

The Effect of Income on Turnout: Evidence from Public Salary Records and Administrative Voting Data*

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Abstract

How does income affect voting behavior? Existing work is clear: there is a large turnout gap between high- and low-income voters. A widely-held assumption derived from this between-person relationship is that changes in any given person's income should cause a change in that same person's political participation. Recent work has interrogated this assumption using within-person economic windfalls, however, and found no subsequent effects on turnout. Using public records requests for data on the salaries of US public employees and other administrative data on over 570,000 people, I examine whether enduring changes to annual income between 2014 and 2020 affect political participation. The public employee data span everyone from custodial workers to college professors and cover a total of over 13,000 different occupations. I link the salary records to administrative data on voter registration and turnout to track within-person changes in income and participation over time. Results suggest that for a given person, changes in annual income correspond to extremely small changes in turnout. The effect is at most one-third the size of the gap estimated using conventional cross-sectional designs that compare the turnout rates of otherwise similar voters with marginally different incomes. Data on salary trajectories within occupations suggests turnout patterns may be more a function of selection than individual economic incentives. These findings have implications for our understanding of what drives political participation as well as the types of policy interventions that might affect it.

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1 Introduction

Election after election, turnout in America follows a simple pattern: people who make more money turn out to vote more than those who make less. Our understanding of how income affects turnout is overwhelmingly based on differences in turnout between individuals. Scholars have largely assumed that the same pattern holds within-person, assuming that when *the same* individual makes more money it causes them to become more likely to vote in subsequent elections. There is scarcely any work examining whether enduring increases to salary over a person’s career affect levels of political participation. Knowing whether the within-person relationship exists is critical for understanding whether resources like income actually drive political participation or whether other, largely unobservable, attributes or endowments shape both income and participation. Sorting this out has implications for whether interventions like more progressive taxation or guaranteed income can reduce existing inequalities in voting—and therefore in representation in government.

Some scholars suggest that income should have a causal effect on voter turnout within person. Foundational accounts stress that with less money there is less free time for nonessentials like voting and that income plays a large role in determining people’s neighborhoods, social circles, and behavioral norms (Wolfinger and Rosenstone 1980). This “Resource Model” explains that participation in politics stems from money, civic skills, and time (Brady et al. 1995). Consequently, as these resources ebb and flow so too should voter turnout and participation more generally.

Other scholars stress the importance of early life experiences on political participation later in life and show that childhood economic circumstances have a large impact on political attitudes and behaviors (Ghitza et al. 2023; Ojeda 2018), and once individuals reach voting age, their course is set (Akee et al. 2020) and their decision to vote becomes largely habitual (Coppock and Green 2016).

However, observed between-person income and turnout patterns are observationally equivalent with both accounts. People might earn more money and turn out more in elections

because income drives participation—or they may do both these things because their personality type drives both. The inferences that it is possible to draw from the sorts of cross-sectional data typically employed do not sort this out. Researchers who have used individual-level survey data to get around some of these limitations have found that these data come with their own set of drawbacks in the form of the misreporting of both income (Angel et al. 2019) and turnout (Enamorado et al. 2019) making it difficult to make much progress sorting out the drivers of participation.¹ The evidence from the best panel study of both income and turnout in American elections, the Panel Study of Income Dynamics (PSID), does not clearly resolve this debate as scholars have reached different conclusions about whether changes in income result in changes in turnout (Jungkunz and Marx 2021; Ojeda 2018).

Recently, however, a handful of scholars have taken a different approach to uncovering the within-person effect of income on turnout, leveraging random shocks to income in the form of lottery winnings (Geys and Sørensen 2025) and other plausibly exogenous one-time payouts (Akee et al. 2020). These “treatments” have yielded no effects on political participation. Similarly, a randomized experiment assigning a guaranteed income level to some subjects for a short period of time returned no discernible increases in turnout (Broockman et al. 2024). Isolating more enduring and naturally occurring changes to a person’s income is the next step in understanding how resources may affect turnout.

In this paper, I take this next step by building a dataset that includes repeated observations of the annual salaries of 574,226 Americans. Using public records requests in four states and five cities, I assembled data on the salaries of people who worked as public employees between 2014 and 2020. Using name and age within geography, I linked these salary data to administrative data on voter registration and turnout curated by states. I use a model with individual-level fixed effects to show that as people earn more money they become more likely to vote, but only extremely minimally. An average increase of \$10,000 in annual earnings be-

¹Leading political science surveys overestimate voting rates by up to 20%, and income misreporting is similarly pervasive (Enamorado et al. 2019; Angel et al. 2019).

tween 2014 and 2020 is associated with, at most, a third of a percentage-point increase in the probability of turning out to vote. This effect is a fraction of the difference in turnout rates between two unique individuals whose salaries differ by the same average amount. If making more money causes individuals to vote, it is doing so at the extreme margins. My findings suggest that inequalities in voting across socio-economic groups are unlikely to be remedied solely by changing people’s levels of income or other such interventions, for example.

2 Existing Research

To isolate the effects of income alone, recent work has examined unplanned changes to people’s lives. Things like exogenous income shocks consistently return minimal or null effects on voting behavior, challenging pure resource-based explanations, but echoing the nuance offered by Brady et al. (1995). Akee et al. (2020) study Eastern Cherokee tribal members who received substantial unconditional payments (20-25% income increases) after casino construction on reservation land. While these payments had no effect on adults’ voting behavior, they increased participation among recipients’ children which suggests that early-life economic conditions matter more than adult income fluctuations (Chetty et al. 2016). Similarly, Geys and Sørensen (2025) analyze decades of Norwegian lottery data and find that winners were only one to two percentage points more likely to vote—a fraction of the between-person income-turnout gap. And Broockman et al. (2024) conduct unconditional cash transfer experiments in Texas and Illinois, paying 1,000 low-income individuals \$1,000 monthly for three years, finding no effect on 2022 election turnout despite substantial income increases. Collectively, these studies using different methodologies across diverse contexts converge on a striking conclusion: increasing financial resources alone does little to boost voting participation.

While these studies offer compelling evidence on windfall income effects, they leave open the question of how increases to earned income affect political participation. Work-related

earnings differ from windfalls in three theoretically meaningful ways related to the nuance in Brady et al. (1995) and offered by the other models of voting. First, earned income may carry psychological significance as a reflection of personal effort and achievement, potentially affecting political efficacy and self-actualization differently than unearned income (Piketty 1995; Deci and Ryan 1985). Margalit also suggests something similar, demonstrating that when individuals see the change in their material conditions as connected to the government or their own efforts it has a larger impact on their voting behavior (Margalit 2019). Second, salary increases typically accompany changes in job responsibilities, work schedules, and workplace social networks—all factors that independently influence political engagement (Verba et al. 1995). Third, earned income changes tend to be gradual and persistent rather than sudden and temporary, potentially allowing effects on a different timescale than windfall designs can capture (Heckman 2006). Within-individual salary changes tend to occur on some track or career trajectory and as people work up this trajectory they may have more time to develop habits and routines that are conducive to voting.

3 Data and Linkage

In order to isolate the within-individual effect of income on turnout, I need repeated individual-level observations of both income and turnout. I avoid survey estimates because of sample selection biases and consistent misrepresentation of sampled voters and nonvoters (Ansolabehere and Hersh 2012). I need as much individually identifying information as possible to link the two sets of records. Administrative voting records including demographic and turnout information are available nationally—I use voter files compiled by L2, a data vendor.

For income, I need some measure that is public, longitudinal, and accurate. While it would be possible to impute income from information like credit data or addresses, these measures are too imprecise and do not accurately capture individual-level variation.² I

²The correlation between voter file commercial data estimates for individuals’ salary and the observed salary from public records is $\rho = .33$ (see Appendix Figure E.1), underscoring the importance of using directly

use over-time salary records for government employees whose salaries are public record. Public sector employees are a useful group to study because they are more connected to government than those in the private sector, and so if there is some individual-level effect of income on turnout that exogenous windfall designs have missed, it should be visible in this group. Furthermore, public employees capture wide swaths of the American public; blue collar and white collar, old and young, urban and rural. This diversity allows for a comprehensive examination of how income dynamics interact with civic participation across different demographic and occupational contexts.³

3.1 Salary Records

I separately requested all public employee salary records from every state, 33 cities, and 20 counties across the country.⁴ While some subnational governments have components of their records available online, none contain age, an important variable for my match process as I explain below in the section on linking data. Upon request, four states and five cities provided complete salary data on employees including their full name and age for the years 2014, 2016, 2018, and 2020. Some agencies reject requests from nonresidents and some keep employee age confidential. The states that offered me complete records including employees' ages are Connecticut, Florida, North Carolina, and Texas. The cities are Austin, Houston, Chicago, Seattle, and Oklahoma City. Appendix Table A.1 contains the exact agency within the subnational government that provided the records and the date I received them. Appendix Table C.1 demonstrates the representativeness of the sample along basic demographics. All

observed measurement. A Pew Research Center study found that income is missing or inaccurate in 68% of voter file data looking across multiple voter file vendors (Mitchell 2018).

³In Appendix Figures D.1 and D.2 I show using CPS supplements how public employees' income and turnout patterns compare to the general population's income and turnout patterns. Public employees have slightly higher average income and turnout than the general public, as work like Geys and Sørensen (2022) shows, but the cross-sectional relationship between income and turnout is identical between public and private employees.

⁴For cities and counties, two research assistants and I separately requested each city or county and started with the largest cities and counties and worked our way down the list. In the municipalities' records request portal we would write: "I would like all public salary records annually going back to 2014. Please include columns for first, middle, and last name, as well as employee age, date of hire, and total salary/benefits."

of the records contain a field for total gross earnings in years 2014, 2016, 2018, and 2020. Many of the records contain additional fields of interest that I use in follow up analyses like date of hire, job title, and department.

3.2 Voter Files

To study participation I rely on voter files spanning 2014 to 2020 from data vendor L2. Counties and states are constantly updating their voter rolls, so L2 collects overtime “snapshots” for each geography. I construct a dataset of all voter records for the four general elections between 2014 and 2020. For each election I reference the most complete available voter file snapshot—far enough after the election to have collected as much data as possible from counties, but close enough to the election date to have an accurate tally of votes.⁵ Most snapshots are from between February and May of the year following the November election. In the main text I focus on general election turnout, but in the Appendix I show the main finding with lower salience elections like primaries and local or municipal elections. The voter files contain fields for first name, middle name, last name, age, address, party affiliation, and turnout in all elections.

3.3 Data Linkage

The next step is to link the salary records to the voter files. For state employees I match salary records to the entire state voter file and for city salary records I match to voters in all counties bordering the city.⁶ I link on first name, middle initial, last name, and age. To illustrate the importance of having a panel dataset that has been matched on age in addition to name, consider, for example, that 0.66% of North Carolina voters belong to duplicated name-age records whereas 9.24% of voters belong to duplicated name records.⁷

⁵This follows voter file best practices (Kim and Fraga 2022). Appendix Table B.1 contains the exact date of the L2 voter file snapshot used for each state under study.

⁶Chicago has residency requirements and so I only match to voters with addresses in Chicago. I confirm that matching city records to the entire state voter file does not add or drop observations.

⁷Geys (2023) illustrates the importance of having a high cardinality variable like age for matching records.

I match the data in two ways; the first prioritizes match accuracy and the second prioritizes scope. The first linking approach uses exact matching on full name and age for employees who work for their agency for all four years, 2014–2020. It only retains fully registered individuals who match uniquely between the two datasets. The second approach fills in the gaps left by the first. I use probabilistic record linkage to merge the datasets on full name and age. This method allows for some flexibility in the matching process and keeps every salary observation. If there is some effect of salary on not just turnout but also registration, this second panel will pick it up.

3.4 Exactly-Matched and Registered Panel

The first and primary panel dataset is all continuously employed and registered individuals who appear in both sets of records for the entire period and have one unique match. This restrictive dataset, which includes only registered voters, aims to minimize the false positive match rate. If John A. Doe age 28 matches to two John A. Doe age 28 voters, I drop him.⁸ Salary records including a middle initial must match exactly on it, and I only admit salary records without a middle initial if there is one voter in the file with their first and last name.

