover, we also find that the earnings and wealth of these 400 surnames are not significantly different from those of the reference population in 1427. This evidence suggests that the selection bias due to the five-occurrences cut-off is reasonably negligible as these unmatched surnames had similar earnings and wealth at the origin and represent a small fraction of the current taxpayers.

In Table 6 we also perform a more direct test, examining the robustness of our findings to the privacy cutoff by imposing other increasing (placebo) thresholds. According to these results, the earnings and wealth elasticity are remarkably stable across the different specifications, thus confirming that the drop of surnames with at most five occurrences does not seem to be an issue in our case.

5.2.2 Selection with respect to the 2011 population

Considering the population of current taxpayers, we observe current outcomes at the surname level only for those surnames we are able to find pseudo-ancestors in 1427. If unmatched units are different from those examined in our sample and this difference is correlated to the outcome variables, our estimate will be biased. Beyond privacy rules, the main source of sample selection is internal migration, i.e. taxpayers who migrated into Florence from other parts of Italy during the period under examination. These individuals, obviously, have not ancestors in the 1427 Census. However, if they display different observables with respect to those included in our sample, this might raise some concerns about our long-run estimates. We now examine in depth the internal migration issue and, then, provide further descriptive evidence and adopt an econometric strategy that directly addresses the selection issue, irrespective from its source.

We start by examining whether, among individuals currently living in Florence, those born in Florence are similar to those who immigrated in the last decades. To this end, we exploit the Survey of Household Income and Wealth (SHIW), managed by the Bank of Italy. The SHIW includes information on each individual's province of birth and province of residence. Moreover, it contains detailed information on earnings, wealth, and the family background of each household head. In order to increase the sample size, we pool data from several waves (from 1993 to 2014). Then we regress (the log of) earnings, (the log of) real wealth, and two proxies of family background (i.e. the fathers' years of schooling and their predicted earnings[16]) on an indicator for the immigrant status and some controls (age, age squared, and wave fixed effects). The dummy variable for the immigrant status is equal to one if an individual lives in Florence on the date of the interview but he/she was born in a different province, and zero otherwise. Table 7 shows that there are not statistically significant differences between natives and immigrants in terms of either economic success or family background (columns 1-4). This evidence, while encouraging, does not necessarily imply that (missing) immigrants display the same intergenerational mobility. To address this concern, we use the same data to compute the intergenerational elasticity between two successive generations for those born in Florence and those that have migrated to Florence. The estimated elasticity (0.46, column 5) is close to the national average, consistently with what found in Güell et al. (2018a). More interestingly to our aim, the elasticity for Florence-born individuals is not statistically different from that estimated for immigrants (column 6). Therefore, these findings are reassuring about potential structural differences in intergenerational mobility between natives and immigrants. One potential criticism of this exercise is that we are able to define the immigrant status only from the 20th century while we do not know whether someone immigrated to Florence in the previous centuries. While this is certainly true, we also notice that the most important demographic changes in the city occurred in the last century; moreover, other studies point out that internal migration was negligible in the pre-modern era (Breschi and Malanima, 2002) and reached its peak during the 1950s and the 1960s (Bonifazi and Heins, 2000).

[16] Earnings are predicted using retrospective information on fathers' years of schooling, occupation, sector of activity and geographical areas, using the TS2SLS approach, as in Mocetti (2017).

Selection bias might also arise without immigration if, for example, unmatched Florence-natives are positively selected. Simple descriptive evidence suggests, however, that this does not seem to be the case. The average income (earnings plus real estate income) in our sample is slightly above 26,200 Euros, 5% higher with respect to the figure published by the tax authority at the city level. However, if we exclude foreigners (who earn considerably less), our figure is perfectly in line with the average income declared by Italian taxpayers. Summing up, the results above do not suggest the existence of significant differences between our sample and the reference population, although this comparison is clearly limited to the observables at our disposal.

Finally, we directly address selectivity issues with a two-stage Heckman strategy. As exclusion restriction, we exploit several characteristics of the surnames that might be correlated with the probability of having an unmatched surname but (plausibly) uncorrelated with unobserved determinants of current economic outcomes. These characteristics are: (1) the (log of the) number of occurrences of a surname; (2) the length of a surname; (3) the disproportion between the number of vowels and consonants, i.e. whether the vowels represent less than 25% or more than 75% of the total characters of a surnames; (4) whether there are two identical adjacent letters in the surname (e.g., *bb*, *cc*, etc.), other couplings of letters often subject to grammatical errors (e.g., *cq*) or the presence of other characters (e.g., accents and apostrophes) subject to recording errors; (5) whether the surname is composed by (at least) two different words. The underlying idea is that less frequent, longer, or more complex surnames are more subject to mistakes when recorded in the municipal office of vital statistics.[17] This, in turn, might explain why we are unable to match some current taxpayers to their ancestors while there are no particular reasons to suspect that such indicators of complexity can affect earnings.[18]

Results of the estimates with the Heckman corrections are reported in Table 8; the dependent variable is earnings in Panel A and wealth in Panel B. The reference population is the universe of Italian surnames recorded in Florence in 2011 and the regressions are again at the surname-level. In the first stage the exclusion restrictions are always significant and have the expected sign. More importantly, the coefficient of interest in the second stage is largely unaffected: some form of selection is at work but it does not shape our result on long-run

[17] Feigenbaum (2018) shows that transcription errors are the most likely obstacle to link individuals between historical data.

[18] It is also worth noting that these variables have been introduced separately as exclusion restrictions and that they are only weakly correlated among them (in most of the cases the pairwise correlation is below 0.1). Simple correlations at the surname-level also show that these variables are not correlated (with the exception of the surname frequency) with earnings.

intergenerational mobility.

5.2.3 Selection with respect to the 1427 population

The second type of selection we address is about the fact that we fail to find a pseudo-descendant for a number of the surnames existing in 1427. This is clearly a reflection of the demographic processes that are involved in the analysis of intergenerational mobility in the very long run: the families' survival rate depends on migration, reproduction, fertility, and mortality, which, in turn, may differ across people with different socioeconomic backgrounds. As far as migration is concerned, some of the families recorded in the 1427 Census might have decided to migrate during the following centuries. Since they are not necessarily a random sample of the original population, this might bias our estimates. Borjas (1987) provided a theoretical model that shows that migrants are mainly drawn from the upper or lower tail of the skill (i.e. income) distribution. Analogously, a dynasty's reproduction rate (i.e. fertility/mortality rate) may be correlated with income and/or wealth. Jones et al. (2010) showed a strong and robust negative relationship between income and fertility, though they also argued that, in the agrarian (pre-industrialization) economies, the reverse could have been possible, as documented, for example, by Clark and Cummins (2009). On the other side, it is reasonable to expect that the wealthiest families were those better equipped to survive across the centuries (and therefore, those that can be matched to the current tax records).

