Land Lotteries, Long-term Wealth, and Political Selection

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Abstract

Does personal wealth translate into political power? This study exploits the random allocation of wealth following a large-scale land lottery held in Georgia in 1805 to investigate the hypothesis that wealth increases the probability of officeholding. Most eligible males participated in the lottery and more than one-in-ten participants won a land lot prize worth over $800, representing over half of median property wealth at the time of the lottery. Quantile regression estimates show that winning confers a $330 increase in future slave wealth for participants near the median of the wealth distribution, 95% CI: [$58.34, $602.97], which is equivalent to the median wealth of the sample. The upper bound of the confidence interval represents an increase in wealth sufficient to satisfy the freehold qualifications for serving in the legislature. Despite evidence that the wealth shock produced by the lottery is meaningful ex-ante, the estimated effect of winning on officeholding is not practically different than zero.

Keywords: Political selection; wealth effects; long-term wealth; natural experiment

JEL codes: D72; N31; N41

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A problem in representative democracies is that elected officials might use their power to defend vested interests rather than the interests of their constituents. Personal wealth is expected to reduce the opportunity costs of holding office and may make it more important for the wealthy to hold office.

Does personal wealth cause individuals to select into office? Purely observational studies cannot answer this question because officeholding may be correlated with unobserved qualities that are also related to wealth accumulation. Natural experiments that exploit exogenous variation in personal wealth can be used to identify causal effects of wealth on officeholding. State-run lotteries that randomly distribute land titles to individuals satisfy this objective because winning a land title represents an exogenous shock to personal wealth. Rossi (2014), for instance, exploits the random allocation of land in 16^{th} century Argentina to identify the causal relationship between wealth and posterior political power. Wealth is proxied by the distance of randomly allocated land from the city of Buenos Aires and political power is represented as a binary variable indicating whether heads of household or their relatives held a position in city government. The author finds that an increase of one standard deviation in the distance of the land to the city decreases the likelihood of posterior political power by about 12%. The linear model used to obtain this estimate problematically assumes that distance of the land to the city is uncorrelated with unobserved predictors of political power, such as the supply of time available for officeholding.

The present study uses the 1805 Georgia Land Lottery as a natural experiment to investigate the hypothesis that personal wealth increases the likelihood of officeholding. In 1805, the state of Georgia conducted the first public land lottery in U.S. history by randomly distributing 1.3 million acres of land. Approximately 85% of eligible adult white males living in Georgia at the time of the 1800 Census participated in the lottery, and about 15% of participants won a land prize that could be readily sold in a secondary market for public land. The estimated mean value of a land lot prize is over $800, which represents approximately

55% of median property wealth at the time of the lottery.[1]

Georgia's land lotteries have previously been used to study the relationship between lottery wealth and the economic outcomes of lottery winners and their descendants. Bleakley and Ferrie (2016) link a list of winners in Georgia's 1832 lottery to lottery-eligible men in the full-count 1850 U.S. Census and find no evidence of a treatment effect on the wealth, literacy, or occupational standing of the descendants of lottery winners. Comparing lottery winners to lottery-eligible men not found in the list of winners is typically nonexperimental comparison because lottery players self-select.[2]

Bleakley and Ferrie (2013) use the same linked sample to investigate effect of lottery wealth on the long-run wealth distribution of lottery winners. The authors show that, on average, lottery winners have about $750 more combined real estate and slave wealth in 1850 than individuals in the control group. The authors find no long-run effect of treatment on the lower 40% of the wealth distribution; instead, lottery wealth appears to exacerbate wealth inequality in the long-run by shifting mass from the middle of the wealth distribution to the upper tail. Consistent with these results, I find no long-run effect of treatment on the lower 40% of the wealth distribution in 1820.[3]

If personal wealth influences officeholding, we should be able to find evidence in antebellum Georgia. Gehlbach et al.'s (2010) model demonstrates that when the quality of democratic institutions is comparatively weak, economic elites may select into office themselves to avoid the cost of influencing policy through political investment or lobbying. Institutional quality was considerably lower in the antebellum Georgia compared to southern border states or northern states (Grosjean, 2014). Qualitative evidence points to the dominance of slaveholding planters in state politics (e.g., Helper, 1860, pp. 159). However, I find no evidence in support of the hypothesis that wealth increases the likelihood of officeholding. I argue

[1]Table OA-2 provides information on the estimated lot value per county and a description of how the mean value of land lot prize calculations are made.

[2]The authors, however, estimate a near-universal participation rate for the 1832 lottery (97.8%).

[3]The authors attribute the long-run insensitivity to treatment on the lower tail of the wealth distribution to differences in the abilities of lottery winners and the possibility that winnings were spent on short-term consumption rather than invested.

that the null result is still informative because the estimate is not practically different than zero.

This paper proceeds as follows: Section 2 describes the development and implementation of the 1805 lottery; Section 3 describes the data; Section 4 tests the assumption of random treatment assignment and provides treatment effect estimates; Section 5 discusses concerns regarding statistical power, the importance of treatment, and possible mechanisms in which wealth influences political selection; Section 6 concludes and offers avenues for future research.

