SHOCKING BEHAVIOR: RANDOM WEALTH IN ANTEBELLUM GEORGIA AND HUMAN CAPITAL ACROSS GENERATIONS*

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Does the lack of wealth constrain parents' investments in the human capital of their descendants? We conduct a nearly 50-year follow-up of an episode in which such constraints would have been plausibly relaxed by a random allocation of substantial wealth to families. We track descendants of participants in Georgia's Cherokee Land Lottery of 1832, in which nearly every adult white male in the state took part. Winners received close to the median level of wealth—a large financial windfall orthogonal to participants' underlying characteristics that might have also affected their children's human capital. Although winners had slightly more children than did nonwinners, they did not send them to school more. Sons of winners have no better adult outcomes (wealth, income, literacy) than the sons of nonwinners, and winners' grandchildren do not have higher literacy or school attendance than nonwinners' grandchildren. We can reject effects implied by the cross-sectional gradient of child outcomes by paternal wealth. This suggests only a limited role for family financial resources in the formation of human capital in the next generations in this environment and a potentially more important role for other factors that persist through family lines. *JEL* Codes: I24, N32, J13.

I. Introduction

Investment by parents in the human capital of their children can be constrained when capital markets are imperfect and borrowing against future labor earnings is precluded. In such situations, much of the productive potential of the children may go unrealized, leaving them and society as a whole poorer as a result. Nevertheless, the importance of such constraints is

*For comments on early versions, we thank Lou Cain, Greg Clark, Robert Pollack, Chris Roudiez, Rachel Soloveichik, and seminar and conference participants at the NBER Cohort Studies meeting, the Stanford SITE meeting, the University of Washington, the University of Michigan, the University of Chicago, and the Minnesota Population Center. We also thank Steven Ruggles for access to a preliminary version of the 1850 full-count file. For research assistance, we are grateful to Lucy Shen, Ian Morey, Ee Xin Wong, James Baatz, Joe Mihm, Karen Zainal, Minjae Kim, Shruthi Venkatesh, Xiafei Zhang, Jason Wong, Matthew Lee, Sid Biswas, Wooju Lee, and Mercedes Looser. Bleakley gratefully acknowledges research support from the Center on Aging (funded under NIH grant P30 AG012857), the Center for Population Economics (funded under NIH grant P01 AG010120), the Stigler Center, and the Dean's Office of the Booth School of Business, all at the University of Chicago.

The Quarterly Journal of Economics (2016), 1455–1495. doi:10.1093/qje/qjw014.
Advance Access publication on March 12, 2016.

difficult to assess in the absence of randomized perturbations to family wealth, as parental wealth might predict child outcomes for reasons other than borrowing constraints, such as parents' possession of underlying characteristics (e.g., ability, ambition, or access to better investment opportunities) that helped them accumulate more assets in the first place. If these characteristics are passed on to their children, those children would exhibit better outcomes regardless of their parents' direct investments in them.

We examine the effects of a substantial, random wealth disbursement to parents—winning one of more than 18,000 parcels of land in a large-scale lottery in the U.S. state of Georgia (the 1832 Cherokee Land Lottery)—on their children and grandchildren over nearly 50 years after the windfall.[1] This allows us to separate the impact of parental resource constraints from other influences on the children's outcomes.

At this time in Georgia, as in most modern settings, parents' resources and outcomes are linked to the outcomes of their descendants; correlations were substantively large and statistically significant between parental resources and the school attendance, literacy, wealth, and occupations of their children and even grandchildren.[2] We use the lottery's rules to identify

1. See Figure I for the location of Old Cherokee County, the area settled through this lottery, as well as the counties in the Southeastern United States to which the population we examine had moved by 1850. It bears mentioning that the Cherokee tribe was displaced from Old Cherokee County and forcibly marched to Oklahoma in what was known as the Trail of Tears. There is a small literature treating the economic history of the Cherokee in the antebellum Southeast. For example, Wishart (1995) shows that corn production by the Cherokee in this area compared favorably to that of white farmers in DeKalb County, Georgia, and in the valley and mountain regions of Virginia. Gregg and Wishart (2012) calculate the full costs of the removal of the Cherokee.

2. See Figure III for one example: the strong, positive relationship between a father's position in the wealth distribution and the probability that his children attended school. The existence of intergenerational correlations in outcomes in the nineteenth-century United States is clear: for example, Long and Ferrie (2013) show the links between the occupations of fathers and sons in the United States 1850–1880. Sacerdote (2005) examines father–son links after the Civil War and finds that it took roughly two generations for the descendants of those who were born into slavery in the United States (and faced severely limited opportunities for human or financial capital accumulation) to converge to the human capital outcomes of blacks who were born free. In Britain, Clark and Cummins (2014) use evidence from rare surnames to show how advantages in educational opportunities (attendance at Oxford and Cambridge) persist for eight centuries. Clark (2014)

14,000 adult white males in 1850 who had been eligible to participate in the 1832 lottery. From these eligibles, we identify winners—our treated group—using a list published by the state of Georgia (Smith 1838), while lottery-eligible men not found in the list serve as a control group. The lottery's losers and winners look quite similar in a series of balancing tests using outcomes determined prior to the lottery.[3] The net present value of land won was in the neighborhood of $600, representing perhaps half a decade's work for an unskilled laborer.

Becker and Tomes modeled the decision made by parents to invest in their children, subject to a budget constraint and the presence of "a family's cultural and genetic 'infrastructure'" (1986, p. S6). In this setting, wealthier and better-educated parents face a different budget constraint than do poorer parents, resulting in a correlation in outcomes across generations even if all families possess the same infrastructure. Conversely, parents facing identical budget constraints might also see different outcomes for their children if their "infrastructures" are different. Some of the advantages enjoyed by certain parents might be dissipated (i.e., not exclusively generate better per child outcomes) to the extent that they result in greater fertility. Nevertheless, on the basis of studies available in 1986, Becker and Tomes (1986) predict that any earnings advantage would be erased within three generations.

A large number of studies have examined contemporary correlations in human capital across generations (Chevalier 2004;

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shows persistent correlation of outcomes within family lines in a variety of settings over many generations. Olivetti and Paserman (2015) show high intergenerational income correlations for the nineteenth-century United States, and Lindahl et al. (2015) show the persistence of outcomes within family lines for up to four generations in Sweden.

3. We performed sensitivity analyses in addition to these balancing tests. First, using a placebo sample constructed by counterfactually assuming that the lottery required residence in South Carolina for three years before 1832, we find no pseudo-treatment, in contrast to what we find for Georgia (Online Appendix B). Second, analysis of a sample linking the full 1830 and 1850 Georgia census indexes—a differently constrained population than our sample of eligibles—yields essentially the same results on outcomes for children as our original sample (Online Appendix C). Finally, we control for characteristics of the person's name throughout the analysis, as the identification of winners in our eligible population and the observation of children's and grandchildren's outcomes in post-1850 censuses relies on matching individuals by name, and we find that all results are robust to inclusion of these controls.

Black, Devereux, and Salvanes 2005; Oreopoulos, Page, and Stevens 2006; Wantchekon, Klašnja, and Novta 2015). These exercises are centered on plausibly exogenous variation in educational attainment that is not the product of variation in ability or other unobserved family characteristics. Studies that examine instead the relaxation of the family's budget constraint are less common, but examples include Mayer (1997), Baird, McIntosh, and Özler (2011), Dahl and Lochner (2012), Bursztyn and Coffman (2012), Akresh, de Walque, and Kazianga (2013), and Cesarini et al. (2016). In the present study, we focus on a random shock to financial resources to separate these effects.

The 1832 land lottery offers several advantages when it comes to studying this question. First, the lottery generated a shock to an individual's wealth that we can plausibly expect was exogenous to his characteristics. Second, registration in this lottery was cheap and widespread (nearly all white males age 18+ in the state in 1832 were registered, by our calculations), unlike many studies of lotteries where participants are a small and selective subset of the population. Third, the prize in this lottery was a claim on a parcel of land with an average value close to the median wealth of the period. Fourth, lottery winnings were essentially a pure wealth shock—there was no homesteading requirement, and the claim could be readily liquidated without ever setting foot on the land. Finally, we are able to undertake a long-run follow-up on the effects of this wealth shock and examine family outcomes up to five decades after the lottery.

In the case studied here, the shock came well after parents had completed their schooling, leaving little scope to alter their own human capital. Relaxing their budget constraint would have allowed them to invest more in the human capital of their children, if indeed such investment would have otherwise been constrained by lack of parental wealth. If instead human capital were unaffected in the next generations, this suggests that the intergenerational correlation in outcomes is driven not by wealth constraints but by more fundamental, perhaps family-specific, effects (such as the "family's cultural and genetic 'infrastructure'" in the Becker and Tomes 1986 model).

We find that lottery winners had only slightly higher (postlottery) fertility than losers, but were no more likely to send their children to school.[4] As we follow these children into adulthood in 1860, 1870, and 1880, we find that sons' 1860 and 1870 wealth is not statistically distinguishable between control and treatment groups.[5] Furthermore, in 1880, we do not find differences in occupational standing or literacy as a function of their father's lottery status. In 1880, the grandchildren do not have significantly greater literacy or school attendance if their grandfather was a lottery winner. Treated families have fewer grandchildren per son in 1880, which roughly offsets the small fertility effect in the previous generation, leaving a statistically similar number of grandchildren by lottery status.

The absence of a lottery effect on descendants' human capital contrasts with (i) cross-sectional evidence of returns to skill in nineteenth-century Georgia (Bleakley and Ferrie 2014), (ii) intergenerational correlations in other outcomes, and (iii) narrative evidence of an awareness among Georgia's poorer farmers of the value of investment in education. Indeed, we can reject effect sizes that are considerably smaller than those implied by the cross-sectional gradient of child outcomes with paternal wealth.[6] This indicates that winners did not use their windfall to relax a financial resource constraint on human capital investment in their children. This is instead consistent with intergenerational correlations reflecting the presence of deeper

4. Contextual influences on fertility and school attendance are not apparently an important mechanism for these results: effects are not sensitive to controlling for county of residence, nor do we find evidence that lottery winners move to counties with unusual fertility, schooling, land value, slave intensity, farm sizes, land improvement, urbanization, or transport access. See Online Appendix G.