How do we address these issues? First, we start again with some simple descriptive evidence. In Table 9, Panel A, we compare the average values of a number of observable variables for the surnames that are still present in the tax records of 2011 and those that are not in order to have a general assessment of the relevance of this type of selection issue. It turns out that demographic variables as well as residential choices (potentially capturing unobservable characteristics correlated with urban segregation) are well balanced. The balancing of age and number of children, in particular, suggests that the two groups do not differ at the baseline as to mortality and fertility, respectively, which are two key determinants of this type of selection. As to professions, the share of artisans is higher among survivors, that of merchants and entrepreneurs is lower while other jobs – including member of guilds and unskilled, which represent the two polar case in the economic layer – are equally distributed. Concerning our dependent variables, average earnings are higher for matched surnames, while the difference in the real wealth is not significant. The latter two results bring us to extend the comparison to the whole distributions. Figure 2 shows that the two distributions of earnings are rather similar, although the density of missing families has a larger mass of probability for the lower level of earnings. As far as wealth is concerned, the two distributions largely overlap each other (Figure 2). Table 9, Panel B, reports more formal tests (Kolmogorov-Smirnov and Mann-Whitney) indicating that the distributions of earnings and wealth between surviving and missing families are statistically different. Overall, the evidence in Table 9 alleviates selection concerns with respect to the 1427 population even if the evidence on the whole distributions calls for further analysis.

In the following, we propose two tests aimed at addressing this kind of potential selectivity bias. The first is aimed at fixing a lower bound for our estimates, incorporating a downward bias induced by selective migration. The test goes as follows. Since empirical studies have found the elasticity to lie between 0 and 1, we assume that for missing families, the elasticity is 0, meaning that the migrated families were able to cut the Gordian knot of socioeconomic inheritance. Note that this is the most unfavorable assumption we can make; moreover, this working hypothesis is also not very plausible, because the available evidence shows a significant socioeconomic persistence across generations also among migrants.[19] We add these missing families to the estimating sample and, having assumed that the elasticity is null, impute their earnings/real wealth in 2011 by randomly drawing from a lognormal distribution whose moments are taken from the corresponding distribution of the current taxpayer population. Then, we regress equation (2) on the augmented sample and repeat this procedure (drawing and regression) 1 million times. The parameters of interest are still significant from a statistical and economic point of view: the average estimated elasticity equals 0.016* (with standard deviation equal to 0.009) for earnings and 0.010*** (with standard deviation equal to 0.004) for real wealth. These parameters represent a lower bound to intergenerational elasticity estimates, as far as selection with respect to the 1427 population is concerned.[20]

Second, we estimate a selection equation similar to that used in the previous Subsection and we use the same set of exclusion restrictions. The main difference is that in this case the reference population is the surnames of ancestors in 1427

[19] Borjas (1993) showed that the earnings of second-generation Americans are strongly affected by the economic conditions of their parents in their source countries. According to Card (2005), the intergenerational transmission of education is about the same for families of immigrants as for other families in the US.

[20] Instead of imputing 0 elasticity to unmatched surnames we also replicate the simulation exercise to compute how low this parameter would need to be so that our results on long-run elasticity are null. The estimated parameters are -0.030 and -0.018 for earnings and wealth, respectively. It is worth noting, however, that negative elasticities are rather implausible. Indeed, negative elasticities can occur with a revolution that dramatically change the entire social hierarchy, an event that did not occurred in Florence in the historical period considered in this study.

(rather than those of the current taxpayers). Results are reported in Table 10. The regression is again at the surname-level. We do find, as expected, that less frequent, longer and more complex surnames are less likely to survive; after correcting for selection, the coefficient of interest for earnings (Panel A) and for wealth (Panel B) are largely stable across specifications and very similar to the baseline estimates.

5.2.4 Selection within surnames

The potential biases examined so far concern selection *across* surnames. However, selective migration may also occur *within* families. For example, if the most skilled individuals within a given surname emigrate (i.e. do not survive) then our methodology erroneously treats those individuals as pseudo-ancestors of the taxpayers with the same surname currently living in Florence. The same argument holds if those at the bottom of the skill distribution emigrate (i.e. do not survive) and those at the top stay (i.e. survive).[21]

In order to deal with this further selectivity issue, we run some further exercises. First, we impute earnings (or wealth) in the first stage using the median instead of the mean (as in the TS2SLS approach). Indeed, in the presence of influential outliers (i.e. individuals significantly under- and/or over-performing with respect to the other individuals sharing the same surnames), the median is more informative than the mean about the socioeconomic conditions of the ancestors.

However, within surname selection mechanisms might also be asymmetric. Therefore, we also perform three additional exercises by dropping from the sample the (*i*) top 10% of pseudo-ancestors within each family income or wealth distribution, (*ii*) the bottom 10% of the same distribution and (*iii*) both the top and bottom 10%. The common idea behind these choices is that, to the extent that migration is not random, then family members that stay will be more alike than including those that have left, and the within-surname heterogeneity will be lower. Even forcing the sample to exclude tails (more prone to migrate), the elasticity estimates largely confirm our main findings (Table 11).

6. Further results and discussion

6.1 Dynasties in elite occupations

[21] Another example is an opprobrium mechanism: individuals who deviate from some family norm change (or are forced to change) their surname.

In this subsection we enrich our analysis by providing further evidence on the existence of some degree of persistence in certain (elite) occupations. On the one hand, this represents a further perspective (beyond earnings and wealth) on intergenerational mobility. On the other hand, we provide some insights on the channels behind intergenerational mobility processes and helps to explain the observed long-run persistence in socio-economic status.

