Fairness and Cooperation:
Draft Dodging by the Rich and War Morale in World War II

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Abstract

This paper demonstrates that draft avoidance by the rich undermined war morale in the US during WWII. Using newly digitized individual-level draft records, I show that men whose richer neighbors received more draft exemptions volunteered less, filed more draft appeals, and deserted more. To identify the causal effect, I use an instrument based on randomly assigned draft lottery numbers. By randomly setting the sequence of case reviews, the lottery gave some men more time to prepare their case for an exemption. Heterogeneity analyses and evidence from local newspapers are consistent with a mechanism where individuals perceived the unequal, class-based sharing of the wartime burden as unfair.

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1. Introduction

During WWII, an anonymous American mother wrote scathingly to her son's draft board: "Of all the *unfair* draft boards, yours is the worst ... the government said it would leave it to the American mother to see that her *neighbor*'s boy went as well as her own ... I don't think I've seen where one *rich* boy has been drafted. But plenty of poor ones."[1] This mother's outrage captures a foundational element of the social contract. The capacity of modern states to demand high-stakes contributions, like heavy taxation and military service, often relies on the voluntary cooperation of their citizens (Besley, 2020; Levi, 1988). This cooperation, in turn, is affected by the perceived fairness of the state's demands (Fehr and Schmidt, 1999). When these demands are seen to be shared unfairly—especially when the rich appear to escape obligations imposed on the poor—it erodes the cooperation that underpins the social contract (Luttmer and Singhal, 2014; Acemoglu and Wolitzky, 2020; Besley and Dray, 2024).

Military mobilization has often been a salient example of this violation of fair burden sharing (Levi, 1997). Throughout history, from the French practice of paying a conscript substitute in the 19th century to the university deferments of the Vietnam War, the burden of service has fallen disproportionately on the less advantaged. Despite this historical prevalence, causal evidence on its consequences for the social contract is scarce. How does an unequal distribution of the ultimate sacrifice affect citizens' willingness to contribute to the common good? This is no longer a purely historical question. It has become a pressing contemporary problem as the war in Ukraine compels Western democracies to expand their militaries and consider conscription.[2]

This paper provides the first causal evidence on this question by studying the US mobilization for WWII. The setting is ideal, because the burden of service was starkly skewed: only 10% of Army soldiers came from the top income quartile, while 32% came from the bottom.[3] Using newly collected archival data on draft exemption decisions, I find that men whose richer neighbors received more draft exemptions showed lower war morale. To overcome endogeneity concerns, I exploit the draft lottery. By randomly determining the order of case reviews, the lottery granted some men more time than others to build a case for an exemption.

To measure war morale, I focus on three distinct, high-stakes behaviors. First, as a sign

[1] *Fremont Morning Guide*. 10 December, 1940, p. 1, with emphasis added.

[2] See R. Cohen's article "The Return of the Military Draft" published by *Foreign Policy* on 10 July, 2024.

[3] The over-representation of Black service members was not a feature of WWII: the share of Black soldiers in the Army (9.2%) was nearly identical to their proportion of the national population (9.6%). These figures are based on my calculation from NARA Army Enlistment Records linked to the 1940 Census.

of high morale, I use voluntary enlistment. Despite the profound personal risks, over six million Americans volunteered for service.[4] The second is the filing of a draft appeal. After receiving a draftable classification, an individual could formally appeal for an exemption and express "a somewhat rancorous feeling" (Selective Service System, 1942, p. 222). This action, which paused the induction process and had a nearly 40% success rate, served as a legal channel to reduce one's chance of service.[5] The third, for men already inducted, is desertion from service. This act represents an extreme failure of morale and carries severe consequences for the cohesion of a military unit (Costa and Kahn, 2003).

My key explanatory variable is the draft exemption rate gap among an individual's neighbors, comparing neighbors richer than himself to those poorer. This measure proxies for the salience of class-based draft dodging: a larger gap implies a higher frequency of anecdotes where richer neighbors avoid service while poorer ones are drafted.[6] I use income in relative terms to align with recent findings on "self-centered fairness," where individuals judge fairness based on their own position relative to others (Hvidberg et al., 2023; Lü and Scheve, 2016; Bolton and Ockenfels, 2008).[7]

A central identification challenge is the "reflection problem" (Manski, 1993). This occurs when unobserved factors simultaneously influence both neighborhood exemption rates and an individual's war morale. To address this challenge, I construct an instrument based on randomly assigned lottery numbers. Draft boards were required by law to adjudicate exemptions both sequentially, based on lottery number, and slowly, handling no more than 50 cases per day (Selective Service System, 1942, p. 112). Therefore, a higher lottery number exogenously granted an individual more time to adjust his circumstances to meet exemption criteria before his case was heard.[8] Men could use this time to marry, father a child, change occupations, or become the sole breadwinner by having other working household members quit their jobs. Crucially, this channel was not neutral: men with greater resources were better able to leverage the lottery-induced temporal advantage to secure an exemption. I exploit this phenomenon in my research design. Specifically, I instrument for the difference in exemption rates between richer and poorer neighbors

[4]For military personnel in WWII, fatality and injury rates were 2.5% and 4.2%, compared to a 0.3% fatality rate for male civilians aged 20-34 over the same period (Chambers, ed, 2000; Dunn, 1943).

[5]This approach is conceptually analogous to using property tax appeals to measure low tax morale in public finance (Nathan et al., 2025; Avenancio-León and Howard, 2022; Jones, 2020).

[6]To validate that this local variation was perceptible, I show that county-level aggregation of this exemption gap is associated with the frequency of draft-unfairness content in local newspapers (see Section 5.2).

[7]I find similar results with a specification based on absolute income rankings.

[8]I analyze the administrative records from Pratt, Kansas, and find that a man with a lottery number in the 5th decile had 210 more days to prepare his case than someone in the lowest decile. Consequently, his probability of receiving an exemption was 11 percentage points higher.

using the corresponding difference in their average lottery numbers.

The use of the lottery number during WWII differs from its use in the Vietnam War. In the Vietnam lottery, limited manpower demand led to the use of a cutoff, above which individuals were mechanically ineligible (Angrist, 1998; Tarr, 1981; Selective Service System, 1970). In contrast, the massive manpower demand of WWII meant that there was no such cutoff. Instead, the lottery number determined the timing of adjudication: a high number did not grant ineligibility but merely provided more time for an individual to prepare a case for an exemption.

I construct an individual-level dataset by digitizing and integrating previously unutilized archives from Kansas. I focus on Kansas because it is, to the best of my knowledge, the only state where complete WWII military recruitment data for all military branches—including the Army, Navy, Marines, and Coast Guard—are available.[9] To leverage this unique resource, I first digitize draft registration cards, which provide the lottery number and address of all potential recruits born between 1904 and 1919. I define a registrant's neighbors as all other registrants residing within a 4-mile radius. In 1940s Kansas, this distance captures the effective community within which residents observed each other's lives through shared schools, churches, and markets.[10] Second, I digitize exemption records. Third, I collect service list, appeal, and desertion records to construct proxies for war morale. This dataset allows me to track each individual from the initial registration to final service outcome. Following Abramitzky et al. (2021), I link this newly created dataset to the 1940 census to obtain socioeconomic status (SES) proxies such as income. To ensure the external validity of my findings, I also digitize draft cards from Maryland.

I find that the unequal distribution of military obligations in Kansas discouraged volunteering and increased the prevalence of draft appeals. On average, the exemption rate for richer neighbors was three percentage points higher than for their poorer neighbors. My estimates suggest that eliminating this gap would increase the volunteering rate by 10% and decrease the incidence of draft appeals by 30%. These causal estimates are underpinned by a strong first stage, where a one percentage point difference in the lottery number gap caused a 0.3 percentage point increase in the exemption rate gap.

The erosion of morale caused by this exemption inequity was not confined to the home front; it reached the battlefield. Soldiers from Kansas were more likely to desert if their richer neighbors at home received more draft exemptions. Although the average deser-

[9] The nationwide archives were destroyed by the 1973 National Archives fire.

[10] A 4-mile radius approximates the average distance between two randomly chosen points inside a standard six-by-six-mile township under the Public Land Survey System. In 1940s Kansas, the township functioned as the primary locus of community life. I assign greater weight to closer neighbors. The results are robust to alternative cutoffs (2, 3, or 5 miles).

tion rate was only 0.23%, for recruits from neighborhoods with an exemption gap in the top decile, the rate was four times higher (0.99%).

The results are robust. To address spatial sensitivity, the findings hold using tighter and looser radii compared to the 4-mile baseline, as well as nearest-neighbor counts. To mitigate potential selection bias from individual record linkage, I demonstrate results using surname-level median income. The effects also persist when substituting personal income with alternative status signals, such as occupational rankings, household income, and house value. Furthermore, the finding remains consistent when comparing extreme disparities, like the top and bottom 1% of earners. Moreover, the effects are concentrated among socially proximate peers, such as neighbors sharing the same ancestry. Finally, the core findings replicate in Maryland, confirming external validity in a different state.

Why did the higher draft exemption of the rich undermine war morale? I argue that this inequity triggered a perception of unfair free-riding. Before presenting the evidence, a conceptual point needs clarification: how can a procedurally "fair" instrument, the draft lottery, identify a causal channel rooted in perceived unfairness? The key is to distinguish between the fairness of the identification variation itself and the perceived fairness of the outcomes it isolates. As the mother's letter illustrates, the intricate workings of the draft lottery were invisible; what they saw was its tangible outcome: which local boys went to war and which did not. When a random advantage—more time—disproportionately benefited those with more resources, the outcome was perceived not as luck but as systemic bias. This tendency to judge fairness based on outcomes rather than processes is well documented in the literature (Taleb, 2001; Cappelen et al., 2013; Andreoni et al., 2020). My design leverages the lottery because it isolates the causal impact of skewed outcomes that contemporaries viewed as a demoralizing sign of an unfair social contract.

To support the perceived unfairness mechanism, I present evidence from granular behavioral heterogeneity, corroborating newspaper analysis, and aggregate public opinion. First, I infer the mechanism by documenting three forms of behavioral heterogeneity consistent with a fairness response. The reason for an exemption was critical: likely questionable exemptions had a fivefold larger negative effect on morale than justifiable ones. Moreover, impacts of the exemption rate gap on war morale were amplified in regions where pre-war newspapers used more words associated with the moral foundation of fairness; however, this heterogeneity did not extend to other moral foundations (Haidt, 2012; Enke, 2020). Finally, the principle of fair sacrifice extended beyond service status. The negative impact of the exemption gap was larger when richer neighbors who did serve were nevertheless concentrated in safer military branches.

Second, I show that the information environment reflected the same inequity. In

counties with lower service rates among the rich, local newspapers contain more content characterizing the draft as unfair. To systematically analyze sentiment regarding draft fairness, I construct a novel dataset from newspapers.com containing texts of WWII-era local newspapers across counties where lottery numbers are available from Chen (2025). I use GPT-4.1 mini to classify whether each page makes a reader "more likely to think the military draft is being handled fairly or unfairly."[11] For counties in the bottom decile by civilian rate gap between the highest and lowest income deciles (as predicted by the lottery number gap), 29% of newspaper pages are classified as "more fair" and 4% as "more unfair." Among counties in the top decile, the share of "fair" pages falls by half, while the share of "unfair" pages doubles.

Third, I analyze nationwide public opinion using WWII-era Gallup polls. These surveys asked respondents, "Do you think the draft is being handled fairly in your community?" I find suggestive evidence that a higher civilian rate among the rich in a respondent's state is associated with heightened perceptions of draft unfairness.

I find limited support for alternative mechanisms. Outcome-based explanations—which assume the reason for a richer neighbor's absence from service is irrelevant—such as the lack of role models, pure envy, or networking, are inconsistent with my finding that the justification for an exemption actually mattered. Similarly, labor demand effects are unsupported, as the impact does not vary with the local share of employers. Mechanisms rooted in racism or labor supply predict that poorer neighbors' exemptions should drive the results, but I find the opposite. Furthermore, religious pacifism and national origin are ruled out by spatial and demographic heterogeneity. Finally, mainstreaming dodging among the rich contradicts the stronger reaction of lower-income individuals, while the lack of moderation by the moral foundation of authority rules out negative reciprocity towards authority figures.

In the last section, I provide suggestive evidence that a perceived failure of the social contract in one domain can lead citizens to demand compensation in another. Using WWII-era Gallup surveys, I find that respondents in states with lower military service rates among the rich favored a more progressive tax system, indicating a desire to restore fairness through fiscal policy.

This paper is structured as follows. Section 2 reviews the literature, Section 3 provides historical context and describes the data, Section 4 presents the main results, Section 5 examines potential mechanisms, Section 6 analyzes spillover effects, and Section 7 concludes.

[11] I conduct rigorous validations of the GPT classification; see Appendix C for details.