5. Nevertheless, a mechanical split of the "extra" 1832 paternal wealth among his children would suggest a treatment effect in 1870 of $140 (in 1850 dollars), which we can reject for reasonable discount rates.

6. Bleakley and Ferrie (2014) show that literacy and childhood school attendance—themselves strongly predicted by parents' wealth—predict adult wealth, and sons with more siblings tend to have worse adult outcomes. Whether this reflects a causal effect is uncertain, but the standard methodologies for measuring these relationships indicate their presence in one form or another in mid-nineteenth-century Georgia. In a simulation exercise in Online Appendix D, we show that the observed effect on sons' outcomes are smaller than those we would predict based on the relationships between parents' wealth and sons' outcomes we observe in the control sample. That random wealth shocks generate negligible changes to sons' outcomes despite positive correlations between sons' outcomes and parents' wealth in the population suggests that the latter are not causal and may suffer from the omission of some important variable associated with better outcomes for both some parents and for their children.

underlying characteristics that persist through family lines and are associated with superior outcomes.

II. The Cherokee Land Lottery in Northwest Georgia, 1832

Georgia placed most of its land in the hands of the public through a series of land lotteries.[7] As new land was acquired by the state in treaties with the Native American population, lotteries were conducted, the last in 1832 following the eviction of the Cherokee from northwest Georgia, specifically the area shaded in Figure I. The rules were simple: every man age 18 and older who had resided in Georgia for the three years prior to the 1832 drawing was entitled to one draw, and any man who had a wife or had children under 18 and met the three-year residency requirement was entitled to an additional draw.[8] A slip of paper was made out with a name on it for each draw, and these roughly 85,000 slips were placed in a large drum. The location of each 160-acre parcel of land was written on a slip of paper, and these 18,309 slips were placed in a second drum, together with more than 66,000 blank slips, making the numbers in the two drums identical. Slips were then drawn one at a time from each drum after the drums had been mixed and then were paired up. Names matched to a parcel location were winners; names matched to a blank slip were losers. The lottery was thus doubly random: winning was random, as was the quality of the parcel received, conditional on winning. In Online Appendix A, we calculate that 98% of eligible men age 18 and over registered.

Once the lottery was completed, winners could immediately sell their winning draw. Unlike land distributions in many Midwestern states, there was no requirement that the recipient spend any time on the land or make any improvements. The only requirement imposed was that winners register their claim and

7. This summary of the rules is drawn from State of Georgia (1831, pp. 131–139).

8. Widows, orphans, soldiers, and the disabled were also entitled to draws, and prisoners and some outlaws were excluded, but we focus our attention on adult men with children (all of whom will have been eligible for two draws) because for this group it is possible to identify in the 1850 census individuals who had been eligible in 1832 based on the ages and birthplaces of their children reported in 1850. The residency rule for males was four years in the original 1830 law, but was amended to three years in 1831; see State of Georgia (1832, p. 142).

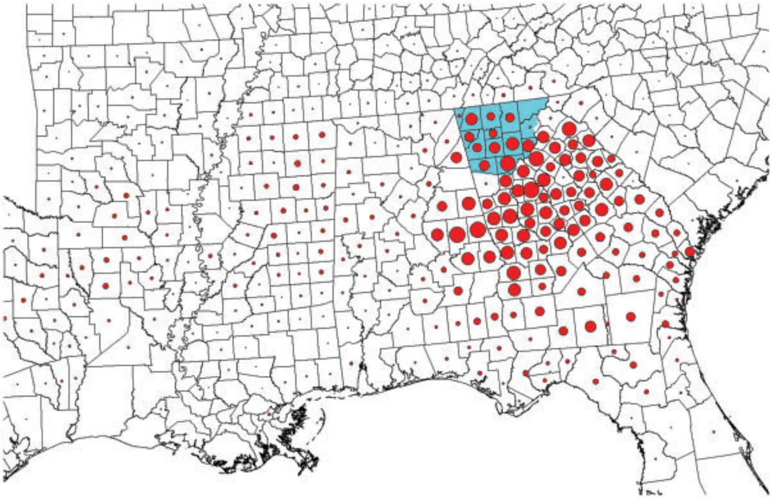

FIGURE I

Old Cherokee County and the 1850 Locations of the Sample.

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 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 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 chose to zoom in on this region to make features legible in this map; see Appendix Figure I for the full country. Data sources and additional variable and sample definitions are found in the text.

pay an $18 registration fee to the state.[9] The land could not be immediately occupied, however, as the Cherokee Nation was engaged in legal action to fight their eviction, and the final ruling in favor of the state did not come until 1838.[10] As a result, some

9. The registration fee need not have been an obstacle to liquidity-constrained winners in that there were many who simply sold the claim itself.

10. Despite the ongoing litigation (the outcome of which was never in doubt, as the Indian Removal Act had already been upheld by the courts and used successfully in numerous prior instances), some settlers still relocated to the area before 1838, and some transactions for land awarded in the 1832 land lottery took place

lottery winners may have exercised their option of immediately "flipping" their property.

We estimate that the value of a winning draw was between \$464 and \$716 in 1850 for a 160-acre parcel. In Bleakley and Ferrie (2013, Table II), we find that winners were in fact between \$572 and \$923 wealthier (depending on the specification we use) than losers by 1850. The midpoint of this range (\$748) is the equivalent of 1,010 days of earnings for an unskilled laborer in the South at this time, or just over five years at 200 work days a year.[11] Even if they sold their parcel between 1832 and 1850 and bought land that rose in value at a similar rate, we would also expect them to be considerably wealthier in 1850 than were lottery losers. Those who sold out before the uncertainty over the timing of the expulsion of the Cherokee might have received somewhat less than this, but the timing was the only source of uncertainty in this process, as the Indian Removal Act of 1830 under which the eviction was conducted had already been applied elsewhere.[12]

III. Data

III.A. Data Sources and Construction

The study follows up on the outcomes of lottery winners and losers and their children and grandchildren. To do this, we need to identify who was eligible and who among them won. We find these individuals, their children, and their childrens' children in

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from 1832 to 1838. Weiman (1991) shows that the market for the 1821 and 1827 land lottery claims was already active before 1830. Old Cherokee County was divided into Forsyth, Lumpkin, Union, Cobb, Gilmer, Murray, Cass (now Bartow), Floyd, and Paulding Counties by the state legislature on December 3, 1832. Though these counties could not be legally occupied until 1838 by winners in the 1832 lottery, the deed records from these places nonetheless report numerous transactions for plots within their borders in the years 1833 to 1837. In new Cherokee County, hundreds of pre-1838 sales occurred (see http://deedsrecords.cherokeega.com/). Shadburn (1990) reports that there were already 11,500 white settlers in Old Cherokee County by 1834.

11. An unskilled laborer in the South Atlantic region earned \$0.74 per day in 1849 (Margo and Villaflor 1987, p. 880), so \$748 in 1850 represented 1,010 days of work by an unskilled worker. Assuming 200 work days/year for such workers, lottery winnings of \$748 represented 5.1 years of unskilled labor.

12. The bottom third of Cherokee County was thought to contain gold and was distributed in a separate lottery in smaller, 40-acre parcels. We focus only on the main lottery of 160-acre parcels.

later, publicly available data sources and ascertain their outcomes. We initially search for these individuals in the census manuscripts of 1850–1880 using a preliminary version of the full-count file for the 1850 census from the Integrated Public Use Microdata Series project, the full-count file for the 1880 census from the North Atlantic Population Project, and indexes to the 1860 and 1870 censuses searchable on Ancestry.com.

The names of winners in the 1832 Georgia land lottery were published in Smith (1838), who listed each parcel in the 1832 lottery area and the name of the winner of that parcel, as well as the county and minor civil division where the winner resided in 1832. There is no surviving statewide list of all participants from which we could construct a control population to compare to those treated by winning the lottery. To create the control population, we exploited the lottery's eligibility requirements and information available in the 1850 Census of Population, which identifies all household members by name, age, and state of birth. The bulk of those eligible to participate in the lottery had to have been men age 18+ in 1832 who had been present in Georgia continuously over the preceding three years. Using the full-count file created by the transcription of the 1850 census, we identified all white men who (i) would have been age 18+ in 1832, (ii) were present anywhere in the United States in 1850, (iii) had at least one child who was born in Georgia 1829–1832, and (iv) had no children born outside Georgia in the same interval. There were 14,306 individuals who met these criteria (see Figure I for their 1850 locations), of whom 1,758 were subsequently linked—using their surname and given name—to exactly one individual in the Smith (1838) list of lottery winners.[13] An additional 1,177 were linked to more than one individual in the Smith list. Thus the population "at risk" to have won land in the lottery was created using the eligibility criteria, and that uniformly created population was then divided into "treated" (winners) and "control"

13. An individual was considered to have been uniquely linked if exactly one individual in the Smith list appeared in the 1850 census group of eligibles with the correct given name and a surname that differed by no more than 15 units in the SPEDIS "string distance" function in SAS (which assigns points to different sorts of transcription errors such as omitting a letter, sums the points, and adjusts for the name length). If several individuals were matched by given name and all had exactly the same SPEDIS value (no more than 15), the individual was considered to have been multiply matched.

(losers) groups based on whether they also appeared in Smith's list of winners.[14]

We were able to create an entirely separate sample of individuals eligible for the lottery and surviving to 1850 by taking all men in Georgia in the 1830 U.S. Census and locating them in the 1850 census. This allowed us to compare our eligibles with a sample of eligibles who were not chosen on the basis of their 1850 household composition. These comparisons, shown in Online Appendix C, reveal that our eligibles are men with somewhat more children, a somewhat compressed age distribution, and wealth portfolios tilted toward land and away from slaves, compared with the universe of all men linked 1830–1850.[15]

For the control and treated populations, information reported on the population schedule of the 1850 census and present in the 100% file (county of residence and the names, number, and ages of all children) was combined with information we transcribed from the slave schedule of the 1850 census (the number, age, and sex of slaves owned) that was created by census marshals concurrently with the population schedule. The value of slave wealth was estimated using slaves' age and sex and contemporaneous slave prices disaggregated by these characteristics, reported by Kotlikoff (1979). We then transcribed occupation, literacy, school attendance, and real estate value from the 1850 population schedule.