2 The 1805 lottery

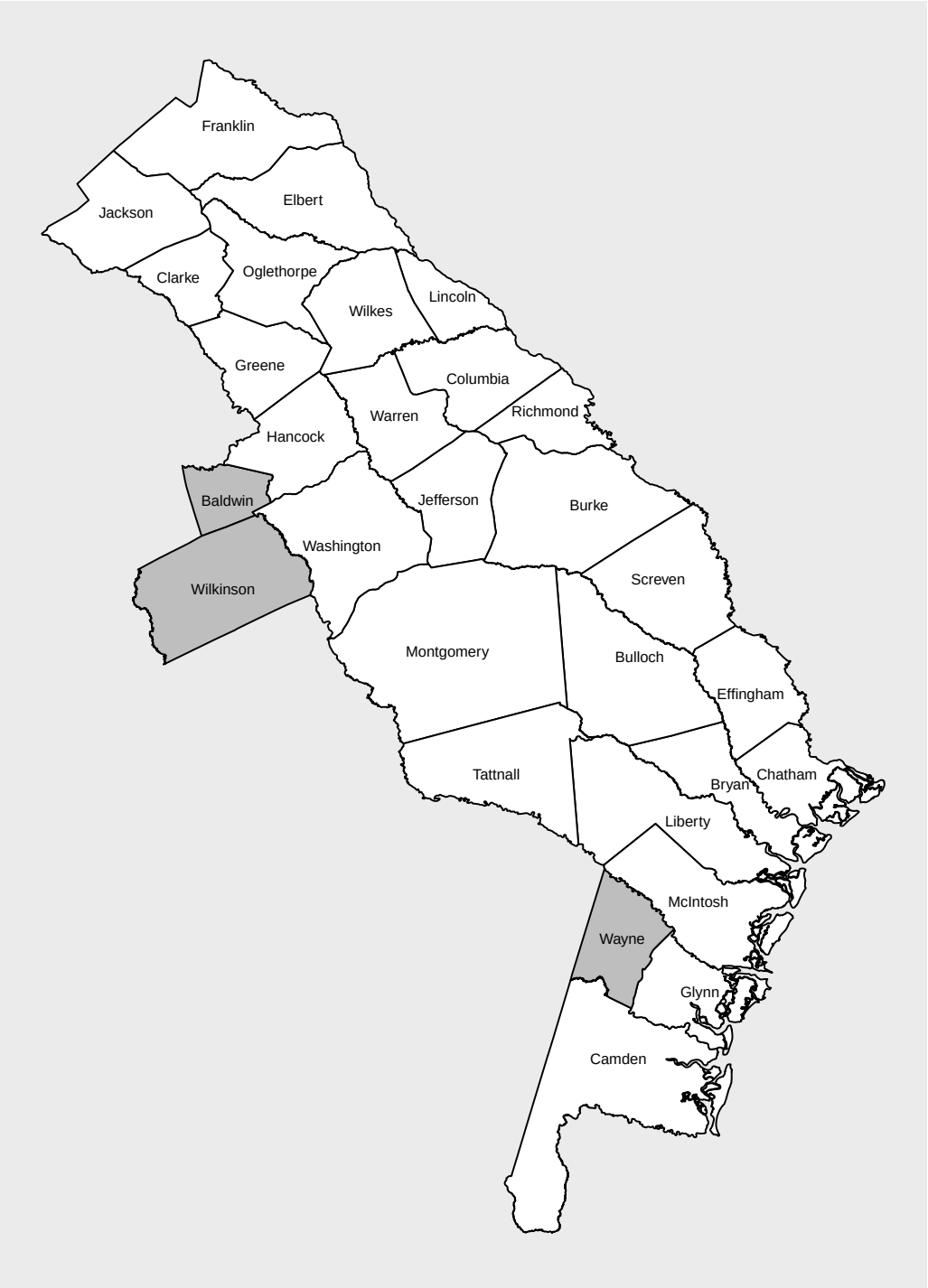
In the wake of two public land fraud scandals, Georgia's legislature created a lottery system to distribute 1.3 million acres of land acquired by the 1802 Treaty of Fort Wilkinson. The lottery created three new counties using the ceded Creek land: Baldwin and Wilkinson counties, each divided into five districts, and Wayne county, divided into three districts (Fig. 1). For each district, a surveyor was appointed by the legislature to map the area into square lots. Tickets representing each land lot were placed in a wooden "lottery wheel" to constitute prizes, along with blank tickets equal in number to the difference between the number of prizes and the number of draws. Tickets were drawn without replacement from the lottery wheel.

Under lottery rules, adult males were eligible to register for one draw and married men with children were eligible to register for two draws.[4] Registration for the lottery was voluntary and required a payment of 12.5 cents per draw. Participants who won a prize (*fortunate drawers*) had 12 months following the drawing to claim their prize. Fortunate drawers were required to pay $4 per hundred acres in order to obtain the title, or "grant", on the land lot. If fortunate drawers did not claim their land, then the lots were reverted to the state and sold in a public auction.

