Up from Poverty?

The 1832 Cherokee Land Lottery and the Long-run Distribution of Wealth[*]

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Abstract

The state of Georgia allocated most of its land through lotteries, providing unusual opportunities to assess the long-term impact of large shocks to wealth, as winning was uncorrelated with individual characteristics and participation was nearly universal among the eligible population of adult white male Georgians. We use one of these episodes to examine the idea that the lower tail of the wealth distribution reflects in part a wealth-based poverty trap because of limited access to capital. Using wealth measured in the 1850 Census manuscripts, we follow up on a sample of men eligible to win in the 1832 Cherokee Land Lottery. We assess the impact of lottery winning on the distribution of wealth 18 years after the fact. Winners are on average richer (by an amount close to the median of 1850 wealth), but mainly due to a (net) shifting of mass from the middle to the upper tail of the wealth distribution. The lower tail is largely unaffected. This is inconsistent with the prediction of an asset-based poverty trap, but is consistent with heterogeneity in characteristics associated with what wealth would have been absent treatment.

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1 Introduction

Wealth disparities interest researchers and policymakers because of concern for the plight of the poor and societal preferences for less inequality of outcomes. Although transfers to equalize outcomes can dull the incentives for productive activity, transfers might improve efficiency if so-called poverty traps prevent the poor from making even very high-return investments. In such cases, unequal circumstances in the past can create inequality of opportunity in the future.

Yet we seldom observe transfers in settings where their long-term effect on the distribution of wealth can be properly assessed. Analysis of the distributional effect of transfers depends both on the measurement of wealth and on credibly exogenous variation in transfers, which are usually an endogenous response to an individual's misfortune. Also, a long follow-up period is necessary if the short-run effects of transfers, which change the wealth distribution purely as a matter of accounting, are to be distinguished from their more persistent effects. Such longer-run effects could either amplify or attenuate the initial transfer, depending on the underlying causes of the initial wealth distribution.

Consider a poverty trap that arises with a limited ability to borrow (thus entrepreneurs can only make investments with their own wealth) and a fixed cost of production (thus entrepreneurs with zero wealth cannot grow incrementally by investing retained profits). Those with low wealth could get stuck in such a poverty trap, which imparts extra persistence to the path of inequality (see Banerjee and Newman, 1993, and Buera and Shin, 2013, for example). An implication of these models is that perturbations of the wealth distribution, particularly when pushing up from poverty, should be highly persistent and perhaps even with a positive multiplier that amplifies the initial shock. The relevance of such a poverty trap in understanding the wealth distribution is a question of interest both in contemporary developing economies and in the historical evolution of today's developed nations.[1] Perhaps paradoxically, a constant lump-sum grant to everyone could actually compress the distribution of wealth (in levels) because the added wealth unlocks high-return investments among those with low wealth at the outset. This thought experiment of dumping wealth on individuals and then examining the later wealth distribution informs our empirical analysis in present study.

We analyze a large-scale lottery to consider the effect of a random disbursement of wealth on the wealth distribution in the long run. Participation was nearly universal, unlike other studies of lotteries whose participants are a selective subset of the population. The prize in this lottery

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[1]See, for example, Carter and Barrett, 2006, and McKenzie and Woodruff, 2006, for empirical studies of asset-based poverty traps in developing countries. Fogel and Engerman, 1973, and Wright, 1979, discuss this issue as a possible cause of wealth inequality in the antebellum Southern United States.

was a claim on a parcel of land. The average value of such parcels was large—comparable to the median level of wealth at the time. Winning in the lottery was close to a pure wealth shock: there were no strings attached to the land (such as a homesteading requirement) and the claim could be liquidated immediately. In addition, we consider a historical episode, which allows us to retrospectively examine the distributional effect in the long run, almost two decades after the lottery took place.

Specifically, we investigate the aftermath of the 1832 Cherokee Land Lottery in the US state of Georgia. In the early 19th century, Georgia opened almost three-quarters of its total land area to white[2] settlers in a series of lotteries. In the history of land opening, this was an unusual allocation method, chosen in large measure for its sheer transparency in the wake of several tumultuous corruption scandals in Georgia in the 1790s. We conduct a follow-up on these random wealth shocks using a sample of over 14,000 men eligible to win land in that lottery. To ascertain the long-term effect on the wealth distribution, we transcribe information on wealth from the 1850 Census manuscripts, measured 18 years after the lottery. The two measures of wealth available in the 1850 Census are real-estate and slave holdings. From this sample of eligibles, we identify winners using a list published by the state of Georgia (Smith, 1838). Those identified in the Smith list comprise the treatment group, and the lottery eligibles that were not linked to the Smith list serve as a control group. While, in theory, not all of the men in our sample of 'eligibles' were technically eligible to win the lottery, our analysis in Section 4 suggests this was a minor subset in practice. Further, in our sample, lottery losers look similar to lottery winners in a series of placebo checks found in Section 4 and 5.5.

As a point of departure, consider first the mechanical effect on the wealth distribution of randomly assigned wealth. If everyone receives (and holds on to) the same dollar amount, this simply shifts the entire distribution of wealth, in levels, to the right by that same amount. It is nevertheless common to treat the wealth distribution in natural logarithms, which would be strongly compressed in such a circumstance. (A wealth shock of a given size represents a much larger fraction at the lower tail of the distribution.) If instead there are much higher returns to capital at the low-end, as argued by some,[3] then a constant-level disbursement would compress the distribution both in levels and, to a greater degree, in logs. A complication, however, comes from the heterogeneity in quality for the lotteried parcels. While this would increase the variance of the treatment wealth distribution relative to control, it would still have the effect of draining mass out of the lower tail

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[2]Slaves and free people of color were excluded from the lottery, as were Native Americans. Indeed, while the present study is focused on distributional changes for white men in Georgia eligible for the lottery, it bears mentioning that the land was expropriated (i.e., redistributed) from the Cherokees, who were subsequently expelled from northwest Georgia in a forced march known as the Trail of Tears.

[3]We review related literature in Section 2.

by the random nature of the lottery, as long as the value of winning the lottery was positive. These cases provide a point of comparison for the empirical results, discussed next.

Almost two decades after the lottery, winners were, on average, $700 richer[4] than a comparable population that did not win the lottery. The gains in wealth, however, are not evenly distributed among the lottery winners. Indeed, the poorest third of lottery winners were essentially as poor as the poorest third of lottery losers. Rather, the gains from lottery winning are almost entirely seen as a (net) shifting of mass from the middle of the wealth distribution to the upper tail. The lower tail is largely unaffected. Therefore this wealth shock tended to exacerbate inequality (at least when considering the poor versus the rest) rather than reduce it. These results are found in Section 5, where we compare the probability density functions (PDFs) and cumulative density functions (CDFs) of control and treatment wealth distributions. Further, in Section 5, we use a quantile-regression estimator to show that winning the lottery affects wealth mostly in the upper half of the distribution.[5] We also show that these results are robust to controlling for various factors, including characteristics of the person's name. The latter strengthens the earlier conclusions in that, although we used the name to link to the list of winners, it did not appear to bias our estimate of the treatment effect.

Whether the wealth transfer actually caused an aggregate improvement for the treated depends on one's taste for equity.[6] Various measures of inequality, such as the Gini coefficient or the standard deviation of log wealth, are higher in the treatment group than in the controls. We use a constant-elasticity-of-substitution aggregator, bootstrapped over both groups, to ask whether the treatment distribution shows a statistically distinguishable improvement over the control group under different preferences about the size of the pie versus how it is sliced. For very large elasticities of substitution (and correspondingly low weights on equity), the treatment group has a significantly higher aggregate outcome than the control group. But we cannot reject equality of outcomes for

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[4]Dollar figures reported in the study are in 1850 dollars unless otherwise specified. We suggest a few different ways to contextualize this number. First, as stated above, this is approximately equal to the median of wealth in our sample. If instead we convert this number to 2010 values using consumer prices, it is approximately $20,200. In contrast, it would convert to $142,000 in 2010 if adjusted by the relative value of the unskilled wage. (This latter figure translates to over ten years of earnings at the 2010 federal minimum wage of $7.25 per hour for a full-time/full-year worker.) These conversion factors come from MeasuringWorth.com (Williamson, 2013).

[5]Absent the property of rank invariance across the distributions of potential outcomes, we cannot literally interpret these effects as the treatment effects at a given point in the control distribution, in that these results could have arisen through more complicated patterns of reshuffling from control to treatment. For example, all of the would-have-been-poor could have become rich and an equal number of the would-have-been-rich could have become poor as a consequence of treatment.

[6]For the purposes of the present study, we set aside issues of broader efficiency. The efficiency loss associated with the lottery could be twofold. First, by not selling the land at its market value, the state of Georgia was foregoing revenues that then would have to be raised from more distortionary taxes. Second, opening the land through a lottery was a peculiar form of "market design" that appeared to constrain land use well into the 20th century. This latter issue is discussed in detail in Weiman (1991) and Bleakley and Ferrie (2013a).

elasticities of substitution much below one, a far cry from a Rawlsian elasticity of zero in which social welfare depends exclusively on the outcome of the lowest-ranked individual.

In Section 6, we ask why we fail to observe the footprint of an asset-based poverty trap—in which the strongest effects of a positive wealth shock should come from the lower portion of the wealth distribution. The possibility of negative selection into treatment should be ruled out by the random nature of the lottery. Further, it is likely that there were indeed important fixed costs and/or minimum-input requirements in this economy that could generate a poverty trap. Subsistence constraints would have made it difficult to gradually improve land on the frontier, for example. Nevertheless, the winnings from the lottery should have been more than enough to eject a winning landless laborer well into the distribution of existing farms; the expected value of winning was around $700, close to the peak of the bell curve of (log) asset holdings. But we fail to find evidence of particularly strong returns from treatment at the low end, and in fact find quite the opposite: an apparently complete dissipation of winnings among those in that range of the wealth distribution.

Rather than invoking inflections of the production function and the asset-based poverty traps they can create, an explanation that better fits the results includes heterogeneity (across the counterfactual wealth distribution) in the characteristics that permit one to hold onto wealth.[7] This heterogeneity could have taken numerous forms. These sources of heterogeneity might also have resulted in a position low in the wealth distribution even in the absence of treatment. Heterogeneity consistent with the absence of a large, positive effect of winning on wealth in the lower tail includes a strong bias towards early consumption (either through very high discount rates or self-control issues),[8] a lack of skill needed to manage a complex venture like a farm, or some tendency to inefficient (read 'reckless') risk-taking.[9] Sorting out which of these is the main source of heterogeneity is beyond the scope of the paper (and no doubt each of these mechanisms is operative to some degree). Nevertheless, the evidence is more consistent with the wealth dissipation in the lower tail coming

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[7]We say "hold onto" here because, across the 1850 wealth distribution, there was a roughly constant rate of claiming land by lottery winners; therefore, those who ended up poor did not do so because they disproportionately failed to collect their winnings at the outset. See Section 6.3.1.

[8]One minor complication is that individuals especially ones with high discount rates may have begun to consume down some of their winnings for lifecycle reasons. But note that, if someone who appears to be in the poverty trap discounts the future heavily enough, there is no trap from his perspective; the very large returns are still not large enough to justify delaying gratification. In any case, almost all of our sample is young enough to be in the ages in which a typical person is still accumulating assets, presumably for later-life consumption or bequests. By 1850, the men in our sample could have expected at least another 20 more years of life. Further, the results below are not sensitive to our accounting for differential fertility or for inter vivos transfers to their children. Nor were lottery winners who wound up in the lower tail less likely to go out to the high-growth frontier (thereby avoiding the hard work of land improvement). This analysis is seen in Section 6.

[9]The risk would have to have been substantially above and beyond what we observe among those who got their wealth non-randomly. We measure the degree of wealth churn (and risk of total loss) in see Section 6.2.

from an inequality of skills, including what James Heckman and others call non-cognitive skills such as the ability to delay gratification and/or avoid obviously bad decisions, and less consistent with the lower tail emerging from an asset-based poverty trap.

Section 7 concludes the study.

2 Related Literature

The condition of the small entrepreneur is a topic that has received attention across a wide variety of contexts and disciplines. We cannot hope to give a proper survey here, so instead in this section we touch on a few relevant examples from various perspectives. For starters, recall that Thomas Jefferson and his later intellectual disciples argued for policies that would encourage yeomen farming (i.e., small-scale and owner-operated farms) rather than large estates or urban factories, both employing landless laborers.[10] This view gave rise to land policies in the 19th century US that distributed small landholdings on the frontier at low, often below-market, prices.

While the intent of the policy was to establish the dominance of small farming, the extent to which it did so may have been limited by other factors. Indeed, Gates (1996) argues the so-called free-land[11] policy was ineffective, because small-scale settlers were often capital constrained and probably were outbid, outmaneuvered, or bought out by those he called "frontier estate builders" (chapter 2 title, on page 23). Indeed, Atack (1988) shows that rates of landlessness among agricultural workers in the Midwest were at similar levels in 1860 (when ostensibly free land was still available on the frontier) and in 1880 (when the frontier was closing). To some extent this is a puzzle; giving free land in relatively small parcels to individuals will have the mechanical effect of compressing the logarithmic land-wealth distribution in the short-run. The question remains, however, whether this compression will persist, or whether it will be unwound by some other feature of the economic environment that makes it difficult for (some) farmers to operate at such a small scale.

In the historical U.S. South, these issues of scale and inequality are particularly stark. The South by the first half of the nineteenth century was characterized by a distribution of farm sizes with less mass in the middle than in other farming regions of the U.S. There were plantations oriented toward market production that covered in some cases thousands of acres and employed

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[10]See, for instance, Jefferson's *Notes on the State of Virginia*, in response to "Query 19", at http://etext.virginia.edu/toc/modeng/public/JefVirg.html.

[11]Calling the land "free" was perhaps more a political slogan then a statement about its price or its value. The Homestead Acts, for example, effectively rationed small parcels to people willing to invest several years in improving and farming them. It would be a mistake to assume that land obtained through this process was not of productive value, even if labeled by politicians as "free."

large numbers of slaves and small family farms producing very small marketable surpluses and oriented mainly toward meeting subsistence needs. But the middle of the distribution was thinner, especially when compared with the Midwest, where the class of yeoman farmers were so prominent. One explanation for this pattern comes from an older literature on antebellum Southern agriculture (discussed by Fogel and Engerman, 1977, and Wright, 1979) which asserts that the presence of large farming units had "privileged access to capital" (Wright, p.63), which thus prevented the growth of small farms into intermediate-sized farms.[12] Another explanation is advanced by Fogel and Engerman (1977), who acknowledge the possibility that small farmers faced such constraints, but instead emphasize the scale economies in employing slaves under the "gang system" (with regimented passage of entire groups of slaves through fields in cultivating and harvesting).

More recently, de Soto (1987) and Khanna (2007) highlight the ubiquity of small-scale entrepreneurs in developing economies, often hidden in the informal sector. De Soto (2002) also presents parallels between the antebellum US and developing economies today. While his central thesis is that economic development is (or was) held back by conflicting property rights, the undergirding theme is that capital markets fail(ed) to direct resources to a large class of small entrepreneurs (including small farmers), who could otherwise make productive use of such capital.

This line of thinking is related to the notion of a "poverty trap," which appears in a wide range of theoretical papers. There are many possible motivations for the existence of such traps; perhaps the easiest one to think about is a simple fixed cost of production. An example from this theoretical literature is by Banerjee and Newman (1993) who, using a model with a poverty trap to analyze occupational choice (being a laborer versus a self-employed entrepreneur versus an employer), demonstrate the possibility of multiple steady-states for the wealth distribution. In this and related models, the wealth distribution is the state variable and can be highly persistent, even to the point of path dependence.

But are such poverty traps empirically relevant for small entrepreneurs in developing economies? In principle, one could take detailed measurements of the production function, although demonstrating the poverty trap requires precise evidence on the third derivative of the production function.[13] An alternative approach would be to randomly disburse capital to entrepreneurs and attempt to

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[12]Evidence presented by Wright and Kunreuther (1975) on the crop mix choices of small Southern farmers is consistent with those farmers facing a binding liquidity constraint—they produced a mix of corn and cotton more in line with a desire to satisfy a subsistence constraint (imposed by an inability to borrow in the short run) than a desire to maximize expected long-run income. It is a short leap from a liquidity constraint on year-to-year borrowing to a capital constraint that effectively barred longer-run investments such as those needed to expand the farm's size. Indeed, according to Govan (1978, p.202), long-term credit in antebellum Georgia was very limited in that the "chief function of the banks was to furnish credit for mercantile operations, and to supply a medium through which payments could be made in distant places at a minimum of risk and expense."

[13]McKenzie and Woodruff (2006), for example, study nonconvexities induced by fixed costs.

measure how this changes the distribution of their outcomes. This is the approach of the present study. Perhaps the most closely related work is by de Mel, McKenzie, and Woodruff (2008, 2012), who examine the impact on profitability of randomly assigned capital grants to a sample of self-employed in Sri Lanka. They find large effects on the profitability of microenterprises in the short and medium runs. (In addition to the obvious difference in location and time period, we note that their grants were approximately one month worth of unskilled wages rather than almost a decade as was the case for winnings in the Cherokee Land Lottery.) More recently, Blattman, Fiala, and Martinez (2013) follow up several years after an unconditional cash transfer (of approximately one year's wages) to find higher earnings and labor supply.

Risk is another central feature of the environment that an entrepreneur faces, perhaps to an even greater degree at a small scale of operation. Banerjee and Duflo (2011) argue that there is "so much risk in the everyday lives of the poor [...] that, somewhat paradoxically, events that are perceived to be cataclysmic in rich countries often seem to barely register with them (page 136)." They provide some illustrative anecdotes of such risk.[14] Note that high returns can exist in the short run perhaps as compensation for high risk that becomes more evident at longer horizons. Thus supernormal returns in the short run are not necessarily an indication of a binding capital constraint. Instead, it might indicate a failure of diversification, a distortion that can itself hold back economic development, as in the model of Acemoglu and Zilibotti (1997). Relatedly, Rosenzweig and Binswanger (1993) study how farmers in India change their crop mix if they face greater weather risk and Karlan, Osei, Osei-Akoto, and Udry (2012) show how agricultural decisions change with the provision of insurance for small farmers in Ghana. Wright (1979) argued that small Antebellum Southern farmers practiced "safety first" farming because their risk exposure was so great. Ransom (2005) labelled the Antebellum period as "the era of walk-away farming," in which small farmers could cope with bad shocks by simply abandoning their land (and presumably their debts as well). In contrast, wealthier farmers were better equipped to self-insure and thus not be obliged to abandon their wealth in response to a transitory negative shock.

Heterogeneity in returns might also arise for reasons that do not bring the specter of inefficiency. Consider Schultz's (1975) argument that ability or human capital helps one take advantage of new opportunities. Indeed, a basic notion of economics is that factors of production should gravitate to their highest valued use. If the experimentalist somehow manages to perturb the distribution of factors away from the baseline, this logic suggests that we should expect a reduction in average returns. It is likely that skill and wealth are complementary, and furthermore that at least some at

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[14]Richer detail, albeit from nonacademic sources, is presented by the journalist Boo (2012), who relates some of the difficult shocks endured by several families in an informal settlement in Mumbai, and by Wilder (1971), who details her own experience as a young mother on the 19th-century US frontier.

the bottom of the (treatment or counterfactual) wealth distribution were there precisely because they lacked the ability to seize opportunities such as winning the lottery.

Finally, there is earlier work that also analyzes the wealth shock coming from lottery winnings. Imbens, Rubin, and Sacerdote (2001) follow up on the consumption behavior of people who had won large jackpots in state-run lotteries in Massachusetts. Hankins, Hoekstra, and Skiba (2010) examine medium-sized jackpots in the Florida lottery and relate this to bankruptcy filings over the following several years. Both of these studies are strongly related to the present one by using lotteries to analyze wealth, although neither considers a developing-economy context and in neither case is the sample size large enough to permit the distributional analysis that we conduct below. Further, a perennial concern about examining the shock from gambling winnings is that one can only analyze the effect on gamblers, who are typically a highly selected population. As we discuss below, participation in the 1832 lottery was so widespread (at least, among white adult men resident in Georgia circa 1830) that this selection issue is less important in our case.

3 The Cherokee Land Lottery of 1832

The state of Georgia is quite unusual in the U.S. in that much of the state's territory was distributed through a series of land lotteries. The initial Georgia colony was concentrated around the Savannah River, and this land was distributed through a more traditional grant-based system. However, a corruption scandal in the 1790s (the Yazoo Land Fraud) provoked such popular outrage that the Georgia Legislature opted to use lotteries as methods of distributing land from then forward. The first lottery took place in 1805 and the last ones were held in the early 1830s.

For this study we consider the 1832 lottery of Cherokee County in northwest Georgia. We choose to focus on the 1832 lottery because the list of winners was available and the later date increases the chance of tracking these people in census data. The land in this area was made available to white settlers by the eviction of the Cherokee from that area.