We examine whether one's probability of being employed in a certain elite occupation today is higher, the more pseudo-ancestors were employed in the same occupation. Namely, we select the occupations of lawyers, bankers, medical doctors and pharmacists, and goldsmiths. We consider only these occupations for several reasons. First, because of data availability, we are forced to focus on occupations that already existed in 1427 and for which we currently have access to publicly available data. Second, they should be elite or niche occupations, consistent with the fact that there should be unobservable variables that favored career following (e.g. specific human capital or guild privileges).[22] Third, the available empirical evidence documents the existence of career dynasties precisely for (some of) these occupations.[23] Notice that in this exercise we observe the universe of the population of taxpayers in the province of Florence and, therefore, this exercise is immune from some of the selectivity issues discussed above.

Table 12 shows the results from the estimation of equation (3). In each column, we consider each occupation separately, and we find a positive and statistically significant correlation for lawyers, bankers, and goldsmiths, and a positive, but not significant, correlation for doctors or pharmacists. The magnitude of the impact is small: a one-standard deviation increase in the independent variable increases the dependent variable by 0.5%, 0.2%, and 0.6% of its standard deviation for lawyers, bankers, and goldsmiths, respectively. Nevertheless, these results are, again, surprising if evaluated across six centuries. They are also consistent with earnings persistence, and in particular, with larger persistence at the top of the distribution. Finally, they suggest the existence of market and non-market mechanisms governing access to certain occupations and contributing to socioeconomic inheritance over multiple generations.

6.2 Intergenerational mobility in the 15th century

[22] In the online Appendix A.3 we show that the earnings in these selected occupations are larger than the average, both in 1427 and nowadays.

[23] See Lentz and Laband (1989) for doctors, Laband and Lentz (1992) for lawyers and Mocetti (2016) for pharmacists.

Low mobility (between two successive generations) in the preindustrial era may help to explain why we still find some degree of inheritance of socioeconomic status after six centuries. In the next Subsection, we provide the conceptual support to this claim, while here we show that mobility in 1427 was lower than today. We do that by relying on the approach by Güell et al. (2015), who developed a novel measure of intergenerational mobility that needs only cross-sectional data and is based on the informational content of surnames (ICS). Specifically, the ICS is defined as $ICS \equiv R_D^2 - R_F^2$. The first term (R_D^2) is obtained from the regression $y_{i,s} = b'D + \mu_{i,s}$ where $y_{i,s}$ is the log of the income of individual i with surname s, and D is an S-vector of the surname-dummy variables with $D_S = 1$ if individual i has surname s and $D_S = 0$ otherwise. The second term (R_F^2) is obtained from the regression $y_{i,s} = b'F + v_{i,s}$ where F is an S-vector of "fake" dummy variables that randomly assign surnames to individuals in a manner that maintains the marginal distribution of surnames. In a nutshell, the ICS measures how much of the total variance of individual outcomes is explained by surnames, conditional on the underlying distribution of surnames.

Following this methodology, we estimate ICS for incomes in the 15th century, and we compare this figure with that drawn from Güell et al. (2018a) and referring to the province of Florence in 2005; to increase comparability, we also provide an estimate of the ICS restricting the analysis to the surnames with at most 30 occurrences (Figure 3). Although these comparisons should be interpreted with some caution – given the different nature of the data sources and the different distribution of surnames – they support the view that, in the past, intergenerational mobility was (much) lower than nowadays.[24]

Although the analysis of the factors behind such low mobility is beyond the scope of the paper, we might provide some tentative explanations. First, pre-modern societies were characterized by higher levels of inequality and by greater social stratification, thus hampering the mobility of people along the economic ladder (Milanovic et al., 2011). Indeed, the existence of a positive correlation between inequality and intergenerational persistence (the ''Great Gatsby curve'') has been documented both at the cross-country level (Corak, 2013) and within country (Chetty et al., 2014). Moreover, according to liberal theory, industrial society is characterized, with respect to the preindustrial society, by more rational procedures of social selection and therefore higher mobility (Erikson and Goldthorpe, 1992). It is also worth noting that democratization of school access,

[24] Alfani and Ammannati (2017) show that economic inequality in the Florentine State increased over time, from the late 14th to the late 18th century. As we know that economic inequality is negatively correlated with mobility, we might expect that the intergenerational elasticity estimated for the 15th century slightly increased (or, at least, remained stable) over the following centuries.

especially in the preschool age and with reference to compulsory schooling, might have attenuated the role of family background. In 1861 (the first census in Italy), 78% of the population was illiterate while there have been several reforms since then that have progressively increased mandatory schooling and favored access to education to those coming from disadvantaged families (Genovesi, 2004). There is some evidence that early school interventions and other inclusive policies had a positive effect on equality and intergenerational mobility (Cunha and Heckman, 2007; Braga et al., 2011; Stuhler, 2018). Finally, one might expect that the expansion of the welfare state and of the scope of public policies have helped the society to be more equal, thus favoring mobility also through this channel. In the earlier centuries, in contrast, the guilds and the family itself provided most types of welfare services, including the acquisition of human capital and technical skills (Epstein, 1998; de la Croix et al., 2018).

6.3 Framing our results within the existing evidence

In this section we examine whether our baseline results can be framed within the existing evidence on intergenerational elasticity and whether they are consistent with the different theories on multigenerational effects.

We preliminarily note that the comparison with other studies is far from straightforward. Indeed, as discussed in Corak (2006), the comparability is challenged by the existence of many specific empirical choices that might severely affect the estimates, such as the parents' and children's age, the number of years used to average earnings, the set of predictors used in the TS2SLS approach, etc. Caution in interpreting the results applies even more in our case, as our study is characterized by data coming from very different sources (and very different historical periods).

We now turn to the reconciliation of our results with the available evidence and focus on earnings elasticity because of the availability of reliable current estimates. Namely, there are three pieces of evidence. First, the long run elasticity is equal to 0.045. Second, the current elasticity (between two successive generations) is around 0.5.[25] Third, the intergenerational mobility in the 15th Century was lower than nowadays, as shown above.

The simplest hypothesis is to infer long-run mobility by the naive exponentiation of elasticity between two successive generations (i.e. the correlation between grandparent and grandchild is the square of the parent-child

[25] According to Mocetti (2007) the current intergenerational earnings elasticity between two successive generations in Italy is equal to 0.5; Acciari at al. (2017) and Güell et al. (2018a) argue that mobility in Florence is not significantly different from the Italian average.

correlation, the correlation between great-grandparent and child is the cube, etc.). This iterated regression procedure and the current available estimates for Florence would imply that the elasticity across 20 generations is almost zero, which is not consistent with our long-run estimates.