Linkage to later censuses was performed to generate multigenerational outcomes for the control and treated populations. The male sons of the 1850 male household heads previously identified as the control and treatment groups were sought in the 1880 U.S. Census of Population in two ways: (i) the characteristics of 1850 sons (name, year of birth, birthplace, and parents'

14. Because the sample of the population "at risk" to have participated in the lottery relies on a measure of household composition (children of specific ages born in or out of Georgia) in 1850, we were concerned that this procedure could produce a sample with more children observed postlottery (one of the outcomes we examine) if infant and child mortality was lower among lottery winners. We linked 4,978 households from 1840 to 1850 and again used linkage to the Smith list to divide them into winners and losers. We then compared the number of children age 0–4 in 1840 to the number of children 10–14 in 1850. This change (which should reflect mortality, as these children were still too young in 1850 to have left the households where they were initially observed in 1840) does not differ in either a statistically or economically significant way between winners and losers.

15. This was possible because after our original submission, Ancestry.com made the index to the entire 1830 U.S. Census publicly available.

birthplaces) were used to locate them in the 1880 U.S. Census 100% file; and (ii) individuals not successfully linked 1850–1880 were located in the Ancestry.com 1850 U.S. Census index, where any hints to their 1880 record were followed.[16] These hints are generated by Ancestry.com on the basis of actual links among individuals made by genealogists in the construction of their family trees and links generated by Ancestry.com through a machine learning process in which actual genealogist-generated links were used as training data and the system then generated links automatically for individuals not previously linked by genealogists. When 1850 sons were identified as 1880 household heads through either of these mechanisms, the 1880 information on their entire 1880 family was transcribed (occupation was extracted from the 100% file, and we transcribed literacy and school attendance).

The male sons of the 1850 male household heads we previously identified as the control and treatment groups were sought in the 1860 and 1870 U.S. Census of Population in three ways: (i) individuals linked to the 1880 100% file in the manner described above were located in the Ancestry.com 1880 U.S. Census index, from which any Ancestry.com suggestions to their 1860 and/or 1870 record were followed; (ii) individuals not successfully linked 1850–1880 in the manner described above had their suggested links forward from their 1850 census record on Ancestry.com followed; and (iii) those who could not be traced through hints were sought directly in the 1860 and 1870 indexes. When 1850 sons were identified as 1860 and/or 1870 household heads through either of these mechanisms, their real estate and personal estate in those years were transcribed.

The initial sample drawn from the 1850 census yielded 47,749 children age 5–17 whose school enrollment was observed (as the lottery occurred in 1832, the number of children under 18 years of age was also an outcome that we observed for families of winners and losers). The linkage to 1880 yielded 14,963 male children of lottery winners and losers whose outcomes could be observed in both 1850 and 1880 (46% of the 32,738 males under age 18 from 1850 sought), together with 40,658 grandchildren in

16. When multiple matches were found in 1880 for the same 1850 individual, the match that minimized the SPEDIS "string distance" between the 1850 individual sought and the 1880 individual located was chosen; if multiple 1880 individuals minimized this distance, the observations were rejected.

1880 of the original lottery winners and losers. The linkage to 1870 yielded 12,235 sons of winners who were adults in 1870 (37% of those sought), so their 1870 real and personal wealth was observed.[17] These rates of linkage compare very favorably with other studies that have used linkage across U.S. census manuscripts in the second half of the nineteenth century.

III.B. *Summary Statistics and Balancing Tests*

Table I presents the sample's summary statistics. Each variable appears in its own row, and each panel contains related variables.[18] Column (1) displays values (means and standard deviations) for the entire sample, and columns (2) and (3) report the corresponding values for lottery losers and winners, respectively. Column (4) reports p-values for a test of the null hypothesis that the means in columns (2) and (3) are identical. The test is a simple bivariate regression on a dummy for lottery winner. Clustered standard errors are calculated throughout the analysis when the data have a grouped structure. Sample sizes are reported in square brackets.

We use two measures of winning land in the 1832 Cherokee Land Lottery (Table I, Panel A). If an individual was uniquely matched to the list of winners (Smith 1838), the first measure is coded to 1; otherwise this measure is coded to 0. By this measure, 12.4% of our observations are lottery winners. This measure has a mean of 0 for losers (column (2)) and a mean of 1 for winners (column (3)) by construction. The second measure is designed to account for the few cases where more than one individual is matched to the list of winners. If n individuals are matched to the same winner, the match variable is recorded to $\frac{1}{n}$. Our maintained assumption in constructing this measure is that one of the "tied" individuals in fact won a parcel, but in the absence of additional information, we can do no better than assigning equal probabilities of this event to all n individuals in the "tied" set. The mean value for this measure of the probability of winning is 15.5%, which is 3 percentage points higher than the original measure but similar to the winning rates in Columbia and Oglethorpe Counties, where we have actual lists of both lottery participants and lottery winners.

17. Linkage to 1860 yielded 3,306 sons who were household heads.

18. Portions of Table I also appear in Bleakley and Ferrie (2013).

TABLE I
SUMMARY STATISTICS

	(1) Whole Sample	(2) Lottery "Losers"	(3) Lottery "Winners"	(4) 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 $\frac{1}{n}$ in case of ties	0.155 (0.335)	0.037 (0.121)	0.995 (0.053)	0.000 [14,375]
Panel B: Predetermined outcomes				
Age, in years	51.2 (8.5)	51.3 (8.5)	50.9 (8.6)	0.122 [14,375]
Born in Georgia	0.497 (0.500)	0.497 (0.500)	0.498 (0.500)	0.889 [14,375]
Born in South Carolina	0.212 (0.408)	0.210 (0.407)	0.222 (0.416)	0.263 [14,375]
Born in North Carolina	0.180 (0.384)	0.180 (0.384)	0.178 (0.383)	0.804 [14,375]
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 [14,375]
Cannot read and write	0.147 (0.354)	0.147 (0.354)	0.142 (0.350)	0.593 [14,340]
Number of letters in surname	6.19 (1.61)	6.20 (1.62)	6.13 (1.51)	0.072 [14,375]
Frequency with which surname appears in sample	36.2 (46.3)	36.3 (46.9)	35.3 (41.9)	0.380 [14,375]
Surname begins with "M" or "O"	0.101 (0.302)	0.101 (0.301)	0.104 (0.305)	0.740 [14,375]
Mean wealth of families in the South with same surname	1,203.4 (445.4)	1,204.5 (455.1)	1,195.7 (370.5)	0.373 [14,093]
Median wealth of families in the South with same surname	184.1 (162.3)	184.6 (167.3)	181.0 (121.8)	0.276 [14,093]
Mean illiteracy of adults in the South with same surname	0.175 (0.043)	0.175 (0.044)	0.176 (0.039)	0.124 [14,093]
Mean school attendance of children in the South with same surname	0.323 (0.052)	0.323 (0.052)	0.323 (0.049)	0.998 [13,975]
Panel C: Fertility and school attendance				
Number of children in household born after the 1832 lottery	3.955 (2.546)	3.930 (2.539)	4.135 (2.586)	0.002 [14,375]
School attendance among children aged 5–17, inclusive	0.342 (0.474)	0.342 (0.475)	0.341 (0.474)	0.799 [47,749]

TABLE I

(CONTINUED)

	(1) Whole Sample	(2) Lottery "Losers"	(3) Lottery "Winners"	(4) p-Value, Mean Difference [N]
Panel D: Other outcomes				
Spouse cannot read and write	0.235	0.236	0.231	0.676
	(0.424)	(0.424)	(0.421)	[11,563]
Resides in Georgia	0.723	0.722	0.729	0.548
	(0.447)	(0.448)	(0.445)	[14,375]
Resides in Alabama	0.144	0.144	0.145	0.935
	(0.351)	(0.351)	(0.352)	[14,375]
Resides in Old Cherokee County	0.113	0.111	0.126	0.074
	(0.317)	(0.314)	(0.332)	[14,375]
Panel E: Measures of wealth in 1850 (18 years after the lottery)				
Real estate wealth	1,999.0	1,970.8	2,198.2	0.068
	(4,694.2)	(4,422.0)	(6,290.1)	[13,094]
Slave wealth	1,339.1	1,297.3	1,635.3	0.021
	(5,761.0)	(5,329.7)	(8,189.0)	[14,375]
Total wealth (sum of wealth in real estate and slaves)	3,323.7	3,245.5	3,876.5	0.006
	(8,691.0)	(7,952.9)	(12,734.4)	[13,094]
	{100,800, 3,000}	{100,800, 3,000}	{100,1,000, 3,550}	
Panel F: Select variables for those with below $300 in 1850 total wealth				
Number of children in household born after the 1832 lottery	3.905	3.878	4.098	0.063
	(2.471)	(2.453)	(2.591)	[4,506]
Number of slaves in 1840	1.4	1.3	2.3	0.074
	(6.7)	(6.6)	(7.4)	[1,761]
Has at least one slave in 1840	0.190	0.179	0.255	0.012
	(0.392)	(0.384)	(0.437)	[1,761]

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 0. The second measure takes individuals that "tie" for a match to the Smith list with (n – 1) other observations and recodes them to $\frac{1}{n}$. 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 the measure of 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 a preliminary copy of the 1850 100% census file to construct average fertility, school attendance, and real estate wealth among households in Alabama, Florida, Georgia, Kentucky, Mississippi, North Carolina, South Carolina, Tennessee, and Virginia, 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 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 genders of slaves on the schedule. In curly brackets in Panel E, we report the 25th, 50th, and 75th percentiles of wealth. The number of slaves in 1840 was reported in the 1840 census population manuscripts to which these families were linked. Data sources and additional variable and sample definitions are found in the text.