This is a very conservative match approach. Some recent work links employee payrolls to voter files without age, and when employees match to dozens of voter file records, they record turnout based on one of the many matches at random. When employees do not match to the voter file for any reason, they are considered nonvoters (e.g., Markovich and White 2022). However, individuals might not match to the voter file because of name discrepancies (e.g., maiden names, A.J. instead of Andrew) when they are in fact voters. Using only confidently matched registrants limits the sample but improves the data. This panel has over 150,000 individuals observed across four election cycles, representing 55% of individuals present in

⁸I only use matched records where the ages are within two years of each other. Because I have to use age in years instead of date of birth, this window allows for slight discrepancies in how states and cities record age and how the voter file records age. For example in the November 2016 election data, the voter file could come from March of 2017 whereas the age column in the salary data could come from a January 2016 data pull.

the salary records for all four years. Appendix Table F.1 breaks down the share within each geography who show up in all four years of salary records and the share among them who match uniquely to the voter file. Table 1 shows the descriptive statistics for each city and state included in the panel. Appendix Figure A.1 shows the salary distributions visually.

Table 1: Summary of Average Salary and Raise by Place

		Salary (Total Gross, 2020 Constant Dollars)			Raise (2014 - 2020)	
	Individuals	Mean	Median	SD	Mean	Median
States						
Connecticut	23, 101	\$87, 844.77	\$84, 059.29	\$38, 944.88	\$23, 988.99	\$16, 830.14
Florida	23, 297	\$48, 157.39	\$42, 201.65	\$21, 015.25	\$6, 469.20	\$4, 148.04
North Carolina	52, 744	\$50, 579.67	\$43, 938.92	\$22, 715.30	\$6, 514.66	\$4, 123.52
Texas	34, 052	\$59, 835.95	\$51, 920.32	\$29, 713.77	\$17, 179.49	\$11, 650.18
Cities						
Austin	4, 285	\$83, 278.59	\$81, 430.61	\$35, 524.42	\$29, 751.08	\$20, 615.80
Chicago	11, 907	\$93, 675.07	\$92, 784.59	\$17, 846.55	\$12, 238.53	\$8, 928.73
Houston	7, 944	\$61, 413.28	\$59, 995.34	\$19, 272.43	\$8, 827.97	\$6, 309.96
Oklahoma City	1, 341	\$79, 125.69	\$80, 450.91	\$24, 447.69	\$10, 453.44	\$9, 283.41
Seattle	4, 942	\$108, 443.95	\$106, 574.67	\$44, 587.82	\$37, 271.48	\$29, 919.18
Overall	163, 613	\$63, 923.25	\$55, 232.61	\$32, 858.71	\$13, 293.80	\$7, 634.87

3.5 Full Voter Probabilistic Panel

The second panel dataset retains all employees from the records and matches probabilistically with **fastLink** (Enamorado et al. 2019), an implementation of the Fellegi-Sunter record linkage model (Fellegi and Sunter 1969). This is to address the possibility that salary has an effect on registration and to use the full universe of data provided by state and city governments. If the entirety of the effect of earned income on turnout were concentrated in causing unregistered individuals to register and then vote, a panel of consistently registered individuals would miss this, for example. Because the match is happening by first name, middle initial, last name, and age, I aggregate the voter file to this level for the probabilistic merge. Fortunately, very few individuals are nonunique within their state or city by full

name and age.⁹ At various threshold parameters for the merge, this method could admit a “Jim” in one set of records matching to a “James” in the other if they are the same person and other fields closely match. By that same token, the process can admit false positive matches if there are close string distance scores between different individuals. In the results that follow I focus on the first exactly-matched panel of registrants and use the probabilistic panel to confirm the findings. Detailed results from the full voter probabilistic panel are in Appendix Section H.

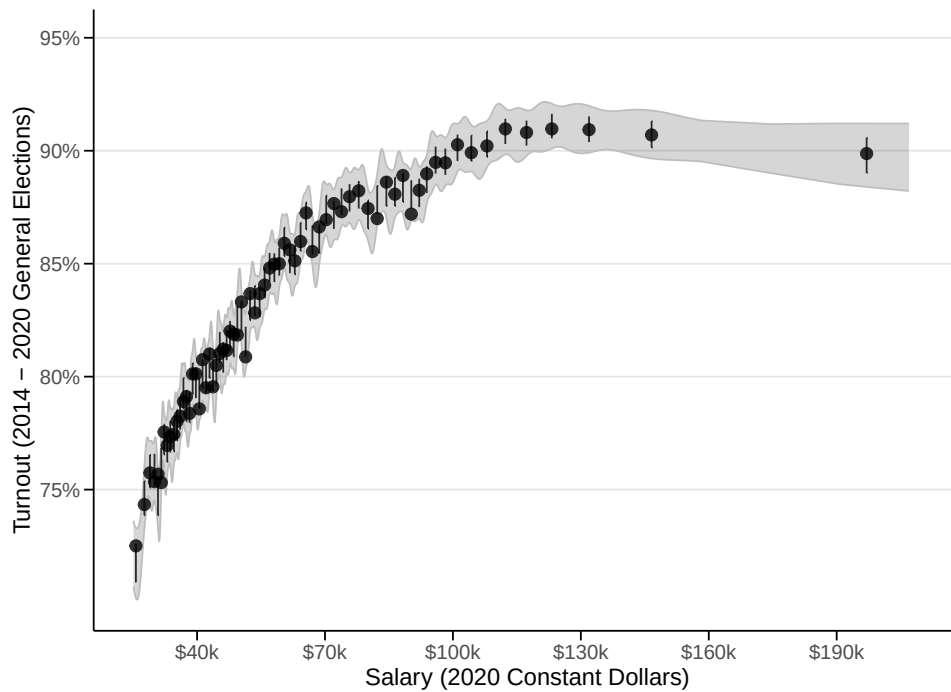
⁹See Appendix Table H.4 for probabilistic match diagnostics.

4 Results

4.1 The Income-Turnout Distribution

The exactly-matched and registered panel exhibits the traditional income-turnout distribution. At the very low end of the salary distribution the curve is steep and close to linear. Towards the higher end of the salary distribution the turnout curve flattens out. The shape of the overall distribution appears more quadratic or logarithmic rather than linear. Figure 1 is a binned scatterplot of turnout by salary using turnout data for all four general elections between 2014 and 2020. This curve demonstrates that when looking between different registered voters, those who make more money vote more on average.

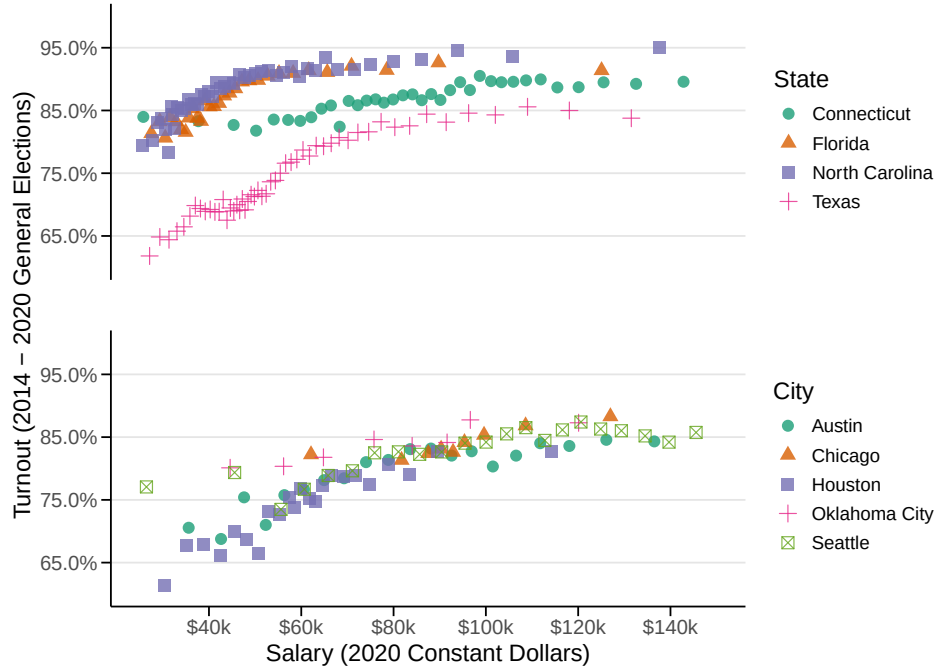
Figure 1: Average Relationship between Salary and Turnout, 2014 - 2020



Note: The binned scatter plot divides the range of salary in the exactly-matched and registered sample into equally populated bins (quantile-spaced, $\sim 8,700$ observations per bin). Each point represents the mean of turnout within a given salary bin, plotted against the bin's average salary. Confidence band and intervals are created from a cubic B-spline. The averages adjust for three covariates (place, age, and election year) using semilinear residualization as described in Cattaneo et al. (2024).

These data align with existing work on the income-turnout relationship (Wolfinger and Rosenstone 1980; Ojeda 2018). This is a pattern with overwhelming consistency—both across time and place. Figure 2 shows the same turnout data but plotted separately for each set of salary records. The shape of the curve is generally steepest below \$100,000 or so and then flattens off. Each city or state has its own salary distribution among public employees as Figure A.1 shows, but the general pattern is the same.

Figure 2: Average Relationship between Salary and Turnout by Geography, 2014 - 2020



Note: The binned scatter plot divides the range of salary in the exactly-matched and registered sample into equally populated bins (quantile-spaced). Each point represents the mean of turnout within a given salary bin, plotted against the bin’s average salary. The averages adjust for three covariates (place, age, and election year) using semilinear residualization as described in Cattaneo et al. (2024).

The income-turnout gap across individuals captures the difference in turnout rates between two individuals whose salaries differ by a certain amount. However included in the between individual income-turnout gap are all the features of people that correlate with salary

and voting: homeownership, family status, neighborhood, occupation, education, race, age, and gender for example.

4.2 Modeling Between vs. Within-Individual Effects

To quantify how much of the across individual income-turnout gap is due to salary changes, I estimate two models. The first (“Between-Individuals”) is a model that only considers one datapoint per individual, with turnout measured by average turnout in all four elections and salary as average salary in all four years. This is the class of model that is pervasive in the literature on income and turnout (e.g., Brady et al. 1995; Schlozman et al. 2012; Ojeda 2018) which conflates selection and the income effect. The second model (“Within-Individual”) treats each annual observation as a separate data point and therefore has variation in salary and turnout within-person. It estimates the effect of salary changes on turnout while including individual fixed effects.

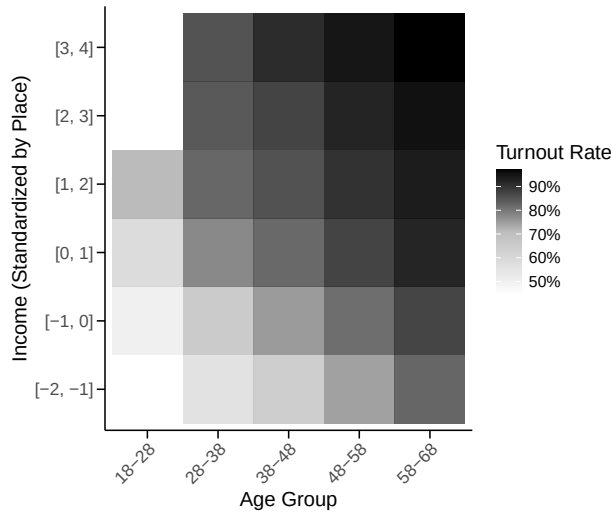
$$\overline{\text{Voted}}_i = \alpha + \beta f(\overline{\text{Salary}}_i) + \lambda_{b(i)} + \gamma_p + \varepsilon_i \quad (\text{Between-Individuals})$$

$$\text{Voted}_{it} = \alpha + \beta f(\text{Salary}_{it}) + \theta_i + \tau_{b(i)t} + \varepsilon_{it} \quad (\text{Within-Individual})$$

Voted_{it} is a binary variable for whether individual i in place p in year t voted. Salary_{it} is the total gross salary of individual i in year t , inflation adjusted to be in consistent 2020 dollars, and $f()$ imposes a plausible functional form, either quadratic, logarithmic, or linear. $b(i)$ represents individual i ’s birthyear and so $\lambda_{b(i)}$ are birthyear fixed effects and $\tau_{b(i)t}$ are birthyear-by-election fixed effects. Figure 3 shows why birthyear-by-election fixed effects are valuable—there is also an age-turnout gradient. Birthyear-by-election year fixed effects mean the salary coefficient will not be confounded by the nonlinear impact of the individual growing older nor will it be confounded by presidential elections having higher

average turnout than midterm elections. γ_p are place fixed effects. The key addition in the within model is θ_i , individual fixed effects.