Winning a prize in the lottery represents a pure wealth shock because there was no homesteading requirement and fortunate drawers could easily sell their grants in a secondary market for public land (Weiman, 1991). In some cases, fortunate drawers sold their grants to land speculators, who in turn sold the land to out–of–state settlers (Davis, 1981). Land speculators often sought out fortunate drawers who drew particularly valuable lots (Cadle, 1991).

[4]Orphaned children and widows with children were also entitled to draws, but the focus of the present study is on adult males because only they were eligible for officeholding under the 1798 state constitution, which was in effect for the entirety of the Antebellum period. Table OA-1 provides details for lottery eligibility. Fig. OA-1 describes the sequence of the lottery process.

Figure 1: Original 1805 Lottery counties.



Notes: Map of Georgia with 1807 county boundaries (Long, 1995). The shaded counties are original counties created by the 1805 lottery.

3 Data

The primary source of data for this study is a complete list of participants of the 1805 lottery ($N = 23,927$), which include information on participants' name, county of residence, draw record, and prize record (Graham, 2005). Generational titles, occupations, and information on orphan and widow status are informed using identifying remarks next to the participants' names. I also rely on fortunate drawer records for the 1805 and 1807 lotteries (Graham, 2004, 2011), which provides information on whether grants are claimed or sold at public auction as a reverted lot. This information is used to identify fortunate drawers whose land lot prize reverted to the state due to failure to assert rights to the lot.

3.1 Officeholding response variable

In order to test the hypothesis that wealth increases the probability of officeholding, I construct a response variable on officeholding derived from a historical roster of officeholders. The roster includes information on name, jurisdiction, and term date for all elected and appointed officeholders from Georgia's Trustee Period to 1990 (Archives, 1978, 1990). I consider officeholders whose first term began in 1805 to 1847, inclusive.[5]

In the full sample, about 5% of lottery participants were successfully matched to officeholder records.[6] A large majority of these officeholder-participants first served in the Georgia House (88%), while the rest started their political careers in the state Senate, U.S. House, or state executive offices. About a quarter of officeholder-participants held office before the drawing of the 1805 lottery. Excluding these pretreatment officeholders from the sample along with widows and orphans yields a restricted sample size of $N = 21,667$ with a mean officeholding rate of 4%.[7]

[5]Considering that the white male life expectancy at age twenty in the early 1800s was approximately forty years (Hacker, 2010), the youngest participants who were both eligible for the lottery and public office were 21 years old and were expected to live 40 additional years.

[6]The procedure for linking datasets is described in Section OA-6.

[7]Table OA-9 provides summary statistics on response variables by treatment and compliance status.

4 Treatment effect estimates

I first check for balance across pretreatment covariates, including generational titles, occupations, and county of registration, to verify the assumption of random assignment of treatment. Fig. OA-6 demonstrates that counties are balanced at the 5% significance level, with the exception of Lincoln County, $p = 0.012$, which registered significantly more fortunate drawers. Assignment is also unbalanced in favor of the treated for bricklayers ($p = 0.025$), participants with military backgrounds ($p = 0.043$), and participants listed with the suffix "senior" ($p = 0.028$). However, these differences are not significant when accounting for the multiple comparisons made for the balance tests.[8]