Table 13 shows the elasticity between two successive generations, under different assumptions on the underlying model, which is consistent with our long-run estimate. For sake of brevity we comment only on Panel A (where we assume that our time span covers 19 generations of about 30 years each) and the combinations of assumptions that are consistent with the three pieces of evidence mentioned before. We initially assume an AR(1) process and variable elasticity over time, allowing for a downward change around the 20th century (Table 13, column 1). Although we do not have direct evidence on the trend of elasticity, this assumption is consistent with the structural changes that occurred between the 19th and 20th centuries and discussed in Section 6.2. In more detail, we assume that elasticity is 0.5 in the last generation and smoothly declined to this value during the 20th century, and ask what is the prevailing IGE before the 20th century that is consistent with our long-run estimate. According to these simulations, we would need a two-generation elasticity slightly larger than 0.9 up to the 19th century to replicate our documented empirical facts.

Recent papers have questioned the assumption that the intergenerational transmission process has a memory of only one period (Solon, 2018). Specifically, two distinct theories have gained significant attention. First, grandparents can directly transmit their cultural capital to their grandchildren through childrearing or other forms of interactions; similarly, they can directly pass their wealth to their grandchildren or invest in their human capital (Mare, 2011). Finally, the genetic transmission of family traits whenever they skip a generation might determine a direct effect of grandparents on grandchildren's economic outcomes. We do not have direct evidence for Florence (and Italy) on the grandparent-grandchildren elasticity; following Lindahl et al. (2015), we assume that it is one-third of that between parents and children. Thus, assuming a variable elasticity and AR(2) process (Table 13, column 2), we would need a two-generation elasticity slightly larger than 0.6 up to the 19th century and around 0.5 in the 20th century.

An alternative theory is that supported by Clark and his coauthors. They argue that elasticity might not decline geometrically, as commonly thought, because of the existence of a latent factor (also called "endowment") whose persistence is very high (around 0.8). Following Braun and Stuhler (2018), we can derive the heritability of the underlying unobserved endowment. With simple algebra, it is possible to show that in our case this parameter is in the interval 0.8-0.9, depending on the underlying assumption on the number of generations and on the potential distortion of the long-run elasticity estimate.[26]

Another example that challenges the geometric decline rule is a society of perfect status inheritance (e.g., a pure caste system) in which children, parents, grandparents, and earlier ancestors are identical in their social and economic positions; in this society, the perfect correlations between each generation make alternative types of intergenerational effects (e.g., children-parents, children-grandparents, etc.) indistinguishable. Zylberberg (2013) studies a mathematical model of semi-caste society in which individuals inherit careers and shows that income persistence will decay less than geometrically if mobility is high within but low between distinct blocks of careers.[27] Interestingly, in subsection 6.1 we have shown that some form of dynastic (multigenerational) transmission of occupations underlies our empirical case.

Summing up, our long-run earnings elasticity, although higher than expected, is broadly consistent with different multigenerational mobility models. Unfortunately, we are not able to disentangle them, although this is an important issue as different theories have different policy implications particularly with respect to the role of institutions and other context factors.

6.4 Surname-grouped estimator

Our estimation strategy is based on the use of surnames as grouping variable (see equation 2), an approach that has become increasingly popular in the literature on intergenerational mobility.

In parallel, scholars have been increasingly aware of some potential limitations of this approach, as opposed to the individual-based one. Some argue that the grouping estimator is biased upward if two conditions hold: (i) the grouping variable is inheritable, and (ii) it is associated with the outcome. In such a case, the within-group mobility is not accounted for, while persistent differences between surnames drive the intergenerational correlation up (Solon, 2018; Güell et al., 2018b). In the case of surnames as grouping variable, two notable examples are the geographical origin and the ethnic group. In the former case, a surname

[26] Braun and Stuhler (2018) show that calling β_{-m} the elasticity between children and their ancestors who are distant m generations, the heritability of the latent factor is $(\beta_{-m}/\beta_{-1})^{1/m-1}$.

[27] More generally, many social institutions contribute to status inheritance over multiple generations, especially at the bottom (e.g. due to ethnic or social discrimination) and at the top (e.g. membership in exclusive clubs and/or elite professions) of hierarchies. Borjas (1992) shows that the skills of the children depend not only on parental inputs, but also on the average quality of their ethnic environment. In particular, if the external effect of ethnicity is sufficiently strong, ethnic differences in skills are likely to persist for many generations. Tilly (1998) argued that all forms of long-term inequality are based on categories that allow social groups to monopolize opportunities at the expense of other groups by means of category-based exploitation and social closure.

average can persistently be larger than another one because it is more common in a richer region. In the latter case, the idiosyncratic differences in surname averages might reflect different racial groups, whose belonging is inherited across generations. In our setting, these potential concerns are largely mitigated because (*i*) we have data on a single city, thus avoiding issues related to potential geographical differences and (*ii*) we consider a society without explicit sources of racial discrimination.

Santavirta and Stuhler (2019) has recently enriched our comprehension of the surname-grouped estimator's properties by stressing other sources of bias, related to the sampling scheme. They analyze a continuum between two polar cases: (*i*) the "inclusive" one, in which the group mean (i.e. the estimation of the ancestors' earnings) is defined over the ancestors of the sampled descendants with a perfect overlap between the two populations; and (*ii*) the "leave-out" case, in which the group mean is defined over ancestors who merely share the surname. They show that in the inclusive case the intergenerational elasticity is upward biased while the opposite holds in the leave-out case. They also explicitly discuss our case and argue that is likely a mix between the "inclusive" and "leave-out" variants, without any clear ex-ante priors on the direction of the bias.

On the empirical side, those papers that compared estimates from individually linked data with those based on surname averages failed finding large differences. The appendix to Chetty et al. (2014) show that, in their data, a significant upward bias emerges only when restricting the sample to very common surname (i.e., those occurring more than 20,000 times) while it is almost negligible when restricting the sample to very rare surnames (i.e., those with at most 100 occurrences). Our case is much closer to the latter as our surnames have, on average, 65 occurrences and 80% have less than 100 occurrences.[28] Moreover, Feigenbaum (2018) finds no systematic bias in grouping estimates as compared to the corresponding direct ones in Iowa. This result might be partly due to the fact that he focuses on a relatively small geographical area, as in our case, thus minimizing the geographical content of surnames and allowing a better match across generations using surnames.[29]

All in all, while we are fully aware that surname-grouped estimates may in

[28] It is worth noting that this comparison is not fully conclusive as the rareness of a surname depends on the reference population.