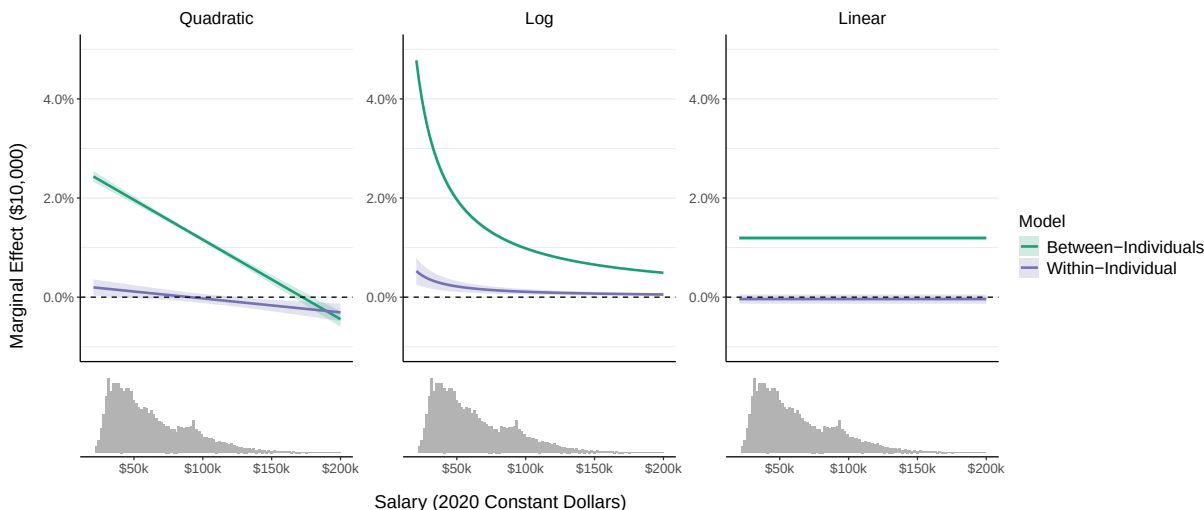
Figure 3: Age and Salary Both Correlate with Voter Turnout



Note: Figure shows within each cell of age and standardized salary, the overall rate of voter turnout across general elections 2014 - 2020. Cells with fewer than 50 observations are excluded. The salary is normalized to have a mean of zero and a standard deviation of one within each place to account for differences in the salary distributions across states and cities in the sample. Moving from lighter to darker corresponds to moving from lower to higher turnout.

Figure 4 and Table 2 present results from fitting these models. The graphs in Figure 4 show the marginal effects from quadratic, log, and linear specifications. AIC and BIC scores suggest logarithmic and quadratic models fit the data comparably well and better than linear models; however, I present all results since the point is to compare between to within estimates while remaining agnostic about the exact functional form of the relationship. The marginal effects associated with a \$10,000 salary change are plotted with 95% confidence intervals and the histogram below each subplot shows the distribution of salary data in the sample.

Figure 4: The Marginal Effect of \$10,000 Increase in Salary on Turnout (General Elections 2014 - 2020)



Note: These figures show marginal effects of a \$10,000 increase in salary on the probability of turning out to vote in general elections between 2014 and 2020. Individuals in this dataset are present in all four years of salary records and match exactly on name, age, and geography to one registrant in the voter file. The left panel shows the marginal effects from models fit on salary and salary squared, the middle panel on log salary, and the right panel is linear. The histogram at the base of each plot shows the distribution of salary in the sample. The marginal effects are plotted with 95% confidence intervals. For tabular regression results, see Table 2.

Between and within-individual models show the effect shrinking in salary. At the lowest end of the full time employees' salary distribution, a \$10,000 increase in salary is associated with between one and four percentage points increased probability of turning out to vote in the model without individual fixed effects. This is not an "effect," but rather a way to describe the large turnout gap between income strata as displayed in binscatters in Figure 1. The effect of \$10,000, estimated in the model with individual fixed effects, is largest in the log specification. But more often than not it's indistinguishable from zero.

Effects are shown in terms of \$10,000 because it is between the median and mean raise from 2014 to 2020 (see Table 1). However, the main takeaway comes from the ratio of the effects between the models, not any one particular raise or its associated turnout effect. Across the salary distribution the within-individual model's salary effect is only a fraction of the gap in turnout between two different people whose salary varies the same amount.

Table 2: Marginal Effect of \$10,000 Increase in Salary on Turnout (General Elections 2014 - 2020)

	Quadratic		Log		Linear	
	(Between)	(Within)	(Between)	(Within)	(Between)	(Within)
Salary (\$10,000)	0.028 (0.001)	0.003 (0.001)			0.012 (0.000)	0.000 (0.000)
Salary ² (\$10,000)	-0.001 (0.000)	0.000 (0.000)				
ln(Salary)			0.098 (0.002)	0.011 (0.003)		
N Observations	163,613	654,452	163,613	654,452	163,613	654,452
FE: Birthyear	✓		✓		✓	
FE: Election x birthyear		✓		✓		✓
FE: Place (city/state)	✓		✓		✓	
FE: Individual		✓		✓		✓

Note: The table reports results from the models described above. Individuals in this dataset are present in all four years of salary records and match uniquely to one registrant in the voter file. The dependent variable is turnout in the general election. The independent variable is salary. Robust/individually-clustered standard errors in between/within specifications respectively.

Under the log functional form the ratio of the salary coefficient in the model with individual fixed effects to the coefficient in the model without is about one-ninth. This is similar to the ratio of effects found using this same design with administrative data from Bologna, Italy (Schafer et al. 2022) and within the confidence interval for the turnout effect of a large cash transfer experiment in Texas and Illinois (Broockman et al. 2024).

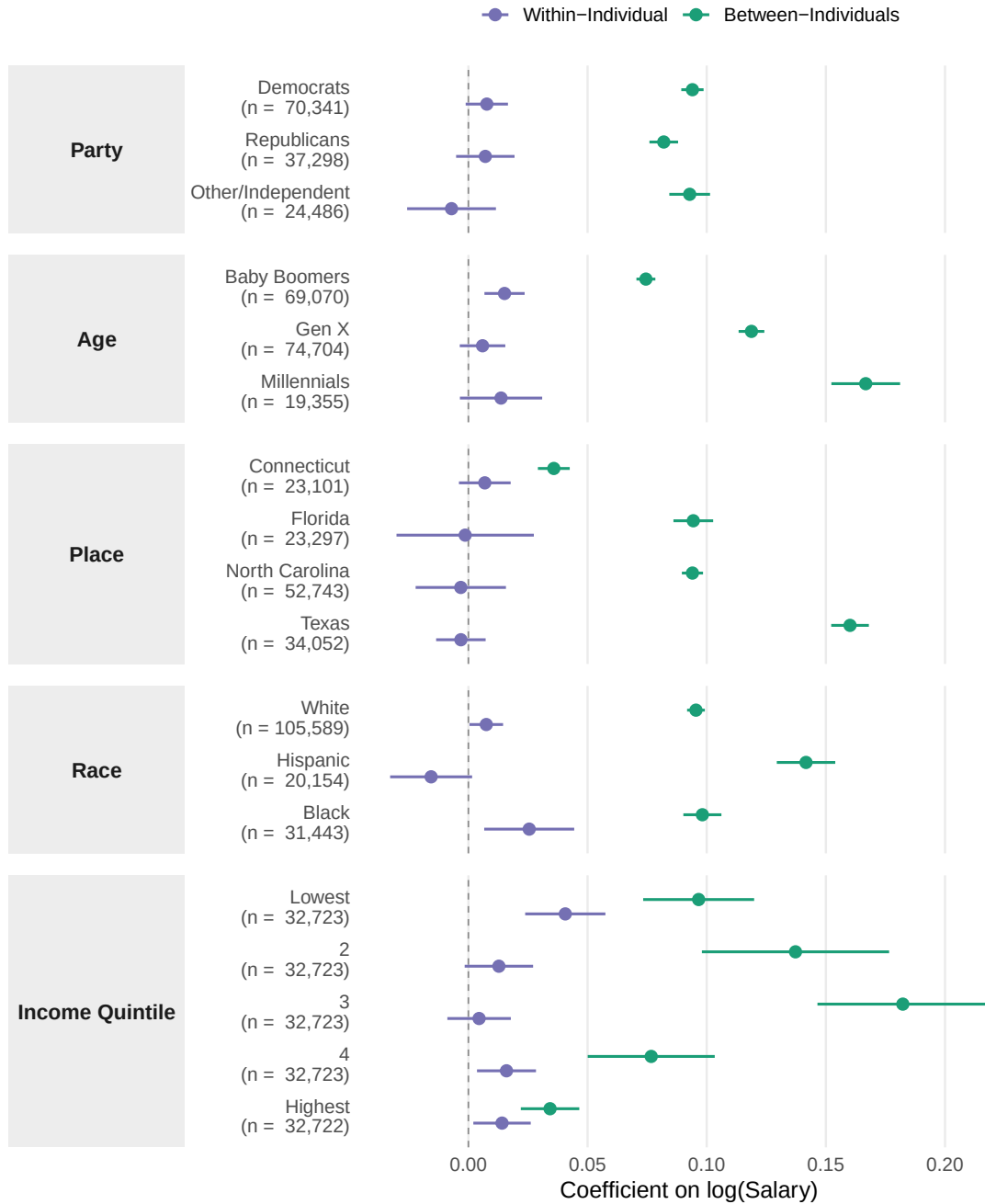
While the coefficient on salary in the within-individual model under the log salary specification is statistically significant, hypothetical unobserved confounders that explain more than 0.27% of the residual variance of both salary and turnout would be strong enough to bring the estimate to a range where it is no longer significant (Cinelli and Hazlett 2020). In the Appendix (Figure I.1) I use de Chaisemartin and D’Haultfœuille (2026)’s estimator to speak to some of the shortcomings of two-way fixed effects, namely allowing for heterogeneous and dynamic effects. Using this design I find insignificant placebos and an average total cumulative turnout effect of \$10,000 raise is 0.31 percentage points (one-fourth of the

cross sectional gap) under this specification, mostly concentrated two periods after a salary change. I also present an additional test of the plausibility of the parallel trends assumption, or specifically that absent salary changes, the turnout trajectory of the group that did in fact receive salary changes would have been similar to the turnout trajectory of the group without salary changes. To be sure that the individual salary effect is as low as Table 2 shows, I want to confirm that individuals do not increase their turnout behavior in anticipation of a salary change; in that scenario, the within-individual model would be biased downwards. Reassuringly, Appendix Table J.1 uses a lead of the salary variable to show that future salary changes are not predictive of present turnout.

4.3 Effect Consistency

The models capture varying marginal effects throughout the salary distribution. In particular, that if there is a very small effect to be attributed to salary itself, it exists for lower income individuals. In this section I analyze various subsets of the data to show that after including individual fixed effects salary accounts for very little of the variation in turnout for the entire sample. For most groups, the within-individual effect of salary is statistically indistinguishable from zero. Figure 5 shows the coefficients on log salary from between and within models fit to one subset of the data at a time. These subsetting exercises show that the small within-individual effects of salary are not masking a demographically heterogeneous effect. No one generational cohort, party, racial group, or state has a large within-individual effect of salary on turnout that the overall regression covers up.

Figure 5: Turnout Effects by Subsets of the Data



Note: Figure shows coefficients on log(Salary) from between- and within-individual models as described in the main text, fit to specific subsets of the overall dataset.

5 Discussion

In this paper, I have isolated the effects of earned annual income on turnout in elections between 2014 and 2020 for hundreds of thousands of Americans across four states and five municipalities. Across all these areas—and other breakdowns—the results show that increases in income have minimal or nonexistent effects on participation. The within-person analysis demonstrates that an average increase in income over a seven year period produces a change in turnout that is only a fraction of the size of the turnout gap observed in between-person assessments of income levels and turnout. The direction of this finding agrees with an emerging consensus from Schafer et al. (2022), Akee et al. (2020), Broockman et al. (2024), and Geys and Sørensen (2025).