2. Literature Review

This paper contributes to four strands of literature. First, I contribute to the literature on the determinants of the social contract. Besley (2020) develops a theoretical framework of reciprocity among three parties in a social contract—the state, citizens, and their fellow citizens—which Papaioannou (2020) characterizes as involving civicness and social capital. Complementing this framework, Alesina et al. (2020) emphasize how the state proactively invests in nation-building to foster the nationalism required for war morale. Recent empirical studies have provided compelling evidence on the vertical, state-citizen dimension of this contract. They find that supportive state policies boosted citizens' trust in government and public good contributions (Besley et al., 2025; Caprettini and Voth, 2023), whereas discriminatory ones undermined them (Qian and Tabellini, forthcoming; Fouka, 2020) or activated efforts to reconfigure the social contract (Ang and Chinoy, 2025; Koch et al., 2021). I contribute by examining the horizontal, citizen-citizen dimension of this contract. I show how the social contract's stability depends not only on what the state demands, but on how these demands are shared among citizens.

Second, this paper speaks to the literature on social capital, especially in higher-stakes settings. Costa and Kahn (2003, 2007) demonstrate that Civil War soldiers in more cohesive units were less likely to shirk, and social networks allowed them to survive the prisoner-of-war camps. Ager et al. (2021) find that German fighter pilots during WWII responded to their former peers' public recognition by taking more risks. Dippel and Heblich (2021) find that local leaders such as "Forty-Eighters" mobilized more volunteers from their communities for the Union Army. Cagé et al. (2023) analyze the role of Marshal Pétain's network in gathering Nazi collaboration in WWII France. Grosjean et al. (2023) show that French soldiers serving in the same regiment during WWI converged in their post-war voting. While this literature powerfully demonstrates that social ties matter, it offers less insight into which social contacts matter most and why. I contribute by showing that not all neighbors' outcomes affected war morale: only those of richer neighbors mattered, as their ability to secure exemptions updated individuals' perceptions of fairness.

Third, I contribute to the literature on tax fairness. Conscription has long been understood as a form of tax in kind (Becker, 1957; Friedman, 1962). A vast literature in public finance shows that tax morale is influenced by perceptions of fairness. When citizens believe the tax system is unfair, or that the rich are engaging in evasion and avoidance, tax morale erodes (Holz et al., 2025; Nathan et al., 2023; Besley et al., 2023). I extend this line of inquiry to military conscription, showing that higher draft exemptions for the rich

lowered war morale.[12]

Finally, a substantial body of research examines the effects of military service on the lives of veterans. This literature explores a wide range of outcomes, including those related to the labor market (Angrist, 1990; Imbens and Klaauw, 1995; Angrist, 1998; Angrist et al., 2011; Autor et al., 2011; Siminski, 2013; Bauer et al., 2012; Card and Cardoso, 2012; Goodman and Isen, 2020; Greenberg et al., 2022; Ronconi and Ramos-Toro, 2025; Barrera et al., 2025; Chen, 2025),[13] human capital (Cipollone and Rosolia, 2007; Blattman and Annan, 2010; Angrist and Chen, 2011; Cousley et al., 2017), health (Bedard and Deschênes, 2006; Autor et al., 2011; Siminski and Ville, 2011; Johnston et al., 2016), crime (Galiani et al., 2011; Hjalmarsson and Lindquist, 2019; Siminski et al., 2016), family dynamics (Conley and Heerwig, 2011; Bitler and Schmidt, 2012; Cesur and Sabia, 2016), and social and political preferences (Erikson and Stoker, 2011; Patulny et al., 2015; Bauer et al., 2017; Fize and Louis-Sidois, 2020; Ertola Navajas et al., 2022; Bagues and Roth, 2023; Ronconi and Ramos-Toro, 2024). However, few studies examine the antecedent question of how the military service burdens are allocated. Notable exceptions are Asoni and Sanandaji (2013) and Carter et al. (2017). They provide descriptive evidence on the economic and demographic backgrounds of recruits in the modern all-volunteer military. Building on this literature, I analyze the socioeconomic representation of the largest manpower mobilization in history and its causal effect on the social contract.

3. Background and Data

3.1 Background

The US military was an all-volunteer force until the Selective Training and Service Act of 1940 reinstated the draft. The Act established the Selective Service System to administer the conscription of men for military service. This system operated through 6,443 local draft boards. The legislation mandated one board per 30,000 people, with at least one in every county. These local boards managed the mobilization of manpower through a three-stage process: registration, lottery, and adjudication.[14]

[12] Relatedly, draft avoidance incentives increased college enrollment and fertility during the Vietnam War or cheating on medical exams in Argentina (Card and Lemieux, 2001; Bailey and Chyn, 2020a; Cruces et al., 2023). Unlike these studies on individual outcomes, I isolate the negative spillovers of such avoidance on neighbors' morale.

[13] In particular, Chen (2025) provides a comprehensive analysis of the WWII draft lottery's effect on earnings in 1950. Given his extensive treatment of these economic returns, I focus on the consequences of draft unfairness.

[14] This section is based on Selective Service System (1942).

Registration. On October 16, 1940, all male residents born between October 17, 1904 and October 16, 1919 were required to register with their draft boards.[15] Each registrant filled out a registration card. Registration was a legal obligation with penalties for non-compliance, leading to near-universal participation: 99.47% of the 16.4 million eligible men registered. To prepare for the national lottery, officials at each local board first shuffled the registration cards and then assigned sequential serial numbers to registrants.

Lottery. Thirteen days after the registration day, the government conducted a public national lottery in Washington to establish the order of adjudication by draft boards (see Figure A.1A). The lottery created a national sequence for pre-assigned serial numbers. This national sequence, in turn, mechanically determined each registrant's local order number (from 1 to N_{max}, where N_{max} was the maximum number among all registrants in a board). For example, the first serial number drawn was 158, so every registrant across draft boards with that serial number was assigned the order 1. In this paper, the order number normalized by N_{max} is referred to as the "lottery number."

Adjudication. Before induction, draft boards had to adjudicate whether each registrant qualified for an exemption from service. Given their limited administrative capacity (each board had only three unpaid volunteers), boards processed registrants sequentially according to the lottery-determined order. As illustrated in Figure 1, this process began with the lowest number and proceeded in order. When a registrant's number was called, his board mailed him a questionnaire regarding his personal circumstances. He was required to return the questionnaire within 5-10 days.[16] Based on the returned questionnaire, the board classified the registrant as either exempt from or available for service. Exemptions were granted for reasons such as occupation, family dependency, age, or health. Men who were not exempted were drafted. By law, draft boards were required to send no more than 50 questionnaires per day.

The administrative records of the Pratt draft board in Kansas validate this process. The board was responsible for 1,265 registrants. On the days it mailed questionnaires, the board sent an average of 30 per day. On average, it took 6.6 days for the board to receive the completed questionnaires. The board adjudicated registrants in strict order of their lottery number: compared to registrants with the lowest lottery numbers, those

[15] This was the first and largest registration. While subsequent registrations included younger cohorts to meet military needs, this initial registration accounted for 50% of all draftees and 60% of all volunteers. The treatment of this first, massive cohort would therefore set the precedent, shaping public perception of the draft's fairness for the duration of the war.

[16] An extension can be requested. The return period was extended from 5 to 10 days after May 31, 1941.

with a 5th-decile number were contacted 207 days later (Figure A.2) and returned their questionnaires 208 days later (Figure 2).[17] This delay corresponds to an 11 percentage point increase in the probability of receiving an exemption.

Given the scale of the mobilization, nearly every registrant was eventually adjudicated in Kansas, a state that my primary analysis focuses on (see Table 1). Consequently, exemptions accounted for nearly all men who remained civilians.

Registrants could appeal their local board's adjudication decision to a designated appeal board without cost. An appeal temporarily paused the registrant's conscription process. A successful appeal resulted in an exemption. This process took approximately one month, with a success rate of nearly 40%, according to my analysis of appeal records. Throughout this period, men could still enlist voluntarily.

3.1.1 Comparison to the Vietnam Lottery

The draft lotteries during WWII and the Vietnam War differed in their assignment mechanisms. In the Vietnam lottery, men born in a specific year received numbers based on a random ordering of their birth dates (Selective Service System, 1970). In contrast, the main WWII lottery pooled multiple birth cohorts and randomly assigned numbers regardless of birth date.[18]

More importantly, the lotteries served different functions. During the Vietnam War, smaller manpower needs allowed for an annual cutoff number; men with numbers above this cutoff were mechanically ineligible for the draft and never faced adjudication (Angrist, 1990; Tarr, 1981). In WWII, by contrast, the massive manpower demand meant there was no "safe" number. A high lottery number did not grant ineligibility—as nearly every man was subject to adjudication eventually—but instead determined the timing of adjudication. Thus, the lottery number governed draft eligibility in Vietnam but adjudication timing in WWII. To see this distinction clearly, consider a high-numbered man who remained a civilian. In the Vietnam draft, he was a civilian because his number was never adjudicated. In the WWII draft, he was a civilian because his number being adjudicated late gave him time to secure an exemption.

[17] Registrants with higher lottery numbers had longer turnaround time, days between the board's mailing and the registrant's reply. However, this difference was small. For instance, the turnaround time of those with a 5th-decile number was 1 day longer than that of the 1st decile.

[18] The ordering mechanism for a smaller cohort of men born after July 1, 1924, was based on their birth date (Angrist and Krueger, 1994). This cohort is outside the scope of the present analysis.

3.2 Data

3.2.1 Registration Cards

My first dataset comprises the universe of draft registration cards for Kansas from the October 1940 registration. I transcribe these cards from records provided by Fold3. As illustrated in Figure A.1B, each card records detailed individual-level information: name, residential address, date and place of birth, race, height, weight, lottery number, and local draft board. I geocode residential addresses using the Google Maps API.[19] To ensure comparability across local boards of varying sizes, I normalize each lottery number by the maximum number in its board. This procedure creates a rank variable scaled to the unit interval [0, 1].

To enhance external validity, I transcribe registration cards for Maryland—a state with a distinct historical and socioeconomic profile. These scanned cards are provided by the National Archives and Records Administration (NARA).

The draft cards lack direct SES proxies. To address this limitation, I link individuals to the 1940 full-count census using the automated procedure of Abramitzky et al. (2021). This process relies on name, race, birth year, birth state, and county of residence, and yields a match rate of 55% (See Appendix B for details). Given my large sample, conventional tests of means between the linked and unlinked populations are likely to detect statistically significant yet economically trivial differences. I therefore follow Spenkuch et al. (2023) and employ equivalence tests to assess the balance between the two groups. This method tests the null hypothesis of an economically meaningful difference, defined as a standardized mean difference exceeding 0.1 standard deviations.

Table 2 presents the results. I reject the null of an economically meaningful difference for eight of the nine observable characteristics. The sole exception is race, where the linked sample contains 2.9 percentage points fewer non-white individuals. Crucially, my findings are robust to an alternative SES proxy that avoids individual census linkage (see Section 4.2.4).

3.2.2 Exemption Records

I digitize exemption records in Kansas, which list all registrants whose cases were adjudicated and their resulting exemption status (Figure A.1C provides a sample page). Preserved at the National Archives at St. Louis, these records identify each individual by draft

[19] To validate the accuracy of this procedure for historical addresses, I compare the API's output to a manual geocoding of the 1923 Sanborn Map of El Dorado, Kansas. This comparison yields mean and maximum discrepancies of only 21.6 and 58 meters.

board and lottery number, which enables a nearly comprehensive match to my registration card data. From this linked dataset, I construct my key explanatory variable and its instrument.

3.2.3 Records for Dependent Variables

I use three proxies to measure war morale: military volunteering, draft appeal filing, and desertion. I draw data for these outcomes from three sources.

Selective Service Index. I use the Selective Service Index (SSI) digitized by the Kansas Historical Society. The SSI provides a comprehensive roster of draftees and volunteers from Kansas during WWII. Its principal advantage over the more commonly used NARA Army Enlistment Records (NARA-AER) is its complete coverage of recruits across all branches of the US Armed Forces, including the Army, Navy, Marines, and Coast Guard.[20] Furthermore, the SSI identifies individuals by draft board and lottery number, allowing for a nearly 100 percent match rate to registration cards. In contrast, the NARA-AER lacks comparable personal identifiers, and linking these records to registration cards results in the loss of approximately half of the observations.

Appeal Records. I digitize the records of draft appeal boards in Kansas (Figure A.1D). Stored at the National Archives at St. Louis, these records identify registrants by their draft board and lottery number. The records detail each appeal's outcome, filing date, and the date of the board's final decision.[21]

WWII Army Deserters Pay Cards. I obtain data on Army deserters between 1943 and 1945 from Ancestry.com. These records provide a deserter's service number, which allows me to link individuals to the SSI and, subsequently, to their registration card. This dataset is not a universal list of deserters, as it is limited to Army personnel and only covers three years.

3.2.4 Exemption Rate Gap

My key explanatory variable, the exemption rate gap, proxies for the local salience of class-based draft dodging. It measures the extent to which exemptions are concentrated

[20] At least 30% of recruits served in non-Army branches (The National WWII Museum, 2024).