[29] The debate on the surname-grouped estimator intersects that on the reliability of Clark's estimates. A general conclusion is that the reason why Clark found high estimates of intergenerational persistence is because he chose elite and/or underclass surnames, i.e. groups who historically faced different advantages and/or forms of segregation and discrimination (Chetty et al., 2014; Solon, 2018). Our estimates, in contrast, refer to the universe of surnames that, on average, are also characterized by low occurrences.

principle be biased, we are quite confident that a number of features of our study strongly alleviates such a risk and that the bias, if any, would not be able to invalidate the ultimate sense of our findings.

6.5 Generalizability of our results

Our results are referred to the city of Florence and one might wonder whether they can be generalized to other advanced countries and/or cities: we argue that this is the case because Florence does not seem to be a polar case in terms of social fluidity neither today nor in the past.

As the *current* degree of intergenerational mobility, the figure for the province of Florence is roughly similar to that of other advanced countries. Güell et al. (2018a) have provided evidence on the degree of intergenerational mobility for all Italian provinces; according to their evidence, the (simulated) intergenerational income elasticity for the province of Florence would be between 0.4 and 0.5, a figure that is slightly lower than that of Italy as a whole and broadly comparable with that of the United States, the United Kingdom, and France (Corak, 2013).

We have shown that in the 15th century the degree of intergenerational mobility was much lower. So, one may argue that Florence was a polar case in the earlier centuries. Unfortunately, we do not have comparable measures for other preindustrial societies at that time, but we can still provide some evidence about variables that correlate to social fluidity. Countries with higher income inequality also tend to be countries in which a greater fraction of economic advantage and disadvantage is passed on between parents and their children because more inequality of incomes in the present is likely to make family background play a stronger role in determining the adult outcomes of the children (Corak, 2013). In this respect, Milanovic et al. (2011) showed that the gross domestic income per capita and the Gini index in Florence in 1427 were comparable to those of other preindustrial societies for which we have data, such as England, Wales, and Holland. Therefore, we may reasonably infer that the degree of intergenerational mobility in Florence, in the earlier centuries, was not markedly different from that of other Western Europe societies.

However, it is also worth noting that some of the processes behind such long-run persistence might vary across countries (and time) because of the different institutional setting and of other environmental factors we have not observed. For example, the role of grandparents in affecting grandchildren's economic outcomes might vary across types of households (e.g., whether they contain multiple generations) that, in turn, respond to economic incentives and cultural preferences. Moreover, the existence of group and/or surname effect might be more important in certain societies than in others.

7. Conclusions

We have examined intergenerational mobility in the very long run, using a unique dataset that combines the tax records from the Italian city of Florence in 1427 and in 2011, and exploiting a favorable setting for this kind of analysis.

We have found that earnings elasticity, across generations that are six centuries apart, is positive and statistically significant. Its point estimate is about 0.04. We also find evidence of an even stronger real wealth inheritance and of persistence in certain elite occupations. These results show that the speed of convergence between different initial statuses is much lower that implied by existing estimates on the correlation between parents' and children' status. Simple descriptive analysis from transition matrices also indicates the existence of a glass floor that protects descendants of the upper class from falling down the economic ladder. Our findings on elasticities are robust to a number of sensitivity checks, particularly to the lineage imputation and to the potential selectivity bias. We also provide two tentative explanations (and the related empirical support) for the surprisingly low level of mobility: first, mobility in the past was much lower than it is today; second, social status may also be highly persistent.

Under a more speculative perspective, our paper suggests that institutions matter: on the one hand, the likely increase of mobility observed between the preindustrial society and the industrial society might be connected, first, with the industrialization process itself and, second, with the larger role of the welfare state and public policies during the second half of the 20th century. On the other hand, persistence in some selected professions (historically characterized by a high level of protection of the incumbents) might suggest that labor market institutions are also important. Needless to say, there may well be other, concurrent (and unobserved) factors beyond formal institutions, which have affected mobility over time (Güell et al., 2018a). Understanding of the mechanisms underlying intergenerational mobility in the long run, as well as looking for the same evidence in different cities or nations, might represent exciting directions for future research.

Data availability statement

The data underlying this article are available at:
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Tables and Figures

Table 1. Descriptive statistics

Variable	Mean	Standard deviation
Panel A – 1427 Census for Florence		
Earnings (Florins)	38.03	33.01
Real wealth (Florins)	414.0	591.9
Age (years)	45.83	8.557
Female (share)	0.157	0.171
Lawyer (share)	0.012	0.090
Banker (share)	0.009	0.072
Medical doctor or pharmacist (share)	0.039	0.141
Goldsmith (share)	0.009	0.068
Panel B – 2000s data for Florence		
Earnings (Euros)	24,234	4,929
Real wealth (Euros)	160,729	70,962
Age (years)	58.39	3.03
Female (share)	0.521	0.050
Lawyer (share)	0.006	0.080
Banker (share)	0.001	0.033
Medical doctor or pharmacist (share)	0.010	0.101
Goldsmith (share)	0.002	0.044

Source: In Panel A, data are taken from the 1427 Census. In Panel B, data on earnings, real wealth, gender and age are taken from the Florence statistical office (fiscal year 2011); data on occupations are obtained combining information taken from the Internal Revenue Service (surnames of the taxpayers for the province of Florence in 2005) and data from the registry of the professional orders for lawyers, medical doctors and pharmacists, data from the OR.SO. archive for bankers and data from the National Business Register database for goldsmiths.

Table 2. Persistence in families' socioeconomic status

	2011	1427		
	Euros	Modal occupation	Earnings pct.	Wealth pct.
Panel A – first 5 surnames in 2011				
A	146,489	Member of shoemakers' guild	97%	85%
B	94,159	Member of wool guild	67%	74%
C	77,647	Member of silk guild	94%	86%
D	73,185	Messer (lawyer)	94%	85%
E	64,228	Brick layer, sculptor, stone worker	54%	54%
Panel B – last 5 surnames in 2011				
V	9,702	Worker in combing, carding and sorting wool	53%	45%
W	9,486	Worker in combing, carding and sorting wool	41%	50%
X	9,281	Sewer of wool cloth	38%	19%
Y	7,398	Medical doctor	84%	38%
Z	5,945	Member of shoemakers' guild	55%	46%

Source: 1427 Census of Florence and tax records from the Florence statistical office (fiscal year 2011); last two columns report earnings and wealth percentile, respectively. Surnames are not reported for privacy reasons.