Essentially every adult male residing in Georgia for the three years leading up to 1832 was eligible to one draw in this lottery. Widows, orphans, and certain veterans were eligible for two draws. (Because we would not know in the control group who was a widow, orphan, or veteran, we exclude them from the treated group in our analysis. Practically speaking, this is of little consequence because our sample excludes females and excludes years of birth that the veterans or orphans would disproportionately populate.) A group of highwaymen called the "Pony Club" that operated in old Cherokee County was also explicitly excluded from the lottery, but this group was trivially small compared to the population of the state. In theory, winners in previous lottery

waves were excluded from participating, and there was also a 12.5¢ registration fee. It is not immediately evident the extent to which either of these was enforced, but the numbers suggest that neither was much of an impediment to participating. We do not know the exact population in late 1832 of white men meeting the requirements for age (18+) and residency (3+ years in Georgia), but the 1830 Census reports the white male population of Georgia ages 15 and over in 1830 as approximately 80,000. There were close to 15,000 winners (excluding widows and orphans) in the 1832 Land Lottery (Smith, 1838), which implies a winning rate of around 19%.[15] Lists of the eligible population were constructed by each county government and forwarded to the state capital in Milledgeville.

Concurrent with this, the area known at the time as Cherokee County was divided into four sections, which were further subdivided into dozens of districts. The districts were generally square, except for those that were on the boundaries of the original Cherokee County, which were defined by the state border to the north and west, and by the Chattahoochee River to the southeast. Surveyors were sent to each district with the aim of further subdividing it into an 18×18 grid of square parcels of 160 acres each.

After the surveys were completed and the lists of eligibles were collected, the lottery began. The drawing proceeded as follows. One drum was filled with slips of paper containing the registration information on each eligible person. Another drum was filled with slips of paper specifying a parcel. Blank slips were added to the parcel barrel to equalize the number of pieces of paper in each barrel. A slip of paper was drawn simultaneously from each barrel to determine who had won which parcel. (Thus, lottery losers were those matched to a blank piece of paper.) This implies that winning and losing was assigned randomly, and also that the specific parcel awarded to an individual, even conditional on winning, was random.[16] Over 18,000 parcels were assigned in this manner.

Very few requirements were imposed on the winners of the lottery. They were not required to homestead the parcel for any amount of time. They were not even required to set foot on their parcel. They simply had to register their claim with the state government and pay a nominal fee

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[15]Cadle (1991, page 278) reports that the total number of registrants was around 85,000, but does not give the breakdown by single- versus double-draw categories. We use the distribution of single-draw and double-draw winners in the Smith (1838) book to infer this breakdown, and compute that approximately 75,822 registered for the single draws. The 1830 Census reports 77,968 white men aged 15 and older in Georgia in 1830. Comparison of these numbers indicates that around 97.2% of group in 1830 indeed registered for the lottery. The remaining 2.8% might easily be explained by a combination of mortality or emigration between 1830 and 1832, in-migration to Georgia between 1829 and summer 1830 (thus missing the full three years of the residency requirement), and that few of the 15-year-olds on June 1 of 1830 would have attained 18 years of age by the fall of 1832, when the drawing was held.

[16]Weiman, 1991, argues that the lottery's outcomes appeared approximately random. Both barrels were rolled around to ensure adequate mixing (or proper randomization, in today's parlance). The blank slips of paper further increased the transparency of the process; it was thus more difficult to increase your odds by excluding other names from ever making it into the barrel. There were a few instances of corruption after the fact that were easily discovered by virtue of the transparent nature of the lottery (Cadle, 1991).

($18). If they wished, they could immediately resell title to that parcel. Indeed, it is likely that many of the winners took this route. One factor that made this sort of "flipping" attractive is that it took six years before the state of Georgia could effectively exercise its jurisdiction over this land. The Cherokee nation fought the eviction through the legal system, and the state of Georgia was not able to evict the Cherokees until 1838. Information on the parcels as well as a list of winners was circulated throughout the state and compiled into a single source by Smith (1838).

A rough measure of the value of a winning draw in the lottery can be obtained by calculating the average value of a farm in the 10 counties of Northwest Georgia in 1850, when the U.S. Census first provides the information necessary to make this calculation. These counties (Cass, Chattooga, Cherokee, Dade, Floyd, Gilmer, Gordon, Murray, Union, and Walker) contained 1.289 million acres of farmland (improved and unimproved); the 6,193 farms in these counties had a total cash value of $8.566 million (1850 dollars), of which $357,000 was implements and machinery.[17] Tostlebe (1957, p. 179) suggests that improved land was three times as valuable as unimproved land in the humid states (apart from the Great Plains, Iowa and Illinois where the mark-up was 1.5 owing to the lower cost of clearing land in these states). If we use the 3-to-1 mark-up for 1850, improved acres were worth $12.45 and unimproved acres were worth $4.15, so an unimproved 160-acre plot was worth $664 in 1850. If winners improved their land at the average for these ten counties (27 percent), a 160-acre farm would have been valued at $1,048 in 1850.[18]

Not all of the land in these counties was in farms in 1850, however: 61 percent of the 3.303 million total acres in these ten counties do not appear in the census agricultural schedules. Part of this discrepancy results from non-farm land uses: pine forest that was not used as part of an active farm, town lots, roads, bridges, ferries, and mills. The first of these – pine forest – accounts for the vast majority of the unfarmed land. Another component of the 61 percent discrepancy between total and farmed land is farms that are missing from the agricultural schedules because the farm household was missed entirely by the census. Hacker (2013, Table 4) estimates that 6 percent of white males born in the South were missed in the 1850 census (the number of adults missed will be below this as the total is skewed by a 19.7 rate for age 0-4). Finally, some farms would also have been missed in the agricultural schedules if they were owned by farmers who resided outside the county and were not present when the census marshal visited.[19]

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[17]Numbers for this calculation are reported in ICPSR study 2896 (Haines, 2010). Georgia did not move to an *ad valorem* land tax until 1852 and few of the county tax digests are easily accessible until 1890, so it is not possible to use actual assessment records to recover the value of land as reported by county tax collectors. The figures reported here are based on the Agricultural Schedules of the 1850 U.S. Census of Population.

[18]In 1832, the only improved land in these 10 counties was the 19,320 acres cultivated by the Cherokee. (Wishart, 1995, Table 1, p. 125), or just 1.3% of the total land in farms by 1850. If we assume each farm was only 1.3% improved (rather than 27% improved), a farm was worth $681 in 1850.

[19]For example, in 1851, Christopher Chaney resided in Militia District 583 Appling County, Georgia where he

The 1832 lottery exhaustively partitioned the area that was distributed, so the non-farmed areas must be accounted for and assigned a value in estimating the value of a 160-acre plot. If we assume that the 5 percent of farms that were missed entirely by the census were otherwise identical to the farms included, and that another 5 percent of otherwise identical farms were held by residents outside the ten-county area who farmed the land themselves, the fraction of non-farmed land falls to 51 percent. But this is too high a fraction to which we should assign a zero value, for two reasons: first, land in non-farm uses could have had a positive value (e.g. pine forest adjacent to water or roads that could be used to transport timber to market); and second, the trend from 1850 to 1860 was for an increasing fraction of each county's land to be farmed, suggesting that some land counted as non-farmed in 1850 was in fact farmable but simply had not yet been occupied or had not yet been incorporated into existing farms.[20] This fraction rose roughly 20 percentage points in the counties for which we have comparable data in 1850 and 1860. A conservative estimate of the fraction of zero-value land in these ten counties is therefore no more than 30 percent.[21]

This allows us to estimate the expected 1850 value of a 160-acre plot (which will now comprise 112 acres of positive-value land) won in the 1832 lottery: $464 if completely unimproved and $716 if 27 percent improved. Using the GDP deflator and ignoring capital gains, these values correspond to $375 and $579 in 1832. One measure of capital gains is the New York price for raw cotton, which averaged 10.3¢ per pound 1831-33 and 10.7¢ per pound in 1849-51 (Historical Statistics of the U.S., 2006, Series Cc222). Taking account of this small trend would further reduce the 1832 value of 160 acres only slightly.

An additional measure of the value of land comes from the neighboring counties of Carroll, Coweta, Muscogee, and Troup, which were opened up in the lottery of 1827. Unlike in 1832, fractional parcels (produced in large measure by surveying accidents) were withheld from the 1827 lottery and sold instead at auction. These auctions did not use reserve prices, and therefore the full distribution of prices can be found in the auction records. Weiman (1991, page 845) reports the mean land value per acre ($2.19) in the auction, which translates into $350.40 for a 160-acre

appears in that county's tax records for that year. He is reported to own 160 acres of land in Cherokee County (in Section 2, District 251, Number 5), in addition to his landholdings in Appling County. (Ancestry.com 2011) In the 1850 agricultural schedules for Georgia, Chaney appears only in Appling County, and the acreage he reports (490 acres) is equal to his holdings in Appling County only. (Ancestry.com, 2010) The Cherokee County land would have been missed entirely if Chaney was farming the land himself and had no operator who could report the farm's characteristics to the census marshal.

[20]Bode and Ginter (1986, p. 59, Map 1) show higher ratios of farm acres to total acres for 1860 than we have calculated for every county in the lottery zone. For example, we find that 42 percent of the total area of Chattooga and Floyd Counties was in farms in 1850, but Bode and Ginter find 80 percent of the total area in these two counties was in farms in 1860.

[21]Banks (1905, p. 19) suggests that no more than 25 percent of the plots distributed in Georgia's land lotteries eventually went unclaimed and reverted to the state. Even the unclaimed parcels would have had some value, and many were sold subsequently by state-run auctions.

plot circa 1827. If we adjust this for the 6.5% higher farm values in 1850 for Old Cherokee County versus the four counties just considered, the estimated value of 160 acres rises to $373.18.[22]

If they sold the plot before 1850 and bought land with a similar net present value (NPV), we would expect the same. These effects might be attenuated, however: wealth could be held in other forms, e.g., slaves, which we observe by linking to the 1850 Slave Schedule) or financial assets (very rare, except for the wealthiest); or wealth could be consumed (in a variety of forms: direct consumption goods or larger family sizes). Additionally, those who flipped the land quickly may have received less than the land's NPV because of uncertainty about the exact timing of the expulsion of the Cherokee. There should have been little doubt about their eventual eviction, however. The Indian Removal Act was passed in 1830, and had been applied several times already in the region.

Roughly the bottom third of Cherokee County was distributed in 40-acre parcels as part of a separate lottery (called the "Gold Lottery"). It was thought that this area was particularly rich in gold deposits, an assumption that proved to be overly optimistic. (For this study, we examine only winners in the Land Lottery section of old Cherokee County.

4 Data

4.1 Sources and Construction

The present study follows up on the outcomes of lottery winners and losers. There are two principal ingredients to this exercise. First, we need to identify who was eligible, and who won. Second, we need to find these individuals in later, publicly available data sources, so as to follow up on their outcomes. For the most part, we search for these individuals in the Census manuscripts of 1850 using a preliminary version of the full-count file for the 1850 Population Census from the IPUMS project, indexed and scanned images of the 1850 manuscript pages from Ancestry.com, and an index of the 1850 Slave Schedule on Ancestry.com.

The original source for the names of lottery winners in the 1832 Georgia land lottery is Smith (1838). He lists, in numerical order, each parcel that was available and the associated lottery winner, along with the winner's county and minor civil division in 1832. Smith's list was partially transcribed and available on accessgenealogy.com, which we downloaded, cleaned, and compared with a copy of Smith (1838) that we scanned and transcribed with an OCR program.

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[22]This value might itself be considered an underestimate in that a fractional parcel was probably below the optimal farm size, and its use depended on combining it with a neighboring plot through an illiquid market. The auctions themselves took place typically in the state capital and the participants appeared to be market makers and/or consolidators (Weiman 1991).

In order to generate a control and treatment group for this lottery, we took advantage of the lottery's entry requirements: individuals had to be 18 years or older in 1832 and resident in Georgia for at least three years by 1832. We extracted all males from the complete count file of the 1850 U.S. Census who met two criteria: (1) they had at least one child born in Georgia in the three years prior to 1832; and (2) they had no children born outside of Georgia in those same years. This yielded a population of 14,306 individuals. Of these, 1,758 were then identified in the list of lottery winners based on their surname and given name. These individuals were then sought in the 1850 census manuscripts to transcribe their 1850 real-estate value,[23] occupation, and literacy.[24] The complete count file directly provided the other outcomes we will explore below (county of residence in 1850, and marital status in 1850 and the number of children born between the 1832 lottery and the 1850 census). Slave wealth was added by locating households in the 1850 US Census Slave Schedules. Together with data on slave prices by age and sex (taken from Kotlikoff, 1979, Table II), this made it possible to impute a value of slave wealth to each household.[25]

An initial concern regarding our sample design is that individual lottery winners needed to survive to 1850 in order to be at risk to be linked from the lottery to the 1850 Census. Given the age structure of the Georgia population, and new life tables produced by Hacker (2010) for the early nineteenth century U.S., we estimate that over 60% of the males eligible to participate in the 1832 lottery would have survived to 1850. Further, Steckel (1988) finds essentially no relationship between real estate wealth and survival probabilities 1850-60, so we argue that lottery winners are no more likely to be found in 1850 than non-winners.

An additional concern is that our reliance on the observed household structure in 1850 to impute lottery eligibility (i.e., the presence of at least one child born in Georgia 1829-32 and the absence of any children born outside Georgia in that window) imparts a bias by focusing our attention on homes where fewer children had left home by 1850 (and were thus present with their fathers and available for us to examine their birthplace and year of birth). Steckel (1996) reports that only 11% of children in the antebellum South departed their parental home by the age of 18, so this, too, is unlikely to contaminate our sample. Nevertheless, children born in 1829-32 must have survived to 1850 to be at risk to be observed, whether within or outside their parental home. Again, the aforementioned lack of a wealth effect on survival should prevent mortality from contaminating the

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[23]Steckel (1994) compares taxable wealth (county records) and census-reported wealth in a sample of individuals in Massachusetts and Ohio located in both sources. There are some discrepancies (more in Ohio than in Mass.), but no association between the size of the discrepancies and any observable characteristics apart from gender.

[24]These variables were double or triple input and then rectified by a different transcriber in case of any discrepancy.

[25]One complication with the Slave Schedule is that slaves were listed with the household/farm where they resided. If an absentee owner were not listed in the household on the Population Schedule, then the slave wealth would be attached to the wrong person. Note that this measurement issue is almost certainly limited to the upper tail of the distribution (e.g., the absentee planter who resides in Charleston rather than on his plantation).

sample of treated versus control households.

We nonetheless take seriously the possibility that differential survival and differential rates of children leaving home by wealth could leave questions as to the extent to which our findings are driven by the wealth differences upon which we focus rather than peculiarities of the sample design. To alleviate these concerns, we perform a series of balancing tests comparing the pre-treatment characteristics of lottery winners and non-winners in Section 4.2.

4.2   Summary Statistics and Balancing Tests

We present summary statistics for the sample in Table 1. Each row presents a different variable, and variables are grouped thematically into panels. Means and standard deviations (in parentheses) are shown for each variable. These values for the whole sample are seen in Column 1, and then we provide decompositions based on each individual's likely lottery status in Columns 2 and 3, which report the summary statistics for lottery losers and winners, respectively. Additionally, in Column 4, we report the p-value of a test of the difference in means between these two subsamples. We implement this test with a bivariate regression on a dummy variable for being a lottery winner. In the cases below in which there is a grouped-data structure, such as the household or surname level, we cluster the standard errors. The number in square brackets in each row reports the sample size used to compute this test statistic.

In the present study, we consider two measures of whether the person won land in the drawing for the Cherokee Land Lottery of 1832. Summary statistics for these variables are found in Panel A of Table 1. The first measure is coded to one if that person is a unique match to a name found on the list of winners published by Smith (1838). Anyone else is coded to zero, including individuals who were among several persons matched to the same winner's name. As is seen in the table, 12.4% of our sample is matched to the list of lottery winners. By construction, this variable takes on means of zero and one in Columns 2 and 3. In the second measure, we attempt to accommodate the relatively small fraction of individuals that tie for a match to the Smith list with others in our sample. In the case of a tie among n observations, we recode the match variable to $1/n$. This recoding of the variable is motivated by the belief that one member of the tying set did in fact win in the lottery, but we do not know which and thus distribute the probability of winning evenly across the group as if we had a uniform prior. More sophisticated (i.e., nonuniform) versions of assigning partial treatment values within such groups are possible, but we shied away from this approach because of the lack of appropriate benchmark data with which to calibrate such an approach. The average value for this variable is 15.5% in our sample, which is approximately 3% higher than the binary match variable and just slightly below the rates discussed above. The vast

majority of differences occur because numerous groups of small-n ties were recoded from zero up to $1/n$. These two lottery-status variables are extremely highly correlated: the regression coefficient of the second measure on the first has a t statistic of 329.

Next, we consider in Panel B of Table 1 a series of outcomes that were determined prior to the realization of the lottery, and therefore should be unaffected by whether the individual won land in the 1832 lottery. Analysis of these outcomes therefore serves as a balancing test when comparing the control and treatment samples. The lottery-eligible men in the sample are approximately 51 years old in 1850, and average age is similar between winners and losers. Almost 50% of the sample was born in Georgia, with the bulk of the remainder being born in the Carolinas. These fractions are statistically similar across groups. By the construction of the sample, these individuals have at least one child born in Georgia in the three years prior to the 1832 lottery. But there is no reason why lottery status should correlate to the number of children born in this earlier period, if our sampling design has drawn an appropriately matched treatment and control group. Indeed, we do find that the sample has approximately 1.33 children born in the pre-lottery window, and this number is quite similar between the two subsamples.

The next variable that we consider is whether the individual could read and write. While this variable is measured in 1850 and could theoretically be affected by the lottery some 18 years prior, literacy was more likely realized in childhood. These men, if they had won the lottery or not, would be unlikely to undertake remedial education in literacy given that they were already adults in 1832 and had on their shoulders the demands of supporting a family in a largely agrarian society. By this measure, almost 15% of our sample was illiterate, with insignificant differences between the control and treatment groups. (Note that this was probably a fairly weak test of literacy in that many enumerators classified someone as literate if they could read and write their name. Rates of illiteracy were considerably higher if a more modern standard of literacy was applied.)

In the rest of Panel B, we examine characteristics based on the individual's surname, which was inherited from the father at birth and therefore predates the lottery. As there was probably very little phonetic change in the surname over the life course (or even across generations), the low rates of literacy and somewhat lax orthography of the time might have occasioned some drift in how the surname was spelled. For example, in the census manuscripts the surname "Blakely" has variants "Blakeley," "Bleakley," "Blakelee," and others, as does "Ferry" have the variant "Ferrie." To accommodate this heterogeneity in spelling, we use the Soundex version of the name, which reclassifies names that are phonetically similar into a single code. The first surname-based outcome that we consider is the number of letters in that name (and for this outcome alone we use the original surname rather than the Soundex version). On average, surnames have 6.2 characters, and this

average is indeed slightly lower in the subsample of lottery winners. Next we find that the average person has a surname that appears 36 times in the sample, and this is not significantly different between subsamples. We also find that 10% of the sample has a surname that begins with the letter 'M' or 'O' (correlated with Celtic origin), and this rate is insignificantly different between the group of winners and losers. Indeed, for a cross tabulation of lottery status and the first letter of last name, a chi-squared test (d.f.=26) of the equality of distribution across groups has a value of 20 (p=0.8).

The final set of surname-related outcomes that we present in Panel B are constructed from the average characteristics of others in Georgia with the same surname. We restrict ourselves to Georgia in part to maintain similarity with our sample and also because we had access to a full transcription of the 1850 census for the counties in Georgia starting with the letters A-J that was provided to us by the IPUMS project. We took this transcription file and formed averages by surname (again using the Soundex recoding of surname) for various outcomes. To prevent any mechanical contamination from our lottery-eligible sample, we exclude anyone in our sample from the construction of the surname-level averages. The mean surname-average of real estate wealth for our sample (again, not their real estate wealth but the average wealth of those people with the same surname) is approximately $1200. Because wealth is right-skewed, the mean presents a somewhat misleading picture, and accordingly we find the median wealth among individuals with the same surname is considerably lower: less than $300. The surname-level illiteracy rate is almost 22%. None of these surname-level outcomes show a statistically significant difference when comparing the lottery winners versus losers. (Some readers might argue that this is a weak test because perhaps the surname-level averages are measured with considerable noise. Nevertheless, we show below in Section 6.2 that the surname averages are strong predictors of individual-level behavior, even when conditioning on demographic and locational covariates. We also test for interactions of winning the lottery with these surname averages below as a test of heterogeneity in the response to wealth shocks.)