[21] To validate the appeal proxy, Figure A.3 shows that individuals who filed an appeal were 2.5 percentage points less likely to volunteer (28% of the mean). Conversely, they were 2 percentage points more likely to desert (twice the mean).

among an individual's richer neighbors relative to their poorer counterparts, capturing the relative frequency of observable anecdotes where richer neighbors avoid service while poorer ones are drafted. This variation was directly observable in the small communities that comprise my sample. Draft boards were administered by three local volunteers, and exemption outcomes—who served and who did not—were common knowledge within a neighborhood (Davis and Dolbeare, 1968). Consistent with this, I show in Section 5.2 that county-level aggregates of the exemption gap are associated with the frequency of draft-unfairness content in local newspapers.

I define neighbors as registrants residing within a 4-mile radius of an individual's address in the same draft board with different surnames.[22] This radius approximates the average distance between two randomly chosen points inside a standard six-by-six-mile township under the Public Land Survey System in Kansas.[23] It also corresponds to the distance an adult can walk in approximately one hour (Alves et al., 2020). The results are not sensitive to the choice of this cutoff (see Section 4.2.4). Figure A.4 shows the median number of neighbors within each 0.5-mile distance interval, from 0–0.5 miles to 3.5–4 miles.

For each individual i, the exemption rate gap is calculated as:

$$\text{Exempt Rate Gap Neighbors}_i \equiv \frac{\sum_{j \in N_i} w_{ij} \cdot \text{Exempt}_j \cdot \mathbf{1}[\text{Income}_j > \text{Income}_i]}{\sum_{j \in N_i} w_{ij} \cdot \mathbf{1}[\text{Income}_j > \text{Income}_i]} - \frac{\sum_{j \in N_i} w_{ij} \cdot \text{Exempt}_j \cdot \mathbf{1}[\text{Income}_j \leq \text{Income}_i]}{\sum_{j \in N_i} w_{ij} \cdot \mathbf{1}[\text{Income}_j \leq \text{Income}_i]}, \tag{1}$$

where N_i is the set of neighbors of individual i, w_{ij} is a weight function detailed below, $Exempt_j$ is an indicator variable equal to one if neighbor j is exempt and zero otherwise. The first term is the weighted exemption rate of neighbors with incomes higher than individual i's, while the second term is the corresponding rate among neighbors with weakly lower incomes. My analysis sample is restricted to individuals for whom both denominators in Equation 1 are non-zero, ensuring the exemption rate gap is well-defined.

To capture that a neighbor's influence declines with geographical distance, I define the weight w_{ij} as:

$$w_{ij} = \exp(-\text{Distance}_{ij}), \tag{2}$$

where $Distance_{ij}$ is the physical distance between individual i and neighbor j, expressed

[22] I exclude individuals with the same surname to ensure that the variable captures the influence of neighbors rather than extended family members.

[23] This system explains the rectangular shape of counties and townships in Kansas, see Pattison (1957) for details.

relative to the neighborhood radius. Figure A.5 illustrates how w_{ij} declines as $Distance_{ij}$ increases: a neighbor located at 20% of the radius receives approximately twice the weight of a neighbor at the radius boundary. I present results if $w_{ij} = 1$ in Section 4.2.4.

3.2.5 Summary Statistics

Table 3 presents summary statistics for my main sample of 59,658 men. On average, the exemption rate of richer neighbors was 2.8 percentage points higher than that of poorer neighbors (standard deviation: 0.14).[24] In terms of war morale measures, 8.3% of individuals volunteered and 10.7% filed an appeal. Among those recruited, 0.2% deserted.

4. Results

Figure 3 illustrates a negative relationship between the exemption rate gap and war morale. Specifically, individuals exposed to a wider gap between the exemption rates of their richer and poorer neighbors were less likely to volunteer for military service, more likely to file a draft appeal, and, for those who served, had a higher propensity to desert.

4.1 OLS Results

To formally test the relationship in Figure 3, I estimate the following OLS model:

$$War\ Morale_{ib} = \beta \cdot Exempt\ Rate\ Gap\ Neighbors_i + \theta X_i + \delta_b + \epsilon_{ib} \tag{3}$$

where the dependent variable, $War\ Morale_{ib}$, is one of the war morale indicators for individual i in draft board b. The coefficient of interest, β, captures the association between the exemption rate gap and war morale. The term δ_b denotes draft board fixed effects, which absorb unobserved heterogeneity at the draft board level. The vector X_i includes a set of control variables: the average exemption rate among all neighbors of i, to account for potential social spillovers; i's own lottery number, to account for the sequence of adjudication; and neighbors' mean income and income Gini coefficient, to account for local economic conditions. Standard errors are clustered at the draft board level. I also report p-values from randomization inference as a more robust alternative for hypothesis testing (Young, 2018).

[24] Figure A.6A plots the exemption rate gap against individual income. I observe substantial variation in the gap across the income distribution.

Table 4 reports the regression results. Columns 1 and 2 demonstrate that a ten percentage point increase in the exemption rate gap is associated with a 0.73 percentage point decrease in the probability of volunteering and an 8.67 percentage point increase in the probability of filing a draft appeal. In Column 3, I construct a war morale index using the first principal component of the volunteering indicator and the negative of the draft appeal indicator. I find that a ten percentage point increase in the exemption rate gap corresponds to a 0.032-unit (11% of the standard deviation) decline in this composite morale index.

4.1.1 Potential Sources of Endogeneity

Despite the inclusion of an extensive set of controls, the OLS estimates remain potentially vulnerable to omitted variable bias. Two primary concerns could introduce estimation biases in opposite directions.

A downward bias (i.e., an overestimation of the negative effect) could arise from unobserved neighborhood norms, such as patriotism. In communities with strong patriotic norms, richer individuals may feel social pressure to serve and thus be less likely to pursue exemptions. This would decrease the exemption rate gap. Simultaneously, these same patriotic norms would independently foster higher general war morale. Because this omitted patriotic norm is negatively correlated with the exemption gap and positively correlated with war morale, its omission would bias my estimate of β downward.

Conversely, an upward bias (i.e., an underestimation of the negative effect) could be introduced by local economic conditions. In neighborhoods with limited employment opportunities outside the military, poorer individuals might strategically avoid exemptions. This behavior would increase the exemption rate gap. At the same time, the lack of economic alternatives would drive higher rates of volunteering. In this scenario, the omitted variable is positively correlated with both the independent and dependent variables, leading the OLS estimate to understate the true adverse impact of the exemption rate gap on war morale.

4.2 2SLS Results

To identify the causal effect of the exemption rate gap on war morale, I use the difference in average lottery numbers between an individual's richer and poorer neighbors as an instrumental variable. The lottery number gap, $Lottery\ Number\ Gap\ Neighbors_i$, is constructed in the same way as the exemption rate gap in Equation 1, replacing $Exempt_j$ with $Lottery\ Number_j$ and using the same distance-based weights. The mean lottery number

gap is close to zero, with a standard deviation of 0.07 (see Table 3).[25]

I estimate the following two-stage least squares (2SLS) model:

$$\textit{Exempt Rate Gap Neighbors}_i = \phi \cdot \textit{Lottery Number Gap Neighbors}_i + \theta X_i + \delta_b + \nu_{ib} \quad (4)$$

$$\textit{War Morale}_{ib} = \alpha \cdot \widehat{\textit{Exempt Rate Gap Neighbors}}_i + \theta X_i + \delta_b + \epsilon_{ib} \quad (5)$$

where Equation 4 is the first stage. The predicted values of the dependent variable from the first stage, $\widehat{\textit{Exempt Rate Gap Neighbors}}_{ib}$, are then used in the second stage (Equation 5). X_i denotes the same set of variables as in Equation 3 to enhance the comparability between OLS and 2SLS estimates. The coefficient of interest, α, captures the causal effect of the exemption rate gap on war morale. Standard errors are clustered at the draft board level, and I report p-values from randomization inference.

As illustrated by Figure A.7, the validity of this instrument rests on two assumptions. First, the random assignment of lottery numbers underpins the exclusion restriction: the lottery number gap affects war morale only through the exemption rate gap. Second, the temporal advantage of higher lottery numbers, detailed below, underpins the instrument's relevance.

4.2.1 Exclusion Restriction

To establish the exogeneity of the lottery number gap as an instrument, I first verify the micro-foundation: random assignment of lottery numbers at the individual level. Figure 4 plots standardized coefficients from separate regressions of individual characteristics on an individual's lottery number, controlling for draft board fixed effects. The characteristics are grouped into three categories: demographics (birth year, foreign born, minority status, height, weight, and residential mobility), socioeconomic status (education, income, home ownership, home value, and home rent), and family characteristics (marital status, number of children, co-residence with parents, number of siblings, parental nativity, and veteran father). The coefficients are shown with 95% confidence intervals. They are both economically and statistically insignificant.

I also test whether they are jointly statistically significant. The p-value is 0.96. The joint test p-value is computed using Fisher's combined probability test (Yu et al., 2024). For each characteristic, I regress it on the lottery number with draft board fixed effects and standard errors clustered at the board level. Fisher's method combines these p-values using the test statistic $\chi^2 = -2\sum \ln(p_i)$, with degrees of freedom equal to twice the number of

[25] Figure A.6B plots the lottery number gap against individual income.

characteristics. Positive correlations among outcomes are likely, making this test biased toward finding significance. Thus, the high p-value provides robust evidence supporting random assignment of lottery numbers.

With confidence in individual-level randomization established, I now examine whether the instrument, lottery number gap between an individual's richer and poorer neighbors, is correlated with either the individual's own characteristics or the average characteristics of their neighbors. Using the same set of variables as in Figure 4, I regress each outcome on the instrument with draft board fixed effects. Figure 5 plots the standardized coefficients and their 95% confidence intervals. The Fisher p-value for own characteristics is 0.94. The Fisher p-value for neighbors' average characteristics is 0.53. Both results support the exogeneity of the instrument.

4.2.2 Relevance: Temporal Advantage

The relevance of my instrument relies on the positive relationship between the lottery number gap and the exemption rate gap in Figure 9A. This aggregate relevance is driven by a specific individual-level mechanism: the temporal advantage conferred by the draft lottery. During WWII, a higher lottery number granted a temporal advantage—individuals with higher numbers were adjudicated for exemptions later. This delay provided registrants with more time, a critical resource for altering one's circumstances to qualify for an exemption.

This temporal advantage meant that individuals with higher lottery numbers had a greater likelihood of receiving an exemption. Figure 6 provides direct evidence for this relationship. The binscatter plot, which is conditional on registrants being adjudicated, shows a significant positive relationship between a registrant's lottery number and the probability of receiving an exemption.[26]

This positive slope is noteworthy when contrasted with Figure 4, which shows that lottery numbers are uncorrelated with exemption-relevant characteristics measured in the 1940 census—at least six months before the lottery. Without a behavioral response enabled by the temporal advantage, the relationship in Figure 6 would be flat. The observed upward slope thus points towards the temporal advantage mechanism.

I provide additional evidence on how individuals with higher lottery numbers utilized

[26]Each draft board comprised three members, regardless of the number of registrants in its jurisdiction. Consequently, boards with a larger caseload processed registrants at a slower pace. This implies that for larger boards, a given increase in the lottery number translated into a greater temporal advantage. Figure A.8 plots the average marginal effect of the lottery number on the exemption probability across different board sizes. The results demonstrate that the effect of the lottery number on the likelihood of exemption is more pronounced for boards overseeing a larger number of registrants.

their temporal advantage to secure an exemption, by linking registrants to the 1940 and 1950 censuses and comparing the change in personal circumstances. Note that the 1950 Census was conducted five years after V-J Day. Some changes in personal circumstances could have been reversed during 1945–50. Any such reversion would attenuate the estimated slopes toward zero, so the 1940–50 comparison yields *lower-bound* estimates.

Marriage. A central tenet of the exemption policy, particularly during the initial years of the war, was the protection of family units. This rendered married men highly probable candidates for exemption and precipitated a "marriage boom" following the draft's announcement (Modell, 1989, Figure 13). However, securing a marriage requires time, a resource more available to individuals with higher lottery numbers. Consistent with this, Figure 7A analyzes men who were single in 1940 by plotting the relationship between their lottery number and their probability of being married by 1950. The sample excludes military recruits to isolate the behavioral response from the effects of service. The resulting binscatter plot shows that a ten percentage point increase in the lottery number causes a 0.32 percentage point increase in the probability of marriage.

Fertility. The principle of family protection also extended to fathers, who held an advantage in obtaining draft exemptions. This incentive precipitated a "baby boom."[27] Given that conceiving a child requires time, men with higher lottery numbers were better positioned to leverage this exemption channel. Focusing on civilian men without children in 1940, Figure 7B demonstrates a positive relationship between a man's lottery number and the likelihood of fathering a child by 1950. A ten percentage point increase in the lottery number causes a 0.2 percentage point increase in the transition to fatherhood.

Breadwinners. Household breadwinners, particularly sole earners, were favorably treated by draft boards. Consequently, anecdotal evidence suggests that other family members would leave their jobs to strengthen a registrant's claim to breadwinner status (Dickson, 2020). A higher lottery number provided more time for households to orchestrate such an adjustment while minimizing the loss of family income. Figure 7C provides empirical support for this behavior, showing that moving from the lowest to the highest lottery number causes a decrease of 0.044 persons in the difference in the number of employed persons within a household between 1950 and 1940.

