Land Lotteries, Long-term Wealth, and Political Selection

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Abstract

Does personal wealth cause individuals to select into office? This study exploits the 1805 and 1807 Georgia land lotteries to investigate the hypothesis that wealth increases posterior political power. Most eligible males participated in the lotteries and more than one-in-ten participants won a land lot prize worth over half of median property wealth. I find no evidence that lottery wealth increases the likelihood of officeholding or running for office, and argue that these null findings are informative because the estimates are not practically different than zero. I estimate that receiving treatment in the 1807 lottery increases slave wealth in 1820 by $282 [$156, $408]. This treatment effect, which represents sufficient property wealth to serve in the state legislature, provides evidence that the wealth shock produced by the lottery is meaningful ex-ante.

Keywords: political selection; candidacy; long-term wealth; land lottery; 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 advance the interests of their constituents. Personal wealth is expected to reduce the opportunity costs of holding office and may also make it more important for wealthy citizens to hold office. While several studies have studied the effect of officeholding on wealth accumulation (e.g., Eggers and Hainmueller, 2009; Querubin and Snyder Jr, 2013; Truex, 2014), research on the extent to which wealth affects officeholding is more limited.

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, where land closer to the city is more valuable, 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%.[1]

The present study uses the first two Georgia land lotteries as natural experiments to investigate the hypothesis that personal wealth increases posterior political power. The 1805 lottery was the first public land lottery in U.S. history, only to be replicated in 1807 and six subsequent lotteries in Georgia.[2] The state distributed over one million acres in the 1805 lottery, two million acres in the 1807 lottery, and 23 million acres in all eight lotteries (Cadle, 1991). A sizable majority of eligible adult white males participated in the lotteries,

[1]The linear model used to obtain this estimate assumes that distance of the land to the city is uncorrelated with unobserved individual-level predictors of political labor supply, such as the opportunity costs associated with holding office.

[2]The six subsequent lotteries in Georgia occurred in 1820, 1821, 1827, 1832, 1832 "Gold Lottery", and 1833 ("Fractions Lottery").

and about one-in-ten won a land prize with an estimated mean value of least $800, which represents more than half of median property wealth at the time of the lotteries. Since land prizes could be readily sold in a secondary market for public land, this random assignment of land generates an ex-ante exogenous shock to personal wealth. However, I find no evidence in support of the hypotheses that wealth increases the likelihood of officeholding or candidacy. I argue that these null results are still informative because the estimates are not practically different than zero.

Georgia's 1832 lottery has 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 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 lottery winners or their descendants.[3] 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 find that lottery wealth exacerbates wealth inequality in the long-run by shifting mass from the middle of the wealth distribution to the upper tail.

While I find no evidence of that winning a prize in the 1805 lottery increased the future wealth of lottery winners, I find that winning a second prize in the 1807 lottery significantly increases slave wealth in 1820 by $282, 95% CI:[$156, $408]. The treatment effect estimate is similar in size to Bleakley and Ferrie's (2013) estimate that winning a prize in the 1832 lottery significantly increases 1850 total census wealth (i.e., combined slave and real-estate wealth) by $200.

This paper proceeds as follows: Section 2 provides a theoretical discussion of the channels through which wealth may or may not impact posterior political power; Section 3 provides a historical background, including details on the freehold qualifications for holding office, as well as the development and implementation of the 1805 and 1807 lotteries; Section 4

[3]Comparing lottery winners to lottery-eligible men not found in the list of winners is typically nonexperimental comparison because lottery players self-select; however, the authors estimate a near-universal participation rate (97.8%).

describes the data and record linkage procedure, and provides summary statistics on the wealth of officeholders; Section 5 specifies the treatment effect estimator, tests the assumption of random treatment assignment, and provides treatment effect estimates; Section 6 discusses concerns regarding statistical power and the importance of treatment; and Section 7 concludes.