In Panel C, we present summary statistics for measures of wealth in 1850. Note that this panel and the rest of the table can no longer be considered part of a balancing test in that we examine outcomes that might very well be affected by winning the lottery. For this panel, the numbers in curly brackets display the 25th, 50th, and 75th percentiles, respectively. The first measure that we consider is real estate wealth. The whole-sample mean is approximately $2000 and the median is $650. Unlike many of the outcomes above, here the mean differences by lottery status is significant for an $\alpha = 10\%$ level. Real-estate wealth also shows differences at the median, although not in the upper or lower tails. Next we consider statistics for slave wealth, which had a mean of approximately

$1340, and a statistically significant difference in means by lottery status. The final row of Panel C displays the sum of these two wealth components, which we label "total wealth" throughout the paper.[26] This variable, whose mean is over $3000, shows a several-hundred-dollar difference between control and treatment groups, which is both economically and statistically significant. The mean difference in total wealth that we observe between lottery winners and losers is close in magnitude to our earlier back-of-the-envelope estimate of the value of the land won in the lottery. The median and 75th percentile is higher in the treatment versus control, but the 25th percentile is the same. Further, a Kolmogorov-Smirnov test rejects equality of the control and treatment wealth distributions at an $\alpha = 5\%$ level.

Finally, the vast majority of the sample still lived in Georgia in 1850, and the bulk of the remainder resided in Alabama. (Appendix Figure 1 displays the geographical distribution of our sample by county in 1850.) Nevertheless, we do not see significant differences across treated versus control subsamples in the propensity to be in either of these states. However, a chi-squared test overwhelmingly rejects the equality of the distribution of the subsamples across counties. One main aspect of this difference is the increased propensity of lottery winners to be in a county whose land was opened up by the 1832 Cherokee Land Lottery.

5 Estimated Change in the Wealth Distribution

In this section, we characterize the difference in the control versus treatment distributions of 1850 wealth using a variety of estimators. In Section 5.1 we define a simple regression equation that forms the basis of our empirical analysis. In Section 5.2, we show that the treatment group of lottery winners had, almost two decades after the lottery, higher mean wealth than the control group of lottery non-winners. This result is robust to a variety of controls derived from the characteristics of surnames and given names. However, results from quantile regressions show that the effect of the lottery on the treatment group is concentrated in the upper part of the wealth distribution. Then, in Section 5.3, we present estimates of the PDF for control and treatment groups, as well as estimates of the difference in the CDF (ΔCDF) between the two groups. Again, we show that the treatment associated with lottery winnings perturbs the distribution of wealth primarily in the upper half of the distribution. Using both the quantile and ΔCDF estimators, we find very little effect of treatment on the lower 40% of the wealth distribution. (To be clear, we are thinking of the distribution itself as an object that is being treated. None of our results in this section is meant

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[26]Plainly, this is not a global total; there are other components of wealth that we cannot measure, such as non-slave personal property (which was only reported in the 1860 and 1870 censuses) and the individual's human-capital wealth. Below we show that these results are not sensitive to using occupation to impute physical capital or to accounting for investments in children.

to imply anything about the mapping from control to treatment, in the sense of characterizing the precise relationship between potential outcomes.) Next, in Section 5.4, we evaluate the gains from treatment (relative to control) under various preferences for distributional equity. Finally, in Section 5.5, we conduct a placebo exercise using a sample defined by having children born within the pre-lottery window, but within South Carolina instead of Georgia. Matches to the Smith list in this case are entirely spurious, and, accordingly, this placebo variable does not predict differences in wealth between the control and treatment groups.

5.1 Estimation strategy

The basic research design of the study is to compare the long-run outcomes of winners and losers among participants in the 1832 Cherokee Land Lottery. Above we discussed how we assign lottery status (winning vs. losing) in a sample of men who, by their characteristics, were eligible to participate in the lottery. With such a sample, estimating the treatment effect of winning the lottery is as simple as a comparison of means across the subsamples of winners and losers or, equivalently, a bivariate regression with the outcome on the left-hand side and a dummy variable for winning the lottery on the right-hand side. Throughout the present study, we opt for the regression approach, which is able to accommodate additional control variables on the right-hand side as well as the $1/n$ measure of lottery status, which is not dichotomous. At some level, the random nature of the lottery should obviate the need for control variables as fixes for omitted-variable problems. Nevertheless, controls might be useful to absorb some of the residual variation and perhaps improve the precision of the treatment estimates. Further, the methods that we use for tracking the lottery-eligible sample and imputing lottery status might introduce biases that control variables could clean up. (The fact that lottery status is not predictive of predetermined variables, as seen in Section 3 and Table 1, casts doubt on this supposition, but we can never rule it out entirely. We return to this issue in Section 5.5 with an alternative placebo test.)

The basic regression equation, which we generally estimate using OLS, is as follows:

$$Y_{ik} = \gamma T_i + B X_{ik} + \delta_a + \delta_k + \epsilon_{ik} \tag{1}$$

in which i, a, and k index individuals, ages, and 1850 counties of residence. The variable of interest, T_i, is a binary variable that denotes treatment—meaning winning the lottery—and the control variables are as follows: δ_a is a set of dummies for age; δ_k is a set of dummies for location (county×state k), which we include to account for differences in settlement patterns in the control and treatment groups; and X_{ik} is a vector of other control variables, as specified below. The random

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assignment of treatment by the lottery allows us to recover an unbiased estimate of γ.

A principal alternative specification used below also incorporates characteristics of the surname (last name). The main variant of the specification includes fixed effects at the surname level. The specification controls for a host of differences that might persist across patrilines. One way of thinking about the specification is measuring the impact of lottery winning within extended families (again, defined patrilineally). Recent work by Clark and Cummins (2012) and Güell *et al.* (2012) highlights the persistence in outcomes across patrilines, and this effect would be absorbed by surname fixed effects. Furthermore, specification problems that are introduced by our use of surname in constructing the lottery variables would also be absorbed by these fixed effects. (As we discussed above, we use the Soundex version of names to account for minor spelling differences.) Note that this is a stronger test to pass in that we effectively ignore individuals whose surnames are unusual enough that the sample does not contain both a winner and loser with that surname.

5.2   Baseline regression results

We estimate a large effect on 1850 wealth from having won the lottery almost two decades earlier. Table 2 presents the estimates of equation (1) with total wealth (the sum of real estate and slave wealth) as the dependent variable, and results are shown for both levels and natural logs. The baseline estimates are found in Column 1. On average, lottery winners have approximately \$750 or 14% more wealth in 1850. This number is similar in magnitude to the unconditional difference seen in Table 1. It is also similar to, perhaps a bit smaller than, the back-of-the-envelope estimate of the value of land won in 1832. It is possible that the winnings were partially spent or saved in some other kind of wealth, although there was a relatively limited set of assets that could be used to store value in the rural Deep South at this time, and we are measuring two of the most important components (land and slaves).

In any event, the baseline estimates suggest substantial persistence. The remaining columns of Table 2 report specifications that use different sets of fixed effects as controls. In Columns 2-4, we control for characteristics of the surname: the first letter, the number of letters, and the frequency of that surname in our sample. These estimates are within a third of a standard error of the baseline. In Column 5, we report specifications that include a full set of dummies for each surname (using the Soundex concept, as discussed above). Estimates drop by about half the standard error in this case, but we still estimate that lottery winners were almost \$600 richer 18 years after the lottery. In Column 6, we control for a full set of dummies for given (first) names rather than surnames, and we see that the estimates instead rise by about half a standard error relative to the baseline. Finally, in Column 7, we include fixed effects both for given name and for surname. (Note that

these are two sets of fixed effects; fixed effects for each given-name-x-surname cell would absorb the lottery-status variable, which uses the full name for linkage to the Smith list.) These estimates are a bit below the baseline, but a bit above the estimates that we obtain when controlling for surname alone.

Table 3 continues the analysis of lottery status and wealth by presenting specifications with alternative ways of constructing the wealth variable. Panel A presents results for total wealth in levels or logs. Estimates from the baseline specification are repeated here for reference. Also in this panel, we attempt to adjust this variable for the truncation of the lower tail. Specifically, census enumerators were instructed to leave real estate wealth blank if the value was under $100. It is common in studies of variables that are censored or truncated like this to impute a value of zero in levels and in logs (=ln($1)). In the previous analysis, we assume the blanks were zeros in levels and missing values in logs. It is difficult to check these assumptions, but they seem ad hoc. An inspection of the distribution of real estate wealth reveals that the truncation at $100 is important: there is a nontrivial amount of density at and just above $100. Furthermore, the distribution looks approximately log-normal above $100. If we fit a truncated normal to the distribution above $100, we estimate that the expected value of wealth below $100 is approximately $59.34. We use this number to impute wealth to those whose real estate wealth is below $100 and rerun the regressions from above. As can be seen in Panel A, this adjustment for truncation of the lower tail results in trivial differences in the estimated coefficient on lottery winning. While this adjustment for truncation is also imperfect, the fact that the results change so minutely when moving around the lower-tail imputations by so much suggests that lottery status has very little impact on the lower tail of wealth. We test this directly in the next panel.

Panel B of Table 3 presents the results of quantile regressions[27] that allow us to explore the effect of winning the lottery on wealth at various points in the wealth distribution. We estimate very little effect of winning on wealth in the lower tail, seen in the first row of the panel where we estimate the treatment effect at the 25th percentile of wealth. (Note that the person at the 25th percentile of the wealth distribution in the sample has zero wealth.) In contrast, we estimate an effect of approximately $200 at the median and over $500 midway into the upper tail. We see even larger differences in wealth at the 95th percentile, although this result is only statistically significant for one of the two specifications. At such high levels of wealth, it is likely that any treatment effect of winning the lottery is overwhelmed by noise (be it statistical noise or variations in fortune/endowments/etc.), especially if the noise grows in magnitude as wealth increases even as the dollar value of winnings does not.

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[27]It was not computationally feasible to estimate the quantile regressions with large sets of dummy variables, so the results reported here are from bivariate quantile regressions.

This pattern of results across the distribution is also shown in Figure 1, which presents quantile-regression estimates of lottery winning across the distribution of total wealth in 1850. The points are the quantile-specific estimates of the treatment effect, and the dashed line is a local-polynomial-smoothed mean of these estimates. Here we use the 'unique match to Smith' definition of lottery winning. (Appendix Figure 2 displays analogous results using the $1/n$ match instead.) Again, we see that shifts in the distribution are quite small in the lower tail, become larger in the middle, and then grow quite large in the upper tail. (We omit the display of quantiles above 0.985, where estimates are larger still, so as to not obscure the shape of the curve for the vast majority of the distribution.) Note that, while the average coefficient is $525, the gains are quite concentrated in the upper third of the distribution.

These results are, on their face, inconsistent with the simple hypothesis that the random disbursement of a fixed amount of wealth shifts the distribution equally at all points. For certain, there was variance in the value of lots won, but the random nature of the lottery insures us that both the variance and the expected value would have been independent of a winner's counterfactual point in the control distribution. Thus, if all of the winnings were at least positive, then the lottery should have to some degree drained mass from the lower tail of the distribution, relative to control.[28] In any case, we cannot interpret these estimates as the treatment effects at a given point in the control distribution, unless the mapping from control to treatment (which is inherently unobservable) preserves the relative rank of each observation in the outcome distributions. Absent this rank-invariance property, the interpretation of quantile-regression estimates is somewhat awkward to render in words, so we return to this issue with graphical presentations of the differences in the distributional function in Section 5.3 below.[29]

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[28]Some readers might wonder what would the results look like if most of the parcels were of zero value? First, note that this assumption is extreme. Banks (1905, p. 19) estimates that more than three quarters of the lottery parcels were eventually claimed, which suggests an expected value greater than the filing fee of $18.) Second, note that someone who would have been in the lower tail should have been just as likely to win an unusually valuable parcels as someone who would have been elsewhere in the distribution. The following two simulations are illustrative. In simulation (1), we take the control group and turn a random 12% of them into spurious winners, and then add $500 to their wealth. The effect at the 25 percentile is $500 with a standard error of 40. In simulation (2), we instead add $2000 to a random 1/4 of this group of spurious winners. The quantile regression coefficient is again $500, but now with a standard error of 43. This pattern continues if we decrease the probability of winning a valuable parcel, but maintain the expected value of $500: the coefficient stays at 500, but the standard error increases. Thus, the problem introduced by parcel heterogeneity would seem to be one of precision, not of bias across the quantiles.

[29]See also Appendix A, where we present a partial-identification strategy for putting bounds on treatment effects for those who, absent winning the lottery, would have been in the lower tail of the wealth distribution (call it the 'counterfactual lower tail'). With minimal assumptions, we cannot rule out considerable departures from rank invariance. If we impose that the expected value of treatment was positive throughout the distribution, we can rule very large treatment effects for those in the counterfactual lower tail. But this upper bound is high: roughly $1500. Thus, even with the partial-identification analysis, we cannot rule out that the lower tails of treatment and control are similar because some of would-have-been-poor moved up and an equivalent number moved down to take there place.

In Table 3, Panels C and D, we consider the subcomponents of measured wealth: real-estate wealth and wealth held in the form of slaves. First, consider the intensive margin. We estimate positive treatment effects of winning the lottery for both categories of wealth, with a somewhat higher coefficient on slave wealth. The estimate for real estate is about half the median real estate wealth, while the estimate for slaveholding is considerably larger than the median (of zero) in that category of wealth. Second, consider the extensive margin of wealth. We estimate essentially no effect of winning the lottery on holding real estate valued at least $100 (the truncation point imposed by the census as discussed above). In contrast, lottery winners are four to five percent more likely to own at least one slave.

Finally, estimates are similar when using either the binary or the $1/n$ match variables to impute lottery winning. Throughout the rest of the study, we present only the binary variable to save space. Nevertheless, it does seem from the results in Table 2 that the specification including controls for surname fixed effects is a bit more conservative, and so we present that specification as an alternative to the baseline in the tables to follow.

5.3   Comparison of Density Functions

In this section, we compare the probability density functions and cumulative density functions (PDF and CDF, respectively) of the control and treatment groups. These results are shown graphically in Figure 2. To construct these graphs, we use the same sample of lottery-eligible household heads as above and we define "treated" to be the binary variable indicating a unique match to Smith (1838). The y axes in Figure 2 denote density (or probability) and the x axes measure the natural log of total wealth (displayed in thousands of dollars for legibility).

Relative to control, the empirical PDF of the treated group appears to be missing mass in the middle of the distribution and have extra mass in the upper tail. Panel A of Figure 2 shows the estimated PDFs of both the control and treatment groups (dashed and solid lines, respectively).[30] The vertical line at 0.1 (that is, $100) denotes a level below which some enumerators censored real estate wealth in the 1850 Census. The control PDF is approximately log-normal and roughly similar to the treatment distribution below a few hundred dollars. Between roughly $300 and $2500, however, the treatment PDF is noticeably lower than that of the control. Above $2500, this pattern is reversed, with control below treatment. (As mentioned in Section 4, the two distributions are significantly different from one another in a Kolmogorov-Smirnov test.)

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[30]These densities are estimated using a kernel estimator in stata ("kdensity") with a Epanechnikov kernel and stata's calculation for the optimal bandwidth. We omit those observations with zero wealth rather than using the imputation. Otherwise the assumption of smoothness would be violated for the kernel density estimator. The question of the extensive margin of wealth was treated above.

The CDF for the treated is shifted up relative to control in a statistically significant manner, but only for wealth between approximately two and and ten thousand dollars. This result is seen in Figure 2, Panel B, which displays the estimated differences in CDFs between the two groups at various points. We implement this estimator by constructing an indicator variable for wealth being below a given $\bar{w}$: $d_i^{\bar{w}} \equiv I(w_i \leq \bar{w})$. We then regress $d_i^{\bar{w}}$ on the treatment dummy, with controls as in equation 1. Sweeping the $\bar{w}$ threshold across the distribution of w_i, we recover a treatment effect and confidence interval that estimates the difference in CDFs across a series of wealth levels. (Note that shifting of the treatment PDF will generate negative coefficients in this procedure, by the definition of the CDF.) These estimates are shown in Panel B, with the solid gray line being the point estimate and the dotted lines describing the 95% confidence interval. A solid black horizontal line is drawn at zero for reference. As can be seen, the lower half (or more) of the CDFs are statistically indistinguishable, as are the extreme upper tails. Nevertheless, there is a range of wealth values, in the several thousands but not the tens of thousands, for which the treatment CDF has significantly more mass at higher wealth than has the control CDF. (These results are similar if we use alternative regression specifications. See Appendix Figure 3.)

5.4 Incorporating Tastes for Equity

The magnitude of the improvement for the treated group depends on one's taste for equity (i.e., distaste for inequality). There is greater inequality in the treatment than control distributions, as measured by the coefficient of variation, the standard deviation of logs, and the Gini coefficient, as well as with the Mehran, Piesch, Kakwani, and Theil entropy measures. Furthermore, a statistical test that the standard deviation of log total wealth (with the imputation adjustment) is greater for treatment than control (versus the null of equality) has a p value of 0.0321. But how should we weigh this increase in inequality (a presumed cost) versus the higher average wealth (an obvious benefit)?

A simple and standard way to vary the equity weight when considering a group's distribution of outcomes is to use an aggregator with a constant elasticity of substitution (CES):

$$\bar{U}_j = \left[\sum_{i \in Z_j} \frac{1}{N_j} w_i^{\frac{\rho-1}{\rho}} \right]^{\frac{\rho}{\rho-1}} \tag{2}$$

in which i indexes individuals in our sample, j is an indicator for treatment status ($j = 0, 1$), Z_j is the set of indices i belonging to group j, w_i is i's 1850 total wealth, and ρ is the elasticity of substitution, which relates to the preference for equity in a manner described below. The question

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we ask is whether, for a given ρ, can we reject the equality of the CES aggregator between the two groups or, more formally, $H_0(\rho) : [\bar{U}_0 = \bar{U}_1]$?

Figure 3 shows the relationship between the taste for equity, ρ, and the ratio of the $\bar{U}$ for the treated divided by that of the controls. Importantly, the graph also displays the 95% confidence interval of this ratio, so we can see whether the two groups are statistically distinguishable under varying assumptions for ρ. These statistics are computed with 5000 bootstrapped samples for each ρ. The sample is the same as in Figure 2, Panel B, and the computations use the untruncated level[31] of wealth.

For a sufficiently high ρ we can reject the null hypothesis of equivalence between the two groups. As $\rho \longrightarrow \infty$, $\bar{U}_j$ becomes simply the arithmetic mean for each group. In this limit, a dollar held by any given person becomes a perfect substitute, in social-welfare terms, for a dollar held by someone else. Thus, the taste for equity disappears as ρ gets larger, and social welfare eventually depends only on the average outcome. Average wealth is indeed higher in the treatment group, as seen above. Therefore, as we see in Figure 3, we can reject the null hypothesis of identical $\bar{U}$ for very large ρ. As $\rho \longrightarrow 1$, the $\bar{U}_j$ aggregator becomes the Cobb-Douglas function, with each individual's wealth as its arguments. The H_0 is rejected for this case, which is equivalent to the test above for the natural log of wealth.

Nevertheless, we cannot reject the equality of the two (re-weighted) wealth distributions if we place a greater weight on equity. Specifically, the control and treatment distributions are only statistically distinguishable if we use a ρ much less than one. This is seen in Figure 3, where the confidence intervals overlap with zero for ρ below and including 0.95. This number is very far from, for example, Rawls' (1971) maximin preference, an elasticity of zero in which social welfare depends exclusively on the well-being of the least-well-off individual.

5.5 Placebo test using South Carolina

We perform a falsification exercise using South Carolina rather than Georgia and do not find statistically significant results. One of the challenges in identifying the treatment effect associated with winning the 1832 Lottery is that our method of imputing lottery status via name matching may introduce biases through sample selection. To check for this possibility, we construct a placebo sample using households with only children born in South Carolina (rather than Georgia) during the same pre-lottery window (the three years prior to the Cherokee Land Lottery of 1832).[32] We use the names among this South Carolina sample to impute lottery status per the Smith (1838) list.

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[31]Using natural logarithms would be an inappropriate starting point, tantamount to forcing $\rho \leq 1$.
[32]We are grateful to Petra Moser for suggesting this test.

As above, we use both a dummy for a unique match to the Smith list and a variable that allows for probabilistic matches, deflated to $1/n$ in case of ties. By the eligibility rules of the Cherokee Land Lottery, any matches to this list from the placebo sample must be spurious. It is then reassuring that the fraction of unique matches in the placebo sample derived from South Carolina is only one quarter of the fraction in the Georgia sample. In Table 4, we estimate Equation 1 using this placebo sample, for the different variables indicating lottery status, and using both the basic specification and the one that includes surname/Soundex fixed effects. These results are found in Panels A and B, with analogous results from the Georgia sample provided for reference in Panel C. The first four columns of Table 4 show outcomes that were determined prior to the 1832 Lottery, and there are no statistically significant results. (Note that a series of falsifications checks using pre-lottery variables was also performed for the Georgia sample, as shown in Table 1, Panel B.) The remaining columns show post-lottery outcomes in 1850 such as residing in old Cherokee County and real-estate wealth. There is no statistically significant pseudo-treatment effect for the South Carolina sample, in contrast to what we find for Georgia.