Table 3. Earnings and wealth mobility: baseline

Panel A – Dependent variable: log of earnings			
Log of ancestors' earnings	0.039**	0.040**	0.045**
Standardized beta coefficient	*0.084*	*0.070*	*0.077*
	(0.017)	(0.020)	(0.022)
Rank-rank coefficient	0.087**	0.087**	0.091**
	(0.039)	(0.035)	(0.040)
Female	NO	YES	YES
Age and age squared	NO	NO	YES
Observations	806	806	806
R-squared	0.007	0.025	0.048
Panel B – Dependent variable: log of wealth			
Log of ancestors' wealth	0.027***	0.026***	0.018**
Standardized beta coefficient	*0.134*	*0.131*	*0.089*
	(0.008)	(0.008)	(0.008)
Rank-rank coefficient	0.120***	0.118***	0.082***
	(0.039)	(0.039)	(0.038)
Female	NO	YES	YES
Age and age squared	NO	NO	YES
Observations	679	679	679
R-squared	0.018	0.020	0.110

Source: 1427 Census of Florence and tax records from the Florence statistical office (fiscal year 2011). Bootstrapped standard errors in parentheses (1,000 replications); *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Earnings and wealth mobility: transition matrix

Earning classes: Origin ↓ / Destination→	Lower class	Middle class	Upper class
Lower class	32.8	37.1	30.0
Middle class	43.0	29.4	27.6
Upper class	25.4	34.5	40.1
Wealth classes: Origin ↓ / Destination→	Lower class	Middle class	Upper class
Lower class	41.5	29.9	28.6
Middle class	31.2	34.7	34.1
Upper class	27.6	35.4	37.0

Source: 1427 Census of Florence and tax records from the Florence statistical office (fiscal year 2011). The three classes are identified by the 33th and the 66th percentile of the distribution.

Table 5. Robustness related to the characteristics of the surnames

Panel A – Dependent variable: log of earnings				
Log of ancestors' earnings	0.069**	0.050*	0.040*	0.046**
	(0.033)	(0.028)	(0.021)	(0.023)
Controls	YES	YES	YES	YES
Observations	806	806	806	806
R-squared	0.057	0.043	0.052	0.057
Panel B – Dependent variable: log of wealth				
Log of ancestors' wealth	0.021**	0.020**	0.017**	0.017**
	(0.009)	(0.008)	(0.008)	(0.008)
Controls	YES	YES	YES	YES
Observations	679	679	679	679
R-squared	0.127	0.122	0.094	0.116

Source: 1427 Census of Florence and tax records from the Florence statistical office (fiscal year 2011). In column 1 observations are weighted by the inverse of surnames' frequency in 1427; in column 2 observations are weighted by the inverse of surnames' coefficient of variation of the corresponding right-hand side variable in 1427; in column 3 observations are weighted by the Florence-specificity ratio in 2011 (i.e. the ratio between the surname share in Florence and the corresponding figure at the national level); in column 4 the Florence-specificity ratio is included as additional control. Controls include fraction of female and age and age squared. Bootstrapped standard errors in parentheses (1,000 replications); *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Robustness to different frequency thresholds

Panel A – Dependent variable: log of earnings					
Log of ancestors' earnings	0.045**	0.042*	0.043*	0.043*	0.045**
	(0.022)	(0.024)	(0.023)	(0.023)	(0.023)
Controls	YES	YES	YES	YES	YES
Frequencies	>5	>6	>7	>8	>9
Observations	806	769	735	697	669
R-squared	0.048	0.045	0.039	0.036	0.036
Panel B – Dependent variable: log of wealth					
Log of ancestors' wealth	0.018**	0.018**	0.017**	0.017**	0.017**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)
Controls	YES	YES	YES	YES	YES
Frequencies	>5	>6	>7	>8	>9
Observations	679	676	670	659	641
R-squared	0.110	0.110	0.114	0.113	0.116

Source: 1427 Census of Florence and tax records from the Florence statistical office (fiscal year 2011). In each column we restrict the sample imposing increasing (placebo) threshold for the privacy rule. Controls include fraction of female and age and age squared. Bootstrapped standard errors in parentheses (1,000 replications); *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Difference between natives and (Italian) immigrants

Dependent variable:	Individual economic status		Father's socioeconomic status		Intergenerational earnings elasticity	
	Earnings	Wealth	Schooling	Earnings	Earnings	Earnings
Italian immigrant (1)	0.051	0.062	0.188	-0.039		
	(0.061)	(0.233)	(0.523)	(0.039)		
Fathers' earnings (2)					0.457***	0.497***
					(0.088)	(0.105)
(1) × (2)						-0.077
						(0.193)
Controls	YES	YES	YES	YES	YES	YES
Observations	478	470	482	425	422	422
R-squared	0.073	0.045	0.054	0.075	0.131	0.147

Source: Survey of Household Income and Wealth (waves from 1993 to 2014). The sample includes household heads living in the province of Florence. Earnings and wealth are expressed in log, schooling in years. Fathers' earnings are imputed using the TS2SLS approach with fathers' education, occupation and sector of activity as predictors. Italian immigrants are those living in Florence but born in a different province. Controls include wave fixed effects, age and age squared, as well as their interaction with the immigrant status dummy in last column. Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Heckman selection correction for the 2011 population

	Panel A – Dependent variable: log of earnings					
Log of ancestors' earnings	0.045**	0.045**	0.045**	0.045**	0.043**	0.045**
	(0.022)	(0.022)	(0.023)	(0.020)	(0.019)	(0.022)
Correction term	-0.001	0.003	-0.018	0.246***	0.245***	-0.002
	(0.012)	(0.024)	(0.025)	(0.020)	(0.020)	(0.012)
Observations	806	806	806	806	806	806
	Probability of surviving					
Exclusion restriction:	Frequency	Length	Vowels/ consonants	Double letters	Compound surname	All variables
Excluded variable	0.492***	-0.190***	-0.159*	-0.125**	-0.235***	
	(0.036)	(0.020)	(0.094)	(0.048)	(0.075)	
Observations	38,340	38,340	38,340	38,340	38,340	38,340
	Panel B – Dependent variable: log of wealth					
Log of ancestors' wealth	0.018**	0.020***	0.017**	0.015*	0.017**	0.020***
	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)
Correction term	-0.119***	-0.115**	0.425***	0.410***	-0.158	0.107***
	(0.029)	(0.056)	(0.051)	(0.058)	(0.272)	(0.027)
Observations	679	679	679	679	679	679
	Probability of surviving					
Exclusion restriction:	Frequency	Length	Vowels/ consonants	Double letters	Compound surname	All variables
Excluded variable	0.528***	-0.192***	-0.129*	-0.144**	-0.336***	
	(0.039)	(0.020)	(0.069)	(0.062)	(0.108)	
Observations	38,340	38,340	38,340	38,340	38,340	38,340