[27] This coincides with the sustained increase in birth rate starting in 1940, which reversed a long-term decline. See Table 1-1 of Vital Statistics of the United States, 2003, Volume I, Natality. Bailey and Chyn (2020b) document a similar phenomenon during the Vietnam War.

Job Changes. Beyond adjustments related to family status, historical accounts indicate that some men sought exemptions by changing occupations to jobs deemed essential for the war effort. This strategy allowed them to argue that they were contributing to national defense, albeit as civilians. For example, after legislative changes in early 1944 expanded deferments for agricultural workers, many men left higher-paying industrial roles for farm work. In one instance in Kansas, a father purchased land for his sons to help them avoid the draft (Rutenberg, 2019). Similarly, despite poor working conditions, young men flocked to North Carolina's mica mines, which supplied essential wartime materials and offered exemptions to their employees (King, 1949; Rutenberg, 2019). Supporting this narrative, Figure 7D shows that individuals with higher lottery numbers are more likely to have changed jobs between 1940 and 1950. The estimates indicate that a ten percentage point increase in the lottery number leads to a 0.2 percentage point increase in the probability of a job change.

Unobserved Behaviors. The behaviors I document here likely represent only a fraction of the ways individuals exploited the temporal advantage conferred by higher lottery numbers. For instance, Rutenberg (2019) discusses other behavioral adjustments, such as misusing conscientious objector status or feigning medical conditions, which are not captured in my data.

The Role of Income. More importantly, the ability to use the temporal advantage is asymmetric. Higher-income individuals possess greater resources to leverage it: they can more easily find a suitable partner, afford to have children, absorb the income loss from a family member quitting a job, and leverage skills and connections to switch to an exemptible occupation. Figure 8A illustrates this asymmetry with a binned scatter plot of the lottery number against exemption probability, stratified by income in 1940. At low lottery numbers, individuals with above-median income exhibit an exemption probability similar to, or even slightly lower than, those with below-median income. As the lottery number increases, this relationship reverses, and the above-median income group shows a noticeably higher probability of exemption compared to the below-median income group.[28] Figure 8B compares the marginal effects of the lottery number on draft exemption probability for above- and below-median income groups. The above-median income group is nearly twice as effective at converting a higher lottery number into an exemption. This asymmetric ability to leverage the temporal advantage results in a re-

[28] Figure A.9 presents a heatmap of average exemption rates by lottery number decile and income decile. Conditional on the same lottery number deciles, higher-income individuals receive more exemptions.

gressive draft pattern, where richer registrants are ultimately more likely to be exempted from conscription and less likely to be in uniform (see Figure A.10).

Relevance of the Instrument. The temporal lottery advantage establishes the relevance of the instrument. Because a higher lottery number increases the chance of exemption, neighborhoods where richer residents have higher average lottery numbers than poorer residents also have larger exemption rate gaps. Panel A of Table 5 reports the results from the first-stage regression specified in Equation 4. I find that a one percentage point increase in the lottery number gap causes a 0.334 percentage point increase in the exemption rate gap. The rk-LM F-statistic from Kleibergen and Paap (2006) in the first stage is 100.

4.2.3 2SLS Estimates

Before presenting the 2SLS estimates, I briefly synthesize the identification strategy. Its core lies in the random assignment of lottery numbers, which creates an exogenous temporal advantage in the exemption adjudication process. As I have shown, this advantage is asymmetrically beneficial, allowing richer individuals to more effectively secure exemptions. Therefore, the randomly generated gap in lottery numbers between richer and poorer neighbors serves as a valid instrument for the corresponding exemption rate gap.

Panel A of Table 5 presents the 2SLS estimates of the effect of the exemption rate gap on war morale: a greater exemption rate for richer neighbors, relative to poorer ones, significantly undermines morale. On average, the exemption rate for richer neighbors was three percentage points higher than for their poorer neighbors. Columns 1 and 2 show that eliminating this exemption rate gap would increase the volunteer rate from 8.30% to 9.13% (a 10% increase) and decrease the draft appeal rate from 10.7% to 7.53% (a 30% decline). The effect on a composite morale index, constructed from the first principal component of the volunteering and draft appeal indicators, is also statistically significant (Column 3). Eliminating the average exemption rate gap would cause an increase of 6% of the index's standard deviation.

Panel B of Table 5 presents the reduced form estimates. A ten percentage point increase in the lottery number gap leads to a 0.92 percentage point decline in volunteering, a 3.53 percentage point increase in draft appeals, and a 0.07 standard deviation decrease in the war morale index (see Figure 9B and Figure 9C).

4.2.4 Robustness

I conduct a comprehensive series of tests to examine the robustness of the baseline results.

Controls. I examine whether the results depend on the specific set of covariates. I re-estimate the baseline model excluding the vector of controls in Equation 5. As Table A.1 shows, the 2SLS estimates remain statistically significant and quantitatively stable.

Weighting. I assess the sensitivity of the results to the spatial weighting. While the baseline specification weights neighbors by distance to capture social influence, Table A.2 demonstrates that the results hold—and the magnitudes remain comparable—when assigning equal weights to all neighbors within the bandwidth.

Alternative Radii. I examine that the findings do not depend on the specific definition of the neighborhood. I replicate the analysis using tighter 2- or 3-mile radii as well as a looser 5-mile radius, rather than the 4-mile baseline. The estimates, presented in Tables A.3–A.5, are consistent with the main results.

Nearest N Neighbors. I also consider an alternative approach to define neighbors. I use the nearest N individuals within the same draft board. Tables A.6–A.8 show the results when N is 10, 50, or 100. These estimates align with the baseline.

Record Linkage. I employ a measure based on surname-level median income (the median income for a surname in a given county). This measure does not rely on individual-level record linkage. It therefore mitigates concerns that the results arise from selection bias in the linkage process. Row 2 of Table 6 provides results for this measure.[29]

Other SES Proxies. I use alternative proxies for socioeconomic status. Row 3 of Table 6 presents results using total household income instead of personal income. I also use status signals that are more visible publicly. These include occupational rankings based on median income and the Duncan Socioeconomic Index (Rows 4—5)

Age. I address the concern that age acts as a confounder. Older registrants tend to be richer and are more likely to receive exemptions. Consequently, the exemption rate gap

[29] Table A.9 provides detailed results.

between higher- and lower-income neighbors might simply reflect an age gap. I perform two tests to rule this out. First, I adjust individual income for life-cycle earnings profiles by calculating the deviation from birth-cohort averages. The negative effect on the war morale index persists (Table 6, Row 1). Second, I conduct a placebo test using the exemption rate gap between older and younger neighbors. As reported in Table A.10, I find no significant effect. This supports the interpretation that economic status, rather than age, drives the response.

Home Value. I address measurement error in the 1940 Census income data. This is particularly relevant for farmers and business owners. Previous studies, such as Collins and Wanamaker (2022) and Abramitzky et al. (2024), impute farmer income based on the relative earnings of farmers and farm laborers in 1960, adjusted for region. I depart from this approach for two reasons. First, this imputation assigns identical income levels to all farmers within the same broad areas, thereby eliminating the local income variation within a 4-mile radius that is central to my identification strategy. Second, this method does not correct measurement errors for business owners. Instead, I utilize the house value variable reported in the 1940 Census. While imperfect, this variable serves as a useful proxy for wealth and circumvents the specific measurement limitations associated with these self-employed groups (Table 6, Row 6).

Top 1% vs. Bottom 1%. I show that the effect of the exemption rate gap on war morale remains robust, if I define the gap as the difference in exemption rates between the richest 1% and the poorest 1% of neighbors (Table 6, Row 7). This contrasts with the baseline measure, which compares richer and poorer neighbors

Neighborhood + Ancestry. I introduce an additional layer to the neighborhood network: ancestry. While individuals presumably know neighbors living within 2–4 miles, the intensity of these interactions likely varies with social distance. Ancestry is a primary determinant of this distance (Bertrand et al., 2000). To capture this, I divide neighbors into two groups. The first consists of those sharing the same ancestry, defined by the father's country of birth. The second includes neighbors with different ancestries. I then estimate the effects of the exemption rate gap separately for each group. Specifically, I examine the gap between richer and poorer neighbors of the same ancestry and compare it to the gap between neighbors of different ancestries. Table A.11 presents the results. The exemption gap among same-ancestry neighbors generates effects that align with the baseline findings. By contrast, the exemption gap among neighbors of different ancestry has no

estimated effect.

4.3 Battlefield Morale: Evidence from Desertion

I next examine how the exemption rate gap between richer and poorer neighbors affects morale on the battlefield, using desertion rates among Army soldiers from Kansas as my primary outcome. Although the average desertion rate is only 0.23%, for recruits from neighborhoods with an exemption gap in the top decile, this rate is 4.3 times higher. Figure 3C presents a positive correlation between the exemption gap in soldiers' home communities and their desertion decisions. Specifically, a ten percentage point increase in the exemption rate gap leads to a 0.87 percentage point increase in a soldier's probability of desertion (Column 2 of Table 7).[30] The reduced form, illustrated by Figure 9D, shows that a ten percentage point increase in the lottery number gap causes a 0.28 percentage point increase in the probability of desertion (Column 4 of Table 7).

Given the rare nature of the outcome (49 desertion cases), I address two specific concerns regarding outliers and linear estimation. First, a single influential observation (e.g., a deserter with a high exemption gap) could drive the estimated effect. To test for this, I conduct a leave-one-out analysis. I re-estimate the baseline 2SLS model 49 times, dropping a different deserter in each iteration. Figure A.11 plots the coefficients and 95% confidence intervals. The point estimate remains stable, and the confidence interval excludes zero in every iteration. This shows that my findings do not arise from a single influential case.

Second, linear estimators can be biased when the outcome is extremely rare. To address this, I employ the penalized maximum likelihood estimator proposed by Firth (1993). This method corrects for rare event bias in logistic regression. Table A.12 reports the Firth logit average marginal effects corresponding to the OLS, 2SLS, and reduced form specifications in Table 7. The marginal effects from the Firth specification (Column 2) are slightly more conservative, at approximately 70% of the linear baseline magnitude. However, they remain statistically significant and economically large.

4.4 External Validity

My baseline analysis focuses on Kansas, a state that in 1940 was more rural, less affluent, and more politically conservative than the median US state, though similar in moral foun-

[30] A remaining question is whether soldiers were aware of these exemption patterns before deployment or learned of them through correspondence while serving. My desertion data lack a temporal dimension, which prevents me from empirically distinguishing between these two information channels.

dations and racial demographics (Table A.13). To test the external validity of my findings, I replicate the analysis using data from Maryland, a state that contrasts with Kansas by being more urban, affluent, and politically progressive.

This replication is subject to three data limitations. First, lacking digitized exemption records, I use civilian status from the linked data between draft cards and NARA-AER (see Section 3.2.3) as a proxy for exemption. 98.4% of civilian registrants received an exemption and 93.2% of exempted registrants remained civilians throughout the war, according to the Kansas data (see Table 1). However, this proxy has measurement errors because the NARA-AER only covers Army recruits, and the record linkage results in missing individuals. Registrants would be coded as civilians in my data if they joined another branch or if their records could not be linked. Second, I can only measure Army volunteering. Third, draft appeal records from Maryland are unavailable for digitization.

Despite these limitations, Table 8 shows qualitatively similar results to my analysis in Kansas. A higher civilian rate among a registrant's richer neighbors causes lower rates of volunteering and higher rates of desertion.

5. Mechanism: Perceived Unfairness

Motivated by the sentiment of the American mother quoted in Section 1, I hypothesize that perceived unfairness is the primary mechanism driving the negative reaction to the exemption gap. To validate this mechanism, I provide evidence from three sources, moving from granular behavioral heterogeneity to corroborating evidence from local newspapers and national public polls. First, I document behavioral heterogeneity that aligns with fairness concerns: the negative morale effects are driven by likely questionable exemptions and amplified in regions with fairness as a moral foundation. Second, I analyze local newspapers to establish that in counties with lower service rates among the rich, local reporting contained more content characterizing the draft as unfair. Third, I use nationwide Gallup surveys to provide suggestive evidence that these patterns extend to broader dissatisfaction with the draft's fairness. Finally, I develop a conceptual framework in Appendix D that guides me to empirically rule out alternative mechanisms.

Before presenting the evidence, a conceptual point warrants clarification: how can a procedurally "fair" instrument, the draft lottery, identify a causal channel rooted in perceived unfairness? The resolution lies in distinguishing between the fairness of the identification variation and the perceived fairness of the outcomes it isolates. To the average citizen in the 1940s, the intricate workings of the draft lottery were invisible; what they observed was its tangible outcome: which local boys went to war and which did not. As

the mother's letter illustrates, judgments were based on this stark pattern of outcomes, not on an abstract procedural logic.

When a random allocation of "more time to prepare a case" disproportionately benefits those with more resources, the resulting exemptions do not appear as a product of luck. Instead, they reinforce a perception of a system that favors the rich. This observation aligns with research showing that judgments of fairness are often based on salient outcomes rather than complex processes (Cappelen et al., 2013; Andreoni et al., 2020), particularly when the chance interacts with underlying social structures (Taleb, 2001). Therefore, my research design does not assume the lottery itself is seen as unfair. Rather, it leverages the lottery's random assignment to isolate the causal impact of skewed outcomes that, in the eyes of contemporaries, are a demoralizing sign of an unfair social contract.