6   Discussion

Paying attention to their differential effects across the wealth distribution, we consider in this section various mechanisms for the results above. We do not present evidence that can rule out any one of these mechanisms, but we do discuss several channels that pertain to them. To have such similar lower tails of the control and treatment distributions two decades after the lottery, there must have been dissipation of the lottery winnings by some. Painting with a broad brushstroke, we suggest four distinct channels of dissipation. First, there may have been differences in the physical returns to capital, which might arise because of a fixed cost of production or an interaction with the ability of the individual. The second mechanism is risk. The economy of the period was characterized by much uninsurable risk. This may have been differentially incident on the lower tail if they had less of a buffer against shocks that might take them down to a subsistence level. A third mechanism for wealth dissipation is consumption: some winners may have consumed all of their newfound wealth prior to 1850.[33] A fourth mechanism is leisure: lottery winners may have chosen to live a quiet life rather than leveraging their capital with sweat equity (e.g., by purchasing on the frontier and improving their land).

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[33]Note that, in an environment without access to credit, it is difficult to separate the timing of consumption and investment decisions. Thus, even if a non-convexity in production represents a high return to investment relative to canal bonds, e.g., the return may be low when compared with the individual's subjective discount rate. If so, there is effectively no poverty trap in this case because the individual would chose consumption over investment if he had the wealth.

6.1 Fixed Costs

It is implausible that a fixed cost of production, in and of itself, caused the long-run insensitivity to treatment of the lower tail of the wealth distribution. We say this not because we doubt the existence of fixed costs (we do not), but rather because it is inconsistent with what we know about farming in this period. Without doubt there were farms (plantations, really) that had large fixed costs of start-up and operation. But a prospective farmer who could not produce such a large initial investment would have had the option to farm at a lower scale, perhaps with a different crop and/or in a different area.

Within our data itself, it is difficult to square the results with a large fixed cost of production. Differences in the PDFs do not emerge until over $400 and the CDFs do not significantly diverge until over $2000 in wealth. In contrast, we see farmers in our own data working with much lower levels of wealth. (See Figure 2, Panel A.) The fact that the control wealth distribution is approximately log-normal, even in this low-wealth range, suggests that we are observing something close to the steady-state level of operation rather than some transitory range that farms pass through on the way to either closing or dramatically expanding. In any event, the parcels won in the 1832 Cherokee Land Lottery were roughly 160 acres, which would have been more than enough for small-scale farming, except on the lowest-quality land. Even if 160 acres were too small for some purposes, lottery winners could have simply sold their land in Old Cherokee County (as many did) and purchased a larger farm with cheaper land farther west on the frontier.

While slaves were manifestly difficult to purchase in non-integral units, this nonconvexity need not have been an impediment to productivity using the lottery winnings. First, purchasing a prime-age, male fieldhand would have cost several hundred rather than several thousand dollars in 1850. Second, there were substitutes for purchasing slave labor, such as hired labor or less slave-intensive crops. Furthermore, while some areas of the South were dominated by slave-based agriculture, other were not. Indeed, Old Cherokee County itself was in the Upcountry where smaller yeomen-operated farms were commonplace.

Land improvement was itself an up-front cost, but did not need to be either large or lumpy. Initial land clearance was often done in a low-cost, slash-and-burn fashion. In any case, improving land could be done incrementally, perhaps hiring oneself out as a farm laborer until at least enough land was cleared for subsistence.[34]

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[34]We are grateful to Tim Guinnane for pointing this out.

6.2   Risk and Churn

Another mechanism is exposure to risk. Figure 4 displays estimates of the fraction holding no more than $100 in 1860 as a function of 1850 total wealth. The dependent variable (on the y axis) is a dummy for whether the 1860 total wealth (personal plus real estate) is less than or equal to $100 in 1860 dollars. The independent variable (on the x axis) in this figure is total measured wealth in 1850. The sample size for this figure is 5603, because only 40% of the sample is linked to 1860. The dashed line displays a local-polynomial smoothed estimate of the indicated fraction for each level of total wealth, and the grayed area denotes the 95% confidence interval. For reference, the solid gray line presents the PDF for log total wealth (excluding the imputation for those with zero wealth). Even very wealthy men faced a roughly 5% chance of dropping to the lowest measured wealth status over the course of a decade. This probability reaches one-quarter for the very poor. These results highlight the large degree of risk and churn present in that period.

A simple calculation helps think about the following hypothesis: lottery winners who would have been the lower tail anyway have zero treatment effect because of risk. Suppose that any lottery winners who would have been in the lower tail had they lost the lottery faced some catastrophic risk that would wipe out their lottery winnings. Suppose further that the shock would hit them with 95% probability over the 18 years between the lottery and the 1850 census. If the hazard rate of this catastrophic risk was constant over time, this would translate into a chance of approximately 15% per year $(1 - .05^{1/18})$ of losing their extra lottery wealth. This number seems high to us, although there is very little data on the idiosyncratic risk exposure in the pre-1850 period. Indeed, Wright's hypothesis that small Southern farmers practised "safety first" was based on observations of crop selection rather than direct information about uninsured risk. If this sort of yearly risk prevailed in the linked 1850-60 data shown in Figure 4, the dashed curve (indicating the 1860 wealth had dropped below the reporting threshold) would reach levels of over 80% (0.85^{10}) rather than below 30% as estimated. This suggests that a story purely of risk (perhaps conditional on wealth) cannot explain these results, although it leaves open the possibility of a story of heterogeneity across individuals in risk or returns.

Aggregate risk was also a major factor. The Panic of 1837 saw a large drop in commodities prices, including the price of cotton. (There was also a panic in the late 1850s, which would have affected the 1850-60 transition discussed above.) From a pure portfolio perspective, this should have affected the yeomen farmers less if "safety first" lead them to diversify away from market-oriented crops like cotton. But they might have been more affected if they had less of a buffer to keep them away from subsistence and/or Ransom's "walk away" margin.

6.3 Interactions and Human Capital

One possible mechanism is that wealth shocks alone are insufficient, but rather they must be paired with some complementary skill. Indeed, those at the bottom of the distribution (either control or treatment) may have been there precisely because they lack the ability to seize opportunities. If so, the bottom parts of the two distributions might look similar because the low-skilled winners could not take advantage of their winnings.

6.3.1 Did the would-be poor not claim their winnings?

Failure to claim lottery winnings cannot account for the lower tails of the treatment group being similar to control. Evidence supporting this claim is seen in Figure 5, which displays estimates of the fraction of parcels claimed prior to 1838 (as reported in Smith, 1838) versus total 1850 wealth (adjusted for truncation at the lower tail) for the subsample of lottery winners only. (By definition, lottery losers were not assigned parcels, so parcel characteristics are unavailable for the full sample.) The solid line displays a local-polynomial smoothed estimate of the mean claim rate for each level of total wealth, and the short-dashed lines denote 95% confidence intervals. For reference, the long-dashed line presents the PDF for log total wealth.

Across the distribution of 1850 wealth in the treated group, there was a relatively stable rate of claiming land, as shown in Figure 5. Claiming rates are, if anything, a bit higher in the lower tail of the 1850 wealth distribution. In any event, note the range of the curve: within 5 percentage points of one half for almost all of the distribution. If less than 50% failed to claim their winnings by the end of 1837,[35] this will attenuate the effect of treatment, but it cannot explain the apparent 100% markdown of winnings in the lower tail of treatment. Therefore the lottery winners who ended up poor did not do so because they all failed to collect their winnings at the outset.

6.3.2 Surname-average Characteristics

In this subsection, we consider whether the response to the shock of winning the lottery is related to characteristics of other people who share the same surname and are therefore likely related along patrilines.

To assess this possibility, we construct surname-specific averages of wealth, fertility, literacy, and school attendance as possible proxies for differences across extended families in either preferences or prices. We used the 1850 100% census file to construct the average fertility, school attendance,

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[35]Eventual claiming rates are probably even higher because the Georgia state government processed claims for several more years. Banks (1905) estimates that approximately a quarter of the land lots opened by lottery went unclaimed.

and real-estate wealth among Georgia-resident households for each (Soundex) surname. Those individuals that appear in our lottery-eligible sample are excluded from the construction of the averages. We first check for the statistical power of these proxies by regressing the individual-level outcome on the surname average:

$$Y_{ijks} = \alpha Y'_s + \delta_a + \delta_k + \epsilon_{ijk} \tag{3}$$

where s denotes the surname for each observation, Y'_s is the surname-average of the Y' variable, and each regression contains dummies for age and for state/county of residence. (The 'prime' on Y' allows for the possibility of a different Y variable's average on the right-hand side of the equation.) Furthermore, recognizing the group-level regressor, we adjust the standard errors for clustering at the surname level. The base sample for these regressions is the same as for analogous estimates of Equation 1 displayed in earlier tables, with the exception that some households are omitted if there were no other households in Georgia with the same surname and therefore no one with which to form the surname-level averages. We consider three surname-averaged wealth outcomes: the level of 1850 total wealth, its natural log (both adjusted for truncation in the lower tail), and a dummy variable if total wealth exceeds $5000. The baseline estimate for the treatment dummy is shown in Column 1 of Table 5.

The estimates indicate that the surname-averaged variable is indeed a strong and statistically significant predictor of individual wealth, although results are weaker for surname-average fertility. Estimates of equation 3 are found in even-numbered columns of Table 5. The coefficient of zero is rejected in most cases for conventional confidence intervals. A mechanistic model in which the patrilineal dynasty (as proxied by surname) predicts outcomes one-for-one is even more strongly rejected, however; the coefficients are closer to 1/4th or 1/8th. See, for example, Panel A, Column 2, or Panel B, Columns 4 and 6, for apples-to-apples comparisons. (Note that we do not argue that this is a causal effect of the behavior of their relatives on the individuals' choices, but rather a proxy for some shifter that is common within the group. Thus the interaction term should be interpreted as interacting with the shifter as well.) Surname-average fertility is a weaker predictor of wealth (Column 8). Nevertheless, men have statistically and economically higher wealth if their surnames are associated with higher rates of school enrollment among children aged 5-15 or of literacy among adult men (Columns 10 and 12, respectively). In sum, these surname-level measures are generally strong predictors of own wealth, and thus may be suitable predetermined proxies of own human capital. (Recall from Table 1, Panel B, that lottery status did not predict these characteristics.)

The odd-numbered Columns 3–13 of Table 5 report results in which we interact the surname-

average characteristic with winning the lottery. The specific estimating equation is as follows:

$$Y_{ijks} = \gamma T_j + \beta T_j z(Y'_s) + \alpha Y'_s + \delta_a + \delta_k + \epsilon_{ijk} \tag{4}$$

Note that this is a modified version of Equation 1, to which we add the interaction of the treatment variable (T_j) with the z-score of the surname-average variable ($z(Y'_s)$). The main effect of the patriline loads on to $\alpha Y'_s$, and standard errors are clustered at the surname level. We report the coefficients on treatment, the surname average, and the interaction of treatment with the surname average. The estimated coefficients on being a lottery winner throughout the table are similar to those reported above, and the coefficients on the surname averages are similar to those in the even-numbered columns. The estimated interaction terms are generally of the expected sign but not statistically significant. For example, in Panel B, Column 7, we display estimates in which the outcome variable is the natural log of total wealth and the surname-level variable used to form the interaction with treatment is the natural log of the median total wealth among others adult males in Georgia with the same surname. The coefficient of .009 implies an additional .036 of log total wealth from treatment as we sweep across four standard deviations of the distribution of the surname-average variable. The difference is approximately one third of the main effect of treatment. However, this interaction coefficient is not significantly different from zero. Indeed, only one of the 18 interactions with surname averages yields a significant (at the 10% level) estimate, although 13 of 18 are of the expected sign.

6.3.3 Own Illiteracy

We do not find a significant interaction between treatment and the lottery winner's own illiteracy. While this could in theory have been affected by the lottery, it is unlikely that many men would have become literate during adulthood, whether they won in the lottery or not. Nor does lottery winning predict illiteracy, as we saw in Table 1. With these facts, we take license to use the lottery eligible's illiteracy (defined as unable to read or write) as a predetermined (prior to the lottery, that is) variable. These estimates should be taken *cum grano salis* in case this assumption is incorrect. In Table 5, Column 14, we show that illiterate men indeed had substantially lower wealth by all three measures. However, estimates for the interaction term (shown in Column 15) are not significantly different from zero.

All told, evidence in support of both a statistically and an economically important interaction between treatment and human capital is weak, at least with the proxies of human capital at our disposal.

6.4 Life-Cycle and Family Considerations

The results above are not likely because of a life-cycle-related wealth decumulation (be it for spending or bequests). First, note that *inter vivos* transfers to current members of the same household are already included in measure of wealth used above. Second, the age distribution of and life-profile of wealth do not favor this hypothesis of decumulation. The distribution of age and the average wealth by age, in our sample, are shown in Appendix Figure 4. Wealth accumulation peaks around 60 years, and over 80% of our sample is below this age. In any event, it is probable that some would have maintained their gross asset position (which is what we measure above) into old age to use as collateral against a mortgage or as a kind of social collateral to secure help from their children who would be waiting for their inheritance. Again using Hacker's life table, we compute remaining life expectancy for ages 35-75 in 1850 was an additional 17.7 years, which is similar to the number of years lapsed since the lottery. Thus, our sample of lottery eligibles was nowhere near the end of their planning horizon for consumption. We argue, therefore, that the similarities of the lower tail of the control and treatment wealth distributions are not because people had spent or bequeathed all of their wealth for standard life-cycle motives.

One complication is that some of the wealth might have already been bequeathed to adult children outside the house. Note that our sample would still have children in the household by construction, and it is unlikely that they would have already bequeathed all of their wealth to children outside the household.[36] In any case, we attempt to include *inter vivos* transfers by measuring the wealth of nearby adult sons. (We have no way to track sons that moved far away.) In order to identify potential sons of lottery participants who may have left home by 1850 and established nearby farms themselves, the manuscript pages were searched for all males within 50 individuals of each lottery participant with a surname similar to the participant but a reported 1850 age that was 20 or more years younger than that of the participant. The real estate wealth of these individuals was then transcribed and their wealth was re-inserted into the participant household's total to account for the possibility that some wealth might have been disbursed to these potential sons as they left their father's home and set up farms. Though some were no doubt nephews rather than sons or may in fact have been unrelated to the lottery participants, we do not expect the quality of this indicator of wealth "leakage" to vary systematically between lottery winners and lottery losers. We find that the relationship between having a possible son nearby and the 1850 distribution of wealth is quite similar in the control and treatment groups. (See Appendix Figure 5, Panel B, which is patterned on Figure 4.) Furthermore, when we construct the difference in the

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[36]Primogeniture was abolished by the Georgia colony in 1777 (19 Ga col rec part 2 1912 455), so children were entitled to equal treatment as heirs when their father died intestate. There are numerous examples of trousseau (dowries) being provided to daughters at the time of their marriage, but the practice was not legally mandated.

CDF of wealth including potential sons' wealth (shown in Appendix Figure 3, Panel F) the pattern is similar to the baseline estimates in Figure 2, Panel B.

We also argue that changes in fertility are not an important mechanism in explaining the pattern of results across the distribution. Using a similar research design, Bleakley and Ferrie (2013a) found that lottery winners had a higher fertility rate (approximately 0.1 more children by 1850, significant at conventional levels of confidence) and that the effect on fertility[37] is strongest in the lower half of the wealth distribution. This result is shown in Appendix Figure 5, Panel A, where the control and treatment means show a gap of roughly 0.1 over approximately the same range for which the ΔCDF results in Figure 2, Panel B were insignificantly different from zero. Nevertheless, this coincidence does not imply that the pattern of excess fertility among lottery winners explains the pattern of the wealth results. First, if the average number of additional children is approximately 0.1, it implies that the vast majority of the treated did not have additional children, and therefore had no reason to lose their extra wealth because of extra fertility. Second, even if we ignore the integral nature of childbearing, comparing 0.1 extra children with the approximate value of winnings of $500-$1000 would imply a price of child-rearing on the order of $5000 of wealth per child, which seems implausibly large.

6.5 Locational choice

Finally, we do not find locational choice as an important consideration in explaining differences in wealth between control and treatment. First, note that the lottery winners were slightly more likely to be in old Cherokee County in 1850, by approximately 2 percentage points. But apart from this, lottery winnings did not appear to bring them to a place that had peculiar characteristics, at least across a wide range of observables from aggregate county data, including fertility, schooling, farm values, farm sizes, land improvement, slave density, urbanization, or access to transport. (These results are found in Appendix Table 1.) In any event, we obtain fairly similar results for wealth whether or not we condition on state×county dummies; therefore, the change across the wealth distribution is not because some part of the treatment group happened to pick counties that saw faster appreciation of land values, for example. Note that (as seen in Appendix Figure 5, Panel B) residing in old Cherokee County in 1850 by the treated seems to be strongest at the lower/middle part of the wealth distribution. This is consistent with claiming rates being slightly higher in the lower part of the distribution, as seen above. Generally, this pattern makes sense in that much of

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[37]Note that same study, focused on the child-quantity versus child-quality effects of parental wealth, reports that lottery winners did not change their investments in average child quality. The children of lottery winners did not go to school more in 1850, nor did they hold more wealth in 1870, nor have higher-paid occupations in 1880, nor were they more literate in 1880.

the work of actually going out to improve land would have been labor-intensive, and therefore a task disproportionately taken up by those with a low opportunity cost of time.

At the same time, going out to the frontier and waiting for population growth might have been a high-return investment, albeit one that required the hard work of improving land. We find that treated men tended to live in counties in 1850 that had lower population density in both 1830[38] and in 1850. But the 1830-50 growth in log population density in the 1850 county of residence was not significantly different for control versus treatment groups, and the pattern of density growth is similar across the 1850 wealth distribution for both groups (Appendix Figure 5, Panel E). (We would have preferred to conduct this calculation using the 1830-50 growth in land value, but farm values by county were not available before 1850.) In any event, we do not find that those in the treated group who ended up in the lower tail had opted for the 'quiet life' of purchasing land in already developed (dense) counties (Appendix Figure 5, Panel F).

6.6 Unmeasured assets

The analysis above is limited to two categories of assets: land and slaves. Here we discuss our conclusions that might be affected by unmeasured aspects of the household's balance sheet. First, financial assets (such as stocks and bonds) would not have been an important part of the portfolio, except for the wealthiest households. Access to banks would also have been limited, especially for those in the lower tail in view of the fact that they were more likely to live in sparsely populated counties. Some important assets for those engaged in agriculture, however, would have been farm implements and livestock. While we do not measure them, holdings of such assets would presumably rise in proportion with landownership, which we do measure.

Another important class of assets is physical capital that might have been used by artisans who were not themselves farmers, but whose work was demanded by local farmers. For example, blacksmiths, farriers, and millwrights supplied goods and services to farmers, but these activities were physical-capital intensive rather than land intensive. To identify occupations other than farming that had substantial requirements in terms of physical capital,[39] the 1860 Census of Population was used to generate average personal wealth (reported for the first time in 1860) by occupation in the non-slave states[40] among white, native-born males aged 20-65. Using their 1850 occupation, we add this occupational index of non-farm, non-slave wealth to the total wealth of the sample of

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[38]The 1830 density is computed for the 1850 county boundaries using a raster-based method described in Bleakley and Lin (2012). The underlying data are from NHGIS and ICPSR study # 2896 (Haines, 2010).

[39]We are grateful to Chris Woodruff for suggesting this strategy.

[40]Slaves would have been counted as personal wealth in 1860, so we used the pattern of occupational wealth in non-slave states to avoid double counting.

lottery eligibles. The similarity between control and treatment groups in their lower tails is not affected by this imputation. If anything, the distributions are even more similar at the lower end, with statistically significant differences not emerging until \$2500. (See Appendix Figure 3, Panel E.)

This index of personal wealth was then culled to remove occupations in which the sole source of earnings was likely to be human capital (physicians, lawyers, teachers) or farm implements (farmers, e.g.). We then grouped the occupations into those with above-median and below-median averages of filtered personal wealth. The control and treatment groups have similar propensities to be in occupations with apparently high physical-capital requirements. If anything, the lower tail of wealth has a higher representation of such 'artisan' occupations (Appendix Figure 5, Panel D). These results are inconsisent with the idea that the lower tail of the treatment group was in fact shifted up from poverty, but obscured to us by not measuring artisans' wealth above.

Another issue is that we measure the gross asset holdings, which might be inflated if the farmer could leverage his wealth with debt. Farm mortgages in the first half of the nineteenth century were generally valued at no more than half the value of the underlying property,[41] so the reported gross real-estate wealth can deviate from real-estate wealth net of encumbrances by no more than a factor of two. This is an upper bound in that few farms were at their maximum loan-to-value ratio. Bogue (1976) reports that for the 1850s, between 5% and 40% of all farmland in a set of Midwestern counties was encumbered. The fraction of land encumbered was probably lower in the South in this period, as Ransom and Sutch (1974) report that "as late as 1890, only 5.3 percent of all farms in the South were mortgaged, compared to 36.4 percent of all farms outside of the South." (Ransom and Sutch, 1973, note 4, p. 136). This low number should not be surprising given the antebellum Southern banks' overwhelming focus on short-term commerical affairs. (See footnote 12 above.)