4.1 Effect on officeholding

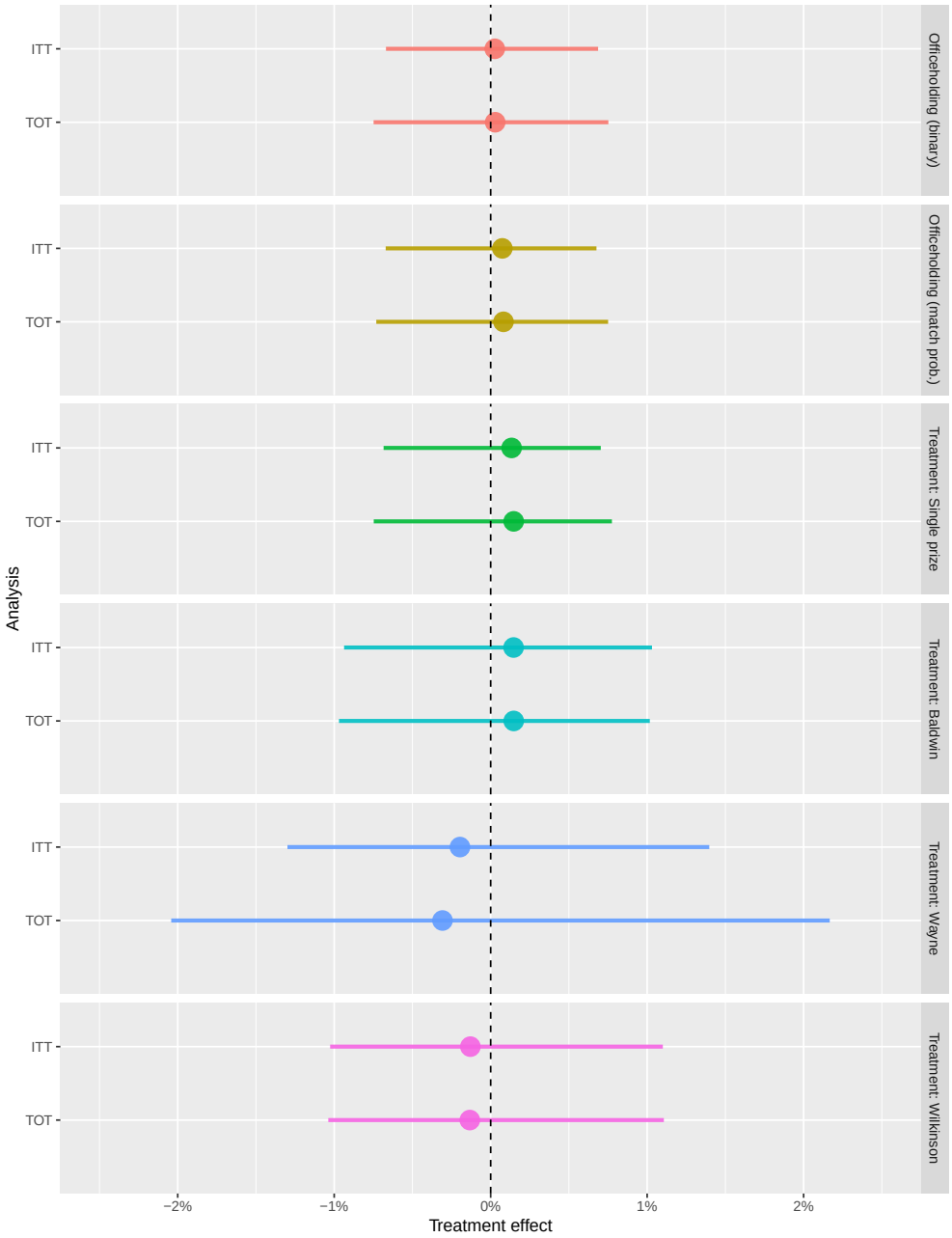
I estimate the effect of winning a lottery prize on officeholding under the Neyman (1923) potential outcomes framework, described in Section OA-7. The nonparametric bootstrapped confidence interval for the intention-to-treat (ITT) estimate on the binary officeholding response contains zero, 95% CI: [-0.006, 0.007].[9] The null result is informative because the estimated effect is not practically different than zero: the upper bound of the interval implies that at most 23 treated participants, less than 1% of the sample treatment group, would select into office as a result of receiving treatment.

The confidence interval on the estimate is unchanged when estimating the effect on the continuous version of the response, which captures the probability of being matched to the officeholder records, or when controlling for noncompliance (Fig. 2). Moreover, the estimates are insensitive to analyses that define treatment as winning exactly one prize or as a function of the geographic location of the land prize.

[8] The significance level for the Bonferroni correction is $\alpha = 0.05/39 = 0.001$.

[9] A randomization confidence interval for the estimate, which is constructed by inverting the exact test of no treatment effect under the assumption of constant treatment effects, yields essentially the same interval.

Figure 2: Sensitivity of treatment effect estimates on officeholding.



Notes: *Officeholding (binary)* and *Officeholding (match prob.)* are the treatment effects on binary and continuous versions of the response variable, respectively. See footnotes to Table OA-9 for response variable definitions and sample exclusions. *Treatment:Single prize* is the effect on the binary response variable when treatment is defined as the receipt of a single prize. *Treatment:Baldwin*, *Treatment:Wilkinson*, and *Treatment:Wayne*, are effects on the binary response variable when treatment is defined as the receipt of a prize in the respective county. Horizontal lines represent 95% bootstrap confidence intervals constructed using 10,000 smoothed bootstrap samples. Smoothed bootstrap samples are generated by sampling observations with replacement and then adding a random normal variate independently to each observation (Hesterberg, 2004). The sample size for each analysis is $N = 21,750$.

4.2 Effect on candidacy

The primary focus of the present study is whether wealth influences officeholding, not whether wealth makes individuals more likely to be candidates for office. To address the latter question, I extract candidate names from two publicly-available election datasets that cover offices from the local level to the federal level, and then link the candidate names to the participant records to form a binary response variable that captures whether participants ran for office between 1805 and 1847, inclusive.[10] I hypothesize that lottery wealth increases the probability of candidacy by decreasing candidacy costs, which follows from the theoretical predictions of citizen-candidate models of political competition (e.g., Osborne and Slivinski, 1996).

Approximately 2% of participants are successfully linked to the candidate datasets; excluding women, orphans, pretreatment candidates, and pretreatment officeholders yields a restricted sample size of $N = 21,639$ with a mean candidacy rate of 1%. Similar to the officeholding analysis, I find no treatment effect on candidacy, 95% CI: [-0.004, 0.004].

4.3 Effect on future wealth

I investigate whether treatment increased long-term wealth by linking the participant records to the full-count 1820 Census and estimating the treatment effect on imputed slave wealth.[11] I successfully match about 23% of the unrestricted participant sample to the 1820 Census ($N = 2,131$). The mean slave wealth in the linked sample is \$1,938 (1820\$), which is more than the market value of two male prime-age field hands in Georgia at the time. I find no treatment effect on slave wealth in 1820, 95% CI:[-0.006, 0.007]. In the discussion below, I estimate treatment effects for each quantile of the 1820 slave wealth distribution.