2 Theoretical considerations

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. Materialist interpretations of oligarchic power (e.g., Winters and Page, 2009) view personal wealth as a source of political power because it is highly concentrated among a minority of the population, can be translated into political influence, and accompanies a set of political interests, such as wealth defense. According to this interpretation, wealthy citizens 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 weak, economic elites tend to select into office themselves to avoid costs associated with political investment or lobbying.[4]

2.1 Political labor supply

To inform a hypothesis of the effect of lottery wealth on officeholding, consider a static model of political labor supply in which citizens' selection into office results from a "wage effect" and a "wealth effect." The wage effect is tied to nonpolitical income, which is positively related to the opportunity cost of officeholding. However, an increase in nonpolitical income

[4]In Grosjean's (2014) study, 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. Indeed, qualitative evidence discussed in Section 3 suggests that that slaveholders held a disproportionate share of the political offices in the antebellum South.

also decreases the importance of earned income in politicians' overall utility profiles and thus decreases the opportunity cost of officeholding. Lottery wealth isolates the wage effect from the wealth effect because land lotteries did not provide winners additional skills which could be turned into labor market earnings, but rather provided them with additional wealth. Land lotteries increased wealth without increasing wages, and is thus expected to increase the probability of participating in politics.

Lottery wealth is also expected to increase future wealth. In this simple model, overall earnings are a separable function of wealth and skill, which implies that an exogenous wealth shock can amplify the effects of some types of skill on earnings. Moreover, there may be credit or liquidity constraints that make it impossible for non-wealthy individuals to capitalize on their skills in the labor market, but are able to overcome these constraints with an exogenous increase in wealth.

2.2 Candidacy and barriers to entry

Citizen-candidate models (Osborne and Slivinski, 1996; Besley and Coate, 1997) demonstrate that 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. The opportunity cost of officeholding affects the incentives of different types of individuals to select into office, 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 either decreasing the costs of running for office or the opportunity costs of participating in politics.

In traditional citizen-candidate models, the set of eligible candidates is the set of eligible voters. In the context of 19^{th} century America, the former was a subset of the latter because many otherwise eligible citizens could not run for office due to freehold requirements for officeholding.[5] Corvalan et al. (2018) build on the citizen-candidate model by adding a

[5]In English common law, a freehold is ownership of land and the structures attached to the land.

freehold requirement for running for office, which results in differing ideological distributions between eligible candidates and voters. Their model predicts that when freehold qualifications are high enough to be binding — i.e., when the qualifications for office exceeded the median wealth of the constituency — removal of these restrictions leads to the election of less wealthy politicians who favor more redistributive policies. As described in further detail in section below, the economic barriers to entry into electoral politics in Georgia were not likely binding in the sense that freehold requirements did not exceed the estimated median property wealth at the time of the lotteries.

3 Historical background

The invention of the cotton gin in 1794 made the cultivation of cotton immensely profitable, and created a new oligarchic power in the slaveholding cotton planter (Simons, 1912). Slaveholders held a disproportionate share of the political offices, despite representing less than 10% of the total white population in the state. Inequality in political represenation was prevalent throughout the antebellum South. For example, Helper (1860) rails against the "slave–driving oligarchy" which dominated political offices in the antebellum South:

> The magistrates in the villages, the constables in the districts, the commissioners of the towns, the mayors of the cities, the sheriffs of the counties, the judges of the various courts, the members of the legislatures, the governors of the States, the representatives and senators in Congress — are all slaveholders....There is no legislation except for the benefit of slavery and slaveholders.

The slaveholders' dilemma was to secure political dominance amid universal white male suffrage. The 1798 state constitution, which was in effect for the entirety of the antebellum period, extended rights to elect members of the legislature to adult white males.[6] Propertyless whites were discouraged from voting on account of a poll tax that the legislature

[6]On the matter of suffrage, the 1798 constitution was a radical document compared to the constitutions of other southern states (Beard, 1913).

maintained during the antebellum period. Slaveholder candidates, however, often paid the poll taxes of poor whites in exchange for political support (Meyers and Williams, 2012). Slaveholding cotton planters found allies in members of the clergy and the professional class, who often owned a few slaves for personal service, and this alliance created a class of "social retainers" who defended the interests of slaveholding planters (Simons, 1912). The three–fifths clause in the 1798 state constitution counted slaves as three–fifths of a person for the purpose of representation in the legislature and thus fortified the slaveholders' control of the legislature (Coulter, 1960).

3.1 Barriers to officeholding

The 1798 state constitution also tied officeholding eligibility to land and property values. Beyond age and residency requirements, requirements for holding a seat in the state House included owning settled freehold estate worth $250 or possessing taxable property to the amount of $500 within the county. The requirements for the office of state senator were double those amounts. Qualifications for governor, an office opened to direct elections in 1824, included owning at least 500 acres of land within the state and other property to the amount of $4,000. Amendments to the constitution removed freehold qualifications for the legislature in 1835 and for governor in 1847.