7   Conclusions

The 1832 Cherokee Land Lottery in Georgia represents an unusual environment in which to assess the long-term impact of shocks to wealth on the wealth distribution. Winning should have been uncorrelated with individual characteristics by the random nature of the lottery, and indeed our model passes numerous falsification tests using predetermined variables and a placebo sample from South Carolina. Further, participation in the lottery was nearly universal (albeit limited to adult white men largely) thus ameliorating some of the possible problems with external validity that can

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[41]Among southern land banks, "mortgages were to represent property of double value" (Sparks, 1932, p. 96).

arise with self-selected samples.

Using wealth measured in the 1850 Census manuscripts, we follow up on a sample of men eligible to win in the 1832 Cherokee Land Lottery. With these data, we can assess the effect of winning that lottery on the distribution of wealth almost two decades after the fact.

We show that winners are, on average, richer, but mainly because the middle of the distribution is thinner and the upper tail is fatter. In contrast, the lower tail is largely unaffected. This stands in contrast with a 'mechanical' short-run effect of the lottery, which would tend to compress the distribution of (log) wealth. The results are also inconsistent with the view that the effect of winning would have been greatest on the lower tail because credit constraints had created a wealth-based 'poverty trap'. Instead, the results are consistent with heterogeneity along a number of dimensions—differences by wealth in risk-taking or the ability to manage resources and delay gratification, for example—that may themselves help account for wealth levels that prevail absent a lottery. As we see in this episode, then, it may take more than just wealth to move the lower tail of the long-run wealth distribution "up from poverty."
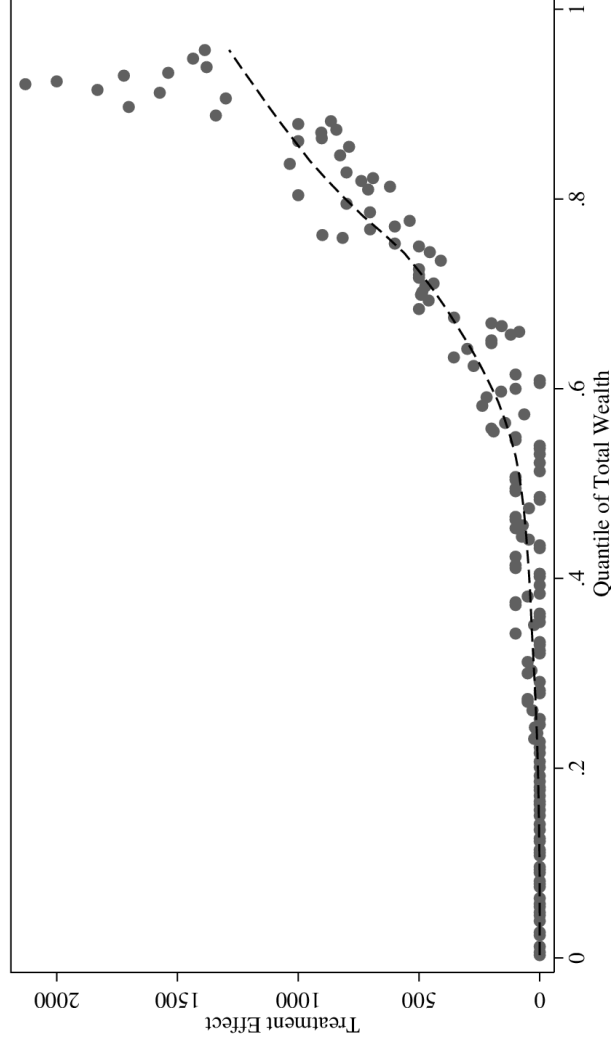
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Figure 1: Quantile Regression Estimates on Total Wealth and Lottery Winning

Notes: This figure displays estimates from a quantile regression of the effect of winning the lottery ("treatment") on total wealth in 1850. The points are the quantile-specific estimates of the treatment effect at various quantile points. The dashed line is a local-polynomial-smoothed (Epanechikov kernel, with a bandwidth of .11) estimate of the treatment effect. The sample consists of all household heads in the 1850 census with children born in Georgia during the three years prior to the Cherokee Land Lottery of 1832 and no children born outside of Georgia during the same period. An observation is coded as a lottery winner ("treated") if he is a unique match to a name found on the list of winners published by Smith (1838); anyone else in the sample is coded to zero ("control"). The dependent variable in this figure is total measured wealth in 1850, the sum of real-estate wealth and slave holdings. Real-estate wealth is as reported on and transcribed from the manuscript pages of the 1850 Census of Population. Slave wealth was estimated by linking the household to the 1850 Slave Schedule and imputing a market value of slave holdings adjusting for the reported ages and gender of slaves on the Schedule. The sample size for this figure is 13094. Data sources and additional variable and sample definitions are found in the text and in the appendices.

Figure 2: Differences in the Distribution of Total Wealth Between Lottery Winners and Losers

Panel A: Estimated Probability Distribution Functions

Panel B: Estimated Differences in the Cumulative Distribution Functions

Notes: This figure displays estimates of the distribution, decomposed by lottery status, of total 1850 wealth for sample of lottery-eligible men. The sample consists of all household heads in the 1850 census with children born in Georgia during the three years prior to the Cherokee Land Lottery of 1832 and no children born outside of Georgia during the same period. An observation is coded as a lottery winner ("treated") if he is a unique match to a name found on the list of winners published by Smith (1838); anyone else in the sample is coded to zero ("control"). The dependent variable in this figure is total measured wealth in 1850, the sum of real-estate wealth and slave holdings. Real-estate wealth is as reported on and transcribed from the manuscript pages of the 1850 Census of Population. Slave wealth was estimated by linking the household to the 1850 Slave Schedule and imputing a market value of slave holdings adjusting for the reported ages and gender of slaves on the Schedule. The sample size for this figure is 13094. Panel A presents the probability distribution functions, estimated using the "kdensity" command in stata. The vertical line denotes $100, the level below which some enumerators censored real-estate wealth. The solid line in Panel B presents the differences in the cumulative distribution function between groups (treatment minus control), estimated using a linear probability model (equation 1) of an indicator for being below 200 quantile cut-points for wealth. Heteroskedasticity-robust 95% confidence intervals are plotted with the dashed lines. Data sources and additional variable and sample definitions are found in the text and in the appendices.
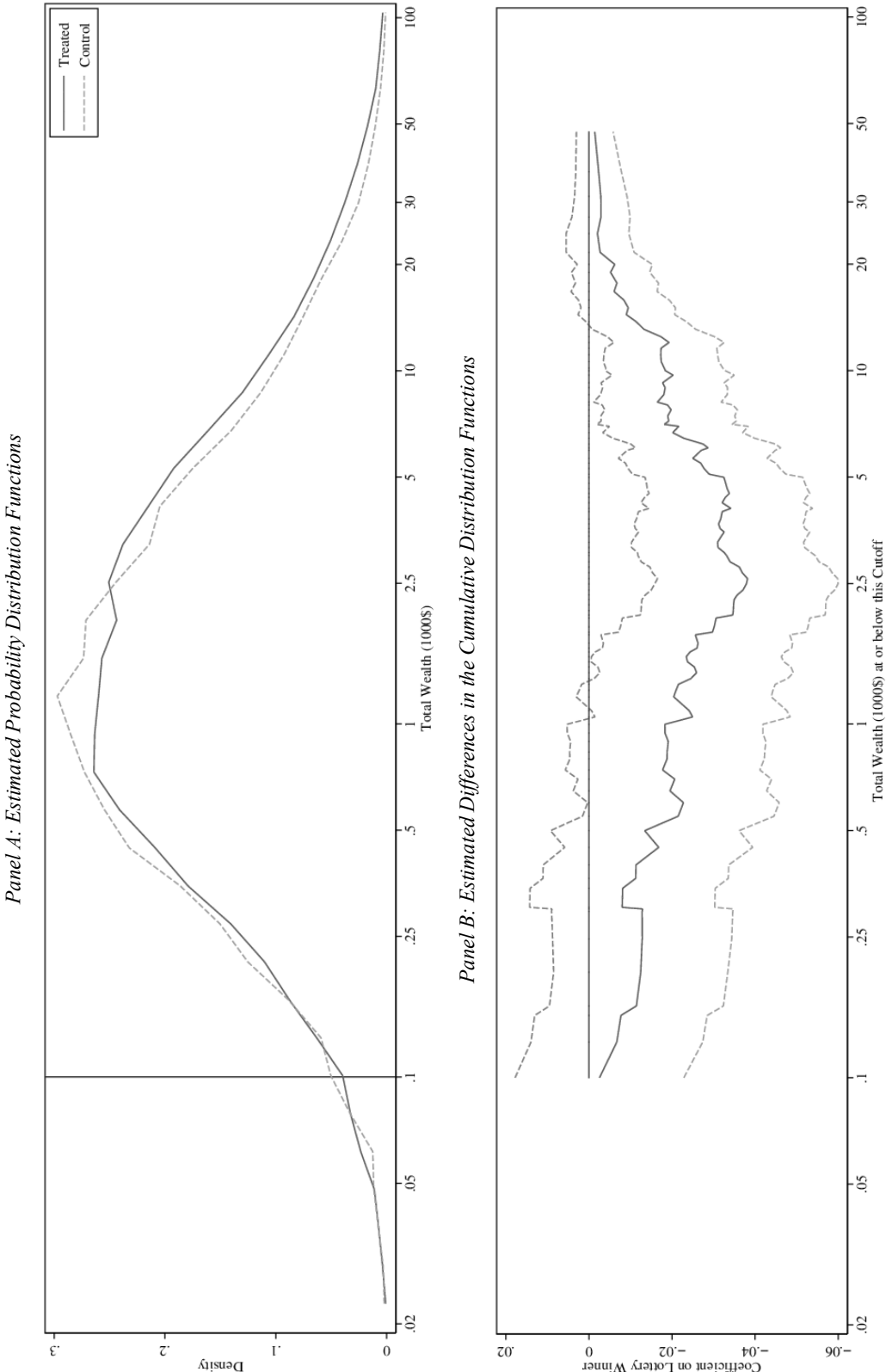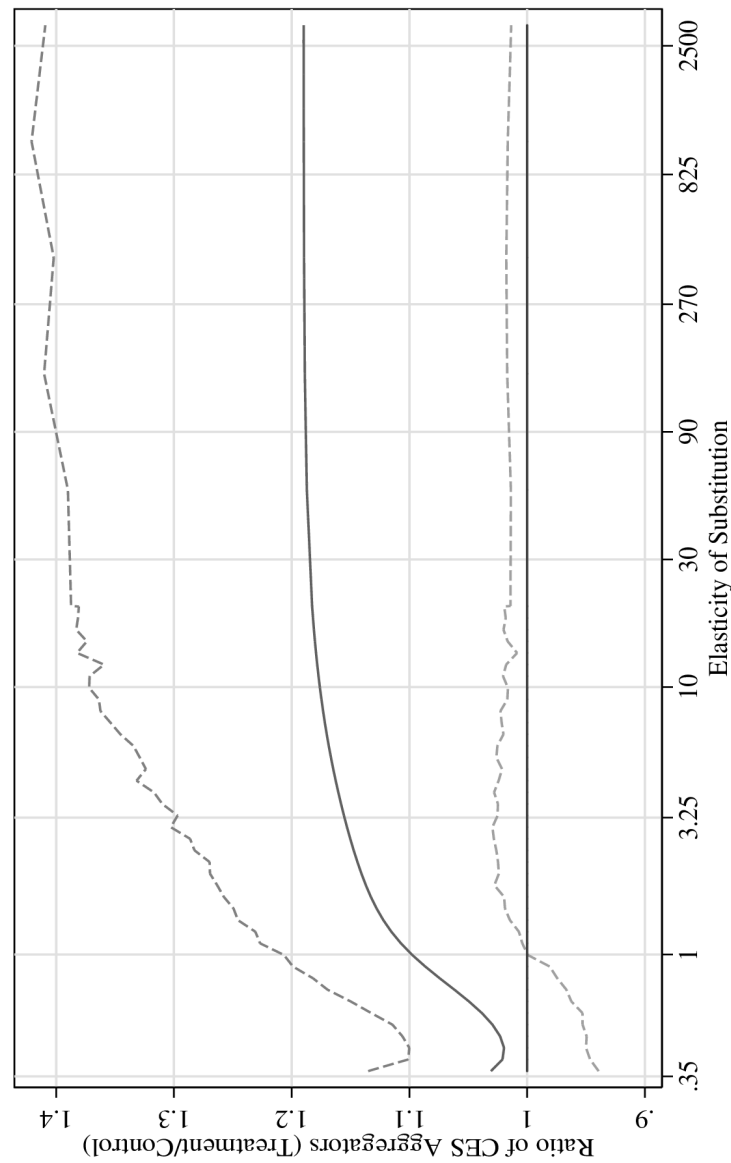
Figure 3: Total-Wealth Differences, Lottery Winners versus Losers, Under Various Tastes for Equity


Notes: This figure compares treatment/control differences using a range of preferences for equity over total wealth. The summary statistic for each group is computed using a constant-elasticity-of-substitution (CES) aggregator. The ratio (treatment divided by control) of this statistic is indicated on the y axis. This ratio is computed for various values of the elasticity of substitution (rho), denoted on the x axis. The graph displays the ratio and 95% confidence interval, computed with 5000 bootstrapped samples for each point in the rho grid. The sample is the same as in Figure 1, Panel B. See the notes to Figure 1 for variable and sample definitions.

Figure 4: Fraction with 1860 Wealth Less Than or Equal to $100 versus 1850 Wealth
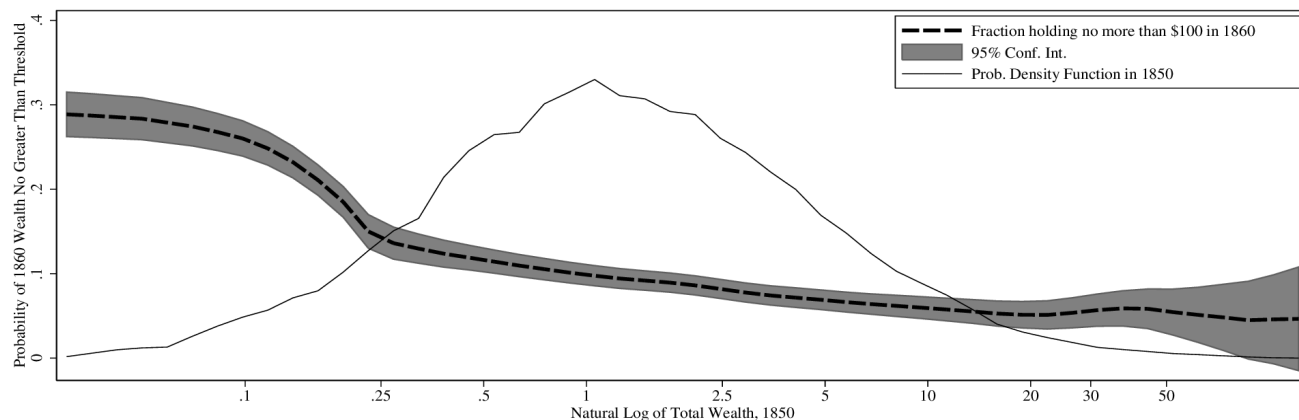
Notes: This figure displays estimates of the fraction holding no more than $100 in 1860 as a function of 1850 wealth. The base sample consists of all household heads in the 1850 census with children born in Georgia during the three years prior to the Cherokee Land Lottery of 1832 and no children born outside of Georgia during the same period. An observation is coded as a lottery winner ("treated") if he is a unique match to a name found on the list of winners published by Smith (1838). The dependent variable (on the y axis) is a dummy for whether the 1860 total wealth (personal plus real estate) is less than or equal to $100. The independent variable (on the x axis) in this figure is total measured wealth in 1850, the sum of real-estate wealth and slave holdings. Real-estate wealth is as reported in and transcribed from the manuscript pages of the 1850 Census of Population. Slave wealth was estimated by linking the household to the 1850 Slave Schedule and imputing a market value of slave holdings adjusting for the reported ages and gender of slaves on the Schedule. The sample size for this figure is 5603. The dashed displays a local-polynomial smoothed estimate of the indicated fraction for each level of total wealth, and the grayed area denotes the 95% confidence interval. For reference, the solid gray line presents the probability distribution function for log total wealth (excluding the imputation for those with zero wealth). These curves are estimated using, respectively, the "lpoly" and "kdensity" commands in Stata version 12. Data sources and additional variable and sample definitions are found in the text and in the appendices.

Figure 5: Probability that Parcel is Claimed by 1838 versus Realized 1850 Wealth among Lottery Winners
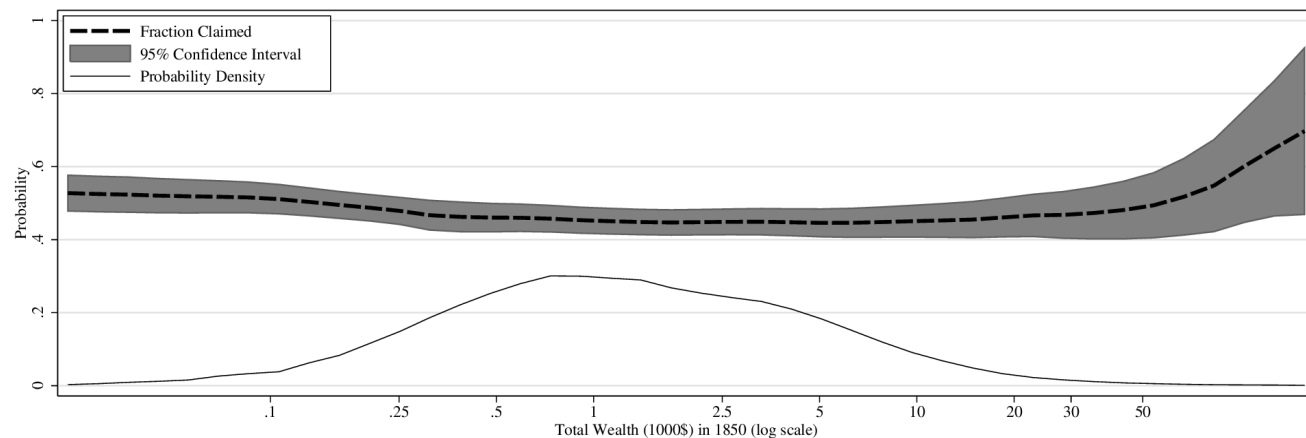
Notes: This figure displays estimates of fraction of parcels claimed as of 1838 (as reported in Smith, 1838) versus total 1850 wealth for the subsample of lottery winners only. (By definition, lottery losers were not assigned parcels, so parcel characteristics are unavailable for the full sample.) The base sample consists of all household heads in the 1850 census with children born in Georgia during the three years prior to the Cherokee Land Lottery of 1832 and no children born outside of Georgia during the same period. An observation is coded as a lottery winner ("treated") if he is a unique match to a name found on the list of winners published by Smith (1838). The dependent variable (on the y axis) is a dummy for whether the lotter winner's parcel had been claimed by the time of publication of the Smith (1838) list. The independent variable (on the x axis) in this figure is total measured wealth in 1850, the sum of real-estate wealth and slave holdings. Real-estate wealth is as reported in and transcribed from the manuscript pages of the 1850 Census of Population. Slave wealth was estimated by linking the household to the 1850 Slave Schedule and imputing a market value of slave holdings adjusting for the reported ages and gender of slaves on the Schedule. The sample size for this figure is 1607. The solid line displays a local-polynomial smoothed estimate of the mean claim rate for each level of total wealth, and the short-dashed lines denote 95% confidence intervals. For reference, the long-dashed line presents the probability distribution function for log total wealth (excluding the imputation for those with zero wealth). These curves are estimated using, respectively, the "lpoly" and "kdensity" commands in Stata version 12. Data sources and additional variable and sample definitions are found in the text and in the appendices.

Table 1: Summary Statistics

	(1)	(2)	(3)	(4)
	Whole Sample	Lottery "Losers"	Lottery "Winners"	p-value, mean difference [N]
Panel A: Lottery Winner or Loser				
Dummy for unique match to Smith (1838) list	0.124 (0.329)	0	1	---
Dummy for match to Smith (1838), deflated to 1/n in case of ties	0.155 (0.335)	0.037 (0.121)	0.995 (0.053)	0.000 [14375]
Panel B: Predetermined Outcomes				
Age, in years	51.2 (8.5)	51.3 (8.5)	50.9 (8.6)	0.122 [14375]
Born in Georgia	0.497 (0.500)	0.497 (0.500)	0.498 (0.500)	0.889 [14375]
Born in South Carolina	0.212 (0.408)	0.210 (0.407)	0.222 (0.416)	0.263 [14375]
Born in North Carolina	0.180 (0.384)	0.180 (0.384)	0.178 (0.383)	0.804 [14375]
Number of Georgia-born children in the three years prior to the lottery	1.333 (0.542)	1.333 (0.541)	1.332 (0.542)	0.910 [14375]
Cannot read and write	0.147 (0.354)	0.147 (0.354)	0.142 (0.350)	0.593 [14340]
Number of letters in surname	6.19 (1.61)	6.20 (1.62)	6.13 (1.51)	0.072 [14375]
Frequency with which surname appears in sample	36.2 (46.3)	36.3 (46.9)	35.3 (41.9)	0.380 [14375]
Surname begins with "M" or "O"	0.101 (0.302)	0.101 (0.301)	0.104 (0.305)	0.740 [14375]
Mean wealth of families in Georgia with same surname	1186.3 (1257.8)	1185.4 (1288.4)	1192.3 (1021.8)	0.811 [13848]
Median wealth of families in Georgia with same surname	289.1 (716.6)	290.0 (717.6)	282.7 (709.9)	0.686 [13848]
Mean illiteracy of adults in Georgia with same surname	0.219 (0.107)	0.219 (0.108)	0.218 (0.098)	0.648 [13848]

Notes: Table continues on next page.