[10]The candidate names are extracted from two election datasets, the first covering all offices from the local level to the federal level from 1787-1825 (Lampi, 2013) and the second covering federal offices from 1788-1990 (ICPSR, 1984).

[11]The 1820 Census is the earliest surviving enumeration of Georgia's population and represents all counties except for Franklin, Rabun, and Twiggs counties. The records include information on the name of the head of household and the number of slaves held by gender and age group, which I use to impute slave wealth. See footnotes to Table OA-5 for the slave value imputation method.

5 Discussion

In this section, I discuss concerns regarding statistical power, the theoretical and empirical importance of treatment, and possible mechanisms in which wealth influences political selection.

5.1 Statistical power

Did winning the lottery not affect officeholding because the magnitude of the winnings — while sizable relative to statewide median property wealth — was not sufficient to move the average fortunate drawer into the elite class who served in office?

In the short-run, treatment enabled fortunate drawers in the lower-end of the wealth distribution to meet the freehold qualifications for holding office in the legislature.[12] I estimate just under a third of the state population otherwise eligible to hold office did not meet the freehold qualifications. Still, the treatment may be too weak to cause a substantively meaningful increase in the likelihood that fortunate drawers would select into office, given how rare an event it is for citizens to become elected officials.

I conduct a power analysis by simulation, described in Section OA-4, to ensure that the research design allows for the identification of a significant treatment effect. Fig. OA-4 plots power estimates for various treatment effect sizes, holding fixed the sample size and significance level. The simulation results imply that if the actual treatment effect size in a hypothetical finite population is 1.3%, the research design provides an 80% chance of rejecting the null hypothesis that treated and control participants are equally likely to hold office. Since I am unable to reject the null in the sample, it follows that the population effect size is most likely less than 1.3%, which is an effect size not practically different than zero.

[12]Freehold qualifications prohibited a sizable proportion of otherwise–eligible citizens from holding office: approximately 13% of adult male heads of household in 1850 had nominal property values below the freehold qualification for state representative ($250) and 28% were below the threshold for state senate ($500).

5.2 Importance of treatment

Is it the case that the null effects on officeholding are driven by the fact that lottery wealth does not translate into higher long-run wealth, but rather increases present consumption?

Fortunate drawers may perceive lottery wealth as a financial windfall and spend the winnings more quickly than earned wealth (Doherty et al., 2006). Fortunate drawers' mental accounting may also vary depending on the quality of the land prize, which is largely a function of the county in which the land is situated. Fig. OA-3 shows an inverse relationship between the time lag in filing grants for fortunate drawers and the quality of the land prize. The median time lag is 78 and 170 days for those who drew land in soil-rich Baldwin and Wilkinson counties, respectively, compared to 350 days for winners who drew land in Wayne county. The lag in filing grants and the relatively low compliance rate for winners who drew land in Wayne county reflects the poor land quality in the county. However, grants for land in Wayne county were sought after by land speculators. Thus, treatment for winners who drew land in Wayne represents the opportunity to liquidate their claim for cash, while winners who drew land in Baldwin or Wilkinson additionally had the opportunity to farm cotton. Differences in the response to treatment may therefore be explained by variation in the quality of land granted.