The second measure is higher than the first, as some individuals who were multiply matched have a zero for the first measure but $\frac{1}{n}$ for the second. In nine cases, there was one unique match to 1850 but several similar quality matches (e.g., the additional multiple matches had full given names but lacked middle initials, whereas the unique match had a middle initial and full given name in the winners list and in the 1850 census). In these cases, there is one observation with the value 1 and $n - 1$ with the value 0 by the first measure, and n observations with the value $\frac{1}{n}$ by the second. Overall, these two lottery winning indicators have a very high correlation.

Table I, Panel B presents outcomes determined before the 1832 lottery, which should not be affected by whether the individual was a lottery winner or loser. The comparisons between columns (2) and (3) here represent a balancing test—an analysis of how well the treated group compares to the control group prior to the application of the treatment. Only 1 of the 13 outcomes in this panel is significantly different at the 10% level. Average age, the fraction born in Georgia, the number of co-resident children present in 1850 and born in the three-year prelottery window, and the fraction of adults who could not read or write are similar in the control and treatment groups.

We then examine characteristics associated with the surname of each individual. Since surname was determined at birth and could not have been affected by the lottery, we would expect no differences between the control and treatment groups in these measures. We account for minor variation in spelling by using the Soundex code for each surname. Surnames (prior to Soundex coding) are 6.2 characters in length on average, though this measure is slightly lower for lottery winners. On average, each individual's surname occurs 36 times in the sample, with no difference between winners and losers. Surnames began with the letter M or O (a rough indicator of Celtic origin) in 10% of all cases, with no difference between winners and losers. (A χ^2 test on the cross-tabulation of surname first letter and lottery status cannot reject that the variable is distributed the same for winners and losers.)

Finally, we constructed average characteristics from other males resident in the South in 1850 with the same surname, excluding members of our sample.[19] Mean real estate wealth of

19. Although an earlier version of our work used only data from the half of Georgia (counties beginning with the letters A through M) transcribed entirely in

those people with the same surname as the sample individuals is \$1,200, and median wealth is below \$300. The surname average rate for illiteracy is 22%. None of these measures differ between the winners and losers.[20]

Summary statistics for fertility and school attendance among the children of winners and losers are shown in Panel C. Outcomes measured here and in the rest of Table I are no longer expected to be the same between winners and losers and thus, unlike Panel B, are not useful as a balancing test. Lottery winners had, on average, 0.2 more children born after 1832 who survived to 1850 than did lottery losers. By contrast, the fraction of school-eligible children who attended school at any time during the 12 months prior to the census reference date (June 1, 1850) did not differ between the winners and losers.[21]

Additional characteristics for spouses and 1850 locations are compared in Panel D. Roughly equal percentages of spouses were illiterate among the winners and losers. Though winners and losers in our sample could in principle have been found anywhere in the United States, most of the sample still resided in Georgia in 1850 (Figure I), while most of the balance outside Georgia was in Alabama. Although the fractions residing in Georgia and Alabama do not differ between the winners and losers, the equality of the distributions of winners and losers across counties is

the 100% file, for this version we took advantage of the full transcription of all fields for the entire South to which we now have access.

20. Although this might be a weak test due to noise in the surname averages, note that surname averages (in results not shown) are statistically significant predictors of individual-level behavior even controlling for a variety of other covariates. As mentioned already, Clark (2014) documents persistent differences in various outcomes across certain surnames, and he uses this phenomenon to construct intergenerational elasticities of outcomes. Solon (2015) critiques Clark's methodology for estimating such elasticities. In contrast, we use the predictive power of surnames as a control or as a specification check. To our knowledge, the predictive power of (at least some) surnames is not disputed by Solon (or anyone else). We do not use surname averages to estimate an intergenerational elasticity and thus our estimates below are not directly affected by this dispute.

21. We observe only children still present by 1850 in the lottery participant's household, so this small positive fertility result might reflect the more rapid departure of children from the households of losers than from the households of winners and no difference in fertility. When we examine fertility separately for children age 10 and under in 1850 (who would have been still present in their parents' home in 1850 regardless of the parents' lottery status), the effect persists, leading us to conclude that it is not driven by differential rates of departure from the homes of winners and losers.

strongly rejected by a simple χ^2 test. Lottery winners were slightly more likely than losers to reside in 1850 in Old Cherokee County (the counties settled through the 1832 Cherokee Land Lottery).

Panel E summarizes our measures of 1850 wealth, 18 years after the lottery. We report real estate wealth, slave wealth, and the sum of these. Although we label the latter "total wealth," there are other forms in which wealth could be held that were not recorded in the 1850 census (a personal wealth question was added in 1860 and 1870). Mean wealth in all three measures (real estate, slave, and total) are several hundred dollars higher for lottery winners than for lottery losers. The economically large magnitude is similar to the value of winning a parcel that we calculated previously. Although winnings could in theory have been invested in a variety of instruments other than children, land, and slaves, such alternative investment opportunities (e.g., bank deposits, mortgages, stocks and bonds) were rare for the common man in the Deep South in the antebellum period.[22] The baseline estimates suggest that the effect of winning a parcel in the lottery persisted for at least the two decades following the drawing.

The effect of winning a parcel can be seen well down into the 1850 wealth distribution. Bleakley and Ferrie (2013) show a positive effect on total wealth from winning the lottery at the median and above in the 1850 total wealth distribution. Even among households with less than $300 in 1850 total wealth, fertility by 1850 was greater post-lottery. Furthermore, wealth in 1840, just eight years after the lottery, was higher—winners had one slave more on average and were 50% more likely to own at least one slave than were losers in 1840. There was thus clearly a strong direct effect of winning across the wealth distribution. The question that we now consider is whether that effect led to better outcomes for winners' children.

22. Ransom and Sutch (1988, Table A.1, pp. 150–151) report that the total value of slaves in the United States in 1860 (the first time the census reported both real and personal wealth) was $3.1 billion. In that year, total real estate and personal estate in the South were $3.4 billion and $4.7 billion, respectively (IPUMS 1860 1% sample: Ruggles et al. 2010). Thus, slaves accounted for two thirds of all personal wealth in 1860, and land plus slaves accounted for 80% of total wealth in 1860.

IV. Estimation Strategy

Our data allow us to analyze outcomes for lottery winners themselves, their children, and their grandchildren—a span of roughly 50 years from the date of the lottery. The treatment effect of winning a parcel in the lottery can be assessed directly by comparing mean outcomes for winners and losers (and their descendants), or by estimating a simple bivariate regression with a relevant outcome on the left-hand side and a dummy variable for winning a parcel on the right-hand side. We adopt the regression-based approach to permit both the inclusion of additional control variables and the continuous $\frac{1}{n}$ lottery status indicator.[23]

We estimate OLS regressions of the following form:

$$Y_{ij} = \gamma T_j + \beta \mathbf{X}_{ij} + \delta_{ai} + \epsilon_{ij}, \tag{1}$$

where i is the individual, j indexes the lottery-eligible person, T_j denotes treatment (winning a parcel in the lottery), which is either a binary variable or a binary variable deflated by $\frac{1}{n}$ to a continuous variable to account for multiple matches, and control variables are δ_{ai} (a set of age dummies) and $\mathbf{X}_{ij}$ (a vector of other control variables specified below). The error term is allowed to vary by both i and j. When we examine outcomes for the original lottery participants, $i = j$. Many of the regressions that follow instead use samples of children or grandchildren of the lottery participants, generating potentially numerous observations (i) for each lottery participant (j). In these regressions, standard errors will be clustered at the lottery participant (j) level. The estimate of γ that we recover should be uncontaminated by omitted variable or endogeneity problems as a result of the random assignment of treatment by the lottery.

We also employ an additional specification that incorporates characteristics measured at the level of surnames, in the simplest case adding a fixed effect for each surname. Such a specification controls for numerous differences that might be constant in family lines (patrilineal lines here, as we only have information

23. Although the random assignment of parcels among participants reduces the omitted-variable problem and thereby diminishes the need to introduce additional controls, such controls can improve the precision of our estimated treatment effect and reduce the residual variation. These controls can also reduce any biases resulting from our process for imputing lottery status, although the inability of lottery status to predict predetermined outcomes reduces this concern.

on surnames), allowing the impact of winning a parcel in the lottery to persist within extended patrilineal families. Clark and Cummins (2014) and Güell, Rodríguez Mora, and Telmer (2012) highlight striking persistence in a variety of outcomes across family lines, an effect that surname fixed effects would absorb. Though the least common surnames no doubt contain more information regarding membership in a patrilineal family line, limiting attention to only the least common names would substantially limit our sample size, so we use all family names instead. At the same time, our imputation process for determining lottery status relies on matching by surname, so some differences introduced by this process can be absorbed by surname fixed effects.

To motivate our interpretation of γ, in Figure II we present a simple model of investments in children. The standard model of education features downward-sloping marginal benefits (MB) and upward-sloping marginal costs (MC) of time in school. (This model and additional cases are explored in detail in Online Appendix E.) The intersection of MB and MC defines the optimal choice of schooling (e^*).[24] But benefits occur in the future, and costs are typically immediate. Thus low-wealth households might be constrained in their ability to finance up to e^*. (The credit system in antebellum Georgia was almost exclusively for short-term mercantile transactions, and the sort of long-term credit required to finance investments in one's own children was nonexistent.) For simplicity, we represent this constraint in Figure II as a simple upper bound on time in school. In Case 1, lottery wealth relaxes the borrowing constraint from A to B and enables additional education as long as MB > MC. Thus, our

24. The assumption of an interior solution seems justified for the majority of children. In our sample, the average school enrollment rate for children aged 5–17 is more than a third, and this rate peaks above half among children 11 years of age. The often noncontinuous path through school in that era means that this "above half" number is a lower bound on the fraction of children attending at least some school. Bleakley and Hong (2013) report that white children in the antebellum South achieved literacy rates of approximately 85%. Note further that the model in Figure II does not exhibit a poverty trap, in contrast to those found in models by Galor and Zeira (1993) and Moav (2005). Issues related to poverty traps are discussed in detail in Online Appendix E. There we argue that the lottery winnings should be enough money to boost at least some children out of a human capital poverty trap. We also fail to find evidence of Moav's claimed mechanism that families with low human capital will respond more along the fertility than education dimension.