Table 1 (continued): Summary Statistics

	(1)	(2)	(3)	(4)
	Whole Sample	Lottery "Losers"	Lottery "Winners"	p-value, mean difference [N]
Panel C: Measures of Wealth in 1850				
Real-estate wealth	1999.0 (4694.2) {0,650,2000}	1970.8 (4422.0) {0,640,2000}	2198.2 (6290.1) {0,700,2000}	0.068 [13094]
Slave weath	1339.1 (5761.0) {0,0,0}	1297.3 (5329.7) {0,0,0}	1635.3 (8189.0) {0,0,326}	0.021 [14375]
Total wealth (sum of wealth in real estate and slaves)	3323.7 (8691.0) {100,800,3000}	3245.5 (7952.9) {100,800,3000}	3876.5 (12734.4) {100,1000,3550}	0.006 [13094]

Notes: This table displays summary statistics for the main data used in the present study. The sample consists of all household heads in the 1850 census with children born in Georgia during the three years prior to the Cherokee Land Lottery of 1832 and no children born outside of Georgia during the same period. Column (1) presents means and standard deviations (in parentheses) of variables for this entire sample. We use two measures of whether the person won land in the drawing for the Cherokee Land Lottery of 1832. The first measure is coded to 1 if that person is a unique match to a name found on the list of winners published by Smith (1838); anyone else in the sample is coded to zero. The second measure takes individuals that "tie" for a match to the Smith list with (n-1) other observations and recodes them to 1/n. These variables are summarized in Panel A. Columns (2) and (3) present means and standard deviations of variables for the subsamples of, respectively, lottery losers and winners (decomposed using the first measure). Column (4) presents the p-value on the test of zero difference in means between the subsamples of losers and winners. In square brackets, we report the sample size used for this test, although the test involving children or surnames adjust for the clustering of errors. With the exception of measure the surname length, we use the Soundex version of each name to account for minor spelling differences. For the variables that are means by surname, we use the 1850 100% census file to construct average fertility, school attendance, and real-estate wealth among Georgia-resident households for each (soundex) surname. (Those individuals that appear in our lottery-eligible sample are excluded from the construction of these indices.) Real-estate wealth is as reported on and transcribed from the manuscript pages of the 1850 Census of Population. Slave wealth was estimated by linking the household to the 1850 Slave Schedule and imputing a market value of slave holdings adjusting for the reported ages and gender of slaves on the Schedule. Numbers in curly brackets in Panel C are the 25th, 50th, and 75th percentiles of the respective wealth measures. Data sources and additional variable and sample definitions are found in the text and in the appendices.

Table 2: Lottery Status versus Total Wealth in 1850

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Binary Match to Smith (1838)						
Levels	723.4	714.4	710.1	632.4	593.6	855.1	677.8
	(325.3) **	(319.5) **	(325.4) **	(311.2) **	(352.3) *	(348.9) **	(385.6) *
Natural Logs	0.127	0.128	0.126	0.121	0.098	0.142	0.098
	(0.043) ***	(0.043) ***	(0.043) ***	(0.043) ***	(0.049) **	(0.045) ***	(0.053) *
	Panel B: Allow for 1/n Matching to Smith (1838)						
Levels	777.7	749.8	762.5	660.2	572.0	922.7	645.6
	(310.7) **	(303.0) **	(310.5) **	(300.2) **	(335.6) *	(331.3) ***	(332.6) **
Natural Logs	0.146	0.147	0.146	0.135	0.112	0.158	0.110
	(0.042) ***	(0.042) ***	(0.042) ***	(0.042) ***	(0.049) **	(0.045) ***	(0.053) **
Additional Fixed-Effect Controls:	None	First letter of surname	Number of letters in surname	Freq. of surname in sample	Surname	Given name	Surname; Given name

Notes: This table displays OLS estimates of equation (1) in the text. Each cell presents results from a separate regression, and only the coefficient on winning the lottery is reported. The sample consists of all household heads in the 1850 census with children born in Georgia during the three years prior to the Cherokee Land Lottery of 1832 and no children born outside of Georgia during the same lapse of time. The dependent variable in this table is total measured wealth. This variable is the sum of real-estate wealth, which was reported to enumerators on the population schedule, and slave wealth, which was computed from the slave schedule. Whether this variable enters the specification in levels or natural logs is indicated by the row headings. The sample size the levels regressions is 13094, and is 10013 for the logs regressions. The baseline specification also includes dummies for age and for (state x county) of residence. Additional sets of fixed effects are included in columns 2-7, as reported in the bottom row. In columns 4-7, we use the Soundex version of each name to account for minor spelling differences. Two variables are constructed to measure whether the person was a lottery winner. The first measure, used in Panel A, is coded to 1 if that person is a unique match to a name found on the list of winners published by Smith (1838); anyone else in the sample is coded to zero. The second measure, which is used in Panel B, takes individuals that "tie" for a match to the Smith list with (n-1) other observations and recodes them to 1/n. A single asterisk denotes statistical significance at the 90% confidence level; double 95% and triple 99%. Data sources and additional variable and sample definitions are found in the text and in the appendices.

Table 3: Lottery Status vs. Various Wealth Measures in 1850

	(1)	(2)
	Binary match to Smith	Allow 1/n match
Panel A: Total Wealth (N=13094)		
Levels	723.4 (325.3) **	777.7 (310.7) **
Levels, Adjusted for Truncation of Lower Tail	723.6 (325.2) **	777.6 (310.6) **
Natural Logs (N=10013)	0.127 (0.043) ***	0.146 (0.042) ***
Natural Logs, Adjusted for Truncation of Lower Tail	0.121 (0.049) ***	0.142 (0.049) ***
Panel B: Quantiles of Total Wealth (N=13094)		
Levels, 25th percentile	0.0 (32.7)	0.0 (25.7)
Levels, 50th percentile (median)	200.0 (39.8) ***	200.0 (26.7) ***
Levels, 75th percentile	550.0 (109.7) ***	511.8 (116.1) ***
Levels, 95th percentile	1503.7 (1114.3)	2022.1 ** (1076.6)
Panel C: Real-Estate Wealth (N=13094)		
Levels	286.3 (159.7) *	295.2 (154.4) **
Indicator for Wealth At Least $100	0.002 (0.011)	-0.003 (0.010)
Panel D: Slave Wealth (N=14375)		
Levels	391.8 (201.8) *	431.8 (192.7) **
Indicator for Wealth > 0	0.039 (0.011) ***	0.052 (0.011) ***

Notes: This table displays OLS estimates of equation (1) in the text, except for Panel B where a quantile regression is used. Each cell presents results from a separate regression, and only the coefficient on winning the lottery is reported. The specification also includes dummies for age and for (state x county) of residence. The sample consists of all household heads in the 1850 census with children born in Georgia during the three years prior to the Cherokee Land Lottery of 1832 and no children born outside of Georgia during the same period. The dependent variables are various measures of wealth, as indicated in the Panel descriptions and row headings. The wealth variable in Panels A and B is the sum of real-estate wealth, which was reported to enumerators on the population schedule, and slave wealth, which was computed from the slave schedule. Panel C reports results for real-estate wealth. Enumerators in 1850 were instructed to record such wealth only if were at least $100, which is the cutoff we used for analyzing the extensive margin in Panel C as well as for estimating the truncated normal used to impute values below $100 in the truncation adjustment in Panel A. Two variables are constructed to measure whether the person was a lottery winner. The first measure, used in Column (1), is coded to 1 if that person is a unique match to a name found on the list of winners published by Smith (1838); anyone else in the sample is coded to zero. The second measure, used in Column (2), takes individuals that "tie" for a match to the Smith list with (n-1) other observations and recodes them with to 1/n. A single asterisk denotes statistical significance at the 90% confidence level; double 95% and triple 99%. Data sources and additional variable and sample definitions are found in the text and in the appendices.

Table 4: Falsification test using South Carolina instead of Georgia to construct sample

Dependent variables:	(1) Born in Georgia	(2) Born in South Carolina	(3) Number Ga.-born children, pre-lottery	(4) Number SC-born children, pre-lottery	(5) Resides in Old Cherokee County	(6) Real-estate Wealth ($)	(7) Real-estate Wealth >$100	(8) Real-estate Wealth >$3000
Lottery-status variables:								
Panel A: South Carolina, basic specification								
Dummy for unique match to Smith (1838) list	0.001 (0.004)	-0.017 (0.013)		-0.019 (0.019)	0.005 (0.007)	-41.1 (236.2)	0.001 (0.016)	0.007 (0.013)
Dummy for match to Smith (1838), deflated to 1/n in case of ties	0.004 (0.004)	-0.016 (0.012)		-0.002 (0.018)	0.010 (0.007)	-15.6 (232.0)	0.001 (0.015)	0.004 (0.012)
Panel B: South Carolina, including surname fixed effects								
Dummy for unique match to Smith (1838) list	0.000 (0.004)	-0.003 (0.014)		-0.016 (0.021)	0.006 (0.008)	-93.3 (229.4)	0.005 (0.016)	0.012 (0.012)
Dummy for match to Smith (1838), deflated to 1/n in case of ties	0.003 (0.004)	-0.004 (0.014)		-0.004 (0.020)	0.009 (0.008)	-72.6 -(72.6)	0.015 (0.015)	0.012 (0.014)
Panel C: Analogous results for Georgia, dummy for unique match to Smith list								
Basic specification	-0.004 (0.012)	0.014 (0.011)	0.002 (0.014)		0.022 (0.008) ***	295.2 (154.4) *	0.002 (0.011)	0.020 (0.009) **
Control for surname fixed effects	0.001 (0.014)	0.012 (0.012)	0.009 (0.016)		0.023 (0.008) ***	315.8 (146.8) ***	0.002 (0.011)	0.021 (0.010) **

Notes: This table displays estimates of equation (1) in the text. Each cell presents results from a separate regression, and only the coefficient on "winning the lottery" is reported. The sample for Panels A and B consists of all households in the 1850 census with children born in South Carolina during the three years prior to the Cherokee Land Lottery of 1832 and no children born outside of Georgia during the same period. The sample for Panel C, which repeats some results from earlier tables, uses households with Georgia-born children in this same window. We use two measures of whether the person won land in the drawing for the Cherokee Land Lottery of 1832. The first measure is coded to 1 if that person is a unique match to a name found on the list of winners published by Smith (1838); anyone else in the sample is coded to zero. The second measure takes individuals that "tie" for a match to the Smith list with (n-1) other observations and recodes them to 1/n. Note that these are spurious measures for the South-Carolina samples because the birthplace of their children implies that they lived outside of Georgia at some point during the three years prior to the lottery, and were therefore ineligible. The basic specification also includes dummies for age. The other specification used includes fixed effects for surname (soundex). The dependent variables are indicated in the column headings. A single asterisk denotes statistical significance at the 90% confidence level; double 95% and triple 99%. All standard errors (shown in parentheses) are heteroskedasticity robust and clustered on the lottery-eligible man in there are multiple observations per household. Data sources and additional variable and sample definitions are found in the text and in the appendices.

Table 5: Interaction with Own Illiteracy and Surname Averages

Additional variables constructed from surname averages in 1850 Census data from Georgia (excluding own realizations)

|	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	Baseline	Mean wealth		Mean log wealth		Log median wealth		Fertility		In school, ages 5-15		Literacy rate		Own illiteracy	
Panel A: Total wealth, Levels, Adjusted for Truncation of Lower Tail															
Dummy for unique match to Smith (1838) list	716 (232)***		717 (298)**		707 (298)**		710 (298)**		682 (283)**		709 (297)**		713 (298)**		826 (251)***
Additional variable		0.158 (0.067)**	0.144 (0.062)**	821 (145)***	684 (150)***	443 (86)***	361 (87)***	126 (223)	182 (224)	1438 (433)***	1260 (469)***	1802 (932)*	1631 (766)**	-2186 (227)***	-2084 (241)***
Interaction term (using z score for surname variables)			44 (216)		-12 (241)		25 (224)		346 (304)		42 (198)		-397 (390)		-811 (650)
Number of observations	13036	12553	12553	12553	12553	12553	12553	12896	12896	12417	12417	12595	12595	13005	13005
Panel B: Total wealth, Natural Logs, Adjusted for Truncation of Lower Tail															
Dummy for unique match to Smith (1838) list	0.115 (0.048)**		0.120 (0.051)**		0.115 (0.051)**		0.117 (0.051)**		0.117 (0.050)**		0.121 (0.051)**		0.118 (0.051)**		0.115 (0.051)**
Additional variable		4.04E-5 (1.75E-5)**	3.83E-6 (1.63E-6)**	0.282 (0.035)***	0.236 (0.036)***	0.164 (0.022)***	0.140 (0.022)***	-0.063 (0.053)	-0.004 (0.052)	0.444 (0.117)***	0.411 (0.119)***	0.911 (0.168)***	0.679 (0.172)***	-0.974 (0.046)***	-0.971 (0.049)***
Interaction term (using z score for surname variables)			0.083 (0.050)*		0.043 (0.050)		0.009 (0.049)		-0.028 (0.063)		-0.056 (0.051)		0.038 (0.049)		-0.024 (0.132)
Number of observations	13036	12553	12553	12553	12553	12553	12553	12896	12896	12417	12417	12595	12595	13005	13005
Panel C: Total wealth Greater than $5000															
Dummy for unique match to Smith (1838) list	0.029 (0.009)***		0.029 (0.010)***		0.028 (0.009)***		0.028 (0.009)***		0.027 (0.009)***		0.028 (0.010)***		0.028 (0.009)***		0.034 (0.010)***
Additional variable		6.96E-6 (3.23E-6)**	7.09E-6 (3.26E-6)**	0.042 (0.007)***	0.038 (0.007)***	0.022 (0.004)***	0.019 (0.004)***	0.017 (0.009)*	0.019 (0.009)**	0.071 (0.022)***	0.047 (0.023)**	0.123 (0.030)***	0.100 (0.032)***	-0.108 (0.009)***	-0.104 (0.009)***
Interaction term (using z score for surname variables)			0.000 (0.010)		0.004 (0.009)		0.004 (0.010)		0.012 (0.013)		0.001 (0.010)		-0.005 (0.009)		-0.037 (0.025)
Number of observations	14306	13780	13780	13780	13780	13780	13780	14144	14144	13615	13615	13824	13824	14271	14271

Notes: This table displays OLS estimates of equations (2) and (3) in the text. This table departs from previous ones in the use of surname-specific characteristics to proxy for differences across extended families ("dynasties") in child and wealth outcomes. We use the 1850 100% census file to construct average fertility, school attendance, and real-estate wealth among Georgia-resident households for each (soundex) surname. (Those individuals that appear in our lottery-eligible sample are excluded from the construction of these indices.) Each panel/column presents results from a separate regression. In addition to the displayed coefficients, regressions include dummies for age and for (state x county) of residence. The base sample for these regressions consists of all households in the 1850 census with children born in Georgia during the three years prior to the Cherokee Land Lottery of 1832 and no children born outside of Georgia during the same period. Households without a corresponding surname in the database of surname averages are excluded from the regressions. The dependent variables are indicated in the column headings. A household is coded as a lottery winner if the head is a unique match to a name found on the list of winners published by Smith (1838); anyone else in the sample is coded to zero. A single asterisk denotes statistical significance at the 90% confidence level; double 95% and triple 99%. All standard errors (shown in parentheses) are heteroskedasticity robust and clustered on the surname level to account for correlation induced by the surname-averages. Data sources and additional variable and sample definitions are found in the text and in the appendices.

Appendix Figure 1: Old Cherokee County and the 1850 Locations of the Sample
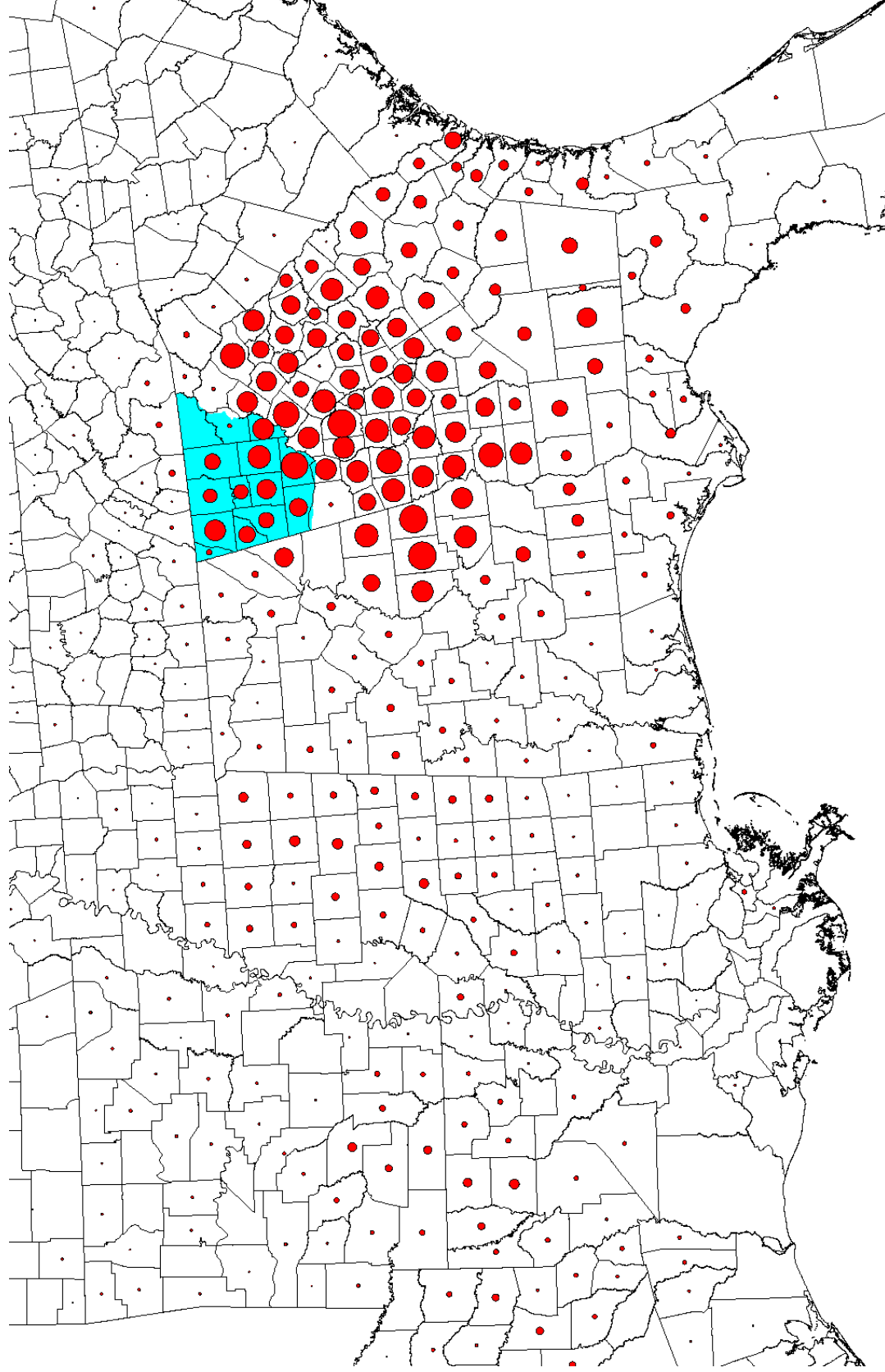
Notes: This figure displays a map of the southeastern United States with information on the location (by county) in 1850 of the lottery-eligible households in our main sample. Black lines indicate the 1850 county boundaries, drawn from the NHGIS database. The area shaded in blue in northwest Georgia denotes old Cherokee County, which was allocated by the Cherokee Lottery of 1832. The sample consists of all household heads in the 1850 census with children born in Georgia during the three years prior to the Cherokee Land Lottery of 1832 and no children born outside of Georgia during the same period. If households in our sample are resident in a county in 1850, we place a red dot at the county centroid. The area of a dot is proportional to the number of sample households resident in that county. A minor fraction of sampled households resides in counties outside the frame of this map. Such households are included in the econometric analysis, but we zoom in on this region to make the features legible in the map. Data sources and additional variable and sample definitions are found in the text and in the appendices.