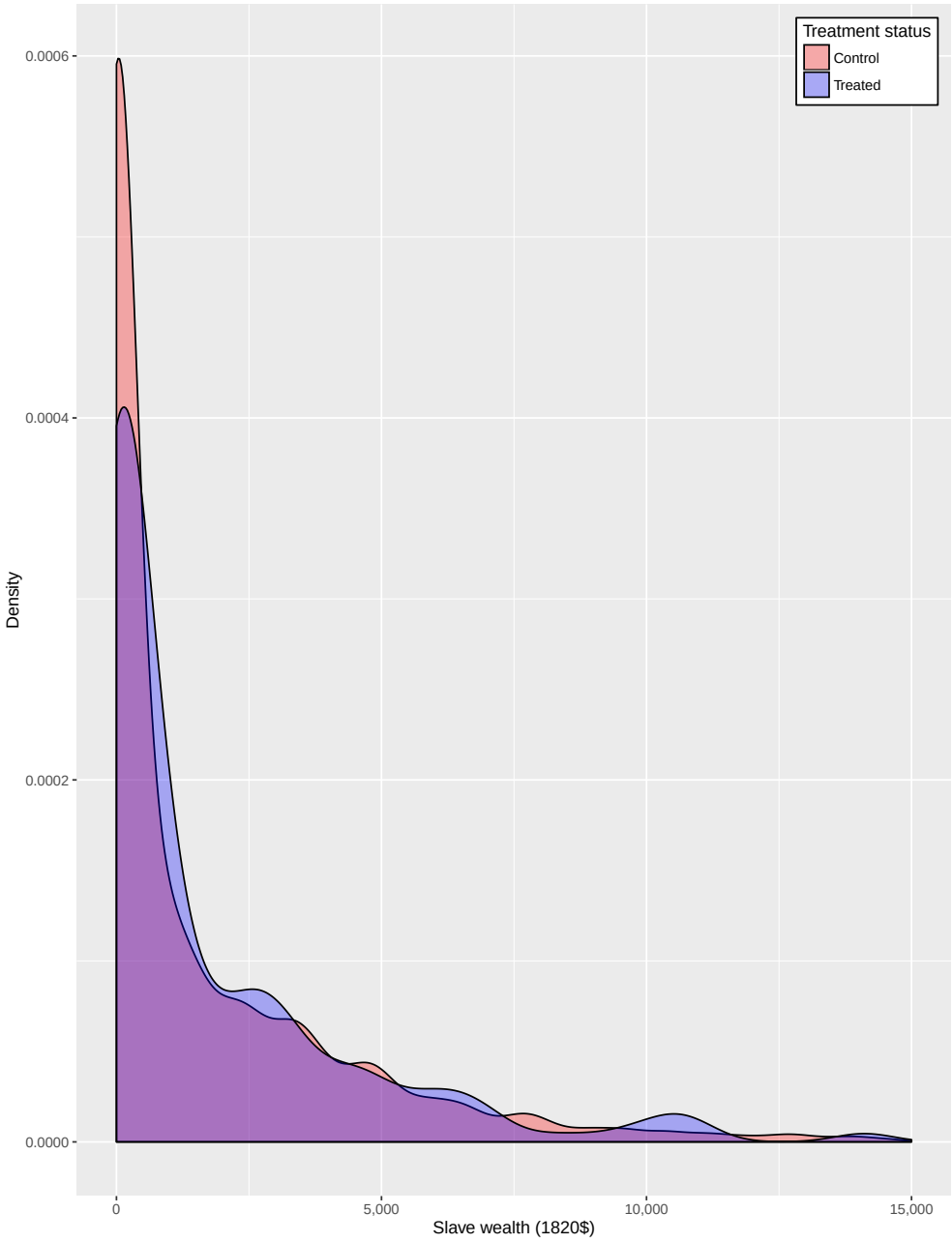
While I find no average treatment effect on future wealth, as reported in Section 4, I provide evidence from quantile regression estimates that treatment increased future wealth for participants near the median of the wealth distribution (Fig. 4).[13] Each point in the plot represents quantile-specific ITT estimates and error bars represent 95% confidence intervals constructed from bootstrapped standard errors. The gains from treatment are concentrated near the median of the wealth distribution; for instance, treatment confers a $330 increase in wealth for participants in the 46% quantile of the distribution, 95% CI: [$58.34, $602.97], which is equivalent to the median wealth of the sample. The upper bound of the confidence

[13]Fig. 3 plots kernel density estimates of future wealth for participants linked to the 1820 Census by treatment assignment. Treated participants are more likely to hold nonzero wealth compared to control participants, and the middle of the distribution is fatter for treated participants.

interval represents an increase in wealth sufficient to own an average-valued male slave and satisfy the freehold qualifications for serving in the legislature. At the median, treated participants are $211 wealthier than controls, although the confidence interval contains zero, 95% CI: [-$71.94, $495.85].