It appears that the things that lead people to hold the jobs they hold are some of the same things that lead people to participate more in politics.¹⁰ There are characteristics of individuals that lead them to different occupations that also lead them to be predisposed to vote in elections. While I have not articulated what these qualities are in this paper, possibilities include the broad ideas generated by foundational models of voting behavior like early-adult attachments or the social transmission of ideas through group memberships.

Because the between-person gap in turnout by income levels is so stark, many people believe income is an important explanation for why people vote in elections. I have shown here that the relationship between income and turnout is mainly spurious. This means that closing the turnout gap between the richest and poorest Americans will require more than increasing the incomes of the poorest citizens.

¹⁰See Appendix K for a demonstration that despite variance in earnings within a given specific job title there is close to no variance in expected turnout.

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Appendix

The Effect of Income on Turnout: Evidence from Public Salary Records and Administrative Voting Data

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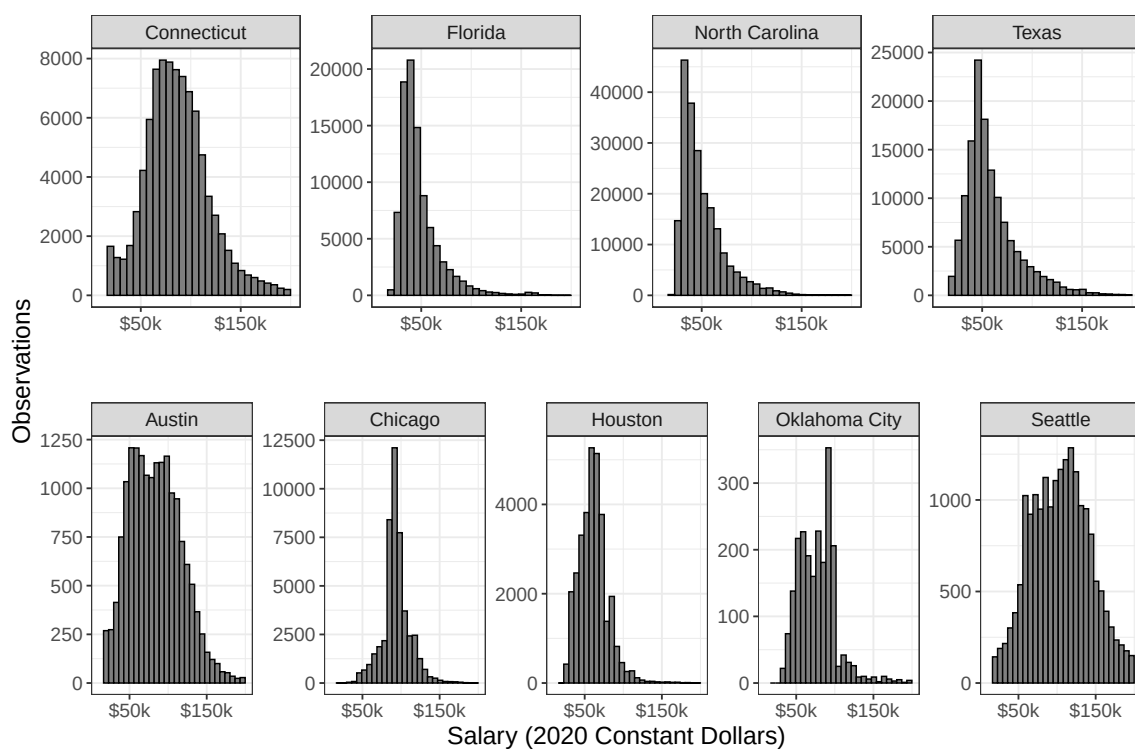
A Salary Records

Table A.1: Salary Record Information

Place	Date Received	Source	Notes
Connecticut	11/07/2024	Connecticut Office of State Comptroller	Link to dataset, separately requested employee age
Florida	08/15/2024	Florida Department of Management Services	Requested & paid \$74.65
North Carolina	10/04/2024	NC Office of the State Controller	Requested
Texas	11/04/2024	Texas Comptroller of Public Accounts	Requested
Austin	12/12/2024	Austin Public Records Center	Requested & paid \$171.00
Chicago	05/17/2024	City of Chicago Records Center	Requested
Houston	11/13/2024	City of Houston Records Center	Requested
Oklahoma City	02/28/2025	Oklahoma City Records Center	Requested
Seattle	12/17/2024	Seattle Public Records Center	Requested & paid \$1.25

Request text to agencies: “I would like to request salary records for all [city/state] employees from years 2014, 2016, 2018 and 2020. Please include first name, middle initial, last name, year of birth/age, job title, agency, salary, overtime, and total annual earnings.”

Figure A.1: Salary Distributions by Place



B Voter Files

Table B.1: Voter File Dates by State and Election Year

State	Election Year	Voter File Date
CT	2014	2015-03-25
CT	2016	2017-06-09
CT	2018	2019-05-08
CT	2020	2021-03-30
FL	2014	2015-05-16
FL	2016	2017-03-06
FL	2018	2019-05-08
FL	2020	2021-02-04
IL	2014	2015-03-02
IL	2016	2017-03-17
IL	2018	2019-05-14
IL	2020	2021-03-05
NC	2014	2015-07-29
NC	2016	2017-05-24
NC	2018	2019-05-10
NC	2020	2021-05-18
NY	2014	2015-07-28
NY	2016	2017-03-13
NY	2018	2019-02-27
NY	2020	2021-03-15
OK	2014	2015-03-26
OK	2016	2017-04-22
OK	2018	2019-05-03
OK	2020	2021-02-08
TX	2014	2015-07-31
TX	2016	2017-03-12
TX	2018	2019-05-24
TX	2020	2021-03-25
WA	2014	2015-05-05
WA	2016	2017-05-24
WA	2018	2019-05-12
WA	2020	2021-07-22

C Comparing Study Geographies to the United States Overall Population

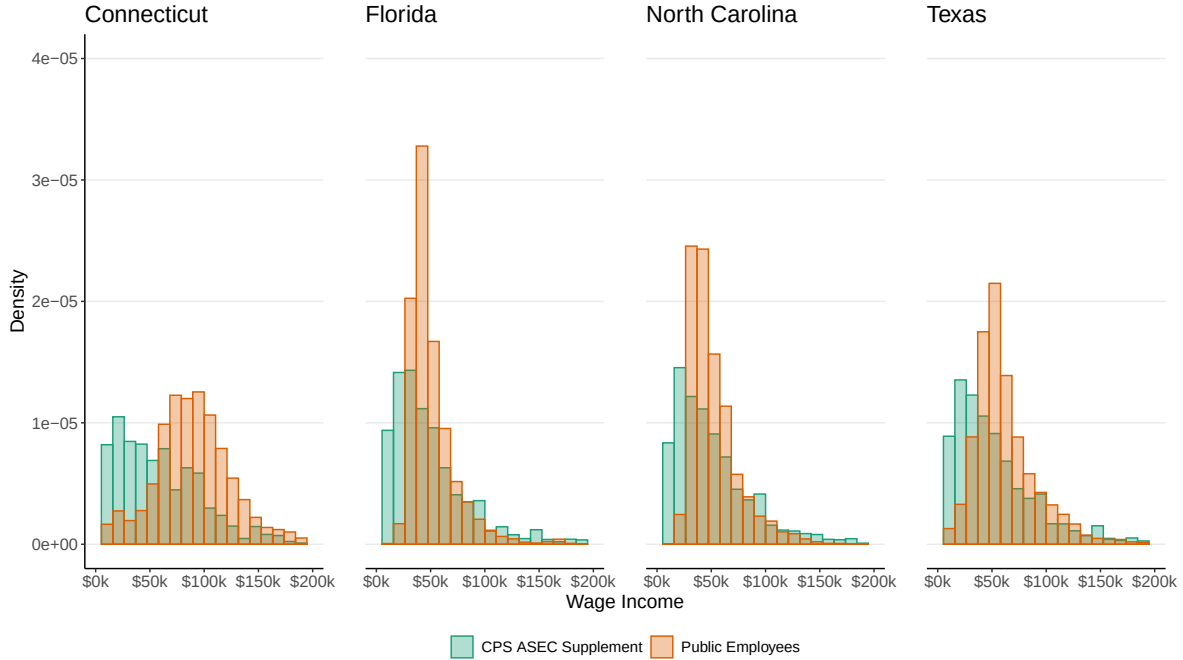
Table C.1: Demographic Comparison of Study Geographies vs. the United States

Geography	% Female	Med. Age	% White	% Black	% Hisp.	% Asian	Med. HH Inc.	% Poverty	% Bach.+
<i>States</i>									
Connecticut	51.2%	41.1	66.0%	9.9%	16.4%	4.5%	\$79,855	9.8%	40.0%
Florida	51.1%	42.2	53.4%	15.2%	25.8%	2.7%	\$57,703	13.3%	30.5%
North Carolina	51.4%	38.9	62.6%	21.0%	9.5%	2.9%	\$56,642	14.0%	32.0%
Texas	50.3%	34.8	41.4%	11.8%	39.4%	4.9%	\$63,826	14.2%	30.7%
<i>Cities</i>									
Austin	49.2%	33.7	48.2%	7.4%	33.3%	7.6%	\$75,752	12.5%	53.4%
Houston	50.2%	33.3	24.1%	22.3%	44.5%	6.8%	\$53,600	19.6%	34.3%
Chicago	51.4%	34.8	33.3%	28.8%	28.6%	6.8%	\$62,097	17.3%	41.1%
Seattle	49.4%	35.2	62.6%	6.9%	7.1%	16.2%	\$97,185	10.2%	65.0%
Oklahoma City	50.7%	34.4	52.8%	14.2%	20.0%	4.4%	\$56,456	15.2%	31.5%
Weighted Sample	50.9%	38.0	52.9%	16.5%	22.8%	4.5%	\$63,573	13.7%	35.0%
United States	50.8%	38.2	60.1%	12.2%	18.2%	5.6%	\$64,994	12.8%	32.9%

Note: United States Census Bureau ACS 2016–2020 5-year estimates. The row labeled “Weighted Sample” weights the overall sample by each constituent geography’s share of the overall total study population.

D Comparing Public Employees to the Statewide Population (CPS)

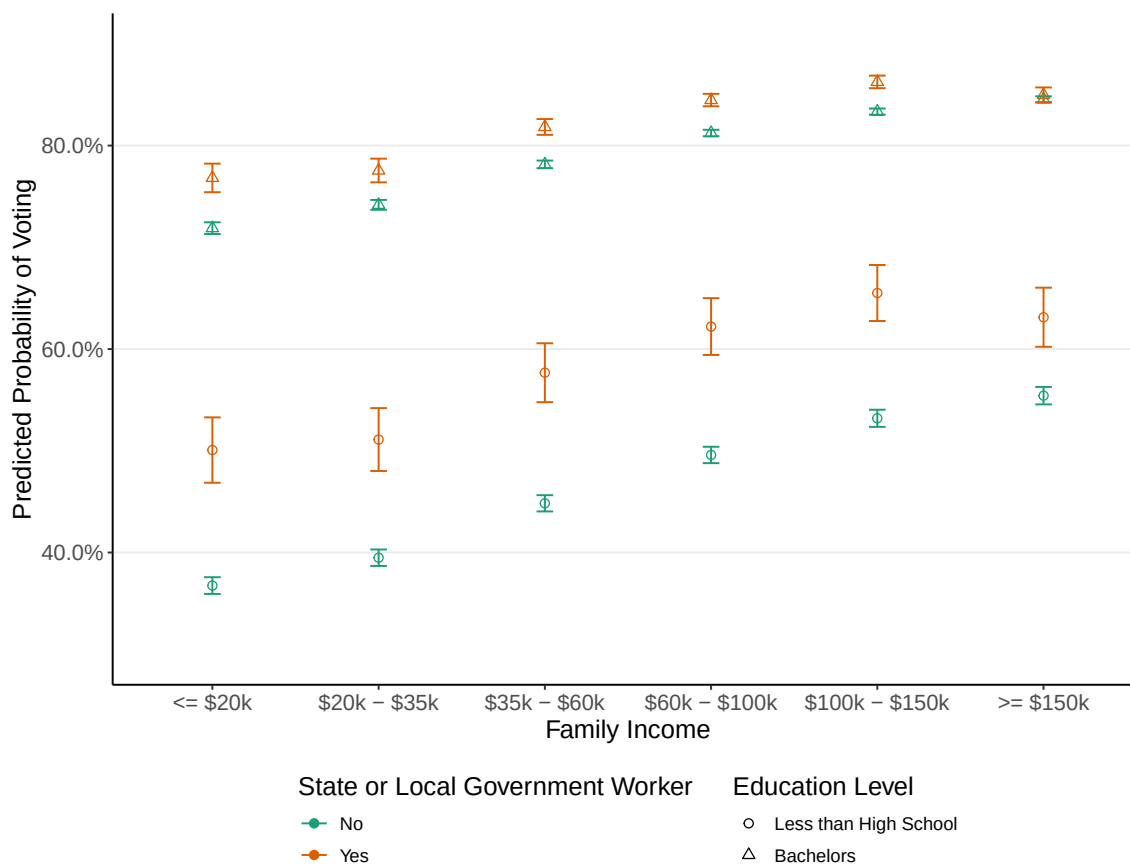
Figure D.1: Salaries of Public Employees vs. Wage Income of CPS Respondents



Note: Figure shows the distribution of 2020 salaries in the exactly-matched dataset of public employees (orange) as compared to the distribution of income from wages as reported in the 2020 March ASEC supplement to the CPS (green).