Appendix Figure 2: Replicate Figure 1 (Quantile Regressions) Using 1/n Match to Smith Instead
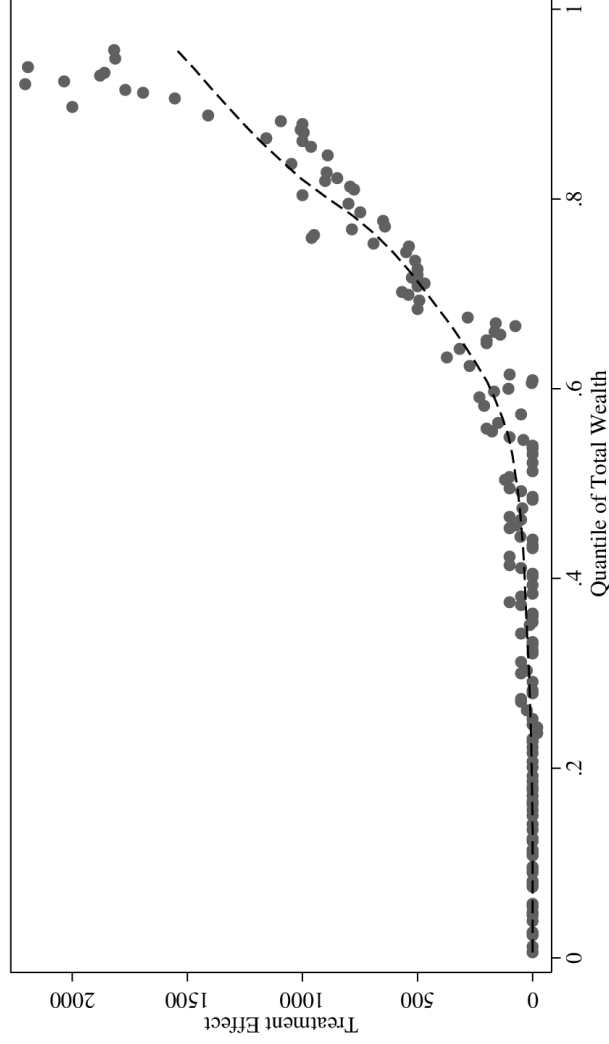
Notes: This figure displays estimates from a quantile regression of the effect of winning the lottery ("treatment") on total wealth in 1850. The points are the quantile-specific estimates of the treatment effect at various quantile points. The dashed line is a local-polynomial-smoothed (Epanechikov kernel, with a bandwidth of .11) estimate of the treatment effect. The sample consists of all household heads in the 1850 census with children born in Georgia during the three years prior to the Cherokee Land Lottery of 1832 and no children born outside of Georgia during the same period. An observation is coded to a lottery status using the 1/n match procedure as described in the text. The dependent variable in this figure is total measured wealth in 1850, the sum of real-estate wealth and slave holdings. Real-estate wealth is as reported on and transcribed from the manuscript pages of the 1850 Census of Population. Slave wealth was estimated by linking the household to the 1850 Slave Schedule and imputing a market value of slave holdings adjusting for the reported ages and gender of slaves on the Schedule. The sample size for this figure is 13094. Data sources and additional variable and sample definitions are found in the text and in the appendices.

Appendix Figure 3: CDF Differences under Various Specifications
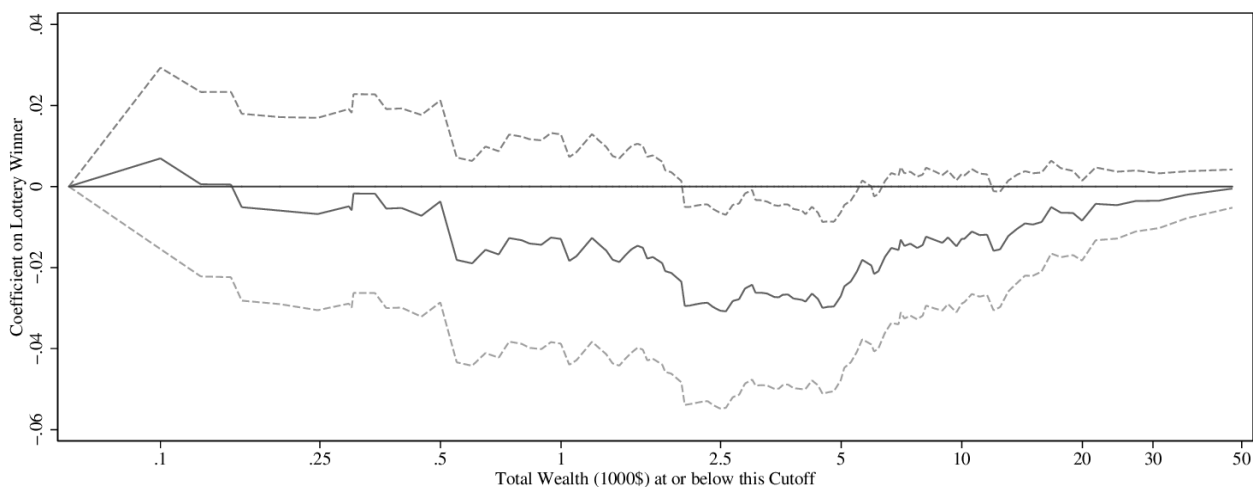
Panel A: Binary Match to Smith List, Baseline Specification (Shown in Figure 2, Panel B)

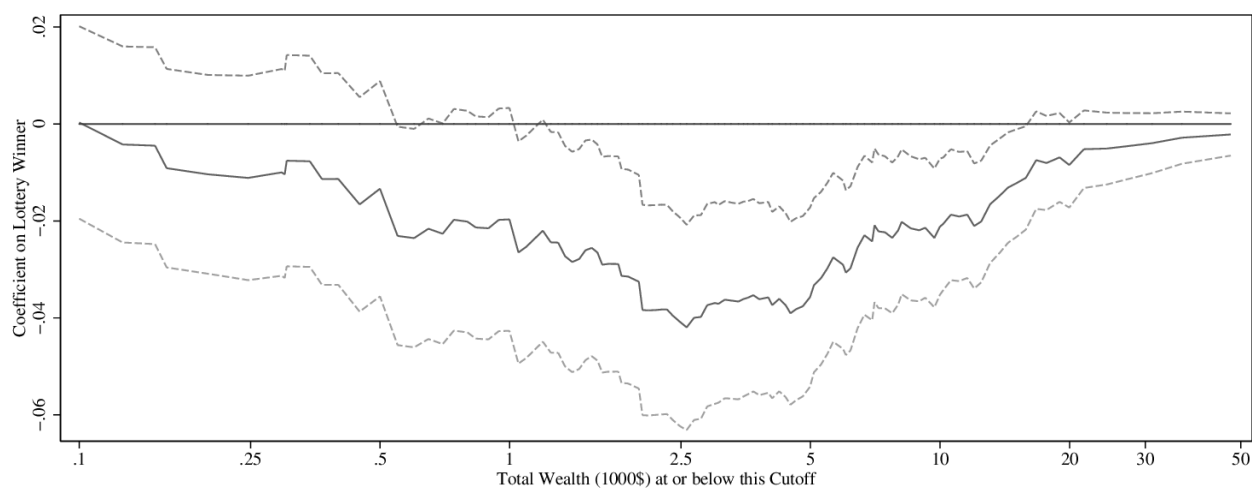*Panel B: Binary Match to Smith List, Bivariate Specification*
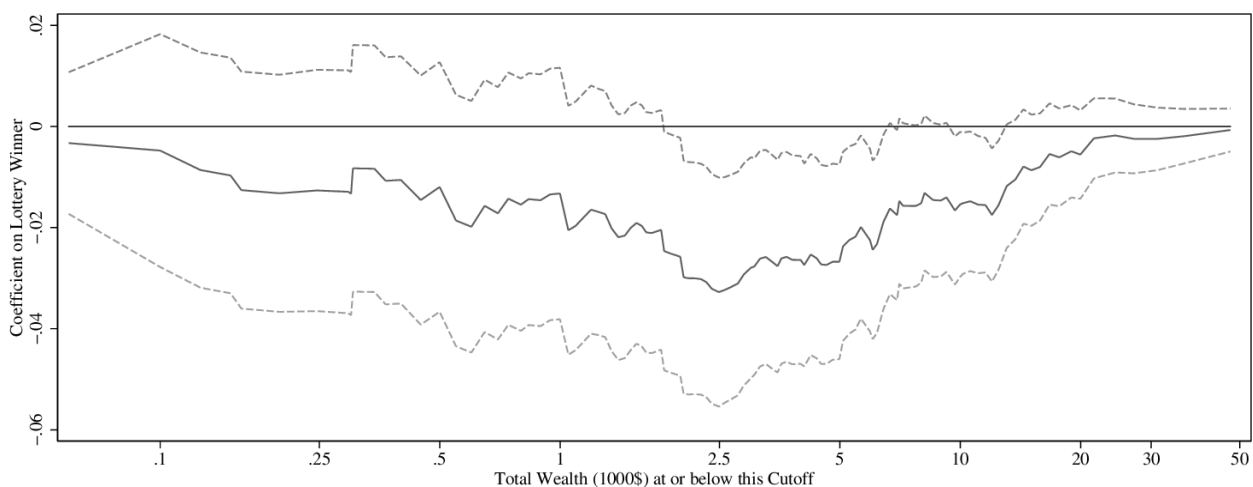
Panel C: Binary Match to Smith List, Baseline Specification with Soundex Fixed Effects
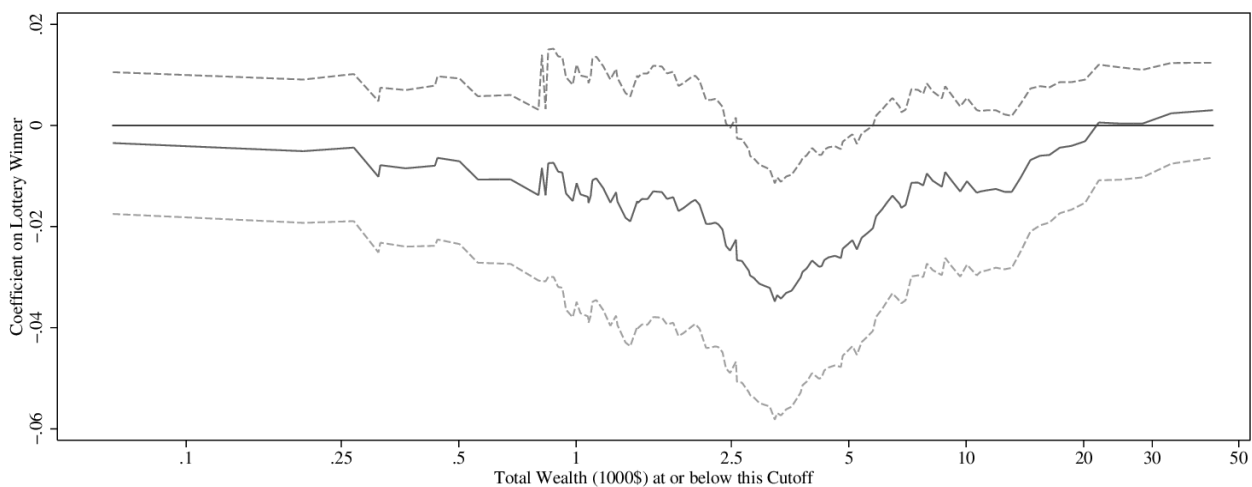

Notes: Figure continues on next page.

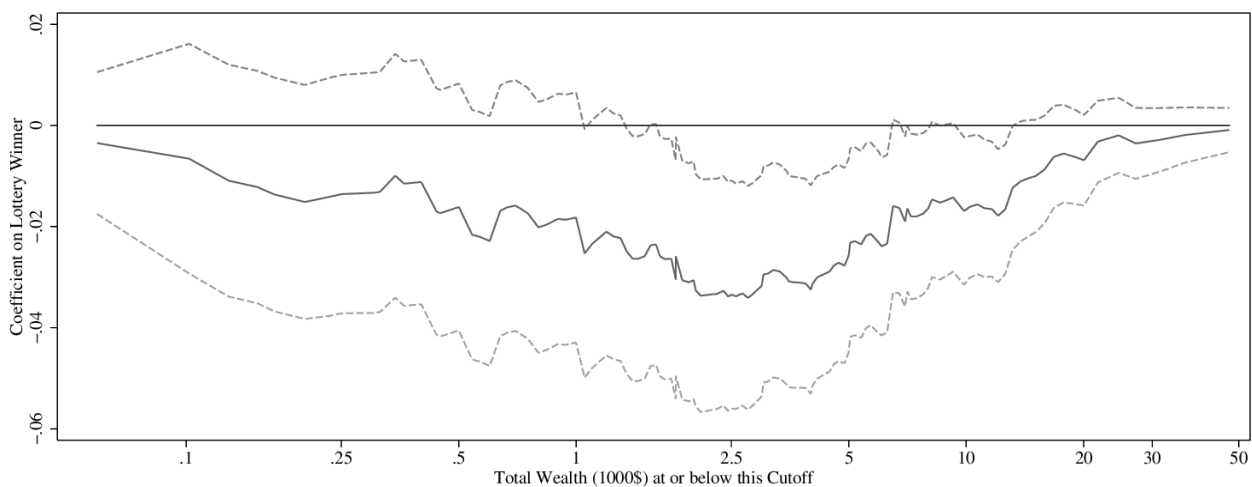Appendix Figure 3 (Continued): CDF Differences under Various Specifications

Panel D: 1/n Match to Smith List, Baseline Specification

*Panel E: Include Capital Imputed by Occupation*

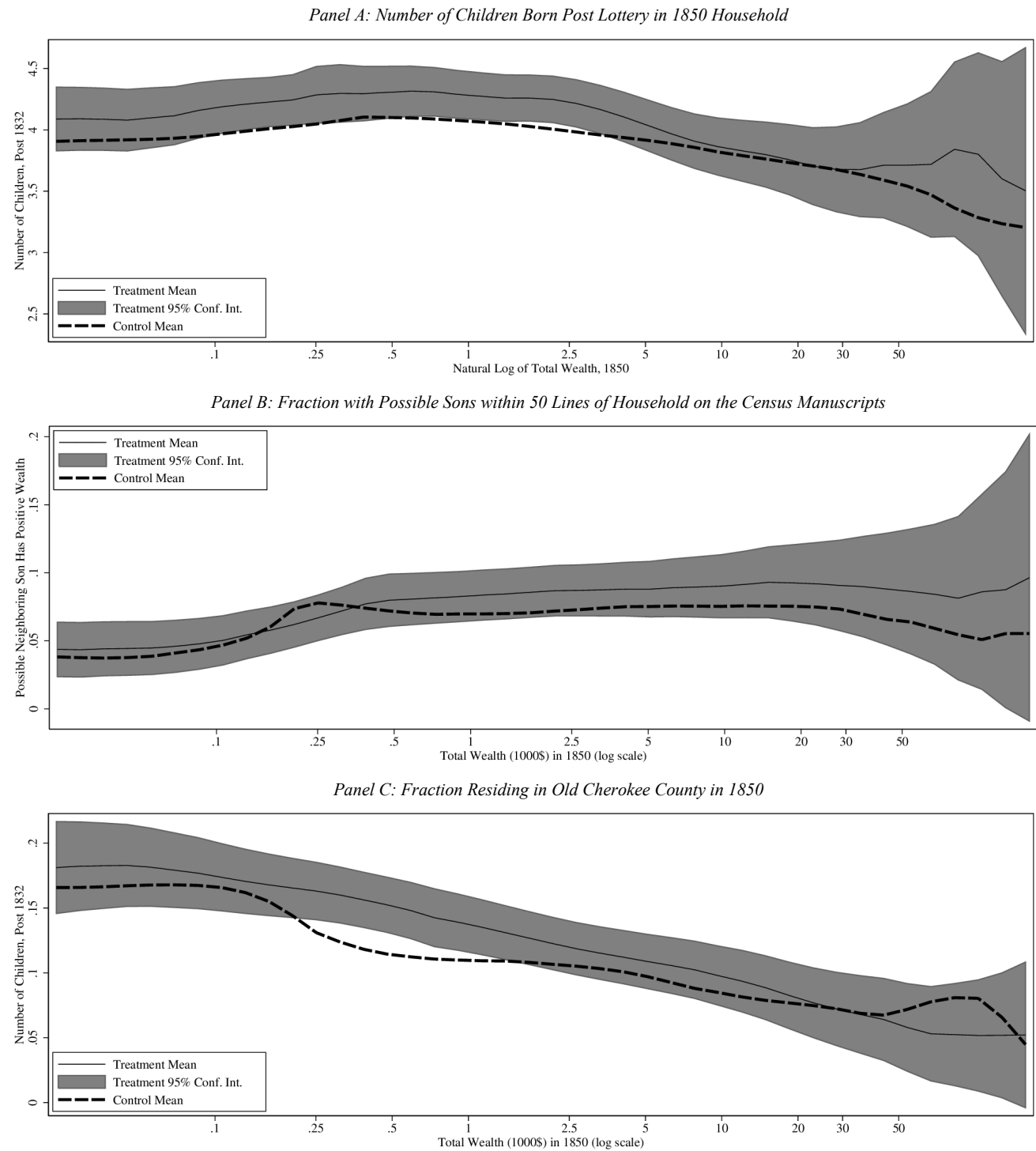*Panel F: Include the Real-Estate Wealth of Possible Nearby Sons*

Notes: This figure displays alternate estimates of the change in cumulative distribution functions in Figure 2, Panel B. The solid lines in each Panel presents the differences in the cumulative distribution function between groups (treatment minus control), estimated using a linear probability model (equation 1) of an indicator for being below 200 quantile cut-points for wealth. Heteroskedasticity-robust 95% confidence intervals are plotted with the dashed lines. See the notes for Figure 2 for definitions of the data and specifications
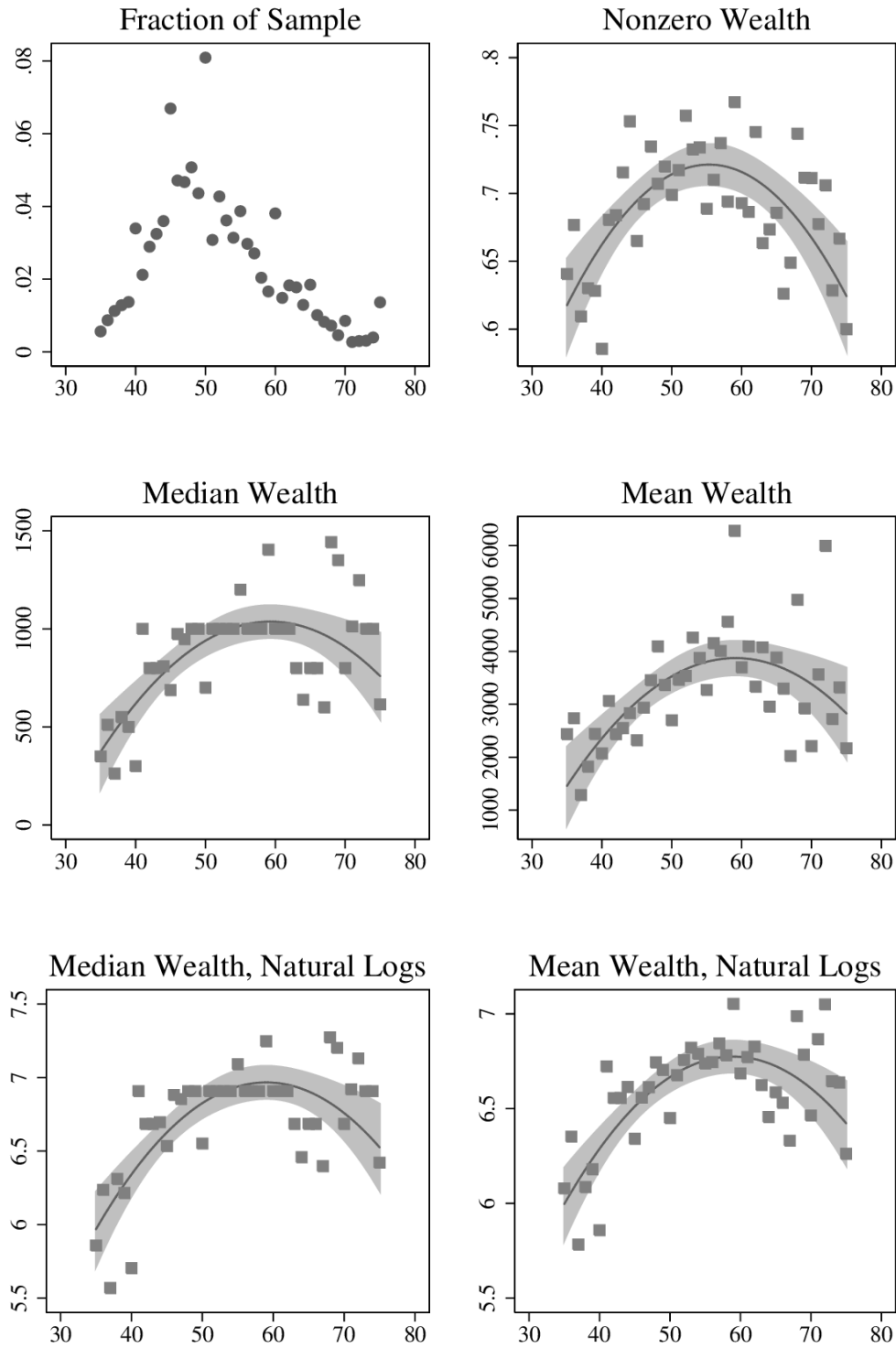
Notes: This figure plots the average total 1850 wealth by age in the sample of lottery eligibles, except for the upper left-hand panel, which displays the fraction of the sample in each age cell. The age-specific averages are denoted with the square symbols. A quadratic fit (plus confidence interval) is displayed with the solid line (and associated shading). The sample and data are defined as in Figure 1-3, except that we do not display results for the handful of observations with ages above 75.
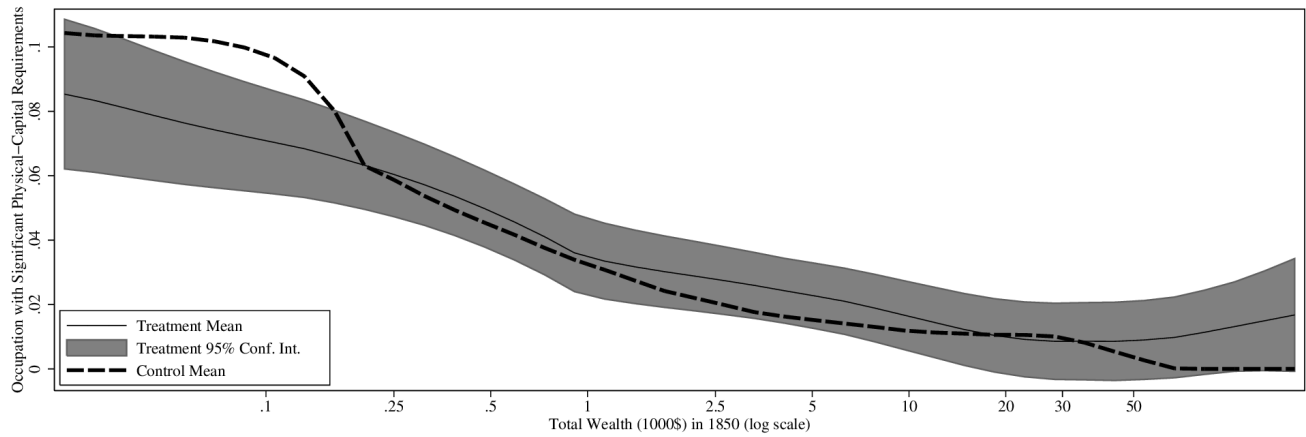
Appendix Figure 5: Various Outcomes versus Realized 1850 Wealth, by Lottery Status