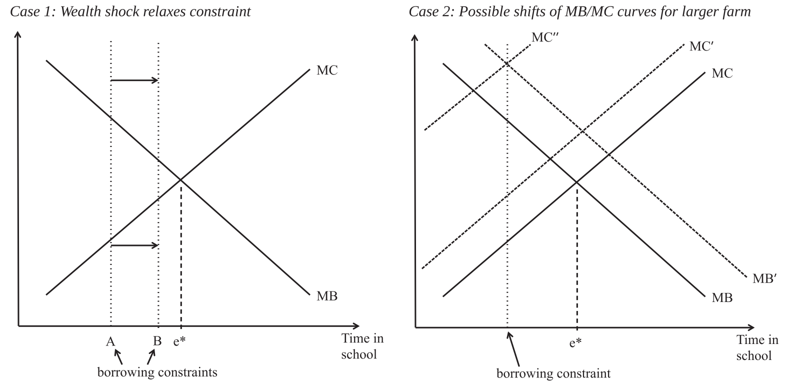

FIGURE II

Standard Model of Education, Augmented with Credit Constraint

This figure displays the marginal benefit (MB) and marginal cost (MC) curves for the standard model of the optimal choice of schooling. Panels show various assumptions about how the lottery winnings affect the borrowing constraint and the MB and MC curves.

preferred interpretation for γ is as a test of parents' response to relaxing a (supposed) borrowing constraint. However, if families used the lottery windfall to buy more land, this could change the optimal amount of schooling for their children. We analyze this in Figure II, Case 2. The opportunity cost of a child's time rises if their labor is more productive on the family farm (in a way that is imperfectly substitutable for hired labor). This is seen, for example, with the new curve MC′. But a larger farm is also a more complex enterprise, and the returns to having an educated child might actually rise, such as to MB′. Nevertheless, such changes in optimal schooling only matter if the schooling choice were unconstrained. The largely null results for child outcomes below might therefore be explained by a relaxation of the borrowing constraint and an exactly counteracting shift in the opportunity cost of child time (to MC″ intersecting with MB′, for example). Such a coincidence would have to be repeated, as we shall see, by sex, for young versus old children, for parents with different human capital, and for fathers of different ages, for all of whom the changes in the value of child labor are likely to be different. Such a coincidence would also not explain the lack of results for the wealth held by future generations, because the lottery windfall increases their lifetime productivity in either case of the model.[25]

V. Effects on Child Quantity versus Child Quality

Lottery winners tended to have (slightly) more children, but were no more likely than losers to send them to school. These results are found in Table II, where we estimate equation (1). Panel A reports results when the dependent variable is the number of children born after 1832 (the year of the lottery) who were still present in the household in 1850. (Recall that the number of children born in the three years prior to the 1832 lottery was not significantly related to lottery status.) In the basic specification, we estimate lottery winners have 0.13 more children on average and, in the specification augmented with surname fixed effects, we estimate instead a coefficient of 0.18. These numbers are consistent with the unconditional difference seen in Table I of 0.2. The Poisson regression coefficients in the last column indicate a roughly 3% higher number of children born after 1832 for winners than for losers.

Table II, Panel B examines school attendance by children in the household aged at least 5 years but not more than 17 years old. (Note that this age range excludes children born prior to the lottery.) These children are linked to the lottery status of their father, and the standard errors are adjusted for clustering at the level of the father.[26] The first column uses the OLS estimator, and therefore this regression is a linear probability model; the last column uses the logit estimator, with marginal effects evaluated at the mean of the observables and assuming the surname fixed effects are all 0. The resulting coefficients imply an effect of

25. Although it might be tempting to discuss the results that follow in terms of Becker's quantity/quality trade-off, the lottery's wealth shock shifts the budget constraint out. Therefore, it does not directly or indirectly identify a trade-off that occurs along an existing budget constraint.

26. To account for the possibility that the fertility and school attendance results are a product of the age of lottery participants (on average 51 in 1850), Online Appendix Table III replicates this analysis but with fathers disaggregated into two groups: age 50 and under in 1850 and over age 50 in 1850. There is no effect of winning on children's school attendance in either group. The effect on postlottery fertility for winners is a bit stronger for the younger group, but this is no doubt an age effect—they are young enough that their family size was more likely to be increasing after 1832 than those over age 50 in 1850.

TABLE II

EFFECTS OF LOTTERY WINNING ON FERTILITY AND SCHOOL ATTENDANCE, 1850 CENSUS

Specification:	Match to List of Winners:	(1) None	Additional Fixed Effects or Alternative Estimators: (2) Given Name	(3) State of Residence	(4) State and County of Residence	(5) Urban Residence	(6) Poisson (A) and Logit (B)
Panel A: Post-1832 fertility of lottery-eligible men [N = 14,306]							
Basic	Binary	0.132	0.146	0.124	0.102	0.126	0.033
		(0.058)**	(0.061)**	(0.058)**	(0.059)*	(0.058)**	(0.014)**
	$\frac{1}{n}$	0.137	0.156	0.128	0.104	0.130	0.034
		(0.056)**	(0.060)***	(0.056)**	(0.058)*	(0.056)**	(0.014)**
Surname	Binary	0.184	0.137	0.106	0.090	0.089	0.028
		(0.073)**	(0.069)**	(0.065)	(0.067)	(0.066)	(0.016)*
	$\frac{1}{n}$	0.175	0.131	0.095	0.075	0.074	0.026
		(0.072)**	(0.068)*	(0.064)	(0.065)	(0.065)	(0.016)
Panel B: School attendance of children aged 5–17 [N = 47,749]							
Basic	Binary	−0.005	−0.002	−0.005	0.001	−0.004	−0.021
		(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.051)
	$\frac{1}{n}$	−0.004	0.000	−0.004	0.004	−0.003	−0.017
		(0.011)	(0.011)	(0.011)	(0.010)	(0.011)	(0.050)
Surname	Binary	−0.005	0.000	−0.005	0.002	−0.004	−0.017
		(0.011)	(0.012)	(0.011)	(0.011)	(0.011)	(0.033)
	$\frac{1}{n}$	−0.006	−0.002	−0.006	0.004	−0.005	−0.023
		(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.033)

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. Estimates are computed using OLS, except in column (6), which uses Poisson for Panel A and logit for Panel B. The basic specification also includes dummies for age of the lottery-eligible man in Panels A and B, as well as age × gender of the child in Panel B. The surname specification includes fixed effects for surname (Soundex). The base sample 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. The sample for Panel A consists of household heads, whereas the sample for Panel B consists of their children at least 5 but not more than 17 years of age. The dependent variables are indicated in the panel headings. Two variables are used to measure whether the person was a lottery winner. The first measure is a binary indicator equal 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 0. The second measure takes individuals that "tie" for a match to the Smith list with $(n-1)$ other observations and recodes them to $\frac{1}{n}$. A single asterisk denotes statistical significance at the 90% confidence level, double denotes 95%, and triple denotes 99%. All standard errors are heteroskedasticity robust (and, for Panel B, clustered on the lottery-eligible man). Data sources and additional variable and sample definitions are found in the text.

winning the lottery of close to 0, and we can rule out effects of more than a few percentage points. Columns (3)–(5) add additional controls for the household's 1850 location, though these are clearly choice variables from the household's perspective.[27] As is evident, locational choice plays little role in understanding these results.

In Table III, we consider some decompositions and possible mechanisms for the quantity/quality result. One hypothesis for these results is that a richer husband might be able to remarry more easily (and/or to a younger spouse) if his first wife had died in childbirth (which was common in this period). This higher remarriage probability could result in higher fertility in families headed by lottery winners. But we see in columns (1) and (2) that there is not a statistically significant difference by lottery status in the wife being present or in the wife's age, if she is present. (We also saw in Table I, Panel D that lottery winning did not predict spousal literacy.) Next we consider the extensive margin of post-lottery fertility in column (3), where we see that lottery winners are more likely to have children after 1832 than were lottery losers. Columns (4) and (5) show that the effect of winning the lottery on fertility persists over the 18 post-lottery years—the number of children 0–10 and 11–17 are both larger for winners. Finally, in columns (6)–(9), we obtain similar school attendance results when decomposing the sample by gender or by broad age groups.[28] Additional results in Online Appendix Table E.1 reveal no difference by father's literacy in the impact of lottery winning on fertility or children's school attendance. We caution against placing undue weight on the fertility results here because of the small magnitude and nonrobustness of the coefficients. Coefficients much less than unity suggests that the vast majority

27. We further investigate this mechanism by examining characteristics of the 1850 county of residence in Online Appendix G. Lottery winners are slightly more likely to end up in Old Cherokee County in 1850, although this difference in probabilities is quite small (2.2 percentage points). Various other county-level outcomes are too similar across winners and losers to plausibly account for the individual-level results.

28. Indeed, the entire CDF of the number of post-lottery children is shifted out for lottery winners, although such differences are strongest when assessing whether winners were more likely than losers to have had one or two additional postlottery children. We also find essentially no effect on the gender composition of children, suggesting that the fertility effect is not due to the differential survival of one gender or the other.

TABLE III

DECOMPOSITIONS, FERTILITY, AND SCHOOL ATTENDANCE, 1850 CENSUS

	Spouse Present	Spouse Age, if Present	Post-1832 Children > 0	Number Children Age [0,10]	Number Children Age [11,17]	Attended School in Past Year			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1. Estimates of the effect of winning the lottery									
Panel A: Basic Specification									
	0.012	−0.071	0.014	0.106	0.089	−0.001	−0.009	−0.011	0.003
	(0.010)	(0.139)	(0.007)**	(0.045)**	(0.032)***	(0.013)	(0.013)	(0.012)	(0.013)
Panel B: Control for surname fixed effects									
	0.009	0.001	0.014	0.083	0.094	0.002	−0.010	−0.015	0.006
	(0.011)	(0.154)	(0.008)*	(0.050)*	(0.036)***	(0.014)	(0.013)	(0.013)	(0.014)
2. Estimation sample									
	Lottery-eligible person, linked to household characteristics [N = 14,375]					Males, age 5–17 [N = 24,350]	Females, age 5–17 [N = 23,072]	Children, age 5–12 [N = 26,575]	Children, age 13–17 [N = 20,847]

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 and, for the case of school attendance, dummies for age × gender of the child. The specification used in Panel B also includes fixed effects for surname (Soundex). The base sample 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. The sample for columns (1)–(5) consists of household heads, while the sample for columns (6)–(9) consist of their children at least 5 but not more than 17 years of age, with subsamples noted in the last row. 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 0. A single asterisk denotes statistical significance at the 90% confidence level, double denotes 95%, and triple denotes 99%. All standard errors are heteroskedasticity robust and clustered on the lottery-eligible man if there are multiple observations per household. Data sources and additional variable and sample definitions are found in the text.

of the sample saw their fertility behavior unchanged by the wealth shock.