A substantial portion of the otherwise eligible population were constitutionally restricted from holding office. I estimate that about one-in-ten citizens otherwise eligible for officeholding had nominal property values below the threshold for eligibility for state representative; one-in-five is below the property threshold for state senate; and about 40% do not meet the qualifications for governor.[7] However, winning a land lottery prize enabled otherwise eligible lottery winners who did not satisfy freehold qualifications to hold office in the legislature. Using county-level data from the 1850 Census (Haines, 2004), I estimate that the mean value

[7]The estimates are made using real estate wealth in 1850 from a sample of adult male heads-of-household who were born in Georgia, living in Georgia at the time of the 1850 Census, and have nonmissing surnames and property values.

of a land lot prize in either of the first two lotteries was at least $800, which represents more than half of median property wealth at the time of the lotteries.[8]

The 1798 constitution only provides for salaries of members of the executive branch and judiciary.[9] The lack of political income for legislators may impose an additional barrier for less wealthy citizens to run for office because of the opportunity costs associated with holding office. Georgia's legislature was not professionalized at the time of the lotteries and was only mandated by the constitution to meet once a year.[10]

3.2 The 1805 and 1807 lotteries

In the wake of public land fraud scandals, Georgia's legislature created a lottery system to distribute 1.3 million acres of newly acquired public land ceded by the Creek tribe. The 1805 lottery carved out three new counties from the land: Baldwin and Wilkinson counties, each divided into five districts, and Wayne county, divided into three districts (Clayton and Adams, 1812).[11] The 1807 lottery extended the boundary between Baldwin and Wilkinson, more than tripling the number of districts within the two counties.

Free white adult men and orphaned children were eligible for a single draw, while married men with children and widows with children were eligible for two draws in the 1805 lottery. The eligibility rules for the 1807 lottery were similar, with the main exception that adult unmarried females could participate and 1805 lottery winners were excluded from participation.[12] Registration for each lottery was voluntary and required a payment of 12.5 cents per

[8]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. The mean value of land lot prize is the mean value of a land lot prize divided by the median real estate wealth in 1850. The 1850 values are deflated to 1805 dollars using a historical consumer price index (Officer and Williamson, 2012).

[9]*Per diem* compensation for members of the legislature is not defined until the 1877 constitution.

[10]The legislature typically met more than once. For example, in 1805 the House and Senate each met about 30 times.

[11]Fig. OA-2 maps the original counties created by the 1805 lottery, and Fig. OA-1 provides a timeline of 1805 and 1807 lottery events.

[12]Table OA-1 provides further details for lottery eligibility. Participants were required take an oath when "doubt exist in the minds of the said justices" regarding the veracity of participants' eligibility. The legislature criminalized making false statements concerning eligibility in the land lotteries. The law specifies that if found guilty in a jury trial, half of the defendant's land went to the informer and the other half is

draw. I estimate that approximately 85% of eligible males living in Georgia participated in the 1805 lottery and 70% of eligible males participated in the 1807 lottery.[13]

Prior to each lottery, a surveyor was appointed by the legislature to map the districts into square lots. During the 1805 lottery, tickets representing each 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. Two lottery wheels were used in the 1807 drawing: one containing the names of participants, and the other containing lot numbers. Blank tickets in number equal to difference between the number of registered draws and prize lots were added to the lottery wheel. Tickets from each wheel were drawn simultaneously to form a combined ticket, and the participant won if the combined ticket included a prize.

Lottery winners were required to pay $4 per hundred acres for lots won in the 1805 lottery, or $6 per hundred acres for lots won in the 1807 lottery, in order to obtain the title on the land lot.[14] Winning a prize in the lottery represents a pure wealth shock because there was no homesteading requirement and lottery winners could easily sell their grants in a secondary market for public land (Weiman, 1991). In some cases, lottery winners sold their grants to land speculators, who in turn sold the land to out-of-state settlers (Davis, 1981). Land speculators often sought out lottery winners who drew particularly valuable lots (Cadle, 1991).

4 Data and descriptive statistics

The primary source of data for this study is Graham's (2004) record of 1805 lottery winners and losers. The 1805 lottery is the only Georgia lottery to have recorded the names of

reverted to the state and auctioned as land fractions.