5.1 Heterogeneity Analysis Aligning with Perceived Unfairness

Ideally, to establish the mechanism, one would link the specific exemption gap faced by an individual to his own perception of fairness. However, individual-level data on fairness perceptions are unavailable. I therefore infer the mechanism from the granular Kansas data by testing for three forms of behavioral heterogeneity that align with the perceived unfairness interpretation. First, the reason for an exemption is critical: likely questionable exemptions have a fivefold larger negative effect on morale than justifiable ones. Second, the moral foundation of fairness moderates the impact of the exemption rate gap on war morale. Importantly, this heterogeneity does not extend to other moral foundations. Third, the principle of fair sacrifice extends beyond service status. The negative impact of the exemption gap is larger when richer neighbors who do serve are nevertheless concentrated in safer military branches.

Heterogeneity Analysis 1 (Questionable vs. Justifiable Exemptions)**.** *The negative effect of the exemption gap on an individual's war morale is more pronounced when the gap is driven by questionable exemptions compared to justifiable ones.*[31]

The perceived unfairness mechanism implies that the reason behind an exemption matters: individuals should perceive a justifiable exemption as fairer than a questionable one. Because I cannot observe exemption rationales directly, I proxy for public percep-

[31] A natural question is how individuals could distinguish between justifiable and questionable exemptions. I argue that the primary criteria for exemptions—such as marital status, number of children, occupation, and health conditions—are often highly visible to neighbors. Individuals in my sample likely possessed more granular, local knowledge of their neighbors' circumstances than I can observe in the data.

tion using pre-lottery characteristics. I assume an exemption is likely perceived as "justifiable" if it aligns with the recipient's *publicly known, pre-lottery* status (e.g., a known father of three), and "questionable" if it contradicts these pre-lottery characteristics (e.g., a man known to be single who secures a marriage exemption).

To implement this, I estimate the probability of exemption, $P(Exempt_i|X_i)$, using a LASSO model on variables from the 1940 census—"public profile" visible to neighbors before the draft. From 578 expanded features (including demographics, occupation, and health metrics), the LASSO selects 14 primary predictors (Table A.14).

I define an exemption as *likely* questionable if the individual receives it despite a *relatively* low predicted probability compared to other exempted men. Operationally, I use a median split on the LASSO residuals among the exempted population: an exemption is likely questionable if $\epsilon_i \geq \text{Median}(\epsilon|Exempt = 1)$. This relative ranking distinguishes between *more* and *less* surprising exemptions, *rather than perfectly separating draft dodgers from legitimate cases*. Exemptions falling below the median residual are classified as likely justifiable.

This relative measure captures the strategic "draft dodging" behaviors discussed in Section 4.2.2. For example, a man observed as single in 1940 has a low predicted probability of exemption; if he marries in 1941 to secure an exemption, he becomes an exempted individual with a high residual. Validating this classification, Figure A.12 confirms that individuals with likely questionable exemptions show stronger patterns of strategic adjustment. Conditional on pre-war status, they are more likely to have married, fathered children, reduced household labor supply, or switched occupations by 1950.

To test if likely questionable exemptions have a stronger effect, I adapt the baseline 2SLS model (Table 5, Panel A, Column 3). Figure 10 shows that the point estimate for the likely questionable exemption gap is five times larger than for the justifiable gap (p-value: 0.001), confirming its more pronounced negative impact on war morale.

Heterogeneity Analysis 2 (Fairness vs. Other Moral Foundations)**.** *The negative effect of the exemption gap on an individual's war morale is more pronounced for those with fairness as a moral foundation.*

The Moral Foundations Theory categorizes the human moral systems into five domains: care, fairness, loyalty, authority, and purity (Haidt, 2012). If perceived unfairness drives the negative effect of the exemption gap on war morale, this effect should be more pronounced in regions where fairness is a more salient moral value. I therefore hypothesize that the draft exemption gap has a heterogeneous effect on war morale that is unique to the fairness foundation and not present for the other four.

To test this hypothesis, I construct a county-level measure for the prevalence of each moral foundation. I apply the Moral Foundations Dictionary to a corpus of pre-1940 Kansas newspapers from the "American Stories" dataset (Frimer et al., 2019; Enke, 2020; Dell et al., 2023). This process yields continuous scores for each of the 42 Kansas draft boards in 29 counties, representing the standardized frequency of words associated with each moral foundation.

I then test whether the effect of the draft exemption gap on war morale is uniquely moderated by fairness. First, I estimate the baseline specification (Column 3 of Table 5, Panel A) separately for each draft board to obtain board-specific treatment effects. Second, I examine how these board-specific treatment effects vary with moral foundation scores by regressing these effect estimates on each score at the board level.

Figure 11 presents the estimate of each moral foundation score with their 90% confidence intervals. The coefficient for fairness is negative and statistically significant. This means that in counties with strong fairness foundations, the exemption rate gap has a significantly more negative impact on war morale. In contrast, the coefficients for the other four moral foundations are statistically insignificant. Formal tests confirm this pattern: the coefficient for fairness is statistically different from the average of the other four (p-value: 0.028), while a joint test of equality for the other four coefficients cannot be rejected (p-value: 0.58). This evidence suggests that fairness is the unique moral foundation, among the five tested, that moderates the effect of the exemption gap on war morale.

Heterogeneity Analysis 3 (Dangerous vs. Safe Service)**.** *The negative effect of the exemption gap on an individual's war morale is more pronounced when richer neighbors serve in safer branches.*

Beyond the fairness concerns raised by unequal service status, a second dimension of fairness involves the distribution of risk conditional on service. During WWII, service branches differed in their casualty risks. For instance, the Army and Marines had fatality rates more than twice as high and injury rates seven times higher than the relatively safer Navy and Coast Guard (The National WWII Museum, 2024). If the perceived fairness of the draft system drives the negative effect of the exemption gap on war morale, this effect should be exacerbated when richer neighbors who do serve are disproportionately placed in safer branches.

To test this hypothesis, I calculate the average mortality burden of richer neighbors by assigning branch-level mortality for servicemen (Marine 5.2%, Army 3.8%, Navy 1.9%, Coast Guard 1.1%) and setting zero for civilians (The National WWII Museum, 2024). I then discretize this continuous measure at its median. This splits the sample into two

groups within the cells of draft board and deciles of richer and poorer neighbors' exemption rates: one where the average mortality burden of richer neighbors is below the median and another where it is above the median. I re-estimate my main 2SLS model, as specified in Column 3, Panel A of Table 5, separately for each group. This approach allows for a direct comparison of the exemption rate gap's effect on war morale under different conditions of perceived risk-sharing.

Figure 12 plots the 2SLS estimates and 90% confidence intervals for both groups. The results support the fairness mechanism. The estimated effect of the exemption gap is 1.8 times as large for individuals whose richer neighbors' average mortality burden is below the median compared to those whose richer neighbors have an above-median mortality burden (p-value: 0.001). This finding suggests that the adverse impact of the exemption gap on war morale is amplified when coupled with the perception that the rich, even when they serve, are shielded from the most dangerous duties.

Frontier Culture. The negative effect of the exemption gap on war morale may be more pronounced in communities with a tradition of skepticism toward state authority. The American frontier experience fostered "rugged individualism" and deep-seated resistance to government intervention. If the exemption gap is viewed as a failure of state-administered justice, this perceived unfairness should trigger a stronger backlash in areas where skepticism of the state is culturally embedded. To test this, I use county-level data on Total Frontier Experience (TFE) from Bazzi et al. (2020), which measures the number of decades a county remained on the frontier. I estimate the heterogeneous effects of the baseline specification (Column 3, Panel A of Table 5) by TFE tertiles. Figure 13 demonstrates that in counties within the highest tertile of frontier experience, the magnitude of the negative treatment effect is 2.3 times larger than in the lowest tertile ($p < 0.01$).

5.2 Evidence from Local Newspapers

While the heterogeneous patterns above are consistent with fairness concerns, they do not directly measure perceptions. Local newspapers provide a complementary window into how communities perceived draft inequities. I hypothesize that in counties where service rates for the rich were relatively lower, local reporting contained more content characterizing the draft as unfair.

I collect 11,281 pages of WWII-era local newspapers discussing the draft from newspapers.com, covering 195 counties.[32] To quantify the sentiment of these texts, I employ

[32]To the best of my knowledge, I am the first to utilize the *complete text corpus* of this website, rather than

a Large Language Model (LLM), GPT-4.1 mini, to perform a classification task. Specifically, I prompt the model to adopt the persona of a 20-year-old US male in the year of publication—the demographic most directly affected by the draft. The model evaluates whether each page makes a reader "more likely to think the military draft is being handled fairly or unfairly."

The sentiment is coded as 1 ("more fair"), 0 ("no change"), or –1 ("more unfair"). Approximately 14.4% of pages are classified as "more fair" and 9.4% as "more unfair" (Figure C.1A). Table C.1 displays examples from each category. To ensure the robustness of the classification, I validate the LLM outputs against human coding and an alternative model (Claude Haiku 4.5) for a subsample of pages. Appendix C provides more details from newspaper data collection, classification, and validation.

I then average these scores to the county level to construct a fairness perception index. It represents the net difference between the proportion of "fair" pages and "unfair" pages.

I measure the publication location of local newspapers at the county level. Restricting the analysis to Kansas yields a limited number of counties.[33] I therefore use lottery number data from Chen (2025), which cover 16 states.[34] I link these data to the 1940 Census and NARA-AER to construct a dataset of potential service members. This dataset includes information on lottery numbers, income, and Army service status.[35] I aggregate the data to the county level to compute two metrics: the civilian rate gap between the rich and the poor, and the corresponding lottery number gap. The civilian rate gap is the difference in the share of individuals not recruited into the Army between the top and bottom income deciles within a county.[36] The lottery number gap is the difference in average lottery numbers between these groups.

The final sample comprises 194 counties with data on the civilian rate gap, the lottery number gap, and newspaper-based fairness perception. Table 9 reports the results. If the civilian rate of the rich is one percentage point higher relative to that of the poor, the sentiment balance in newspapers shifts from neutral toward "unfair" by a margin of 3.6 percentage points (Column 2). The rk-LM F-statistic from Kleibergen and Paap (2006) in the first stage is 20.99, above the conventional threshold. The lottery number gap is uncorrelated with county-level observables (see Figure A.13).

A marginal change in the level of the lottery number or civilian rate gap may not be

simple keyword counts. Details regarding the data extraction are provided in Appendix C.

[33]The number of counties in Kansas is 105, but newspapers.com does not archive newspapers for each county.

[34]These states are AL, AZ, CO, DE, FL, ID, MD, MN, MO, NE, NM, OR, PA, UT, VA, and WY.

[35]A key limitation of the NARA-AER data is that they cover only Army service.

[36]I use civilian status as a proxy for exemption. See Section 4.4 for a discussion of its practicality and limitations.

immediately salient or generate meaningful changes in newspapers' coverage of draft fairness. For instance, a 0.01-percentage-point change in either gap may not be easily perceived. To address this concern, I also use within-state deciles of the civilian rate and lottery number gaps. The results indicate that a one-decile increase in the civilian rate gap leads to a 7.7-percentage-point decline in fairness perception (Column 6, Table 9).

5.3 Evidence from Gallup Surveys

The heterogeneity and newspaper analyses support that fairness concerns drove the negative effect of higher exemption rates of richer neighbors on war morale. However, a question of external validity remains: was this phenomenon unique to specific local contexts, or did it reflect a broader national sentiment? To explore this, I analyze nationwide WWII-era Gallup polls

Three Gallup polls directly surveyed a representative sample of Americans on their views of draft fairness.[37] Each survey asks: "Do you think the draft is being handled fairly in your community?" A negative response indicates a perception that the draft is unfair. This dataset, which I refer to as the "perception data," provides individual-level information on fairness perceptions and state of residence. I also aggregate these data to the state level to calculate the share of respondents perceiving the draft as unfair.

I aggregate the same dataset of potential service members described in Section 5.2 to the state level to compute the civilian rate gap between the rich and poor—the difference in the share of individuals not recruited into the Army between the top and bottom income deciles—and the corresponding gap in average lottery numbers.

Figure A.14 shows a positive correlation between the civilian rate gap and the share of respondents perceiving the draft as unfair. I discretize both variables by their medians and plot the resulting binary indicators on stylized maps of US states. Among the eight states with an above-median civilian rate gap, three-quarters have an above-median share of respondents perceiving the draft as unfair.

To account for potential confounders, I analyze the respondent-level "perception data." The outcome is an indicator for a respondent's perception of unfairness. The key explanatory variable is the civilian rate gap between the rich and poor in respondents' state, and the instrument is the corresponding lottery number gap. I employ a two-step estimation strategy to strengthen the instrumental variable design by leveraging the rich respondent-level data. First, I use one-to-one nearest neighbor propensity score matching to create a

[37] These surveys are USOPOR1943-012, USAIPO1943-0308, and USAIPO1945-0348 from the Roper Center for Public Opinion Research.

sample where individual characteristics (race, gender, education, urban status, income, birth year, and survey date) are balanced between states with above- versus below-median values of the instrument. While not necessary for the validity of the 2SLS estimator, this step can increase the precision of the estimate. Second, I run the 2SLS regression on this matched, balanced sample. Standard errors are clustered at the state level.