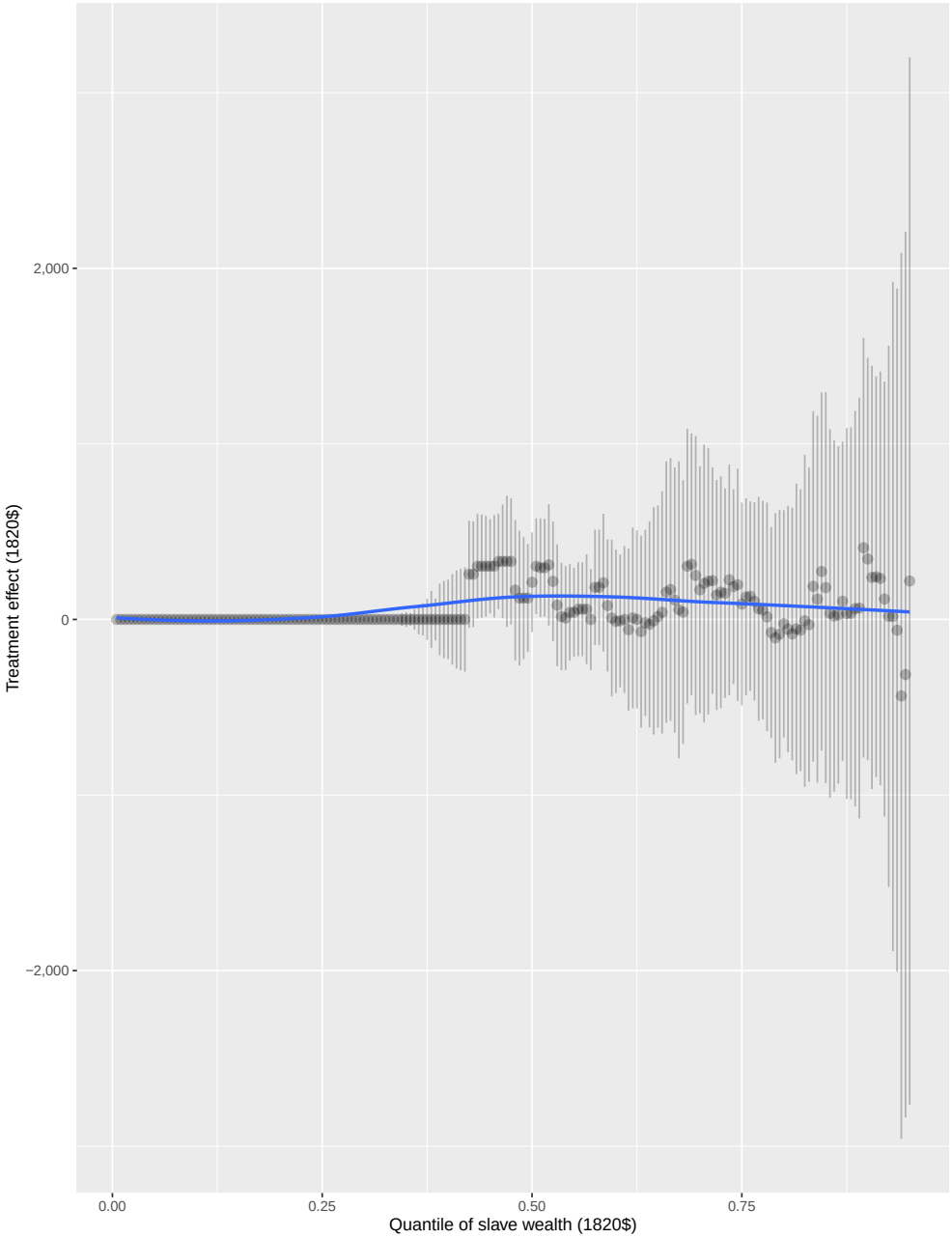
In comparison, Bleakley and Ferrie (2013) finds that winning a prize in the 1832 lottery increases 1850 total census wealth (i.e., combined slave and real-estate wealth) by $200, and that this effect is statistically significant. While Fig. 4 shows the gains from treatment concentrated near the median of the wealth distribution, the authors' results show treatment shifted mass from the middle to the upper tail of the wealth distribution. Differences across studies in how treatment shifts the distribution may be due to the fact that Bleakley and Ferrie's (2013) study includes real estate values in their wealth measure; quantile-specific estimates in the present study may therefore understate the effect of treatment on future wealth. Both studies show no long-run effect of treatment on the lower 40% of the wealth distribution. The authors conclude that the long-run insensitivity to treatment on the lower tail of the wealth distribution is most likely due to differences in the abilities of fortunate drawers (i.e., lower-skilled fortunate drawers could not take advantage of winnings) or short-run consumption spending. The same reasoning applies in the case of the present study. Nonetheless, the results of the present study provide evidence that the size of the wealth shock is meaningful ex-ante.

Figure 3: Distribution of slave wealth by treatment status.



Notes: Kernel density estimate of imputed slave wealth for participants matched to the 1820 Census (see footnotes to Table OA-5 for the slave value imputation method).

Figure 4: Quantile regression treatment effect estimates on slave wealth.



Notes: Estimates from a quantile regression of the treatment effect on imputed slave wealth for participants linked to the 1820 Census ($N = 2,131$). See footnotes to Table OA-5 for the slave value imputation method. The points are quantile-specific estimates of the treatment effect and the error bars represent 95% confidence intervals constructed from bootstrapped standard errors. Quantiles above 0.950 are omitted for display purposes. The line is a LOESS-smoothed estimate of the treatment effect.

5.3 Mechanisms

Through which channel should we expect wealth to influence officeholding, both in general and in the case of the antebellum South? In general, wealth makes it easier to access politics or makes it more important to have political power in order to protect vested interests.

5.3.1 Access to politics

Citizen-candidate models show the number of candidates entering political contests decreases with the cost of running, which is modeled both as campaigning cost and the opportunity cost of running (Osborne and Slivinski, 1996). The opportunity cost of officeholding affects the incentives of different types of individuals to select into public service, with high opportunity costs expected to decrease the likelihood of economic elites selecting into office (Besley, 2005). From these models we expect lottery wealth to increase the probability of candidacy by decreasing candidacy costs or the opportunity costs of officeholding. Indeed, a greater proportion of fortunate drawers ran for office compared to lottery losers (Table OA-9), although the difference is not statistically significant.

Inequalities in wealth that were prevalent throughout the antebellum South may have had a role in stifling access to politics. In 1850, the top decile of property–owners held over half of the total property wealth in Georgia.[14] It may be difficult in this setting for the *nouveau riche* to overcome barriers to entry into politics, such as having requisite political connections in Georgia's factional political system.[15] While there is variation in the level of inequality across counties (Fig. OA-2) and heterogeneity in treatment effects according to county of registration (Fig. OA-7), there does not appear to be a relationship between heterogeneous treatment effects and county-level wealth inequality (Fig. OA-9).

[14]The statewide slave wealth Gini coefficient in 1820 is 0.78 (Table OA-4) and the coefficient for statewide real estate wealth in 1850 is 0.66 (Table OA-7), both indicating high levels of wealth inequality.