We have replicated much of the analysis in this section using a placebo sample, described in Online Appendix B. We combined the list of 1832 Georgia land lottery winners with a sample of eligibles we generated by assuming that the lottery's rules required that participants had resided in South Carolina rather than Georgia in the three years before 1832. In this exercise, then, we identified eligibles as men with at least one child born in South Carolina in this interval and none born outside South Carolina in this interval. This was done to address the possibility that our method of identifying eligibles (based as it is on co-residence in 1850 with a child born during a particular three year period) has biased the sample in a way that spuriously generates outcome differences between winners and losers. None of the effects we see among winners in the "true" sample (higher wealth, a higher likelihood of residence in Old Cherokee County, and more children born post-1832) are present in the placebo sample.

Relatedly, analysis of a sample of males directly linked from 1830 to 1850 without regard for their children's ages or birthplaces in Online Appendix C indicates the foregoing procedure for identifying eligibles in our original sample does not generate a bias toward finding differential outcomes by lottery status. Online Appendix Table C.1 replicates the fertility and school attendance analyses using the sample of households we linked directly from 1830 to 1850. There we find no fertility or school attendance effects. When that sample is divided into households that contained a child born in the five years prior to the lottery (though the lottery requirement was that families have resided in Georgia for the three years prior to the lottery, the census for 1830 uses five-year age bins) and those that did not, fertility and school attendance effects are absent in both. When it is divided into households that met our eligibility criteria (children born in Georgia and only in Georgia in the pre-lottery three-year window, and present in the household in 1850), fertility and schooling effects were again absent for those meeting the criteria and those not meeting it. We conclude that the results are not an artifact of how we constructed the main sample.

VI. Outcomes of the Next Generations in 1860, 1870, and 1880

We follow up on the outcomes in 1860, 1870, and 1880 of children observed in the 1850 households. Many of those in the second generation following the lottery had formed households by 1880, which also allows us to observe the childhood outcomes of the grandchildren of those who were eligible to participate in the 1832 lottery. Note that here we are examining outcomes almost 50 years after the lottery took place.

Following up on these sons requires linkage from the 1850 to the 1860–1880 censuses. We only attempt to link male children across censuses, because female children would almost certainly change their surname at marriage. Linkage rates to 1860, 1870, and 1880 are imperfect (34%, 37%, and 56%, respectively) but are actually higher than for other studies using data linked across nineteenth century sources (e.g., Long and Ferrie 2013). The lower linkage rate for 1860–1870 results from both lacking data on parents' birthplaces (included for the first time in 1880) and the requirement we imposed that they be old enough to be household heads in 1860 or 1870. Approximately 59% of the lottery-eligible men have at least one child in the 1880 sample. The conditions under which lottery winning predicts linkage are discussed below.

The relationship between having a father win the 1832 lottery and various outcomes for their children as adults is presented in Table IV. As before, we present results from a basic specification that includes dummies for age (Panel A) and an augmented specification that controls for surname fixed effects (Panel B). Note in columns (1), (2), and (3) that having a lottery-winning father is a significant predictor for the child being linked to the 1860, 1870, or 1880 census. This differential linkage seems to result from differences in the characteristics of given (first) names as recorded in 1850. (Typically this occurs because occasionally an enumerator would record only first initials on a page, thus reducing the linkage rates to the Smith list and other censuses.) Accordingly, if we condition on a variety of characteristics of the given name, lottery winning no longer significantly predicts differential linkage. Therefore, to our standard set of specifications, we add a Panel C in which we also control for the number of letters in the given name.[29] Online Appendix Table I

29. We find similar results if we use other characteristics of the given name.

TABLE IV

ADULT OUTCOMES OF SONS OF LOTTERY-ELIGIBLE MEN, 1860–1880

	1850			1880		1870			1860		
Match to List of Winners:	Linked to 1880 Census (1)	Linked to 1870 Census (2)	Linked to 1860 Census (3)	Unable to Read and Write (4)	Occup. Score (5)	Total Wealth ($) (6)	Wealth Positive (7)	Natural Log of Wealth (8)	Total Wealth ($) (9)	Wealth Positive (10)	Natural Log of Wealth (11)
1. Estimates of the effect of father winning the lottery											
Panel A: Basic specification											
Binary	0.035	0.036	0.017	0.003	−0.230	−13.2	−0.032	−0.036	−53.0	−0.010	0.030
	(0.010)***	(0.008)***	(0.004)***	(0.009)	(0.304)	(70.9)	(0.013)**	(0.032)	(159.4)	(0.023)	(0.105)
$\frac{1}{n}$	0.038	0.029	0.012	0.006	−0.226	−47.3	−0.027	−0.030	−102.0	−0.009	0.026
	(0.008)***	(0.008)***	(0.004)***	(0.009)	(0.304)	(72.3)	(0.013)**	(0.032)	(158.7)	(0.023)	(0.105)
Panel B: Control for surname fixed effects											
Binary	0.025	0.029	0.013	0.006	−0.217	53.4	−0.021	−0.004	−76.3	0.009	−0.063
	(0.011)***	(0.008)***	(0.004)***	(0.010)	(0.365)	(68.8)	(0.015)	(0.037)	(173.9)	(0.033)	(0.175)
$\frac{1}{n}$	0.029	0.025	0.011	0.010	−0.184	24.2	−0.021	−0.007	−90.6	0.009	−0.001
	(0.008)***	(0.008)***	(0.004)***	(0.010)	(0.362)	(68.3)	(0.014)	(0.037)	(173.1)	(0.032)	(0.173)

TABLE IV

(CONTINUED)

	1850			1880		1870			1860		
Match to List of Winners:	Linked to 1880 Census (1)	Linked to 1870 Census (2)	Linked to 1860 Census (3)	Unable to Read and Write (4)	Occup. Score (5)	Total Wealth ($) (6)	Wealth Positive (7)	Natural Log of Wealth (8)	Total Wealth ($) (9)	Wealth Positive (10)	Natural Log of Wealth (11)
Panel C: Control for surname effects and length of given name											
Binary	0.016	0.014	0.001	0.014	0.190	112.3	−0.006	0.050	46.3	−0.014	0.116
	(0.013)	(0.010)	(0.004)	(0.012)	(0.440)	(82.6)	(0.019)	(0.048)	(287.1)	(0.050)	(0.316)
$\frac{1}{n}$	0.015	0.008	0.000	0.019	0.284	73.7	−0.005	0.048	48.0	−0.016	0.202
	(0.009)	(0.009)	(0.004)	(0.011)	(0.435)	(82.6)	(0.018)	(0.048)	(282.4)	(0.050)	(0.304)
2. Estimation sample											
	Children in 1850	Children in 1850, if adult in 1870	Children in 1850, if adult in 1860	1850 children as adults in 1880	1850 children as adults in 1880	1850 children as adults in 1870	1850 children as adults in 1870	1850 children as adults in 1870	1850 children as adults in 1860	1850 children as adults in 1860	1850 children as adults in 1860
	[N = 40,024]	[N = 38,529]	[N = 14,963]	[N = 14,963]	[N = 14,956]	[N = 12,235]	[N = 12,235]	[N = 12,235]	[N = 3,306]	[N = 3,306]	[N = 3,306]

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) includes dummies for the 1850 ages of the lottery-eligible man and his children. The specification used in Panel B also includes fixed effects for surname (Soundex), and the specification in Panel C adds to this dummies for the length (number of letters) of the given name. The base sample of children in 1850 is as described in prior tables, and this sample is used in columns (1), (2), and (3) to estimate the differential probability of linkage to 1860, 1870, and 1880 censuses. The samples in the remaining columns are drawn from the 1860, 1870, or 1880 households of those male children linked from 1850. The dependent variables are indicated in the column headings. For the binary lottery status variable, 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 0. The second measure takes individuals that “tie” for a match to the Smith list with $(n-1)$ other observations and recodes them to $\frac{1}{n}$. A single asterisk denotes statistical significance at the 90% confidence level, double denotes 95%, and triple denotes 99%. All standard errors are heteroskedasticity robust and clustered on the lottery-eligible man if there are multiple observations per household. Data sources and additional variable and sample definitions are found in the text.

shows balance by lottery status in predetermined observables for the subsample that was successfully linked.[30] Online Appendix Table II shows additional results adjusting for demographics.[31]

Socioeconomic outcomes are shown in the rest of Table IV. The impact of winning on the adult literacy of the winner's children is shown in column (4), measured as whether the lottery participant's son is unable to read and unable to write. In column (5), the outcome variable is the occupational income score, in adulthood (1880), of the children of the lottery participants. Neither of these outcomes is significantly different when comparing the children of lottery winners to those of lottery losers.[32] In columns (6) through (8), we consider wealth outcomes in 1870, the last nineteenth-century census reporting wealth.[33] We

30. Online Appendix Table I compares the pre-1832 characteristics of lottery winners and losers as in Table I disaggregated by whether a father's son was linked to 1880. The only characteristic that differs from the Table I results is the age of the lottery participant: among fathers linked to a son in 1880, winners were 0.6 year younger than losers. Consequently, we include father's age in the regressions in the section with sons as adults.

31. This table provides specifications with controls for birth order and family size, as well as a separate analysis for eldest sons only. We might expect that later-born children have outcomes that differ more between winners and losers, as the winnings were available to their families at a time when investments in children (e.g., schooling) might be expected to produce a higher return. Conversely, we might expect eldest sons to show a less pronounced impact of winning on human capital but a greater impact on wealth if it was not divided equally among the household's children. In no case does any of these additional specifications generate results other than that seen in the baseline case: winning the lottery generates no positive outcomes in the next generation.