Column 2 of Table A.15 presents the 2SLS estimate: a one percentage point increase in the civilian rate gap increases the probability of respondents perceiving the draft as unfair by 1.6 percentage points. The lottery number gap is uncorrelated with state-level observables (see Figure A.15). However, the variation in the state-level instrument is naturally smaller than in the Kansas analysis: the instrument's standard deviation is over four times larger at the neighborhood level than at the state level (see Figure A.16). This is because random deviation from the law of large numbers attenuates with aggregation. Due to the limited variation across 16 states, the first stage suffers from weak instrument concerns. The rk-LM F-statistic from Kleibergen and Paap (2006) is 3.31, below the conventional threshold of 10. To account for the weak instrument concerns, I also conduct a weak-instrument-robust Anderson-Rubin test and the p-value of the 2SLS estimate on the civilian rate gap is 0.052.

Deciles, not Levels. Because variation across states is smaller, small differences—such as one percentage point—in the level of the civilian rate gap or lottery number gap may not be visible and hence are unlikely to affect fairness perceptions. To test whether my results are sensitive to this concern, I use decile ranks of the endogenous variable and the instrument. In other words, instead of comparing individuals from two states with different levels, I compare them from states with different deciles. Column 6 of Table A.15 presents the 2SLS estimate: a one-decile increase in the civilian rate gap raises the probability of respondents perceiving the draft as unfair by 2.5 percentage points.

Size of OLS vs. 2SLS Estimates. In Table A.15, the 2SLS estimate of the levels specification is 5.6 times larger than its OLS counterpart. The larger 2SLS estimate is consistent with the presence of attenuation bias in the OLS coefficient. This bias likely arises from a measurement error, as the civilian rate gap—based only on Army records—is an imperfect proxy for the true gap in military service across all branches. Such measurement error would bias the OLS estimate toward zero. As Figure A.17 illustrates, plausible levels of measurement error could fully account for this difference. The 2SLS estimate, by contrast, uses the precisely measured lottery number gap as an instrument to correct for this attenuation bias, thereby providing a more accurate estimate of the causal effect.

5.4 Ruling out Alternative Mechanisms

While the evidence from newspapers, behavioral heterogeneity, and Gallup surveys converges on perceived unfairness as the driver, several plausible alternative mechanisms could also explain how the exemption rate gap affects war morale (see Figure A.18). To disentangle these potential channels, I develop a conceptual framework in Appendix D. This framework outlines five incentives that affect an individual's war contribution. The first two are economic: (a) a labor supply effect and (b) a labor demand effect. The remaining three are psycho-social: (c) perceived unfairness, (d) a leadership effect, and (e) racism. While these channels can generate similar aggregate relationships, they produce distinct, testable predictions, which I summarize in Table D.1. I rule out alternative explanations by demonstrating that their specific predictions are inconsistent with the data.

5.4.1 Labor Supply Effect

The labor supply explanation posits that high service rates (i.e., low exemption rates) among poorer individuals would reduce the local labor supply, thereby increasing civilian wages and the opportunity cost of military service, which in turn lowers war morale. A key testable implication of this channel is that a higher exemption rate among poorer neighbors (i.e., lower service rates) would increase war morale, implying a positive coefficient on their exemption rate.

To test this, I modify my baseline 2SLS model (specification from Column 3 of Table 5, Panel A) to include the exemption rates of richer and poorer neighbors as two distinct endogenous regressors, each instrumented by their corresponding average lottery numbers. Figure 14A plots the resulting coefficient estimates with their 90% confidence intervals.

The findings do not support the labor supply explanation. The coefficient for the exemption rate of poorer neighbors is positive and statistically insignificant. In contrast, the effect of the exemption rate of richer neighbors remains negative and statistically significant. Furthermore, a formal test rejects the equality of the two coefficients (p-value < 0.001). This finding allows me to rule out the labor supply channel as the primary driver of my results.

5.4.2 Outcome-Based Explanations

I next consider three alternative channels that are outcome-based. These include: (i) a labor demand effect, where higher service (i.e., lower exemption) for rich employers re-

duces labor demand and the opportunity cost of service; (ii) a leadership effect, where high service rates among the rich inspire others to serve through emulation; (iii) a networking motive, where individuals join the military to build social networks with richer neighbors.[38] If richer individuals received more service exemptions, the incentive to volunteer for networking purposes would diminish.

I provide two pieces of evidence to rule out these outcome-based explanations. First, the key distinction is that my proposed "perceived unfairness" mechanism is reason-based, whereas these three alternatives are outcome-based. For these outcome-based channels, the critical event is the absence of a richer neighbor from service; the reason for that absence is irrelevant. This logic implies that justifiable and questionable exemptions for richer neighbors should have similar impacts on war morale.

To test this prediction, I use the same method from Heterogeneity Analysis 1 to distinguish likely justifiable and questionable exemptions. I adapt my baseline model (specification from Column 3 of Table 5, Panel A) by estimating two separate 2SLS models for the exemption rates of richer neighbors: one for likely questionable exemptions and another for justifiable exemptions.

Figure 14B presents the results. The estimated effect of likely questionable exemptions is negative and substantially larger in magnitude than the effect of justifiable exemptions (p-value<0.001). This stark asymmetry contradicts the predictions of those outcome-based hypotheses, lending further support to my reason-based, perceived unfairness mechanism.

Second, I specifically address the labor demand channel. A key prediction of this channel is that the negative relationship between the exemption rate gap and war morale should be stronger in neighborhoods where more richer neighbors are employers. If rich exemptions reduce volunteering by sustaining high local labor demand, this effect should be most pronounced where richer neighbors are business owners rather than rentiers or professionals. To test this, I split the sample by the median share of business owners among richer neighbors and re-estimate the baseline specification (Column 3 of Table 5, Panel A). As shown in Figure 15, I find no significant heterogeneity in the treatment effect (p-value > 0.1). The fact that the effect of exemption rate gap does not vary with the share of local business owners provides further evidence against the labor demand explanation.

Finally, the networking motive is inconsistent with the structure of the US military during WWII. The military was a highly bureaucratized institution in which new recruits had no control over their unit assignments (Flynn, 1993). Consequently, they could not strate-

[38] Alternatively, the presence of exempted richer neighbors creates a valuable opportunity for others to stay home and build connections, particularly when other men have left for the war.

gically choose to serve alongside specific individuals, rendering the networking motive an implausible explanation for volunteering.

5.4.3 Racism

I test an alternative explanation centered on racial prejudice, which posits that non-Black individuals are demotivated to serve if a high number of Black individuals serve and military service becomes increasingly associated with Blackness. This would occur if low exemption rates among poorer neighbors—a group that could be predominantly Black in some areas—led to higher conscription rates for Black individuals.

This hypothesis yields two testable predictions, neither of which is supported by the evidence. First, this mechanism implies that the low exemption of poorer, not richer, neighbors should drive the effect on war morale. However, as shown in Figure 14A, my results are driven entirely by the exemption rate of richer neighbors.

Second, the racism hypothesis predicts that the negative effect of the exemption gap on morale should be magnified in areas with a larger Black population or higher levels of racism. To test this, I re-estimate my main 2SLS model on subsamples split by the median of two variables: the share of Black neighbors and a county-level racism index.[39]

Figures 16A and 16B show that the estimated effect of the exemption gap is remarkably stable across these subsamples. The point estimates for below- and above-median groups are nearly identical. Formal tests for a difference in coefficients fail to reject the null hypothesis of equal effects across groups defined by the share of Black neighbors and by the level of racism (p-value > 0.1). The absence of such heterogeneity contradicts a core prediction of the racism-based explanation.

5.4.4 Other Alternatives

I also consider other alternative explanations that my conceptual framework does not cover.

Mechanical Relationship. A potential concern is that my results are driven by a mechanical relationship. Specifically, a high service rate (i.e., low exemption) among the poor mechanically reduces the pool of potential volunteers. If this were the primary channel, we would expect a positive coefficient on the exemption rate of poorer neighbors when

[39] Following Qian and Tabellini (forthcoming), I measure county-level racism by constructing the first principal component from historical voting patterns (e.g. Democratic vote share, 1900–1940), lynching counts, KKK presence, Black residential segregation (1930–1940), Confederate monument counts, and the White-Black education gap (1940).

the outcome is volunteering. While this channel is plausible, it is not consistent with my findings that the effect is driven by the higher exemption rate of richer neighbors (Figure 14A). Furthermore, this explanation is specific to volunteering and struggles to account for the effects on draft appeals and combat desertion. The persistence of the effect onto the battlefield suggests a psychological mechanism beyond a simple compositional story.

Social Image. An alternative explanation centers on social image concerns, where the prestige of military service is derived from the participation of high-status individuals. From this perspective, high exemption rates among richer neighbors would devalue the social honor of military service, making it a less attractive path for gaining honor. However, this explanation is difficult to reconcile with my finding that the justification for an exemption is critical (see Figure 10 and Figure 14B). The social image mechanism predicts that the absence of a higher-status individual diminishes prestige regardless of the reason. However, I find that questionable exemptions have a greater negative impact on morale than justifiable ones. This suggests that the key driver is not a loss of social honor, but rather a perceived violation of fair burden-sharing.

Mainstreaming Draft Dodging by the Rich. A competing interpretation is the erosion of social norms (Bursztyn et al., 2020). Under this view, high exemption rates of higher-income neighbors signal that draft avoidance is "mainstream" rather than "stigmatized," leading high-income individuals to update their priors regarding social sanctions. If this logic held, the negative treatment effect should be stronger among higher-income individuals. However, Figure 17 shows the opposite: the effect is stronger for lower-income individuals.

Religion. An alternative explanation is that the results are driven by draft exemptions based on religious affiliation, particularly among rich Mennonite communities in Kansas known for their pacifism. However, historical evidence indicates that rather than avoiding contribution entirely like previous wars, these groups channeled their service into specific alternatives. Following the Selective Training and Service Act, Mennonites joined other peace churches to establish 151 Civilian Public Service camps, where 4,665 Mennonites served in forestry, conservation, and public health, while an equal number entered noncombatant military posts (Bailes, 1959; Juhnke, 2010). To empirically test if these communities drive the results, I compare the baseline 2SLS estimate against a subsample excluding counties with concentrated Mennonite populations (Butler, Harvey, Marion,

McPherson, and Reno; see Figure A.19). I find no difference between these estimates (see Figure A.20), suggesting that the erosion of war morale was not driven by religious conscientious objection, but rather by class-based inequities.

National Origin. An alternative explanation is that the results are driven by draft exemptions related to national origin. Specifically, individuals of German or Italian descent might have been classified as "enemy aliens" and interned, thus receiving exemptions. If the observed exemption gap primarily reflected security-based exclusions rather than economic privilege, one would expect the negative impact of the exemption gap on war morale to be distinct in areas with larger populations of Axis origin. However, I find no evidence of differential treatment effects when interacting the exemption rate gap with the county-level share of the population born in Germany, Italy, or the Axis powers (see Table A.16).

Learning. According to this explanation, individuals observe their richer neighbors securing exemptions and learn the specific strategies to do so themselves. This acquired knowledge, rather than a sense of unfairness, could lead them to volunteer less and appeal more. However, this explanation is inconsistent with several key features of my results. The "learning-to-dodge-the-draft" model struggles to explain the increased combat desertion of soldiers who have already been drafted, as knowledge of how to secure a draft exemption is no longer actionable for them. Furthermore, if the mechanism were simple information transmission, the socioeconomic status of the neighbor providing the "lesson" should not matter, yet my effect is driven specifically by the exemptions of richer neighbors. These facts suggest that the mechanism is not one of neutral information transfer, but a normative reaction to perceived class-based unfairness.

Envy. An alternative explanation is pure envy, where individuals resent the privileged outcomes of their richer neighbors. However, envy is outcome-based: it is triggered by the realization of another person's advantage, regardless of how it is obtained. If envy were the primary driver, any exemption granted to a richer neighbor would depress morale equally. My findings contradict this prediction: Figure 14B shows that likely questionable exemptions exert a more negative impact on morale than justifiable ones. This asymmetry indicates that the response stems from a normative judgment about the legitimacy of the exemption—a violation of fair burden-sharing—rather than mere covetousness of civilian safety.

Negative Reciprocity. An alternative explanation is negative reciprocity, where individuals withdraw cooperation to punish misbehaving authority figures. However, in a centralized national mobilization, an individual's refusal to volunteer or decision to desert does not directly punish exempted elites; rather, it penalizes the state. Furthermore, the behavioral response is uniquely moderated by the moral foundation of fairness, not authority (see Figure 11).