[15]Prior to the emergence of political parties starting in the 1830s, Georgia's factional system divided politicians and voters by personal loyalties and regionalism, rather than ideology (DeBats, 1990).

5.3.2 Wealth defense

Personal wealth may make it more important to hold office in order to defend vested interests, which follows from theories of oligarchic power (e.g., Winters and Page, 2009). According to these theories, oligarchs do not need to hold office or explicitly coordinate their political efforts, but instead work toward similar policy goals through political investment or lobbying. Gehlbach et al.'s (2010) model demonstrates that when institutional quality is comparatively low, economic elites select into office themselves to avoid costs associated with political investment or lobbying. Institutional quality as a function of fiscal capacity and newspaper entry was considerably lower in the antebellum Deep South compared to the Border South and Northern States (Grosjean, 2014). Indeed, qualitative evidence suggests that slaveholding cotton planters, who emerged as an oligarchic power following invention of the cotton gin in 1794, held a disproportionate share of political offices at every level (Simons, 1912).

There exists heterogeneity in treatment effects on officeholding according to county of registration: participants registered in counties with lower fiscal capacity, as measured by 1870 per-capita taxation, respond more intensely to treatment than participants registered in counties with comparatively higher fiscal capacity (Fig. OA-10). This finding is consistent with Gehlbach et al.'s (2010) result that economic elites are more likely to select into office themselves when institutional quality is low. Participants registered in counties with lower fiscal capacity are more likely to select into office than participants registered in counties with comparatively higher fiscal capacity. Thus, antebellum Georgia may differ from the general case in that economic elites select into office themselves into office rather than engage in other means of policy formation, and there also appears to be within-state variation according to county-level fiscal capacity. I find no discernible relationship between heterogeneous treatment effects and county-level wealth inequality or slave shares (Fig. OA-9).

5.3.3 Signaling

A third possible mechanism is that personal wealth signals ability to voters (Rossi, 2014). However, increases in wealth resulting from winning the lottery is not informative of fortunate drawers' ability, so it is unlikely that wealth influences officeholding through this channel. Wealth shocks can theoretically improve the human capital development of future generations, which may explain why Rossi's (2014) finding of a significant treatment effect of wealth on posterior political power mostly arises from descendants of lottery winners. Exploring the outcomes of fortunate drawers' descendants is promising since the quantile regression estimates provide evidence that the lottery reduced inequality in the long-run by increasing wealth of median-income participants.

5.3.4 Which mechanism?

To summarize, there is no conclusive evidence as to which mechanism wealth would influence officeholding. We can rule out by logic wealth as a signal of ability as a potential mechanism, and it is unlikely that county-level variation in wealth inequality hampered access to politics. It is most likely that wealth makes it easier to access politics or makes it more important to have political power, or a combination of these two mechanisms.

6 Conclusion

This study uses the first land lottery in U.S. history to estimate the effect of lottery wealth on political selection. I find no evidence in support of the hypothesis that wealth increases the probability of officeholding. I argue that the null result is informative because the estimated effect is not practically different than zero.

What accounts for the null effect on officeholding? Treatment may be too weak to cause a meaningful increase in the probability of fortunate drawers selecting into office, given the rarity of the event. I provide evidence that the size of the wealth shock for fortunate drawers is meaningful ex-ante. Quantile regression estimates provide evidence that the lottery reduced inequality in the long-run by increasing the wealth participants near the median of the distribution. Consistent with the results of Bleakley and Ferrie (2013), I find no long-run effect of treatment on the lower 40% of the wealth distribution. This long-run insensitivity to treatment on the lower tail of the wealth distribution is most likely due to differences in the abilities of fortunate drawers (i.e., lower-skilled winners could not take advantage of winnings) or short-run consumption spending.

Through which mechanisms would we expect wealth to influence officeholding? We can rule out the possibility of wealth as a signal of ability, because increases in wealth resulting from winning the lottery is not informative of fortunate drawers' ability. Rossi's (2014) finding of a significant treatment effect of wealth on posterior political power mostly arises from descendants of lottery winners. An extension of this study might explore the effect of wealth on the officeholding of fortunate drawers' offspring since wealth shocks can theoretically improve the human capital development of future generations. In addition, the quantile regression estimates provide evidence that the lottery reduced inequality in the long-run by increasing wealth of median-income participants, so there is reason to believe that the lottery may have had an effect on dynastic political power.

In general, wealth makes it easier to access politics or makes it more important to defend vested interests. With regard to latter mechanism, qualitative evidence suggests that

antebellum Georgia may differ from the general case in that economic elites select into office themselves into office rather than engage in other means of policy formation. This pattern is consistent with the idea that economic elites select into office themselves when institutional quality is comparatively low. With regard to access to politics, I show that heterogeneous treatment effects on officeholding and county-level fiscal capacity is inversely correlated. Wealth inequality may have had a role in stifling access to politics, although there is no clear relationship between heterogeneous treatment effects on officeholding and county-level wealth inequality. Further investigations in alternative settings would help determine whether the results of the present study would be different in a context of less wealth inequality.

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