[13]The 1805 calculation was made by taking the proportion of adult male participants over the total white male population aged 16 and over in the 1800 Census (Table OA-3). The 1807 calculation subtracts the number of adult male winners in the 1805 lottery from the numerator.

[14]Lottery winners had 12 months following the drawing to claim their prize under law; however, the legislature extended the deadline for claiming prizes on an annual basis until 1815. If lottery winners did not claim their land prior to the deadline, then the lots were reverted to the state and sold in a public auction.

all lottery participants. The records contain information on participants' name, county of registration, lottery draws, and prizes of lottery winners. Identifying remarks next to the participants' names provides additional information, such as generational titles and orphan status. Table 1 reports that 15% of the unrestricted sample of 1805 lottery participants ($N = 23,927$) received at least one lottery prize and were registered for 1.65 draws, on average. Four percent of 1805 lottery winners ($N = 3,707$) won two prizes. Additionally, I use lottery winner grant records for the 1807 lottery (Graham, 2011) to form a sample of 1807 lottery winners ($N = 9,687$), in which 5% of the sample won two prizes.[15]

In order to test the hypothesis that wealth increases the probability of officeholding, I link participant names to a historical roster of officeholders published in the *Georgia Official and Statistical Register* (Archives, 1978, 1990). The roster includes information on name, jurisdiction, and term date for all elected and appointed officeholders from the state's colonial period to 1990. As reported in Table 1, 20% of adult males who participated in the 1805 lottery are successfully matched to officeholder records. About 10% of matched participants held office before the drawing of the 1805 lottery. Three-quarters of these officeholder-participants first served in the state House, while the rest started their political careers in the state Senate, U.S. House, or other state executive offices.

I employ a machine learning approach for linking lottery participants with officeholder records. First, I link 1807 lottery winners with officeholders based on the exact match of surname and Soundex code of the first name, and then manually deduplicate the matched records. Second, I train an ensemble of algorithmic models on the 1807 records to classify correct matches, using participant characteristics (e.g., the frequency and length of surnames) and match characteristics (e.g., the Euclidean distance between participants' county of registration and officeholders' constituency) as features of the model.[16] Lastly, I use the ensemble

[15]Since the 1807 lottery records do not include information on the number of registered draws, I impute the number of draws by assigning two draws to all participants, except for women and orphans, who are assigned one draw. The mean of the imputed number of draws among 1807 winners is almost identical to the mean of the actual number of registered draws among 1805 winners.

[16]The cross-validated mean squared error on the training set is under 3%. Table OA-9 provides information on the record link ensemble's candidate learners, weights, and error estimates.

fit to automatically deduplicate 1805 participant records matched with officeholders on the basis of an exact match on surname and Soundex code of the first name, using a prediction threshold of 50% to classify correct matches. Thus, the *Officeholder* outcome variable is a binary variable indicating whether the participant is linked to the officeholder records. I also use a continuous version of the variable based on predictive probabilities that serves as a proxy for the posterior political power of both lottery participants and their relatives who share the same surname.

4.1 Candidacy

To address the question of whether wealth makes individuals more likely to be candidates for office, I extract candidate names from two election datasets, the first covering all offices from the local level to the federal level from 1787 to 1825 (Lampi, 2013) and the second covering federal offices from 1788 to 1990 (ICPSR, 1984). I link the candidate names to the participant records following the procedure and ensemble weights used for linking participant and officeholder records. The resulting *Candidate* outcome is a binary variable that captures whether participants ran for office. Only two percent of participants are linked to candidate records, since the candidate records are more limited in scope compared to the officeholder records. About a quarter of candidate-participants ran for office prior to 1805.

4.2 Future wealth

Finally, 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. 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.[17] I match 21% of 1805 lottery participants to the 1820 Census ($N = 5,238$). The mean slave wealth in the linked sample is \$1,513, which is more than the market value of two male prime-age field hands in Georgia at the time (Table OA-5).

4.3 Descriptive statistics: Officeholder wealth

How does the wealth of the political class compare with the wealth of the general population? I link the full list of officeholders to the 1820 and 1850 Censuses in order to get a sense of where officeholders stand in the distribution of slave wealth and real estate wealth, respectively. Following the record linkage procedure described in the previous section, I successfully link less than 1% of officeholders to samples of adult heads of household living at the time of the censuses.