32. Of the 36 results for socioeconomic outcomes in 1880 in Tables IV and V, only 4 are statistically significant at the 10% level, only slightly more than the 3.6 we would anticipate finding by chance. For 1860–1870 outcomes, 2 of 36 (5.55%) are significant at the 5% level.

33. The reliability of the 1870 wealth data has been challenged by, among others, Ransom and Sutch (1977). Poor-quality 1870 data would attenuate any relationship between the father's 1850 wealth and the son's 1870 wealth. But this will be true for both the control and treated groups in our analysis. This relationship would also be attenuated by the abolition of slave wealth with the end of the Civil War, but here, too, unless this attenuation is different for winners and losers, we would not expect it to bias a difference in outcomes for the children of winners versus the children of losers. Online Appendix F reports a comparison between 1870 census wealth and 1870 property tax wealth and shows that despite the supposed shortcomings of the 1870 census data, its striking correspondence to wealth reported by tax assessors was evident. Online Appendix Table V reports evidence of robust intergenerational wealth elasticities using these data. This suggests that the attenuation from measurement error is limited.

transcribed both real estate and personal wealth, and the results here are for the sum of these two variables.[34] To compare with the estimates above, we deflate 1870 wealth to 1850 dollars using the consumer price index from measuringworth.com (Williamson 2013).

The 1870 total wealth is statistically and economically similar between control and treatment groups. An alternative point of comparison is a mechanical split of the lottery winnings among the average number of children. This would suggest a treatment effect in 1870 of $140 (in 1850 dollars), which we also cannot robustly reject at conventional levels of confidence. On the other hand, we can reject values larger than that. Note that the deflator adjusts for inflation only and does not convert the 1870 wealth into its present value equivalent; results for 1870 wealth would drop by a factor of two to five for annual interest rates of 3% to 8%. At standard confidence levels, we could handily reject a lottery-winning effect of $140 in 1850 dollars for interest rates much above 3% a year. These estimates for the wealth of the sons are inconsistent with a claim of supernormal returns in intergenerational transmission. The sons of winners were no more likely to have positive wealth in 1870, or to have more wealth in natural logs than the sons of losers in our preferred specification (Panel C). Columns (9)–(11) include results for 1860 wealth. There were no differences between the sons of winners and those of losers in 1860 wealth. As 1860 is prior to the Civil War and slave emancipation, this result cannot be the result of a combination of disproportionate investment in slaves by lottery winners and their subsequent loss of their slave wealth when slaves were freed. (We obtain similar results when using their percentile in the wealth distribution as the outcome. See Online Appendix Table II.) Finally, we show in Online Appendix Figure II, with a quantile regression of 1870 wealth on treatment, that lottery effects are similar across the distribution of 1870 wealth.

In Table V, we turn to outcomes in the third generation (the grandchildren of lottery participants) in 1880. There are two

34. Census enumerators were instructed to leave the wealth response blank for values below $100. Above $100, the logarithmic distribution of wealth appears close to normal; we fit a truncated normal to these values and use it to compute the expected value of wealth below the censoring point. We use this imputation for the blank values when constructing the log of total wealth. Results are similar if we drop the blank values or use an imputed value of $1. See Bleakley and Ferrie (2013) for more on this imputation.

TABLE V

Outcomes of Grandchildren of Lottery-Eligible Men in 1880

	Grandchildren			
Match to List of Winners:	Unable to Read and Write (1)	Enrolled in School (2)	Number Children under 10 (3)	Number Children under 18 (4)
1. Estimates of the effect of grandfather winning the lottery				
Panel A: Basic specification				
Binary	−0.004	−0.021	−0.055	−0.097
	(0.014)	(0.012)*	(0.041)	(0.060)
$\frac{1}{n}$	0.003	−0.012	−0.059	−0.089
	(0.013)	(0.012)	(0.041)	(0.059)
Panel B: Control for surname fixed effects				
Binary	−0.006	−0.026	−0.044	−0.086
	(0.014)	(0.013)**	(0.046)	(0.066)
$\frac{1}{n}$	0.001	−0.020	−0.051	−0.076
	(0.014)	(0.013)	(0.046)	(0.066)
Panel C: Control for surname effects and length of given name				
Binary	−0.005	−0.032	−0.049	−0.120
	(0.016)	(0.014)**	(0.056)	(0.079)
$\frac{1}{n}$	0.007	−0.024	−0.055	−0.099
	(0.016)	(0.014)*	(0.055)	(0.079)
2. Estimation sample				
	Children in 1880, ages 10–19 [N = 23,544]	Children in 1880, ages 5–19 [N = 40,658]	1850 children as adults in 1880 [N = 14,963]	1850 children as adults in 1880 [N = 14,963]

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) includes dummies for the 1850 ages of the lottery-eligible man and for the 1880 age × gender of his grandchildren. The specification used in Panel B also includes fixed effects for surname (Soundex), and the specification in Panel C adds to this dummies for the length (number of letters) of the given name. The base sample of children in 1850 is as described in prior tables. The sample in this table is drawn from the 1880 households of those male children linked from 1850. The dependent variables are indicated in the column headings. For the binary lottery-status variable, 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 0. The second measure takes individuals that "tie" for a match to the Smith list with (n − 1) other observations and recodes them to $\frac{1}{n}$. A single asterisk denotes statistical significance at the 90% confidence level, double denotes 95%, and triple denotes 99%. All standard errors are heteroskedasticity robust and clustered on the lottery-eligible man if there are multiple observations per household. Data sources and additional variable and sample definitions are found in the text.

principal outcomes we consider: illiteracy (column (1)) and school enrollment (column (2)). Differences in illiteracy between the grandchildren by lottery status of their paternal grandfather are not statistically significant and are small in magnitude. In contrast, the grandchildren of lottery winners have a 1–3 percentage point lower probability of attending school. (These two columns use restricted ages in which the variables are measured and/or meaningful.) The result for schooling is the opposite of what one would expect if wealth were relaxing a constraint on human capital investment. Nor are these results consistent with moving along or relaxing a quantity/quality trade-off in that there is lower fertility as well. Men whose fathers had won the lottery had fewer children by 1880 (columns (3) and (4)), although this effect is never statistically significant. Nevertheless, the magnitude of this effect is approximately the same as it was for the previous generation. A regression at the grandfather level of winning on the number of grandchildren cannot reject equality between winners and losers in the 1832 lottery. Apparently the wealth shock induced only a one-generation blip in the size of the dynasty.

VII. Discussion

In this section, we address two distinct questions: (i) is there evidence of a return to skill and were lottery winners aware of these returns? (yes); and (ii) were there intergenerational correlations in outcomes at this time? (yes).

VII.A. Was There a Return to Skill in Antebellum Georgia and Were Winners Aware of It?

A possible response to the results is that antebellum Georgia is not the right environment in which to observe parents investing in skills or facing a quantity/quality trade-off, perhaps because it was too early in that region's path of economic development. But was this indeed the case? The contemporary reader might be unduly influenced by the seemingly moribund state of education in the South after the Civil War. Nevertheless, Bleakley and Hong (2013) show that antebellum rates of school enrollment among white children in the South were considerably higher than postbellum rates, and indeed, the South would have caught up to the North by circa 1890 if the antebellum trends in school enrollment had continued after the Civil War.

Schultz (1975) has emphasized the importance of returns to education in agriculture once farming has passed out of its "traditional" phase (in which prices are stable, long-used production techniques can be employed year after year, and there are no new technological or financial innovations that need to be dealt with).

In light of the nontraditional nature of farming in Georgia from the 1830s forward (with new crops like new cotton varieties being introduced, and increasingly national and international markets for the state's products and wide year-to-year swings in prices), it would not be surprising to find a substantial value for education in this environment.

Lacking an intervention or instrument that specifically manipulates time in school, we cannot provide causal evidence on the return to schooling. Bleakley and Ferrie (2014) apply standard methods using observational data to get a first-pass estimate of these effects and find substantial (correlational) evidence of returns to skill in the same sample we have used here.

The manner in which lottery winners chose to use their windfall no doubt depended on the range of choices they perceived as available to them. If investing in the education of their children was not even part of their choice set (e.g., they were not aware of such opportunities at any cost, let alone at a cost that would be feasible to incur using their winnings), winners may have opted for either other investments (slaves or more land), or immediate consumption. They may have done the same if investing in their children's education was mistakenly viewed as a low-return investment.[35]

It is difficult to know what lottery winners in mid-nineteenth-century Georgia perceived to be the range of options open to them—there were no surveys and very little documentary evidence survives describing what the average farmer was thinking at the time. But what little evidence we possess suggests that the education of their children was indeed a recognized investment opportunity among poor farmers. Moreover, investment in the education of their children was seen as a high priority as budget constraints were relaxed.

Consider the case of Wiley Webb, an initially poor man who used his lottery winnings for education. He participated in the portion of the 1832 Georgia land lottery in which 40-acre lots in a belt of southern Old Cherokee County thought to contain gold deposits were distributed (Williams 1989, pp. 530–532). Webb was a 34-year-old farmer in Walton County, Georgia, in 1832, living with his parents, described as illiterate and a "very poor

35. Jensen (2010) and Bursztyn (2015) provide examples from modern developing economies in which people are either unaware of returns to education or simply prefer that resources be spent in other ways.

man" (Williams 1989, p. 532). A contemporary reported "he was making spinning wheels and chairs with his foot lathe, and cotton baskets to support a large family" (Coulter 1962, p. 199). He won lot 727, which was thought to contain a particularly rich gold deposit and immediately attracted the interest of several mining companies. He auctioned the parcel within days of winning it in the lottery and received $10,000 ($4,000 as down payment and a promise of the remaining $6,000). Upon learning how much his lot had fetched, Webb immediately proclaimed he would use the funds to provide an education for his children. A contemporary reported just five or six years later that Webb, who had by then moved to Gwinnett County, Georgia, adjacent to his home county, "had bought a very good little farm and two negroes, and was educating his children and doing well" (Coulter 1962, p. 200).