6. Suggestive Evidence: Spillover Effects on Taxing the Rich

Scheve and Stasavage (2016) argue that the widespread acceptance of highly progressive income taxes after WWII stemmed from a compensatory argument: the greater sacrifices made by the poor during the war justified heavier taxation of the rich to maintain a sense of fairness. To support this claim, they use Gallup surveys that asked respondents to design a tax schedule by stating the income tax rate that a "typical family of four" should pay at various income levels. The presented income levels—$1,000, $2,000, $5,000, and $50,000—correspond to the 56th, 81st, 98th, and 99.99th percentiles of the 1940 income distribution.[40] Although they show that preferred tax schedules became more progressive after the U.S. formally entered the war, their analysis does not directly link these preferences to inequities in military mobilization.

I bridge this gap by examining whether unfairness in military mobilization correlates with public support for taxing the rich. I measure draft unfairness using the state-level gap in the civilian rate between the rich and the poor. Figure 18 plots the median preferred tax rates for respondents from states with above- and below-median civilian rate gaps. While preferences for the two lowest income brackets ($1,000 and $2,000) are nearly identical across states, they diverge sharply for the highest income tier. At the $50,000 income level, the preferred tax rate in high-gap states is 30 percent, a full 10 percentage points higher than in low-gap states. Figure A.21 shows a similar pattern when sorting states by the proportion of the population that perceived the draft as unfair.

To account for potential confounders, I analyze a matched respondent-level dataset. Following the strategy in Section 5.3, I regress a respondent's preferred tax rate on their state's civilian rate gap, instrumenting the gap with the state-level lottery number gap. Consistent with the graphical evidence, the 2SLS estimates show no statistically significant effect on preferred tax rates for the three lowest income levels, but a large, significant effect for the $50,000 bracket.

While these state-level results should be interpreted as suggestive rather than strictly

[40] See Gallup Poll 1942-0263.

causal due to the weak first stage, they present a compelling narrative. Exposure to draft unfairness appears to foster a targeted demand for a more progressive tax system, indicating a desire to restore the fairness of the social contract through fiscal policy.

7. Conclusion

This paper examines how conscription exemptions favoring the rich undermined U.S. war morale during WWII. By digitizing individual-level draft records, I show that when richer neighbors received more exemptions, individuals volunteered less, filed more draft appeals, and deserted more often. I identify the causal effect using an instrument based on randomly assigned lottery numbers, whose sequential processing created an advantage in securing exemptions. The core mechanism is perceived unfairness. When people saw their richer neighbors receiving more exemptions, they perceived the system as unfair. This perceived unfairness undermined their willingness to contribute to the war. Furthermore, I find suggestive evidence that less service by the rich increased public demand for progressive taxation. This suggests that a failure of the social contract in one domain can lead citizens to demand its renegotiation in another.

Historical evidence indicates that the U.S. government did not intentionally design a conscription system to favor the rich. Instead, they faced a trade-off between fairness and efficiency. The state's need to grant exemptions for wartime production (efficiency) interacted with pre-existing inequality. This allowed the rich to more easily avoid service and created a perception of unfairness.

Perceptions of unfair conscription likely intertwined with a belief that the institution was flawed or that some community members were opportunistic. Such perceptions could, in turn, erode political trust or interpersonal trust. The March 1942 Gallup survey shows that the perception that the rich took "unfair advantage of the war" was negatively correlated with both trust in government and trust in others (see Table A.18 for details). However, a thorough exploration to distinguish the effect of a policy failure on both types of trust would require a setting with better trust data.

My findings challenge the popular "Greatest Generation" narrative. Popularized by Tom Brokaw (1998)'s bestseller, *The Greatest Generation*, it portrays Americans of the WWII era as exceptionally patriotic. Anecdotal comparisons—such as Presidents George H.W. Bush and John Kennedy volunteering for the Navy versus Presidents Bill Clinton and Donald Trump controversially avoiding the Vietnam draft—appear to bolster this narrative. However, using systematic, original archives, I show that the narrative of unconditional patriotism is not empirically supported. This aligns with the broader literature on nar-

ratives, which highlights that narratives are not always factually accurate (Shiller, 2019; Eliaz and Spiegler, 2020; Bursztyn et al., 2022). It would be a fruitful area for future research to explore when and how the "Greatest Generation" narrative formed and why these unfair elements of military mobilization have been collectively forgotten.

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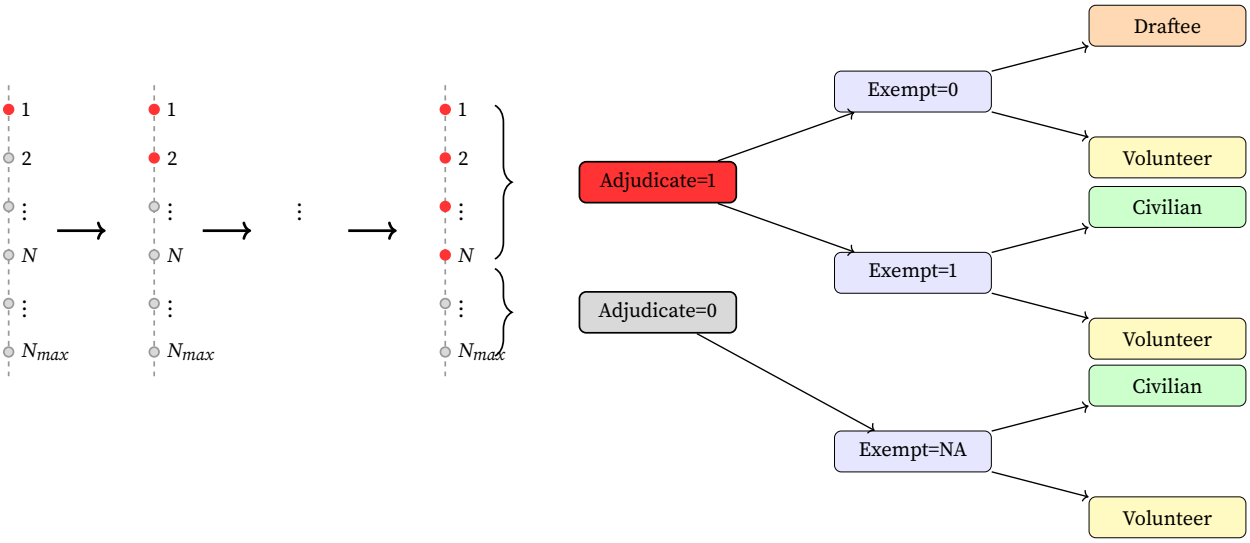
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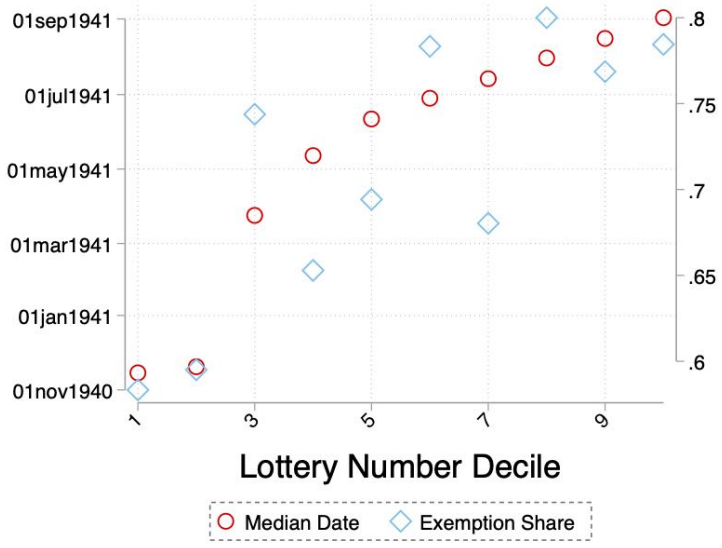
Figures

Figure 1: Adjudication Process



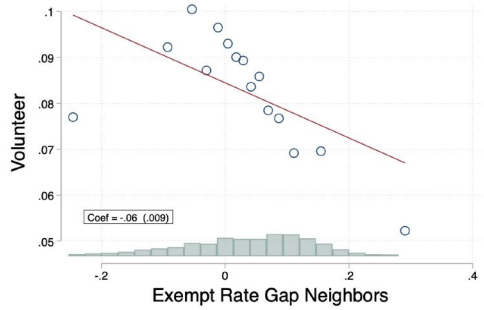
Notes: The numbers are lottery-generated order numbers. The red (gray) dots denote adjudicated (unadjudicated) individuals. N is the current number reached. Individuals with numbers below N are assessed for exemption. N_{max} is the maximum number among all registrants in a board. 98.9% of individuals were adjudicated in Kansas during WWII (see Table 1).

Figure 2: Higher lottery numbers were adjudicated later and received more exemptions

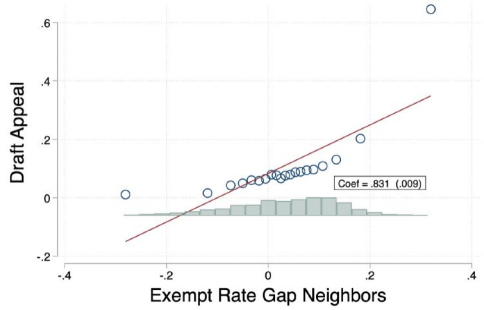


Notes: Binscatter plots of adjudication date (measured by the date the questionnaire was returned) (left y-axis), exemption rate (right y-axis), and lottery number decile (x-axis) in Pratt, Kansas.

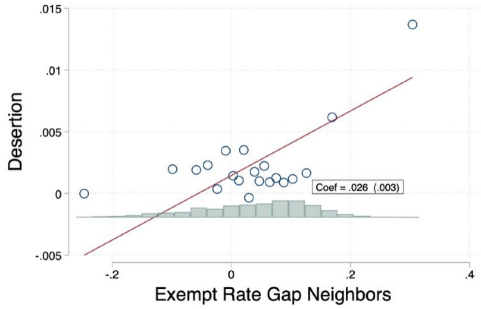
Figure 3: War Morale vs. Draft Exemption Rate Gap



(A) Volunteer vs. Exemption Rate Gap



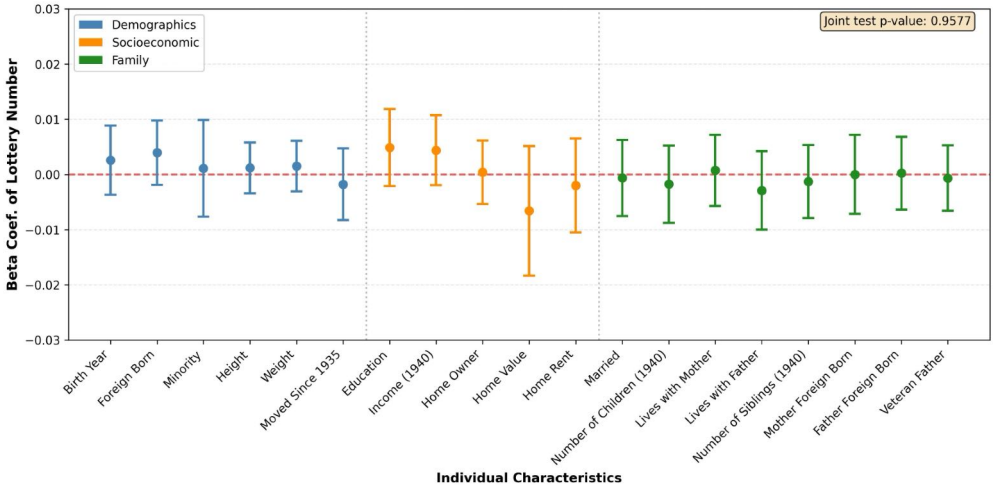
(B) Draft Appeal vs. Exemption Rate Gap



(C) Desertion vs. Exemption Rate Gap

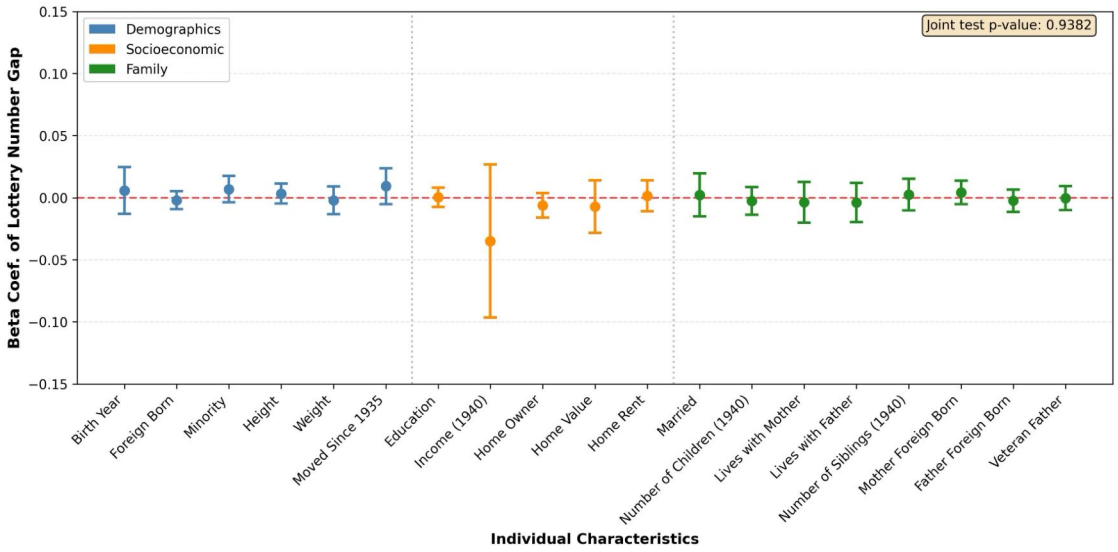
Notes: Binscatter plots. The horizontal axis is the draft exemption rate gap between richer and poorer neighbors. The vertical axis is the volunteer indicator in A, the draft appeal filing indicator in B, and the desertion indicator for servicemen in C. Histograms for x-axis variables are also plotted. OLS regression coefficients and their standard errors are reported. I adjust for draft board fixed effects by regressing each variable on the fixed effects and adding the sample mean back to the resulting residuals.