Table 2 provides basic summary statistics on the wealth-holding of officeholders versus the rest of the sample. Officeholders matched to the 1820 Census hold nearly twice the amount slave wealth on average compared to the rest of the sample, and two distributions are statistically different ($p = 0.002$). Similarly, officeholders matched to the 1850 Census hold nearly twice the value of real estate wealth on average compared to non-officeholders; although, there is no evidence against the null that the distributions are equal ($p = 0.21$).

[17] Access to the full-count data is granted by agreement between UC Berkeley, and the Minnesota Population Center (IPUMS USA). The Minnesota Population Center has collected digitized census data for 1790-1930 microdata collection with contributions from Ancestry.com and FamilySearch.

Table 1: Distribution of pretreatment and response variables by sample.

Variable	Sample	N	Min.	Mean	Max.	S.d.
Pretreatment variables						
1820 Census Match	1805 winners & losers	23,927	0	0.16	0.91	0.31
	1805 winners	3,707	0	0.17	0.9	0.31
	1807 winners	9,687	0	0.64	0.9	0.14
Candidate			0	0	1	0.07
			0	0.01	1	0.07
			0	0	1	0.05
Officeholder			0	0.02	1	0.14
			0	0.02	1	0.14
			0	0.02	1	0.13
Junior			0	0.03	1	0.18
			0	0.03	1	0.16
			0	0.02	1	0.16
Senior			0	0.03	1	0.18
			0	0.04	1	0.2
			0	0.03	1	0.18
Surname Frequency			1	32.61	348	58.62
			1	32.6	348	57.49
			1	12.2	127	20.4
Surname Length			3	6.26	14	1.59
			3	6.28	13	1.59
			1	6.3	15	1.67
Treatment variables						
Treated	1805 winners & losers	23,927	0	0.15	1	0.36
	1805 winners	3,707	0	0.04	1	0.19
	1807 winners	9,687	0	0.05	1	0.23
# draws			1	1.65	2	0.48
			1	1.82	2	0.38
			1	1.81	2	0.39
Outcome variables						
Candidate	1805 winners & losers	21,732	0	0.02	1	0.14
	1805 winners	3,346	0	0.02	1	0.13
	1807 winners	7,797	0	0.02	1	0.14
Officeholder			0	0.2	1	0.4
			0	0.2	1	0.4
			0	0.17	1	0.37
Officeholder (match prob.)			0	0.15	0.89	0.3
			0	0.15	0.86	0.3
			0	0.13	0.95	0.3
Slave wealth (1820$)	1805 winners & losers	5,252	0	1,513.23	45,072.19	2,766.06
	1805 winners	828	0	1,633.39	34,714.06	3,129.27
	1807 winners	9,308	0	219.88	45,072.19	1,395.01

Table 1: See notes to Fig. 1 for definitions of pretreatment covariates. *Treated* is defined as winning at least one prize for the 1805 winners & losers sample, and winning two prizes for the samples restricted to winners. *# draws* is the number of registered draws. *Candidate* and *Officeholder* indicates whether participants ran for office or held office, respectively. The samples exclude women and orphans. *Officeholder (match prob.)* is the probability of being matched to the officeholder records for the same restricted sample as the binary case. *Slave wealth (unweighted)* is the imputed slave wealth for participants matched to the 1820 Census (see footnotes to Table OA-5 for the slave value imputation method); Table OA-7 reports summary statistics for pretreatment counties of registration.

Table 2: Distribution of census wealth by officeholding status.

Variable	**Status**	**N**	**Min.**	**Mean**	**Max.**	**S.d.**
1820 Census						
Slave wealth	Non-officeholder	31,935	0	1,222	116,040	3,037
	Officeholder	79	0	2,215	29,562	4,327
$p = 0.002$	all	32,014	0	1,224	116,040	3,041
1850 Census						
Real estate wealth	Non-officeholder	52,990	1	2,221	310,000	5,408
	Officeholder	4	350	3,962	10,000	4,304
$p = 0.21$	all	52,994	1	2,221	310,000	5,408

Table 2: Slave wealth adjusted to 1850$ values (Williamson, Samuel H, 2018). The footnotes to Table OA-5 describes the slave value imputation method for calculating slave wealth in 1820. p-value is obtained from a Mann-Whitney-Wilcoxon test under the null that officeholder and non-officeholder distributions are equal.