The specification used to compute the predicted probabilities in Figure D.2 is a logistic regression model of voter turnout on class of worker status (public employee or not) interacted with each of the following: family income ($\leq \$20,000$, $\$20,000 - \$35,000$, $\$35,000 - \$60,000$, $\$60,000 - \$100,000$, $\$100,000 - \$150,000$, $\geq \$150,000$), education (less than high school, high school graduate, some college, bachelor's degree, graduate degree), age (continuous), sex, and race (Hispanic, White, Black, Asian, Other). The model is fit to the November CPS supplement data from 2014 to 2020. The predicted probabilities are computed across income and government worker status for low and high education with other variables held at the modal value. A likelihood ratio test comparing a model with and without the interaction between government employment and income yielded no significant improvement in model fit ($\chi^2(4) = 2.06, p = 0.72$), indicating that the cross-sectional relationship between income and turnout does not significantly differ between public and private sector workers.

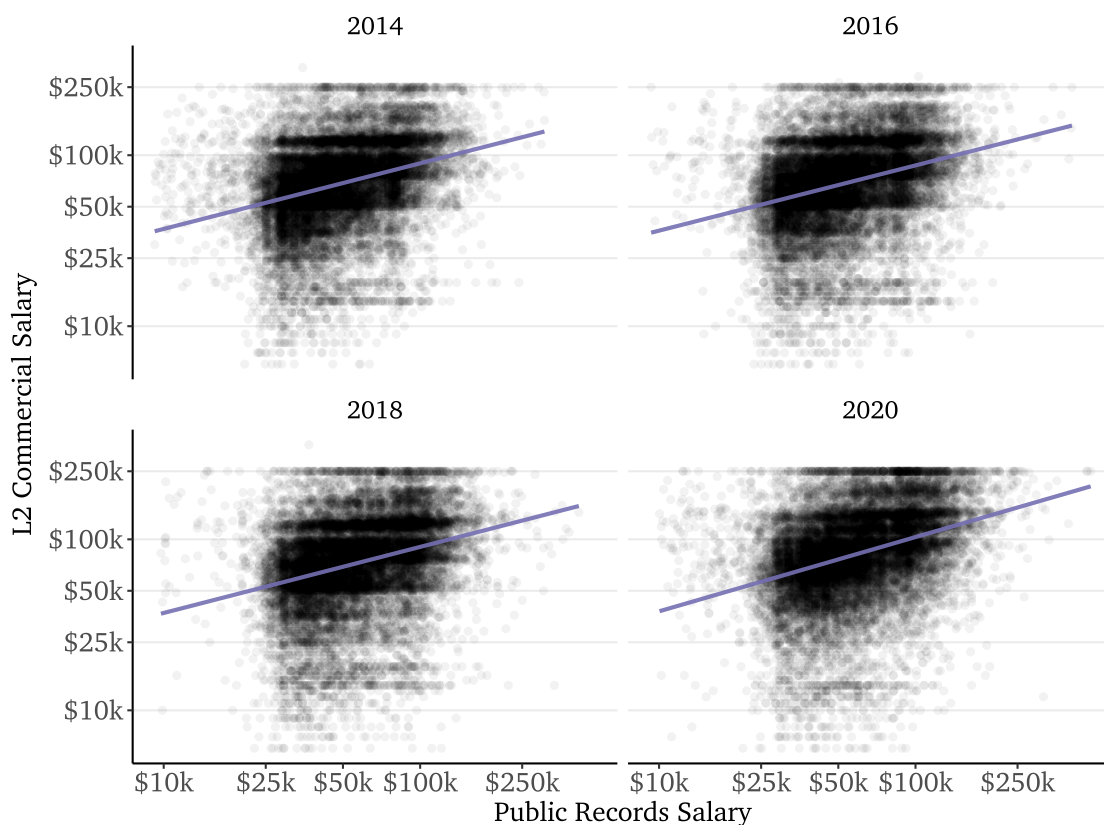
Figure D.2: Interacting Government Employee Status with Demographics to Predict Voter Turnout in the November CPS Supplement



Note: Figure predicts the probability of voting in the November 2020 election using a model fit to CPS November Voting Supplement data from 2014 - 2020. The model interacts a dummy for whether the respondent works for a state or local government with a slate of demographics: age, education, sex, race, and income. The plot above predicts the probability of voting with other variables held at their modal value.

E Comparing Public Salary Records to Voter File Imputed Salaries

Figure E.1: Comparing Salaries from Public Records Requests to Commercial Voter File Imputed Salary



In Figure E.1 the y-axis shows the value of income from L2's voter file. L2 includes commercial data which has their best guess for a person's income. On the x-axis is the salary I observe in the public records obtained directly from state and city governments. $\rho = .33$.

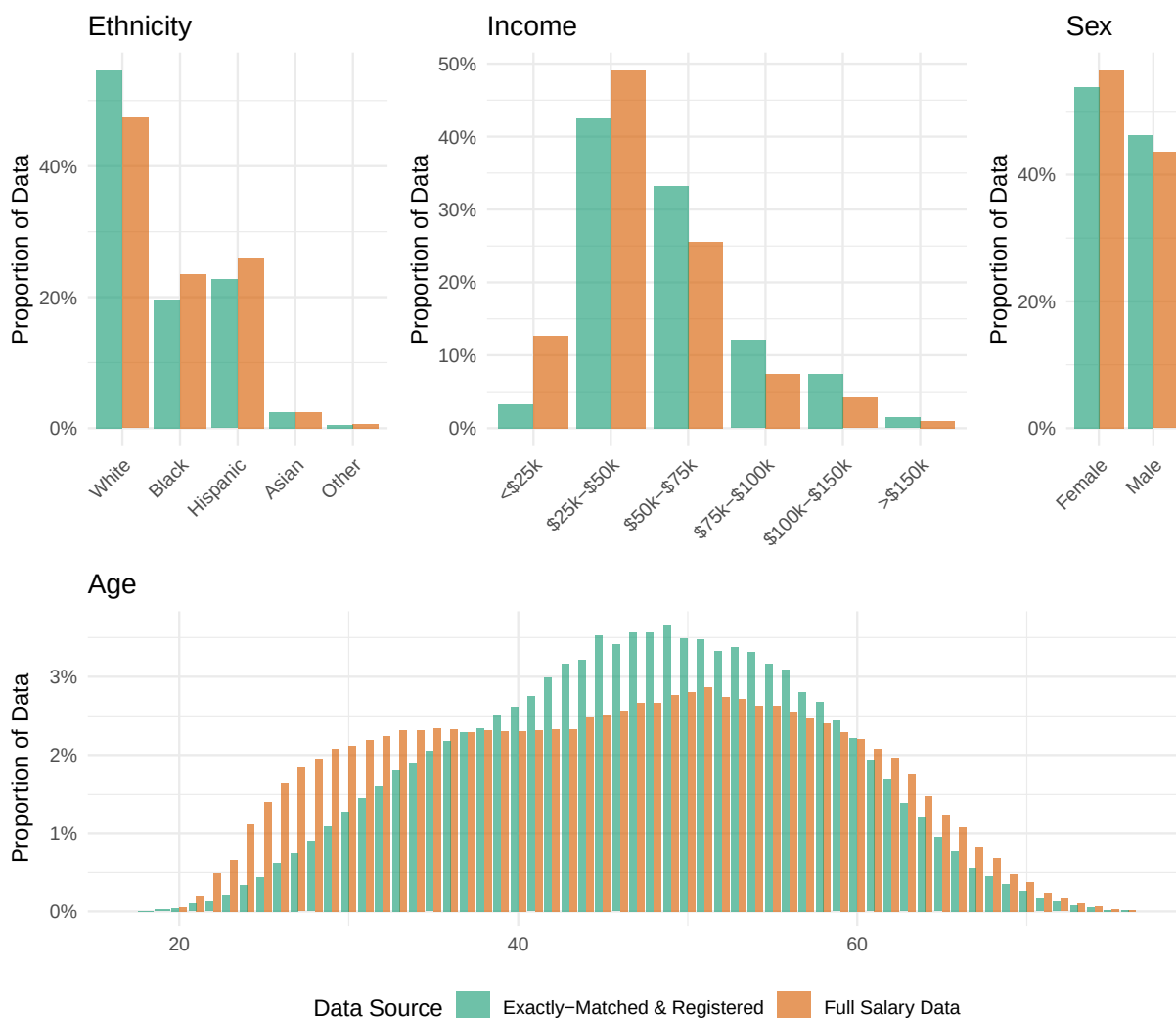
F Construction of The Exactly-Matched Panel

Table F.1: Sample Construction by Place

Place	Full Records		Present All Four Years		Exactly-Matched Set	
	<i>N</i>	Avg. Salary	<i>N</i> (%)	Avg. Salary	<i>N</i> (%)	Avg. Salary
States						
Connecticut	71,706	\$84,540	35,207 (49.1%)	\$90,389	23,101 (65.6%)	\$87,845
Florida	152,480	\$44,764	40,089 (26.3%)	\$48,396	23,297 (58.1%)	\$48,157
North Carolina	131,594	\$47,581	90,634 (68.9%)	\$47,845	52,744 (58.2%)	\$50,580
Texas	261,754	\$49,804	79,334 (30.3%)	\$57,240	34,052 (42.9%)	\$59,836
Cities						
Austin	20,564	\$71,679	7,939 (38.6%)	\$81,602	4,285 (54.0%)	\$83,279
Chicago	32,793	\$90,076	16,985 (51.8%)	\$92,869	11,907 (70.1%)	\$93,675
Houston	30,258	\$57,491	13,636 (45.1%)	\$59,898	7,944 (58.3%)	\$61,413
Oklahoma City	6,100	\$70,637	2,822 (46.3%)	\$75,060	1,341 (47.5%)	\$79,126
Seattle	19,457	\$94,029	7,685 (39.5%)	\$106,355	4,942 (64.3%)	\$108,444

Note: Full Records counts all unique individuals appearing in the salary data across 2014, 2016, 2018, and 2020. Present All Four Years counts individuals with salary records in all four study years. Exact Match Set counts individuals matched to voter rolls via exact string matching as described in text. Percentages are relative to the preceding column.

Figure F.1: Comparing the Exactly-Matched and Registered Panel to the Full Set of Salary Records in Texas



Note: Figure visualizes the distribution of ethnicity, income, sex, and age in the exactly-matched panel compared to the full set of salary records in the Texas state employee data. The exactly-matched and registered set has 34,052 individuals in four years totalling 136,208 observations and the full set of Texas state salary records has 626,059 observations. The exactly-matched panel, which only has 2014 - 2020 consistently employed individuals who uniquely match to the voter file by full name and age, is whiter, wealthier, more male, and older than the full set of Texas state employees.