*Panel A: Number of Children Born Post Lottery in 1850 Household*

Panel B: Fraction with Possible Sons within 50 Lines of Household on the Census Manuscripts

Panel C: Fraction Residing in Old Cherokee County in 1850

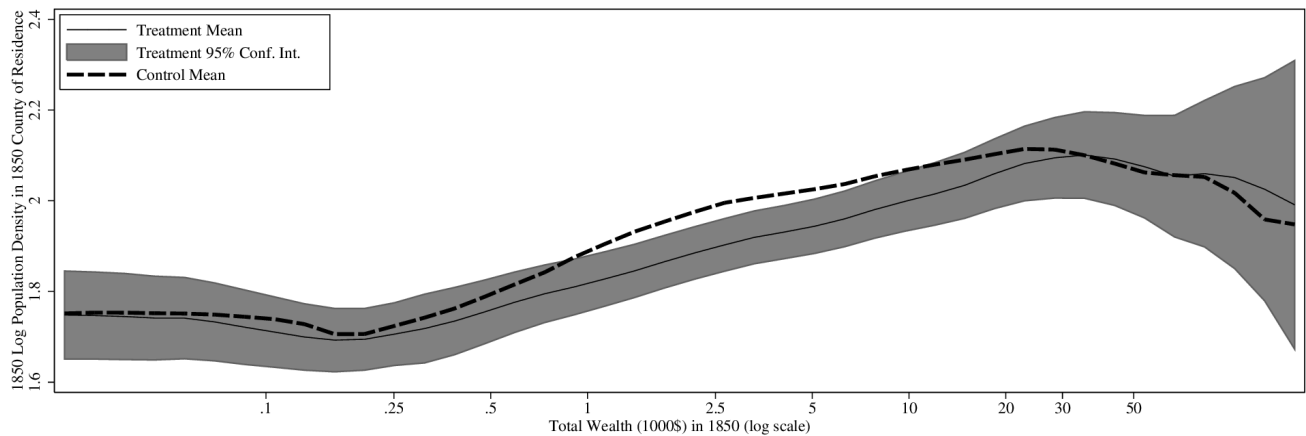Notes: Figure continues on next page.

Panel D: Occupation has Significant Physical-Capital Requirement

Panel E: 1830-50 Change in Log Population Density in 1850 County of Residence
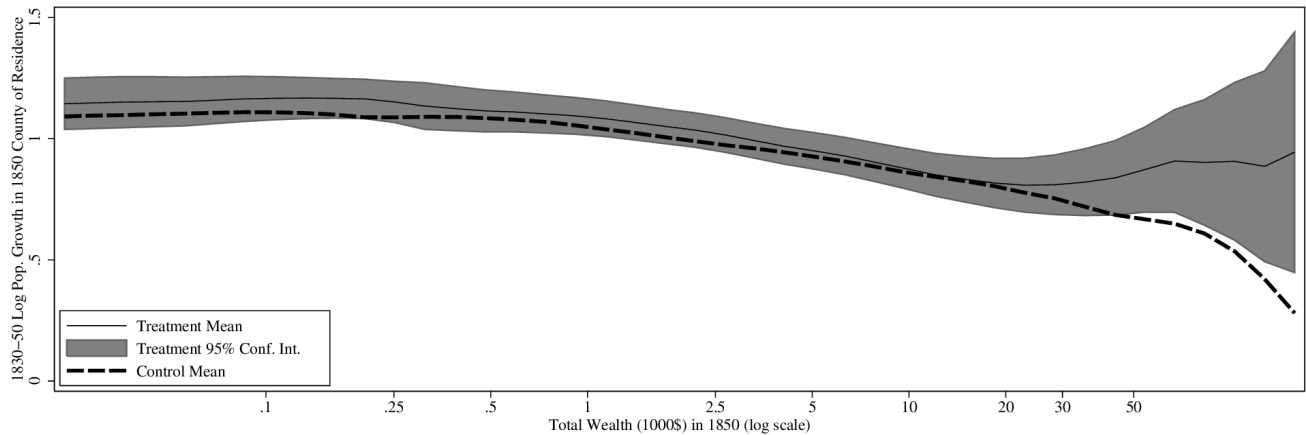
Panel F: 1850 Log Population Density in 1850 County of Residence

Notes: This figure displays estimates for 1850 of the number of children and residence in Old Cherokee County versus total wealth, by lottery status. The base sample consists of all household heads in the 1850 census with children born in Georgia during the three years prior to the Cherokee Land Lottery of 1832 and no children born outside of Georgia during the same period. An observation is coded as a lottery winner ("treated") if he is a unique match to a name found on the list of winners published by Smith (1838). The dependent variable (on the y axis) is an indicated above. The independent variable (on the x axis) in this figure is total measured wealth in 1850, the sum of real-estate wealth and slave holdings. Real-estate wealth is as reported in and transcribed from the manuscript pages of the 1850 Census of Population. Slave wealth was estimated by linking the household to the 1850 Slave Schedule and imputing a market value of slave holdings adjusting for the reported ages and gender of slaves on the Schedule. The solid and dashed display local-polynomial smoothed estimates of the means for each level of total wealth for the treated and controls, respectively. The gray shaded areas denote 95% confidence intervals for the treatment-group conditional mean. These curves are estimated using the "lpoly" command in Stata version 12. Data sources and additional variable and sample definitions are found in the text and in the appendices.

Appendix Table 1: Differences in 1850-County-of-Residence Characteristics by Lottery Status

	(1) Resides in Old Cherokee County	(2) Resides in Georgia	(3) Miles East	(4) Miles North	(5) School Enroll. Rate	(6) Total Fertility Rate (TFR5)	(7) Total Fertility Rate (TFR19)	(8) Log of Farm Value per Acre	(9) Log of Average Farm Size	(10) Log of Improved Land Ratio	(11) Log Slaves per Area	(12) Log Pop. Density in 1850	(13) Log Pop. Density in 1830	(14) Log Fraction Urban	(15) Access to Water Transport	(16) Access to Railroads
Panel A: Basic Specification																
	0.022	0.005	4.320	-4.026	-0.003	0.006	0.011	-0.007	-0.014	-0.017	-0.001	-0.045	-0.111	0.072	-0.007	0.018
	(0.008)***	(0.011)	(3.643)	(2.211)*	(0.003)	(0.004)	(0.012)	(0.021)	(0.017)	(0.017)	(0.026)	(0.029)	(0.052)**	(0.072)	(0.011)	(0.016)
Panel B: Control for Surname Fixed Effects																
	0.022	0.004	4.265	-4.661	-0.004	0.005	0.009	-0.011	-0.005	-0.024	0.000	-0.057	-0.117	0.066	-0.001	0.015
	(0.008)***	(0.013)	(3.997)	(2.306)**	(0.003)	(0.004)	(0.012)	(0.022)	(0.017)	(0.018)	(0.026)	(0.030)*	(0.054)**	(0.062)	(0.011)	(0.016)
Panel C: Basic Specification, Control for Residence in Old Cherokee County																
	---	---	4.654	-5.924	-0.004	0.005	0.007	-0.012	-0.004	-0.015	0.008	-0.048	-0.101	0.072	-0.003	0.018
			(4.343)	(2.781)**	(0.003)	(0.004)	(0.012)	(0.022)	(0.016)	(0.017)	(0.025)	(0.029)	(0.052)*	(0.072)	(0.011)	(0.015)
Panel D: Control for Surname Fixed Effects, Control for Residence in Old Cherokee County																
	---	---	4.560	-6.569	-0.005	0.004	0.005	-0.016	0.004	-0.022	0.009	-0.058	-0.108	0.066	0.002	0.014
			(4.727)	(2.761)**	(0.003)*	(0.004)	(0.012)	(0.022)	(0.016)	(0.019)	(0.026)	(0.030)*	(0.054)**	(0.062)	(0.011)	(0.015)

Notes: This table displays OLS estimates of equation (1) in the text. Each cell presents results from a separate regression, and only the coefficient on winning the lottery is reported. The basic specification (shown in Panel A) also includes dummies for age. The specification used in Panel B includes fixed effects for surname (soundex). Panels C and D repeat specifications from Panels A and B, respectively, but also include a dummy variable for residence in Old Cherokee County. The sample consists of all household heads in the 1850 census with children born in Georgia during the three years prior to the Cherokee Land Lottery of 1832 and no children born outside of Georgia during the same period. The dependent variables are the locational county-specific characteristics denoted in the column headings. Location data used in Columns 3 and 4 are county centroids computed from NHGIS data, and are converted into miles east or north of the NAD83 reference point in central Oklahoma. County data used in Columns 5-14 are drawn from ICPSR study #2896. The number of observations for Columns 1-4 is 14375 and for Columns 5-14 is 14237 because of missing data for some (mostly unorganized) counties. A household is coded as a lottery winner if the head is a unique match to a name found on the list of winners published by Smith (1838); anyone else in the sample is coded to zero. A single asterisk denotes statistical significance at the 90% confidence level; double 95% and triple 99%. All standard errors are heteroskedasticity robust and, in Columns 3-14, clustered at the (state x county) level to account for multiple observations per county. Data sources and additional variable and sample definitions are found in the text and in the appendices.

Appendix A: A Partial Characterization of Treatment Effects Across the Counterfactual Distribution

In this section, we present a partial characterization of mean treatment effects across the distribution of counterfactual outcomes. In plainer language, what were the returns to winning for those lottery losers who ended up with a certain 1850 wealth? Recall that the quantile regressions answer this questions only under the assumption of rank stability. This assumption seems unpalatable, particularly in light of the assertion that some of the poor could make profitable investments if only they could raise capital. Instead, we attempt this analysis with minimal assumptions about the mapping from one's outcome assuming no treatment and one's outcome if treated. (It has come to our attention that this exercise can be called a partial-identification strategy.)

Let the potential outcomes for individual i's total wealth be w_{1i} and w_{0i}, where the subscript 1 denotes his 1850 wealth if he were treated with lottery winning and the subscript 0 denotes his 1850 wealth if he did not win the lottery. Note that one of these variables is counterfactual: we only observe one of the potential outcomes for a given person, who is either treated or not. Nevertheless, we can postulate that they exist and, furthermore, that the potential outcomes can be characterized by a joint distribution function, $F(w_1, w_0)$ and marginal distributions $g(w_1)$ and $h(w_0)$.

For the remainder of this appendix, we consider a discretized version of the marginal potential-outcomes distributions (g and h) that can be summarized with the vectors x_1 and x_0. The joint distribution of the w implies a Markov-process mapping that relates the discretized marginal distributions:

$$x_1' = x_0' \ P$$

where P is the Markov matrix of conditional probabilities. Even with the normalization that $\vec{1} = P\vec{1}$, this is a grossly underdetermined system, with m^2 unknowns and only $2m$ equations. The elements of a Markov matrix being probabilities, it is also the case that $0 \leq P \leq 1$, where the inequalities refer to each element of P. This adds another $2m^2$ constraints, although at most m^2 can be binding.

The vectors x are also unobserved because they depend on both the potential outcomes that we can observe and on those that we cannot. However, because treatment was randomly assigned, the expected value of the discretized marginals are equivalent between the potential outcomes of the whole sample and realized outcomes of the treatment group. Thus, we use the treatment-group distribution to estimate x_1. Similarly, we use the control-group distribution to estimate x_0.

We can characterize the bounds on mean treatment effects using these equations. Note that

this system is linear in the elements of P. Therefore we can transform it into a linear-programming problem that searches over the feasible set of possible P to find the extrema of some linear transformation of P. The expected treatment effect (conditional on the untreated potential outcome) is itself linear in the elements of P.

These results are presented in Appendix Table 2. We discretize the data into M bins that cover the extrema of the total-wealth data. The bins are of equal width in levels to account for the expected value of lottery winnings being a shock in levels rather than logs. We start with a grid of 100 bins (results shown in Panel A), but also verify that our results are similar using grids of 50 or 200 (results shown in Panels B and C, respectively). The rows of each Panel presents results for various sets of constraints on P. In each row, we report on whether the linear program has a feasible solution in the full sample (Column 1) and in what fraction of 1000 bootstrapped[42] re-samples (Column 2). In the remaining columns, we report the lower and upper bounds on the mean treatment effect for various subsets of the counterfactual, untreated state (i.e., for ranges of w_{0i}). We consider mean treatment effects above and below \$800 (roughly the median of total wealth) as well as below \$500 and below \$400.

The first rows, labelled "Basic only", of each Panel displays the results from imposing only the constraints above. These are minimal bounds and therefore a feasible solution is found. As can be see in Columns 3–4, the bounds on returns for this case are quite wide. The upper bound on the treatment effect for the lower tail is around \$9000, while the lower bound is in the single digits. The gap between upper and lowers bounds for the above-\$800 range is about as wide, but is shifted down by around \$6000. This basic restriction rules out very little in terms of explanations for the similarity of the lower tails between control and treatment.

The next case, denoted "no worse off" in the second rows of each Panel, attempts to restrict the treatment to be weakly positive, in an *ex post* sense. In other words, the constraint on P is that everyone who was treated end up with higher wealth than they would have had if untreated.[43] This restriction, it turns out, is not feasible in the main data, nor in more than 99% of the bootstrapped samples. We do not bother reporting the implied bounds for this case because this constraint would

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[42]The bootstrap is stratified by treatment status, so that the size of control and treatment groups remains constant throughout the simulation.

[43]For example, if we discretized using 5 bins, the modified upper-bound restriction would be

$$P \leq \begin{bmatrix} 1 & 1 & 1 & 1 & 1 \\ 0 & 1 & 1 & 1 & 1 \\ 0 & 0 & 1 & 1 & 1 \\ 0 & 0 & 0 & 1 & 1 \\ 0 & 0 & 0 & 0 & 1 \end{bmatrix}$$

with the diagonal and above-diagonal elements of P allowed to be up to unity, but the below-diagonal elements pinned at zero.

seem to be violated in the data.

A related question to ask is what are the minimal and maximal fractions of the joint distribution of potential outcomes that are below the diagonal of P? The answer is that we can obtain feasible solutions if as few as 0.2% are worse off from treatment. But we can also obtain feasible solutions if as many as 76.6% are worse off from treatment.

In any event, this "no worse off" constraint may not be as appealing on *a priori* grounds in that lottery winners might take on more risk and therefore some might end up worse off than if they had instead lost the lottery. A more sensible restriction might be that lottery winners were weakly better off, when viewed from the perspective of moments after the lottery occurring. We test this idea in the third rows of the Panels of Appendix Table 2. There we report results from restricting the expected return from each and every grid cell to be (weakly) positive. There exists feasible solutions about 80% with this constraint applied. Also, by restricting the degree of churn from above, the upper bound for the mean treatment effects is reduced in the lower tail of the counterfactual, untreated distribution. For example, the highest feasible mean treatment effect is $1505 for those who would had a potential outcome of less than $500 in the untreated state (Panel A, Column 5). This upper bound on treatment is greater than our estimate above of the average parcel value won, but only by a factor of 1.67 or a log difference of 0.51. Divided over 18 years, this reflects a maximal difference in returns of 2.85% *per annum*.

Finally, we impose the restriction that no grid-cell's expected benefit of treatment be greater than $2000. We can find feasible solutions in this case for both the original data and for 100% of the bootstrapped samples. Unfortunately, the results are not particularly informative about the maximal returns in the lower tail; the upper bound that comes out is simply the upper bound that we fed in.

In conclusion, with only basic assumptions, we cannot place sharp bounds on the effect of treatment for those who would have ended up in the lower tail if untreated. Put another way, with few restrictions, the data are consistent with very little churning or quite a lot of churning of one's position in the treated versus untreated distributions of potential outcomes. However, if we assume that the expected value of treatment was positive throughout the distribution, the upper bound on treatment effects in the lower tail of w_{0i} is somewhat closer to our estimate of the average value of land won.

Appendix Table 2: Bounds on Mean Treatment Effects for Subsets of the Counterfactual Distribution

	(1)	(2)	(3)	(4)	(5)	(6)
	Feasible solution?		Bounds on treatment effect for various counterfactuals			
	In full sample	Percent in bootstrap samples	At or above $800	Below $800	Below $500	Below $400
Panel A: Discretize using 100-cell Grid						
Basic only	Yes	100.0%	-6047 1259	22 7518	14 8535	10 9453
No worse off	No	0.9%				
Expected value positive	Yes	81.1%	0 1259	22 1271	14 1505	11 1689
Expected value ≤ $2000	Yes	100.0%	-735 1259	22 2000	14 2000	11 2000
Panel B: Discretize using 50-cell Grid						
Basic only	Yes	100.0%	-6046 1244	23 7253	14 8516	11 9431
No worse off	No	0.8%				
Expected value positive	Yes	81.5%	0 1244	23 1257	14 1488	11 1670
Expected value ≤ $2000	Yes	100.0%	-749 1244	23 2000	14 2000	11 2000
Panel C: Discretize using 200-cell Grid						
Basic only	Yes	100.0%	-5720 1191	19 7672	11 9336	9 10045
No worse off	No	0.9%				
Expected value positive	Yes	79.7%	0 1191	19 1338	11 1658	9 1807
Expected value ≤ $2000	Yes	100.0%	-598 1191	19 2000	11 2000	9 2000

Notes: This table displays results from a linear-program consisting of (i) two vectors summarizing the discretized distributions of total wealth in the control and treatment samples and (b) auxiliary constraint matrices, specified in the first column and in the appendix. The number of bins used to discretize the distributions is reported in the panel headings. Column (1) reports whether there exists a feasible solution to the linear program formed from the various constraints and the two vectors using the distribution vectors constructed from the full sample. Results in Column (2) comes from 1,000 bootstrapped samples, each of which was used to recompute the control and treatment distribution vectors and recast the linear program. The remaining columns report lower and upper bounds on the expected (mean) effect of treatment for specified ranges of the distribution of potential outcomes. These effects are weighted by the control distribution within the specified range. See the appendix for details on this procedure. The underlying sample consists of all household heads in the 1850 census with children born in Georgia during the three years prior to the Cherokee Land Lottery of 1832 and no children born outside of Georgia during the same period. The outcome variable is "total wealth," as defined above. An indidivual is coded as treated if he is a unique match to a name found on the list of winners published by Smith (1838); anyone else in the sample is coded as control. Data sources and additional variable and sample definitions are found in the text.