5 Estimation and results

I estimate the following linear model:

$$y_{i,s} = \#\ \text{draws}_{i,s} + \delta\, \text{treat}_{i,s} + \epsilon_{i,s}, \tag{1}$$

where $y_{i,s}$ is takes the form of the officeholding, candidacy, or 1820 slave wealth measure for participant $i = \{1, ..., N\}$ in sample s. $\#\ \text{draws}_{i,s} \in \{1, 2\}$ represents the number of draws registered by each participant. When the sample includes both 1805 winners and losers, $\text{treat}_{i,s}$ is a binary treatment assignment variable that assumes unity for participants who won at least one prize in the 1805 lottery and assumes a zero value for participants who did not win a prize. The coefficient of interest, δ, corresponds to the intention-to-treat (ITT) estimate of the sample average treatment effect. When the sample is limited to winners in either the 1805 or 1807 lotteries, $\text{treat}_{i,s}$ assumes unity for participants who win two prizes in the lottery and is otherwise zero. In this case, the ITT effect captures the marginal effect of winning a second land prize.

5.1 Balance

The model assumes treatment assignment is random conditional on the number of registered draws.[18] While this assumption cannot be directly tested, we can verify that there is balance in treatment assignment with respect to the pretreatment covariates. Statistically significant treatment effects on pretreatment covariates at the level of $\alpha = 0.05$ indicate imbalance in treatment assignment. Fig. 1 plots the p-values corresponding to the treatment effect estimated by running Eq. 1 on each pretreatment covariate summarized in Table 1. Treatment assignment is balanced across all pretreatment covariates among 1805 lottery winners and losers. When the sample is restricted to 1805 lottery winners, only indicators for participants registered in Clarke ($p = 0.02$) and Lincoln ($p = 0.001$) counties are significant. Among 1807 lottery winners, lottery participants who held office ($p < 0.001$) or ran for office ($p = 0.004$) prior to the 1805 lottery are significantly more likely to have won two prizes rather than a single prize, controlling for the number of draws. Imbalance with respect to pretreatment political activity might suggest that the 1807 lottery process was rigged in favor of the political class; although, only the imbalance on pretreatment officeholding is significant when accounting for the multiple comparisons made for the balance tests.[19] Additionally, 1807 lottery winners with generational titles are more likely to win a second prize ($p < 0.001$); as well as winners with more common surnames ($p = 0.006$).