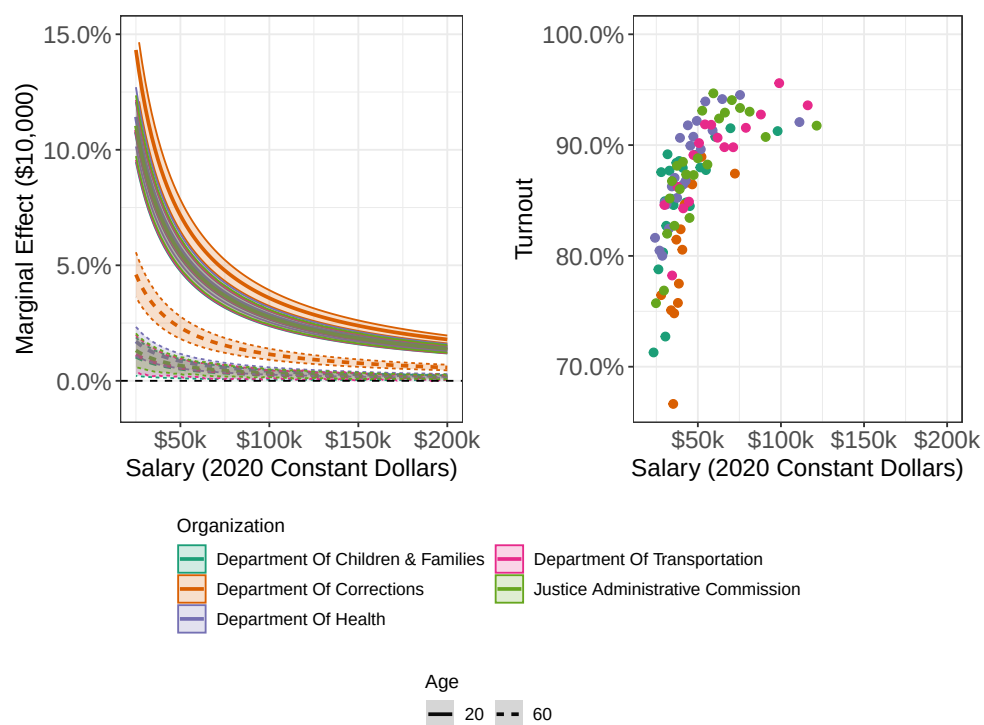
G Exactly-Matched Panel Results

Table G.1: Marginal Effect of \$10,000 Increase in Salary on Turnout (Primary and Local Elections 2014 - 2020)

	Primary		Local or Municipal	
	(Between)	(Within)	(Between)	(Within)
ln(Salary)	0.125 (0.002)	-0.003 (0.004)	0.020 (0.001)	0.000 (0.003)
N Observations	162,785	644,166	161,115	644,460
FE: Birthyear	✓		✓	
FE: Election x birthyear		✓		✓
FE: Place (city/state)	✓		✓	
FE: Individual		✓		✓

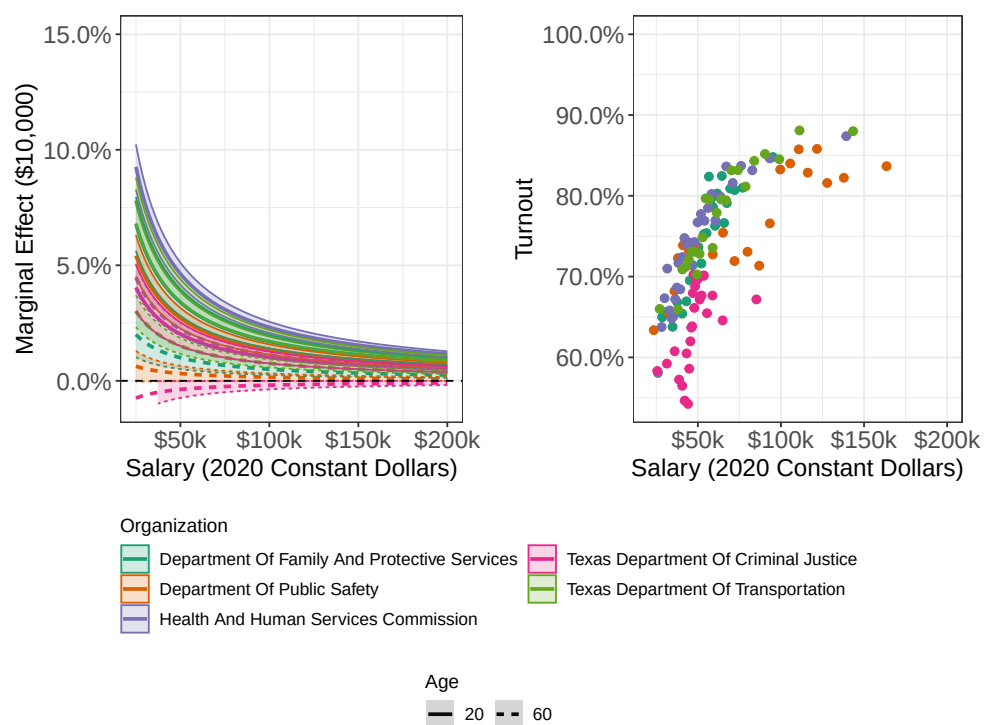
Model results reported from fitting models described in the main text with outcome variables for turnout in primary and local or municipal elections in all geographies. The L2 voterfile reports turnout for all elections recorded by counties in a given year. The outcome variable in the models labeled “local or municipal” is a sum of registrants’ turnout in all elections of the given type in the even year of the salary observation and the odd year following it. This ranges from 0 to 4 given that some voters are eligible to vote in many local elections in the same year, but average local turnout is very low (7.8%). The outcome variable in the models labeled “primary” is simply the voter’s turnout in the even-year primary election corresponding to the salary year.

Figure G.1: Age, Organization, and Turnout — Florida



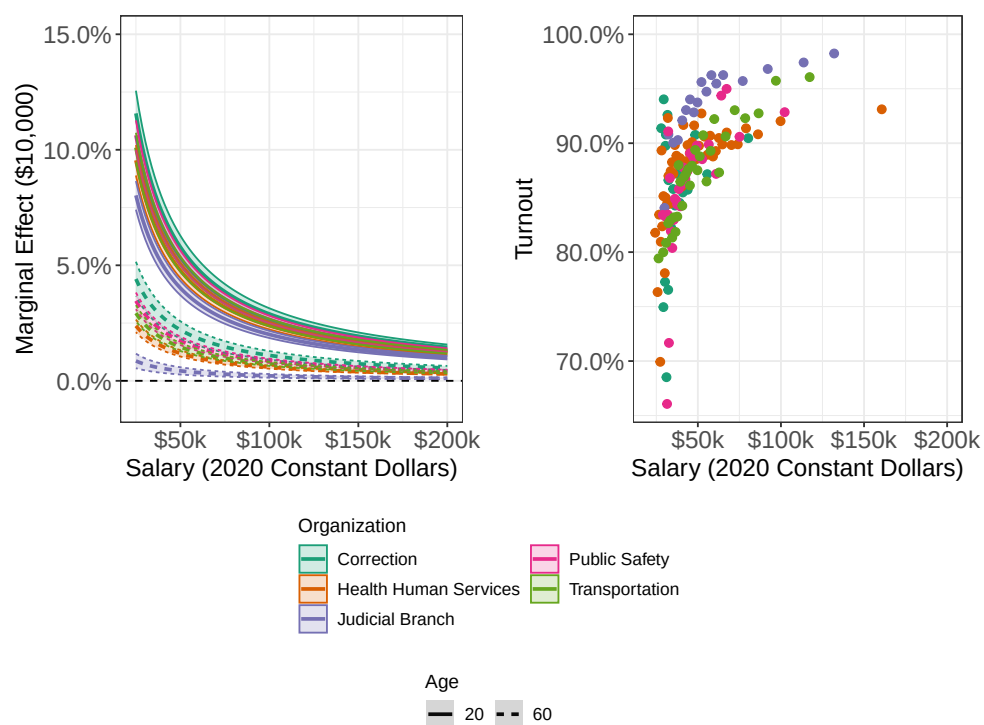
Note: Figure shows on the left, the marginal effect of salary predicted by a model that interacts salary with organization and salary with age. On the right is the binscatter of turnout in general elections 2014 - 2020 by salary separated by organization.

Figure G.2: Age, Organization, and Turnout — Texas



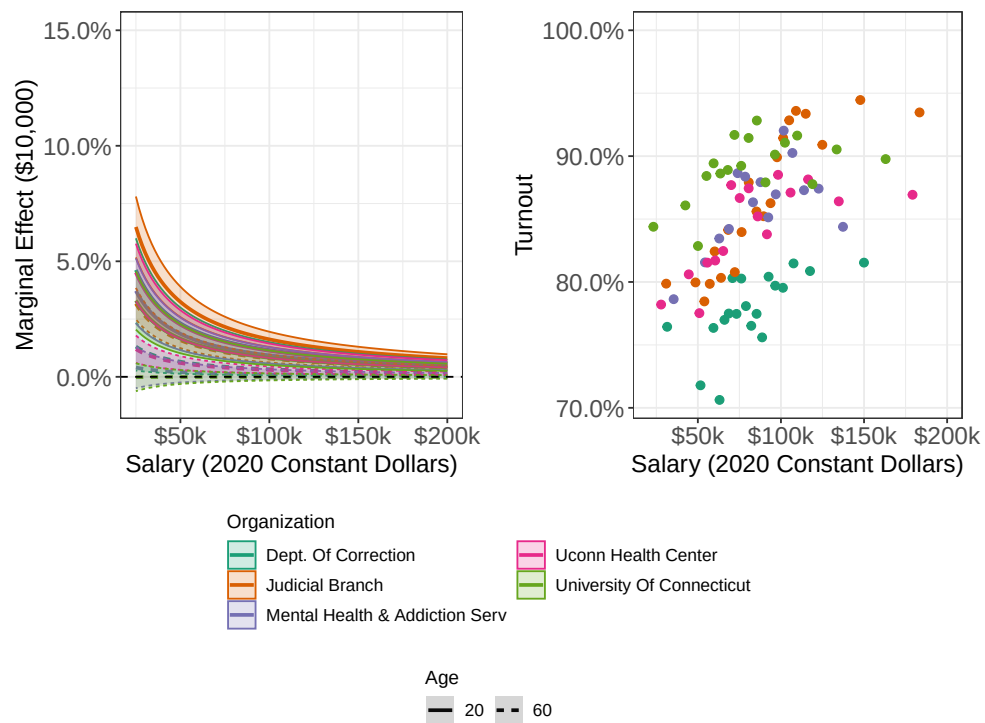
Note: Figure shows on the left, the marginal effect of salary predicted by a model that interacts salary with organization and salary with age. On the right is the binscatter of turnout in general elections 2014 - 2020 by salary separated by organization.

Figure G.3: Age, Organization, and Turnout — North Carolina



Note: Figure shows on the left, the marginal effect of salary predicted by a model that interacts salary with organization and salary with age. On the right is the binscatter of turnout in general elections 2014 - 2020 by salary separated by organization.

Figure G.4: Age, Organization, and Turnout — Connecticut



Note: Figure shows on the left, the marginal effect of salary predicted by a model that interacts salary with organization and salary with age. On the right is the binscatter of turnout in general elections 2014 - 2020 by salary separated by organization.