Another piece of evidence on the value placed on schooling even among the poor farmers of Georgia at this time is a report that the structure of the school system (in which subsidies for tuition were provided only to families deemed "poor" by their county) discouraged some farmers from sending their children to school: "Unquestionably, many a Georgian, poor but proud, let his children grow up in illiteracy, rather than set them apart in the school as 'poor children'" (Coulter 1921, p. 29). These farmers were clearly aware of the availability of schooling and were prepared to send their children to school but for the stigma associated with the nomenclature employed by the state's funding system.

Finally, one of the few farmers from this time and place whose records (a diary and account book) have survived was clearly concerned with the education of his children. "The education of his twelve children was a continuing responsibility for [Edward] Oxford for twenty years or more," though he was himself illiterate until a late age (Biehle 1968, pp. 194–195). His accounts report expenditures that averaged $2.50–4.00 per quarter per student (Biehle 1968, p. 195).

Even seemingly contrary opinions admit of the possibility that at least some farmers were eager to educate their children if they possessed adequate resources. Sunday school was a popular expedient among some but not all rural Georgians in the absence of adequate public schools: "For some of the more ignorant, this method was the only method, since they believed that education was immoral and evil unless linked up intimately with religion. There were others 'so far sunk in ignorance and poverty together, as not to believe that the ability to read was worth the struggle that it would cost them to give it to their children'" (Coulter 1925, p. 26). This description is applied only to "some of the more ignorant," and not the population generally.

Families that won parcels in the lottery clearly did not invest more in their children's human capital, but were they simply exploiting a quantity/quality trade-off by having more children? This is implausible in light of (i) the very small effect of winning on the number of children in winners' households (and the negative sign on the number of children in winners' sons' households), and (ii) the associated high value for children this would imply. For example, winners had between 0.13 and 0.18 children more than losers (Table II), despite their having won as much as $700 in the lottery. This implies that an additional child was worth between $3,600 and $5,300. Kotlikoff (1979) reports that a prime-age male slave could be purchased at auction in 1832 in New Orleans for $701. Given high antebellum infant and child mortality rates, parents would have had to place an implausibly high premium on their own children's labor (and any nonpecuniary benefits from populating their households with their own children rather than slaves) for the measured effect of lottery winning on fertility to be consistent with investment at the extensive margin (quantity) in lieu of investment at the intensive margin (quality).[36]

VII.B. *Intergenerational Correlations*

Next we consider the extent to which outcomes are in fact correlated across generations in this context. For example, in the lottery-eligible sample, the elasticity of son's 1870 wealth with respect to the father's 1850 wealth is between 0.13 and 0.28 (depending on the treatment of zeroes) and statistically significant (in all cases).[37] This connection persists despite the intervening

36. Another way to assess whether parents were exploiting a quantity/quality trade-off is to compute how many children a family could afford at the price such a trade-off implies. The approximate permanent income for an unskilled worker from 40 years of unskilled wages at $18/month would sum to $8,640, which would be below $4,000 if discounted at 5% a year. If the child price were indeed $3,600, only very small families would be possible within the budget constraint. In contrast, unskilled workers routinely had families with more than six children.

37. These results, as well as intergenerational wealth elasticities for 1860 (which range from 0.18 to 0.45 and are statistically significant), appear in Online Appendix Table V. Most of the 1860 and 1870 estimates lie below the modern

Civil War that destroyed much of the South's physical capital (though most had been restored by 1870) and—perhaps more important—resulted in emancipation and the disappearance from slave-owners' balance sheets of a significant quantity of capital.[38] This is perhaps not surprising because (i) the war would not have destroyed the survivors' antebellum human capital and (ii) postwar land reform was minimal. The other intergenerational correlations in the control sample (shown in Online Appendix Table IV) are also substantively large and statistically significant between father's 1850 log total wealth and his children's 1850 school attendance (0.25), 1880 literacy (0.11) and 1880 occupational score (0.11), and his grandchildren's 1880 school attendance (0.04). An example of such an intergenerational correlation can be seen in Figure III: the relationship between parents' 1850 wealth and the probability that a child would attend school is positive and increasing in wealth across the 1850 total wealth distribution. In our control sample, fathers in the lowest wealth category sent their school-aged children to school with a probability of around 0.2. This number rose to approximately two thirds for the highest-wealth fathers.

The observed outcomes for lottery winners fall well short of the outcomes we would predict from the cross section, given the magnitude of the wealth shock they receive. We simulate the relationship between parents' wealth and sons' outcomes in Online Appendix D. The strong cross-sectional relationship (such as in Figure III) contrasts with no effect when wealth is randomly assigned; this indicates that the former relationship is not causal. Instead, the positive correlation between parents' and children's

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estimated intergenerational wealth elasticity of 0.37 reported by Charles and Hurst (2003) for the entire United States. More recently, Boserup, Kopczuk, and Kreiner (2015) examine intergenerational wealth correlations using percentile/percentile plots with Danish administrative data. Relatedly, in Online Appendix Table II, we show that our results are not sensitive to using the sons' wealth percentile as the outcome variable.

38. In an additional analysis (not shown), we interact (i) the fraction of the father's 1850 wealth that was comprised of slaves with (ii) the father's lottery status. If the abolition of slavery and the consequent loss of slave wealth were driving the failure of slave-holding fathers to transmit lottery winnings to their sons, this interaction should be negative. In no specification can we reject the null hypothesis that the coefficient on the interaction term is zero. See note 33 and Online Appendix F for further discussion of the quality of the 1870 wealth data. See Section VI for further discussion of the impact of emancipation on wealth transmission from fathers in 1850 to sons in 1870.

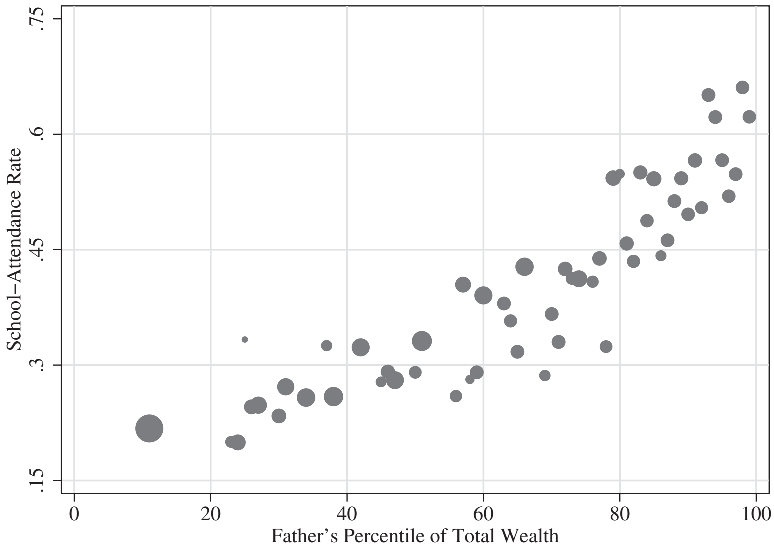

FIGURE III

The Gradient in School Attendance by Father's Percentile of Total Wealth, 1850

This figure displays the fraction of children attending school for each percentile of paternal total wealth (real estate plus slaves) in 1850. The base data are the main sample of households with lottery-eligible men, as used in Table II, for example. We then exclude from this sample those fathers matching to the Smith list to measure the gradient absent a direct lottery effect. Children are ages 5–17 (inclusive of endpoints). Approximately 22% of fathers have zero wealth, and such cases are coded to a percentile of 11. Lumpiness in the data yields uneven cell sizes. The dots are sized in proportion with the cell sizes.

outcomes apparently arises from the presence of an unobserved factor linked within families across generations and associated with better outcomes for parents and their children. Note that our results complement recent work by Cesarini et al. (2016) who examine the intergenerational effects of wealth shocks from Swedish lotteries circa 1990. They find little impact on child development (scholastic achievement, cognitive and noncognitive skills) in the next generation, although they do not follow the children into adulthood, nor do they observe grandchildren. They attribute their null results to a strong safety net. Though such social insurance was not present in antebellum Georgia, we nonetheless find similar noneffects of random wealth shocks on intergenerational outcomes.

VIII. Conclusion

Georgia's 1832 land lottery provides a unique opportunity to assess the impact of shocks to wealth, in that participation was widespread and the random assignment implied that the wealth shock was uncorrelated with individual characteristics. We assess the impact on the winners and their families into the third generation. Using 1850 census microdata, we draw a sample of male household heads that likely were eligible for the lottery. The rate of registration for this eligible population was very high. We identify the lottery winners using Georgia state records and define them as our treatment group. We cannot reject that the treatment variable was randomly assigned in several balancing and placebo tests. We estimate that lottery winners won roughly $700—close to median wealth in 1850 and the equivalent of more than five years of wages for an unskilled laborer in the South.

We focus on child outcomes in response to this wealth shock. Lottery winners slightly increased their family size after the lottery more than nonwinners, but were not more likely to send their children to school. Children of lottery winners did not have more wealth, literacy, or income as adults. Furthermore, the grandchildren of winners were not more likely to be literate or attend school. Indeed, the sons of lottery winners actually have fewer children and, if anything, send their children to school less than did the control group sons. The reduction of treated fertility in the second generation actually leaves the estimated number of grandchildren similar between control and treatment groups, effectively nullifying any fertility effect from treatment in the long run.

Despite the substantial size of the completely random financial windfall received by lottery winners and the presence of returns to human capital seen in cross-sectional data at this time, it does not appear that lottery winners invested more in their children (or that winners' children in turn invested more in their own children) than did losers (or losers' children). These findings are inconsistent with parents' financial resources being a significant explanatory factor for the level or dispersion in intergenerational human capital investments in this period. The results are also inconsistent with a wealth-based "poverty trap" for human capital. The observed intergenerational links are instead consistent with the presence of underlying characteristics that are (i) associated with better outcomes, and (ii) passed down along family lines. Such characteristics could take myriad forms (cognitive ability, social aptitudes, time preferences, *inter alia*) and be transmitted a variety of ways (genetically, culturally, etc.) but were not things that the parents were apparently constrained by wealth in acquiring.

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Supplementary Material

An Online Appendix for this article can be found at QJE online (qje.oxfordjournal.org).

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