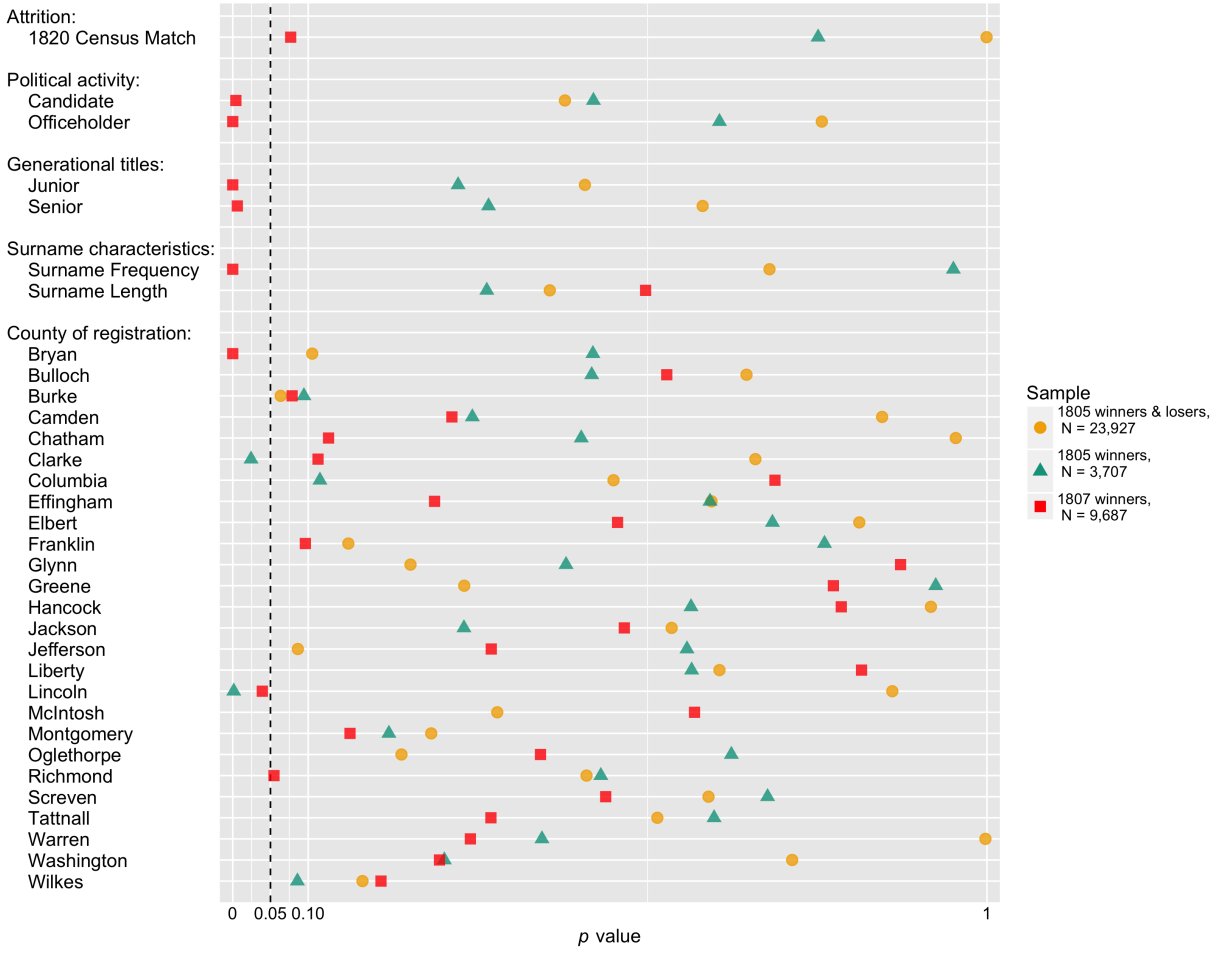
5.2 ITT estimates

Table 3 presents the ITT treatment effect estimates on each outcome of interest. The 95% confidence intervals for the ITT estimates on officeholding tightly surround zero for each sample. In the discussion below, I argue that these null results on officeholding are informative because the estimated effects are not practically different than zero. For example, the upper bound of the interval of the treatment effect estimate on *Officeholder* implies that,

[18] Section OA-6 formally states the assumptions needed to recover an unbiased estimate of δ.

[19] The significance level for the Bonferroni correction is $\alpha = 0.05/33 = 0.001$.

Figure 1: Balance in treatment assignment by lottery sample.



Notes: *1820 Census Match* is the probability of being linked to the 1820 Census; *Candidate* and *Officeholder* indicates participants who ran for office or held office, respectively, prior to the 1805 lottery. Samples include all lottery participants.

at most, 1% of the sample treated 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 represents the probability of being matched to the officeholder records and serves as a proxy for the posterior political power of relatives of lottery participants.

While I find no evidence of a treatment effect on candidacy for the sample of 1805 winners and losers, the point estimates on the samples of 1805 and 1807 winners imply that the marginal effect of winning a second prize in the lottery significantly increases the probability of running for office by 1 to 2%. However, the estimates lose their significance when the outcome is the probability of being matched to candidate records (Table OA-10).

Lastly, I find no evidence that winning a prize in the 1805 lottery increased future slave wealth. In the sample of 1807 winners, however, the winning a second prize significantly increases slave wealth in 1820 by \$282, 95% CI:[\$156, \$408]. This significant finding is robust to including pretreatment covariates in the regression and weighting the slave wealth outcome with the probability of being matched to the 1820 Census (Table OA-11). The treatment effect estimate is similar in size to Bleakley and Ferrie's (2013) estimate that winning a prize in the 1832 lottery significantly increases 1850 total census wealth (i.e., combined slave and real-estate wealth) by \$200.

Table 3: ITT treatment effect estimates.