H Full Voter Probabilistic Panel Results

The results from the exactly-matched and registered panel are based on a selection of employees who reliably match to one single voter in the voter file and are employed for all of 2014 to 2020. This is a very conservative approach to matching the data, but means that the results are based on data that does not contain any unregistered or partially registered individuals. The consistently registered individuals skew older, whiter, wealthier, and more male compared to the full set of public employees (see Appendix Figure F.1, Table F.1). To explore whether salary has some effect on the full sample of public employees, I use the full set of employees matched probabilistically to the voter file on full name and age.¹¹

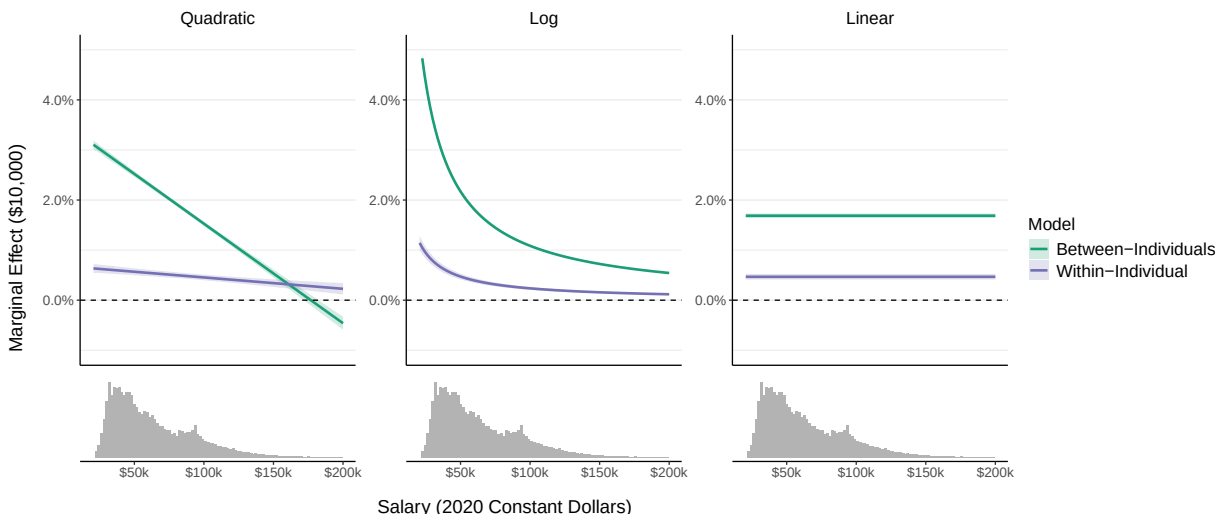
Fitting the same three models as described above on nearly one and a half million observations of salary and turnout yields substantively similar conclusions. Figure H.1 shows, using different functional forms on salary, the marginal effects of a \$10,000 increase in salary on the probability of turning out to vote fit to the entire sample of public employees. While the within-individual effects are higher than in the exactly-matched and registered panel (see Appendix Table H.3 for tabular results), the within-individual effect of salary on turnout remains a fraction of the gap between individuals. Again, it would take very little unobserved confounding to bring the within-individual effect to a range where it is indistinguishable from zero. Unobserved confounders that explain more than 1.54% of the residual variance of both salary and turnout would be strong enough to bring the estimate to zero (Cinelli and Hazlett 2020).

Table H.1: Summary of Average Salary and Raise by Place Full Voter Probabilistic Panel

		Salary (Total Gross, 2020 Constant Dollars)			Raise (2014 - 2020)	
Individuals		Mean	Median	SD	Mean	Median
States						
Connecticut	71,706	\$84,539.91	\$79,499.00	\$40,213.06	\$23,485.53	\$16,194.52
North Carolina	131,594	\$47,581.27	\$40,791.00	\$21,643.12	\$5,567.38	\$3,516.86
Texas	261,754	\$49,804.37	\$45,182.74	\$27,729.96	\$14,198.40	\$8,927.60
Cities						
Austin	20,564	\$71,678.56	\$66,831.26	\$37,572.54	\$25,488.73	\$17,243.15
Chicago	32,793	\$90,075.85	\$91,065.25	\$21,157.35	\$11,349.52	\$8,160.90
Houston	30,258	\$57,490.53	\$56,423.68	\$20,241.18	\$7,519.36	\$4,773.79
Oklahoma City	6,100	\$70,636.94	\$66,971.13	\$24,281.62	\$9,054.93	\$8,011.53
Seattle	19,457	\$94,029.15	\$91,878.80	\$46,781.92	\$33,758.53	\$26,096.72
Overall	574,226	\$59,100.83	\$50,301.60	\$32,978.66	\$13,459.44	\$7,314.45

¹¹Florida records are excluded from the probabilistic approach since they are missing middle initial. See appendix for full probabilistic match diagnostics. The results presented here use a **fastLink** match threshold parameters of 0.9 and 0.7.

Figure H.1: The Marginal Effect of \$10,000 Increase in Salary on Turnout (Full Voter Probabilistic Match Panel)



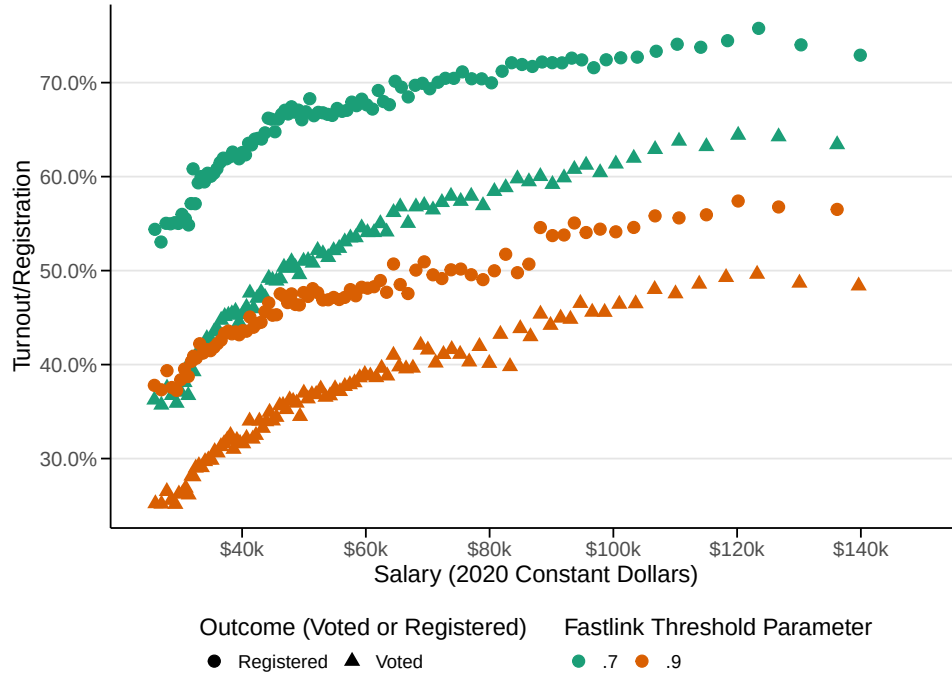
Note: These figures show marginal effects of a \$10,000 increase in salary on the probability of turning out to vote in general elections between 2014 and 2020. This data includes all individuals from the salary records and records their turnout based on a probabilistic record linkage between the voter file and salary records on name, age, and geography (**fastLink** threshold 0.9). The left panel shows the marginal effects from models fit on salary and salary squared, the middle panel on log salary, and the right panel is linear. The histogram at the base of each plot shows the distribution of salary in the sample. The marginal effects are plotted with 95% confidence intervals. For tabular regression results, see Table H.3.

Figure H.2 shows the binned scatterplot of registration (the employee matched to the voter file at all) and turnout (the employee matched to the voter file and voted that year). This is done using all general elections from 2014 to 2020. The binned scatterplot shows the same pattern as the exactly-matched panel and as past literature has documented. Registration and turnout rates are higher for those with higher salaries. Using a lower threshold parameter for the probabilistic match increases the number of registrants and voters compared to a higher threshold value as we would expect.

Table H.2: Individuals Appearing in N Years of Records Full Voter Probabilistic Panel

Place	1 Year	2 Years	3 Years	4 Years
Connecticut	14,992	12,104	9,403	35,207
North Carolina	15,061	14,755	11,144	90,634
Texas	94,194	50,149	38,077	79,334
Austin	5,570	3,835	3,220	7,939
Chicago	5,964	5,646	4,198	16,985
Houston	7,197	5,116	4,315	13,631
Oklahoma City	1,359	1,020	899	2,822
Seattle	5,177	3,672	2,922	7,686

Figure H.2: Average Relationship Between Salary and Turnout (Full Voter Probabilistic Match Panel)



Note: Binned outcome after controlling for city/state. The binned scatter plot divides the range of the independent variable, Salary, into equally populated bins (quantile-spaced). Each point represents the mean of turnout/registration across all elections within that bin, plotted against the mean of salary in the same bin.

Table H.3: Marginal Effect of \$10,000 on Turnout (General Elections 2014 - 2020) Full Voter Probabilistic Panel

	Quadratic		Log		Linear	
	(Between)	(Within)	(Between)	(Within)	(Between)	(Within)
Salary (\$10,000)	0.035 (0.001)	0.007 (0.001)			0.017 (0.000)	0.005 (0.000)
Salary ² (\$10,000)	-0.001 (0.000)	0.000 (0.000)				
ln(Salary)			0.109 (0.001)	0.024 (0.001)		
N Observations	574,226	1,432,101	574,226	1,432,101	574,226	1,432,101
FE: Birthyear	✓		✓		✓	
FE: Election x birthyear		✓		✓		✓
FE: Place (city/state)	✓		✓		✓	
FE: Individual		✓		✓		✓

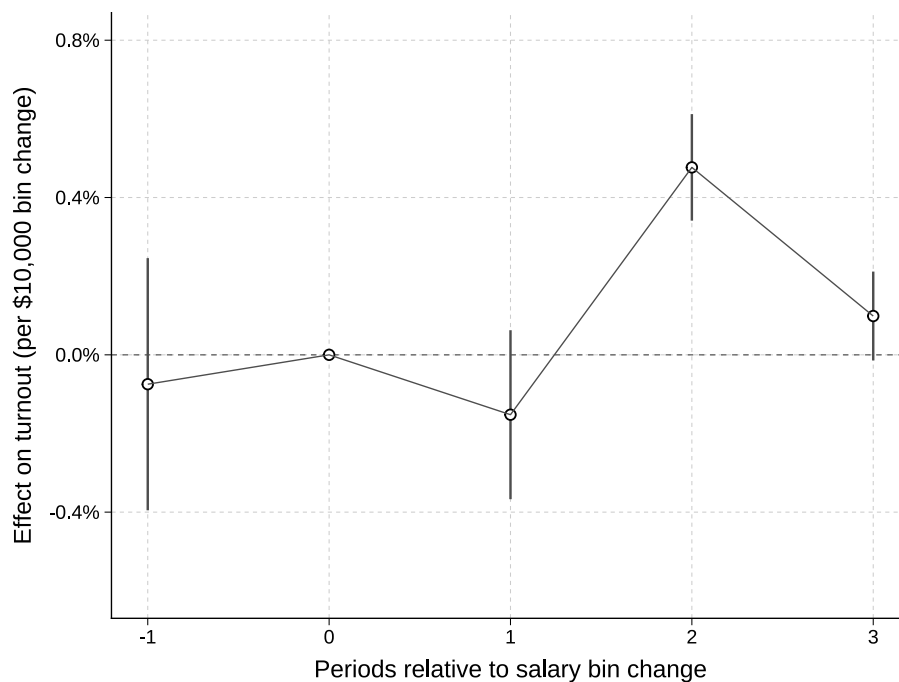
Table H.4: Rate of Matches to Certain Name-Age Records by Place

Place	fastlink 0.9	fastlink 0.7
Connecticut	0.998	0.997
North Carolina	0.997	0.993
Texas	0.991	0.986
Austin	0.999	0.998
Chicago	0.998	0.996
Houston	0.996	0.994
Oklahoma City	1.00	1.00
Seattle	0.999	0.998

Note: The rate of matches to certain name-age records is the proportion of records that either did not match at all to a name-age record in the voter file, or that matched to a name-age record for which everyone with that name-age either voted or did not vote.

I de Chaisemartin and D'Haultfœuille (2026) Estimator

Figure I.1: de Chaisemartin and D'Haultfœuille (2026) Estimator Dynamic Effects Plot



Note: Figure shows the plotted results from `did_multiplegt_dyn` using a discretized version of the salary variable as a treatment dose. Figure shows one placebo period and three periods post-raise which is the maximum possible given the dataset.

The normalized average total effect (ATE) is 0.0031 (95% CI: [0.0010, 0.0053]), or .31 percentage points. This parameter measures the average total effect per unit of treatment, aggregating the contemporaneous effect of a treatment change together with its subsequent lagged effects. In other words, it summarizes the cumulative impact associated with a one-unit increase in treatment intensity over the observed treatment path. The positive and statistically significant estimate indicates that each unit increase in treatment is associated with an average cumulative increase of approximately 0.31 percentage points in the outcome (voting) over time. Interestingly, this is largely concentrated two periods following the treatment dose. This interpretation follows the normalized event-study framework proposed by de Chaisemartin and D'Haultfœuille (2026), where the average total effect captures the sum of current and future treatment effects per unit of treatment change. Even allowing for non-contemporaneous effects of this nature, this individual-level cumulative effect is much smaller than comparable cross-sectional analyses imply.