Source: 1427 Census of Florence and tax records from the Florence statistical office (fiscal year 2011). Each column includes a different exclusion restriction and controls for the fraction of females and age and age squared. The last column includes jointly all the exclusion restrictions. Surviving families refer to surnames that are present both in 1427 Census and in 2011 tax records; the reference population consists of surnames of 2011 taxpayers. Bootstrapped standard errors in parentheses (1,000 replications); *** p<0.01, ** p<0.05, * p<0.1.

Table 9. Earnings and wealth distribution by surviving status

Panel A – Main characteristics by surviving status				
	Surviving surnames	Missing surnames	Difference in means	
Demography:				
Age (years)	46.62	47.29	-0.671	(0.690)
Female (share)	0.162	0.159	0.004	(0.014)
Number of children	3.937	4.009	-0.071	(0.107)
Geography:				
Southern districts (share)	0.406	0.387	0.019	(0.022)
Arno districts (share)	0.333	0.318	0.015	(0.021)
Profession:				
Artisan (share)	0.465	0.345	0.120***	(0.028)
Entrepreneur (share)	0.181	0.220	-0.039*	(0.023)
Government servant (share)	0.050	0.063	-0.014	(0.013)
Lettered profession (share)	0.130	0.136	-0.006	(0.019)
Merchant (share)	0.064	0.099	-0.035**	(0.016)
Unskilled (share)	0.110	0.137	-0.027	(0.019)
Economic outcome:				
Log of earnings (log of florins)	3.416	3.347	0.068**	(0.032)
Log of wealth (log of florins)	4.444	4.594	-0.149	(0.115)

Panel B – Tests for equality of distributions in economic outcomes		
	Kolmogorov-Smirnov	Mann-Whitney
Outcome:		
Log of earnings (log of florins)	0.000	0.003
Log of wealth (log of florins)	0.002	0.016

Source: 1427 Census of Florence. Surviving (missing) families refer to surnames that are present in 1427 Census and (but not) in 2011 tax records. A map of districts is available in the online Appendix A.5. Panel A: standard errors in parenthesis; *** p<0.01, ** p<0.05, * p<0.1. Panel B: each cell reports the p-value of the test for the equality of distributions

Table 10. Selection correction for the 1427 population

	Panel A – Dependent variable: log of earnings					
Log of ancestors' earnings	0.045**	0.045**	0.045**	0.045**	0.040**	0.045**
	(0.022)	(0.022)	(0.023)	(0.020)	(0.020)	(0.023)
Correction term	-0.001	-0.006	-0.021	0.171***	0.161***	-0.001
	(0.006)	(0.018)	(0.014)	(0.016)	(0.016)	(0.006)
Observations	806	806	806	806	806	806
	Probability of surviving					
Exclusion restriction:	Frequency	Length	Vowels/ consonants	Double letters	Compound surname	All variables
Excluded variable	1.256***	-0.275***	-0.219**	-0.170***	-1.712***	
	(0.039)	(0.022)	(0.096)	(0.063)	(0.233)	
Observations	1,992	1,992	1,992	1,992	1,992	1,992
	Panel B – Dependent variable: log of wealth					
Log of ancestors' wealth	0.018**	0.019***	0.017**	0.016*	0.018**	0.018**
	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Correction term	-0.061***	-0.080**	0.280***	0.277***	0.212***	-0.057***
	(0.014)	(0.043)	(0.047)	(0.051)	(0.079)	(0.014)
Observations	679	679	679	679	679	679
	Probability of surviving					
Exclusion restriction:	Frequency	Length	Vowels/ consonants	Double letters	Compound surname	All variables
Excluded variable	1.593***	-0.272***	-0.188**	-0.181**	-1.734***	
	(0.056)	(0.023)	(0.078)	(0.079)	(0.293)	
Observations	1,992	1,992	1,992	1,992	1,992	1,992

Source: 1427 Census of Florence and tax records from the Florence statistical office (fiscal year 2011). Each column includes a different exclusion restriction and controls for the fraction of females and age and age squared. The last column includes jointly all the exclusion restrictions. Surviving families refer to surnames that are present both in 1427 Census and in 2011 tax records; the reference population consists of surnames in 1427 Census. Bootstrapped standard errors in parentheses (1,000 replications); *** p<0.01, ** p<0.05, * p<0.1.

Table 11. Robustness with respect to selection *within* surnames

	Panel A – Dependent variable: log of earnings			
Log of ancestors' earnings	0.038**	0.043*	0.046**	0.045**
	(0.019)	(0.022)	(0.023)	(0.023)
Controls	YES	YES	YES	YES
Observations	806	806	806	806
R-squared	0.049	0.048	0.049	0.048
	Panel B – Dependent variable: log of wealth			
Log of ancestors' wealth	0.016**	0.018**	0.016**	0.016**
	(0.007)	(0.008)	(0.008)	(0.008)
Controls	YES	YES	YES	YES
Observations	679	679	679	679
R-squared	0.111	0.109	0.109	0.109

Source: 1427 Census of Florence and tax records from the Florence statistical office (fiscal year 2011). In column 1 ancestors' earnings and wealth are imputed using the median within surname in 1427; in column 2 (3) we exclude the bottom (top) 10% of individuals – in terms of income or wealth – within each surname in 1427; in column 4 we exclude jointly the top and the bottom 10% of individuals – in terms of income or wealth – within each surname in 1427. Controls include fraction of females and age and age squared. Bootstrapped standard errors in parentheses (1,000 replications); *** p<0.01, ** p<0.05, * p<0.1.