Sample \ Outcome	Candidacy	Officeholder	Officeholder (Match prob.)	Slave wealth (1820\$)
1805 winners & losers	-0.001 [-0.007, 0.003]	0.004 [-0.009, 0.01]	0.003 [-0.007, 0.01]	156.68 [-51.13, 364.49]
1805 winners	0.02 [0.0004, 0.05]	-0.01 [-0.08, 0.06]	-0.006 [-0.06, 0.04]	-155.12 [-1,251.37, 941.12]
1807 winners	0.01 [0.008, 0.02]	0.001 [-0.03, 0.03]	0.002 [-0.02, 0.02]	282.38 [156.74, 408.02]

Table 3: Values in brackets represent 95% confidence intervals for treatment effect estimates derived from the standard errors of the linear model (Eq. 1). Orphans and women are excluded from each sample for candidacy and officeholding outcomes.

6 Discussion

In the short-run, treatment enabled lottery winners in the lower-end of the wealth distribution to meet the freehold qualifications for holding office in the legislature. Still, the treatment may be too weak to cause a substantively meaningful increase in the likelihood that lottery winners 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-3, to ensure that the research design allows for the identification of a significant treatment effect. The simulation results imply that if the actual treatment effect size in a hypothetical finite population is 2.1%, 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 2.1%, an effect size not practically different than zero.

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? Lottery winners may perceive lottery wealth as a financial windfall and spend the winnings more quickly than earned wealth (Doherty et al., 2006).[20] While I find no evidence of that winning a prize in the 1805 lottery increased the future wealth of lottery winners, I provide evidence from quantile regression estimates that treatment increased future wealth for 1805 lottery participants near the median of the wealth distribution (Fig. OA-11). Specifically, I find that treatment confers a $171 increase in wealth for participants at the median, 95% CI: [$24.76, $317.35]. The upper bound of the confidence interval represents an increase in wealth sufficient to satisfy the freehold qualifications for serving in the state House.

In contrast, quantile regression estimates imply that winning an additional prize in the

[20]Lottery winners' 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-4 shows an inverse relationship between the time lag in filing grants for lottery winners 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 comparatively poor land quality in the county (Graham, 2005).

1807 lottery generated large gains in future wealth for individuals at the upper tail of the wealth distribution (Fig. OA-13). For example, treatment confers a $1,820 [$1,195, $2,446] increase in wealth for 1807 lottery winners at the 90 percentile. The quantile-specific estimates for 1807 lottery winners is similar to Bleakley and Ferrie's (2013) quantile-specific estimates, which imply that winning a prize in the 1832 lottery shifted mass from the middle to the upper tail of the 1850 census wealth distribution. Bleakley and Ferrie's (2013) estimates also show no long-run effect of treatment on the lower-end 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 lottery winners (i.e., lower-skilled lottery winners could not take advantage of winnings) or the possibility that winnings were spent on short-term consumption rather than invested. 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.

7 Conclusion

This study exploits the first two land lotteries in U.S. history to estimate the effect of lottery wealth on posterior political power. Personal wealth is expected to reduce the opportunity costs of holding office and may make it more important for the wealthy to hold office. I find no evidence in support of the hypotheses that wealth increases the probability of running for office or holding office; however, I argue that these null results are informative because the estimated effects are not practically different than zero.

What accounts for these null effects? Treatment may be too weak to cause a meaningful increase in the probability of lottery winners selecting into office, given the rarity of the event. I provide evidence that the size of the wealth shock for lottery winners is meaningful ex-ante. Quantile regression estimates provide evidence that the 1805 lottery reduced inequality in the long-run by increasing the wealth of participants near the median of the distribution; in

contrast, the 1807 lottery exacerbated inequality by increasing wealth at the upper tail of the distribution. Consistent with the results of Bleakley and Ferrie (2013), I find no long-run effect of treatment in either lottery 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 lottery winners or short-run consumption spending.

Through which mechanisms would we expect wealth to influence officeholding? In general, wealth makes it easier to access politics or makes it more important to defend vested interests. With regard to the 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.

While winning a land lottery prize enabled otherwise eligible lottery winners who did not satisfy freehold qualifications to hold office in the legislature, pervasive wealth inequality may have had a role in stifling access to politics in Georgia in the early 19^{th} century. In 1850, the top decile of property-owners held over half of the total property wealth in Georgia.[21] It may be difficult in this setting for even the *nouveau riche* to overcome barriers to entry into politics, such as the opportunity costs associated with participating in politics. 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.

[21]This calculation is made using a sample of adult male heads of household who were born in Georgia and were living in Georgia at the time of the 1850 Census. 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-6), both indicating high levels of wealth inequality.

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