J Investigating Pre-Trends

Table J.1: Marginal Effect of \$10,000 Increase in Salary on Turnout (General Elections 2014 - 2020)

	Exactly-Matched Set		Full Probabilistic Panel	
	(1)	(2)	(3)	(4)
Current ln(Salary)	0.011 (0.003)	0.028 (0.005)	0.024 (0.001)	0.032 (0.003)
Lead ln(Salary)		-0.001 (0.004)		0.002 (0.002)
N Observations	654,452	490,839	1,432,101	911,084
FE: Election x birthyear	✓	✓	✓	✓
FE: Individual	✓	✓	✓	✓

Note: Columns (1) and (3) report the baseline specification with individual fixed effects. Columns (2) and (4) add a lead of the salary variable (the individual's salary two years in the future).

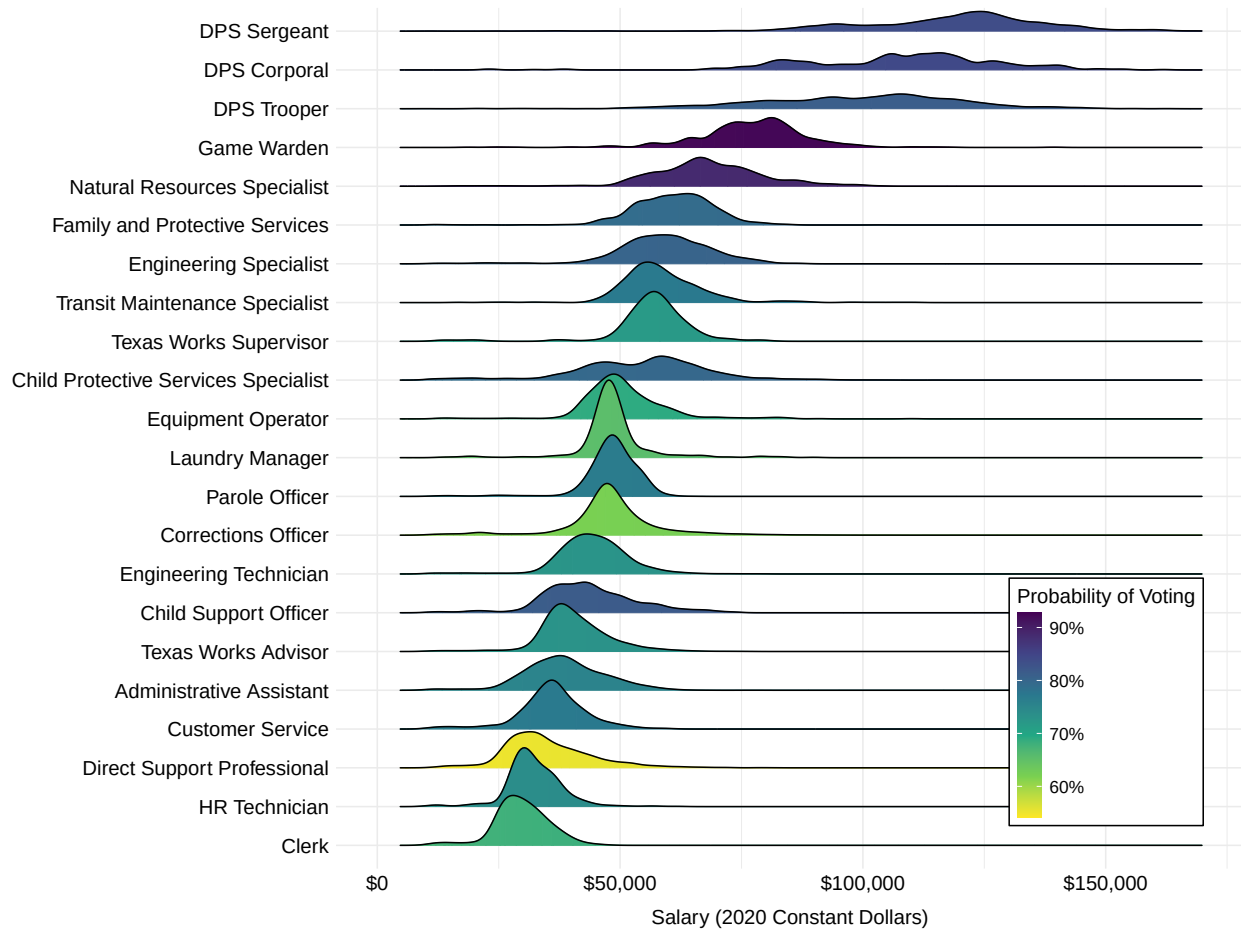
K Income and Turnout within Occupations

To further demonstrate the spuriousness of the income-turnout relationship, in this section I compare turnout rates among people in the state of Texas who have the exact same job title but earn different incomes.¹² Earnings variation within job title ($\sigma = \$9,640.74$) is more limited than across the full working population ($\sigma = \$26,011.68$), but there is still variation because of seniority, overtime pay, and other specific responsibilities. I focus on Texas because of its size and data quality (race, gender, and job title are reported clearly by the state). A job title would be, for example, “Engineering Technician I” or “Accountant II.”

Figure K.1 shows a selection of popular jobs in the Texas state records. On each row is the density curve for the salary distribution among employees with that exact title. The jobs are ordered top to bottom by mean income and shaded (allowing for intra-job variation in color) according to their average turnout between 2014 and 2020. What stands out is that the highest mean salary jobs are not necessarily the highest to turn out and the color gradient is much clearer across titles than within. The colors in each curve appear remarkably solid. Homogeneous participation patterns within a given job title align with theories of socialization and social transmission of political participation and rebukes resource-based theories of turnout.

¹²Although private sector salaries are generally unavailable, I use a unique source of identifiable private sector data to confirm the same pattern outside of public employees: MLB players. See Appendix Section L for an exercise using the Lahman Database.

Figure K.1: Turnout and Salary Within Various Job Titles (Texas)



Note: Each line in the plot shows a salary distribution for a job title in the Texas public employee data. The job titles are ordered by average income. The gradient shading represents the average turnout rate across 2014-2020 general elections. The color gradient is much clearer across titles than within, where colors appear solid and there is close to no variance in turnout rates.

L Equivalent Turnout Among MLB Players Across Salary

Private sector individually identified salary data are generally unavailable, but athletes are a notable exception. In this section I test whether cross-sectional income-turnout gaps exist among a group whose salaries are known but are not public employees, Major League Baseball players. While baseball players have exceptionally high salaries and unique lifestyles, this analysis offers preliminary evidence beyond public sector employees.

The Lahman Database¹³ is a rich source of detailed baseball statistics on every professional player going back to the beginning of the MLB. The records include exact date of birth and full names, with columns for players' given names in addition to common names. For example A.J. Pierzynski is listed with his full name, Anthony John Pierzynski. In addition to game statistics like batters averages and pitching data the database contains salary records through 2016 as well. For each even year between 2000 and 2016 I match all US citizen players by date of birth and first and last name to a complete national voter file¹⁴ and record their turnout in the November general election of that year. Many players move around the country and are registered in multiple states, so I record them as having voted if any of their matched registration records voted.

Table L.1 below breaks down the data by year and shows the number of individuals matched and not matched, as well as both groups' average salaries and ages. About half of the annual player observations in the salary records match to no voter file record, and among the players that do match, turnout is quite low. In Figure L.1 I use the matched sample of MLB players, approximately 300 per year, and separately plot the turnout rate by salary decile. The salary range is massive, from the bottom decile averaging around \$360,000 to the top decile averaging around \$12.8 million. Salary and turnout are correlated at .005 among these MLB players, less than the correlation between player weight and turnout (.032), player height and turnout (.024), and of course player age and turnout (.114).

¹³<https://sabr.org/lahman-database/>

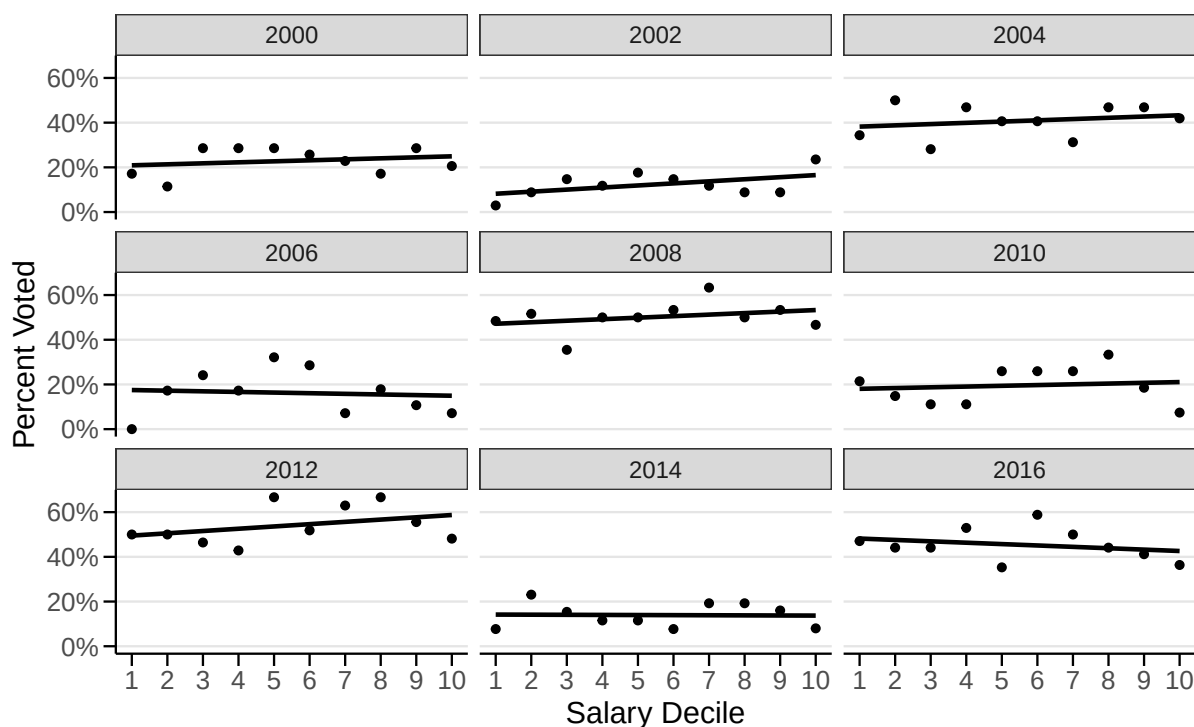
¹⁴For 2016 and 2014 the voter file is the complete national L2 file snapshot from shortly after the election in question. Since 2014 is the earliest year for which I have complete national voter file snapshots, I use this snapshot to observe the earlier elections – meaning players' observed turnout in pre-2014 elections is more subject to error from voter file purges.

Table L.1: MLB Players Matched and Not Matched to the Voter File by Year

Year	Matched				Non-Matched		
	Mean Salary	N	Mean Age	Turnout	Mean Salary	N	Mean Age
2000	\$2,096,252.10	349	30.58	22.9%	\$1,982,617.00	290	30.16
2002	\$2,501,414.72	340	30.59	12.4%	\$2,328,276.08	279	30.18
2004	\$2,546,396.27	319	30.88	40.8%	\$2,267,574.84	282	29.80
2006	\$2,817,755.63	284	30.67	16.2%	\$2,631,596.20	310	29.98
2008	\$2,958,126.22	303	30.25	50.2%	\$2,904,174.81	309	29.61
2010	\$3,124,246.74	271	30.00	19.6%	\$3,075,143.93	326	29.26
2012	\$3,384,647.36	274	29.85	54.0%	\$3,348,096.18	333	29.14
2014	\$3,632,327.70	258	29.75	14.0%	\$4,003,509.06	325	29.21
2016	\$4,236,565.35	339	29.67	45.4%	\$4,204,728.03	277	28.96

Note: Table shows data on the MLB players from the Lahman database matched and not matched to the national L2 voter file. Matches are recorded if players match exactly on first name (either common or given), last name, and birthdate. If there are multiple matches (because baseball players commonly live in multiple states) the player is recorded as voting if any of his voter file records turned out. The salaries of players matched to the voter file and not matched are statistically indistinguishable from each other (t-test, $p = .848$).

Figure L.1: Salary and Turnout are Uncorrelated for MLB Players (General Elections 2000 - 2016)



Note: Plot displays turnout by election for MLB players successfully matched to the voter file. Each point displays the turnout rate for an election among players within a given salary decile. Decile 1 is the lowest salary decile for MLB players (mean \$361,898) and decile 10 is the highest salary decile (mean \$12,875,383).