Table 12. Probability to belong to a given occupation

Dependent variable:	Lawyer	Banker	Doctor or pharmacist	Goldsmith
Share of ancestors in the same occupation	0.004***	0.001**	0.001	0.004***
	(0.001)	(0.000)	(0.002)	(0.001)
Observations	133,193	133,193	133,193	133,193
R-squared	0.000	0.000	0.000	0.000

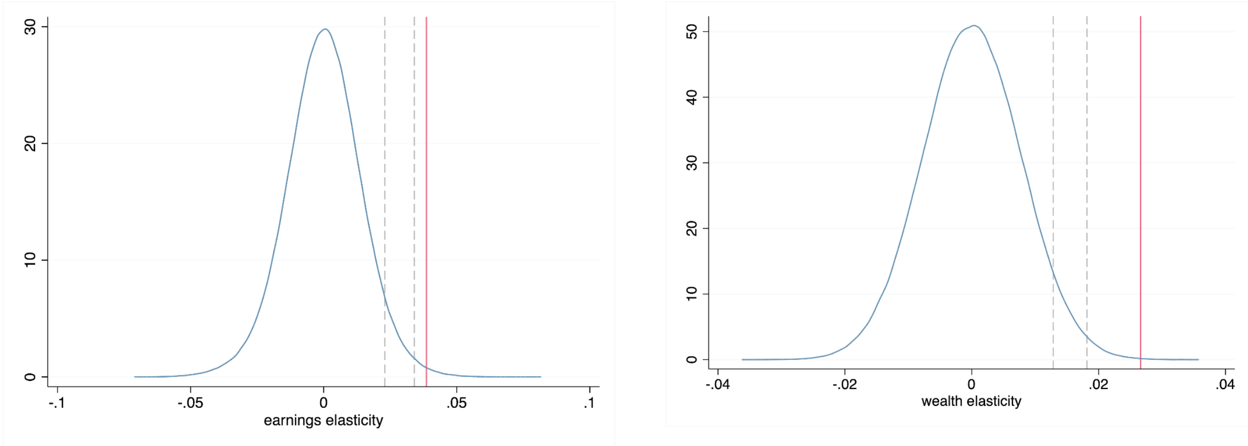
Source: 1427 Census of Florence and tax records on surnames in the province of Florence in 2005 jointly with various sources reporting the surnames in each occupation nowadays. Marginal effects from a probit model are reported. Standard errors clustered at the surname level in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 13. Reconciling long- and short- run evidence

	AR(1) process	AR(2) process
Panel A: 19 generations		
Constant IGE		
All generations	0.85	0.61
Variable IGE		
1st – 15th generations	0.91	0.63
16th – 18th generations	0.71	0.57
19th generation	0.50	0.50
Panel B: 23 generations		
Constant IGE		
All generations	0.87	0.63
Variable IGE		
1st – 18th generations	0.94	0.71
19th – 22th generations	0.72	0.61
23th generation	0.50	0.50

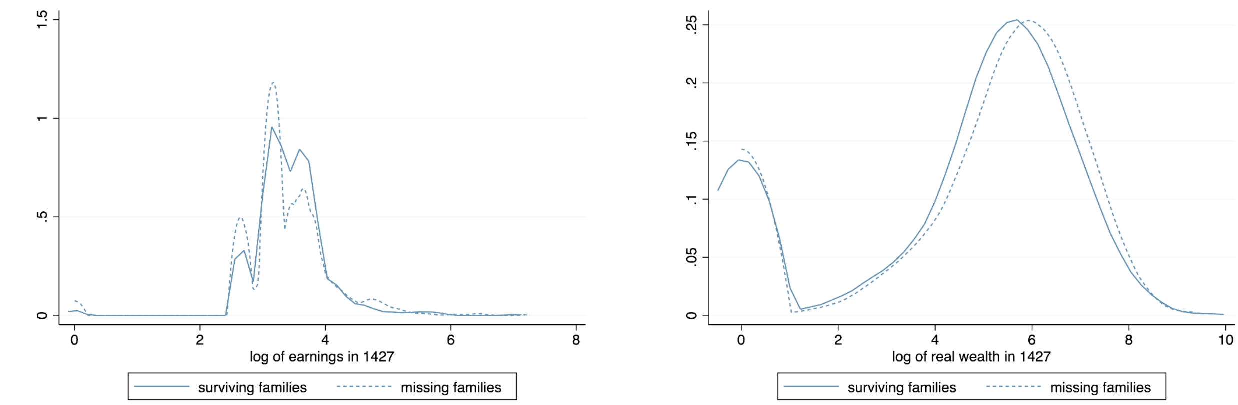
Each entry represents the average intergenerational income elasticity (IGE) that is consistent with a long run value equal to 0.045, under different assumptions. For all entries we assume that our time span covers 19 generations (about 30 years per generation) in Panel A and 23 generations (about 25 years per generation) in Panel B. Grandfather's IGE in the AR(2) process is assumed to be 1/3 of father's one.

Figure 1. Earnings and wealth mobility with randomly assigned surnames



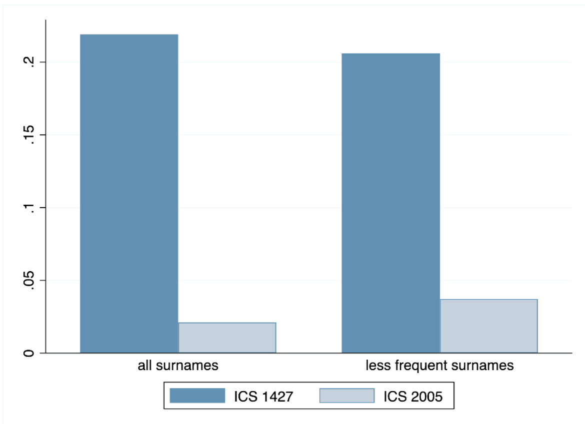
The left (right) panel shows the distribution of estimated earnings (wealth) elasticity randomly matching ancestors' and descendants' earnings (wealth); dashed lines represent 95° and 99° percentile, solid line represents the earnings (wealth) elasticity properly matching ancestors and descendants through surnames.

Figure 2. Earnings and real wealth distribution by survival rate



Authors' elaborations on data from 1427 Census of Florence. Left (right) panel refers to earnings (wealth).

Figure 3. Income persistence in Florence: 1427 vs. 2005



Histograms represent the intergenerational immobility measured by ICS as in Güell et al. (2015); figures for 1427 are estimated from the 1427 Census of Florence and refer to total income; figures for mid-2000s are from Güell et al. (2018a) and refer to taxable income.