Figure 4: Random Assignment of Lottery Numbers

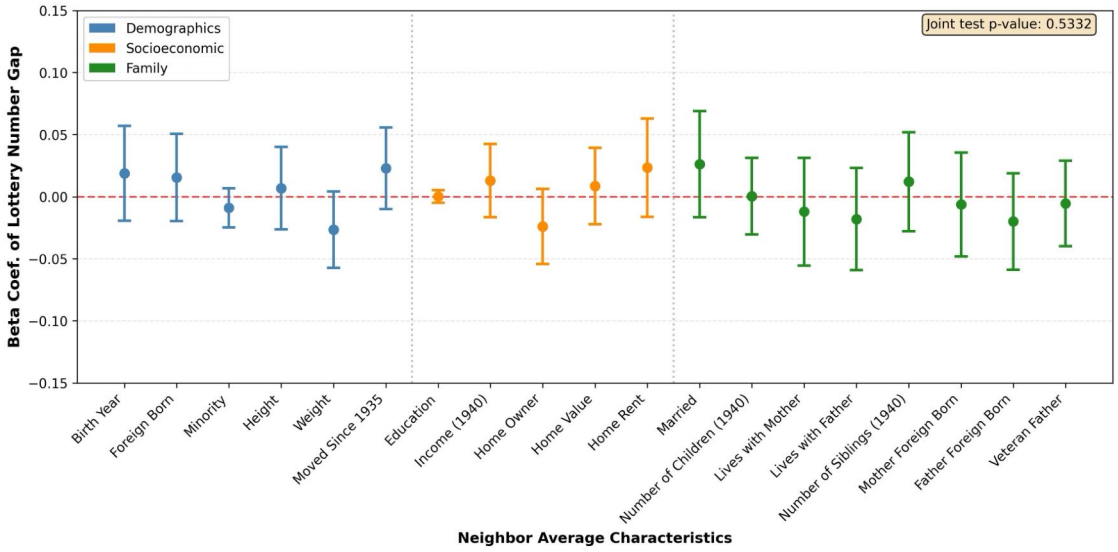


Notes: This figure presents tests for the random assignment of lottery number at the individual level. Each point represents the standardized beta coefficient from regressing a characteristic on the individual's lottery number, with draft board fixed effects and clustered standard errors; the whiskers show 95% confidence intervals. The 19 characteristics are grouped by color into three categories: demographics, socioeconomic status, and family characteristics. The joint test p-value indicates whether the lottery number is jointly significant across all characteristics.

Figure 5: Balance Tests: Lottery Number Gap Neighbors



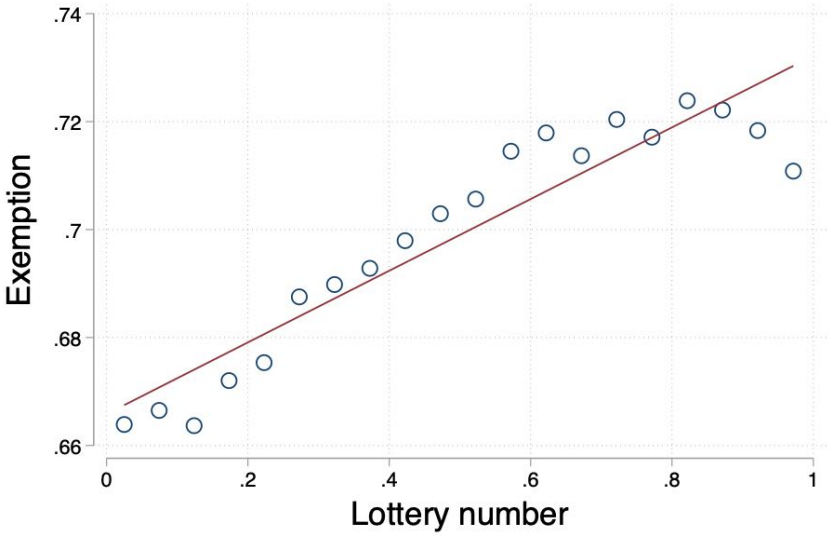
(A) Own Characteristics as Outcomes



(B) Neighbors' Characteristics as Outcomes

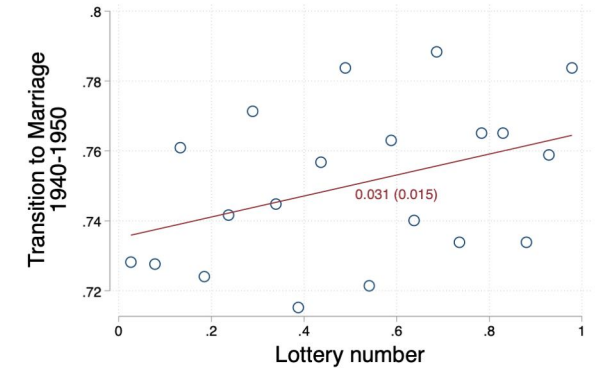
Notes: Each point represents the standardized beta coefficient from regressing a characteristic on the lottery number gap between a salient person's richer and poorer neighbors, with draft board fixed effects and clustered standard errors; whiskers show 95% confidence intervals. In Panel A, the dependent variables are characteristics of the salient person; in Panel B, the dependent variables are averages of neighbors' characteristics. The joint test *p*-value indicates whether the lottery number is jointly significant across all characteristics.

Figure 6: Higher lottery numbers received more exemptions

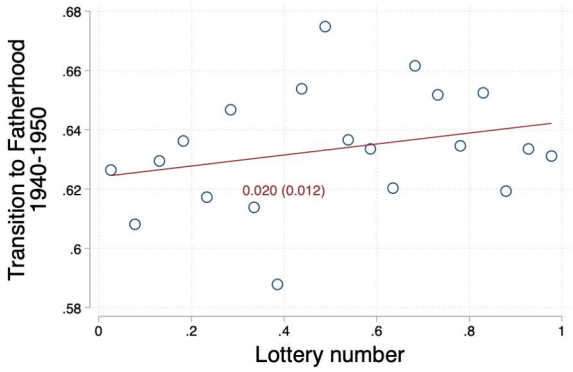


Notes: Binscatter plot. *y*-axis: the indicator of receiving a draft exemption; *x*-axis: lottery number. The solid line represents the linear best fit.

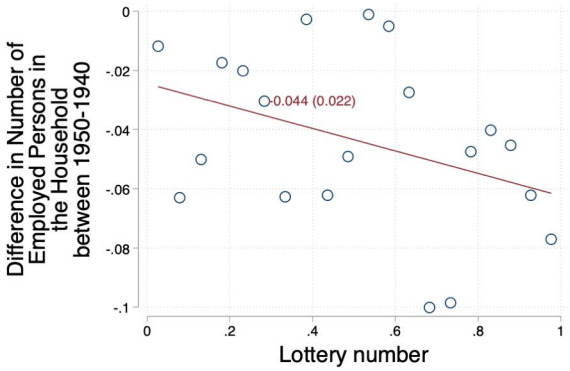
Figure 7: Behavioral Adjustments Associated with the Temporal Advantage



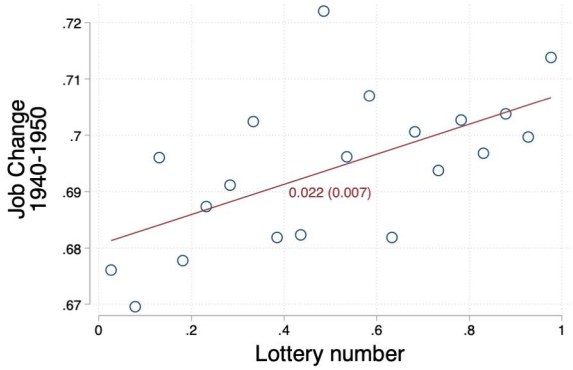
(A) Transition to Marriage



(B) Transition to Fatherhood



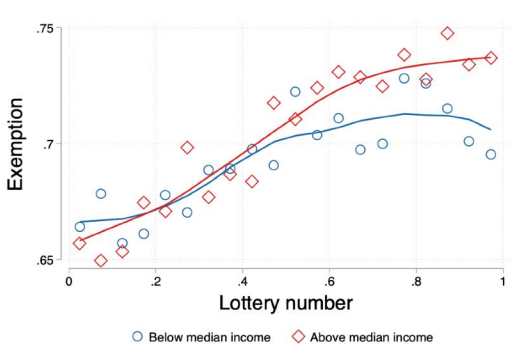
(C) Difference in the Number of Employed Persons in the Household between 1950 and 1940



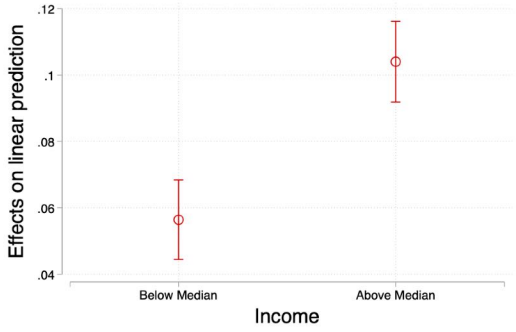
(D) Job Change

Notes: Individuals with higher lottery numbers were more likely to marry (A), father a child (B), strengthen a breadwinner claim with other family members leaving their jobs (C), and change a job (D). Binscatter plots. All exclude service members. A: probability of marriage in 1950 against lottery number for men who were single in 1940; B: probability of becoming a father in 1950 against lottery number for men who had no children in 1940; C: the difference in the number of employed persons in the household between 1950 and 1940 against lottery number; D: probability of having a different job in 1950 compared to 1940 against lottery number. The solid line in each panel represents the linear best fit. The coefficient (with draft board fixed effects) and robust standard error (in parentheses) are shown.

Figure 8: Richer individuals were more likely to use the temporal advantage



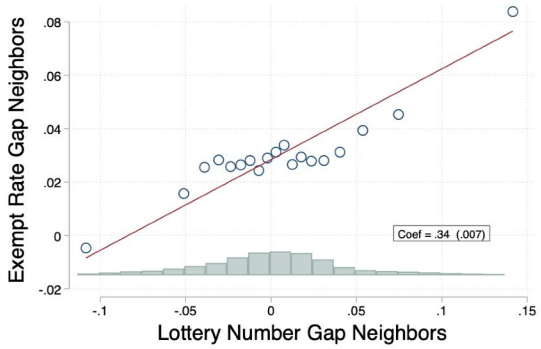
(A) Exemption vs. Lottery Number by Income



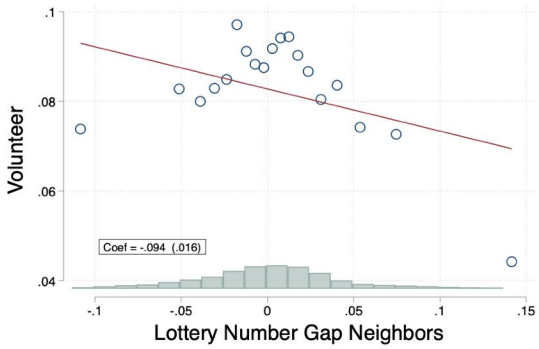
(B) Marginal Effect of Lottery Number on Exemption, by Income

Notes: Panel A: Binned scatterplot of draft exemption status versus lottery number by 1940 income group (below-median in blue, above-median in red). Solid lines are local polynomial smooth plots. Panel B: This figure plots the marginal effect of the lottery number on draft exemption probability for below- and above-median income groups in 1940. The points are coefficient estimates from a fully interacted linear probability model, with vertical bars indicating 90% confidence intervals. Figure A.9 is a heatmap displaying similar relationships.

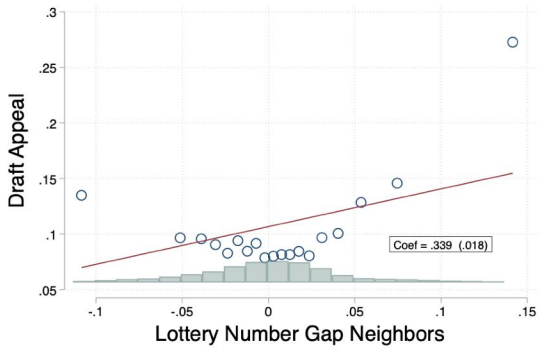
Figure 9: First Stage and Reduced Forms



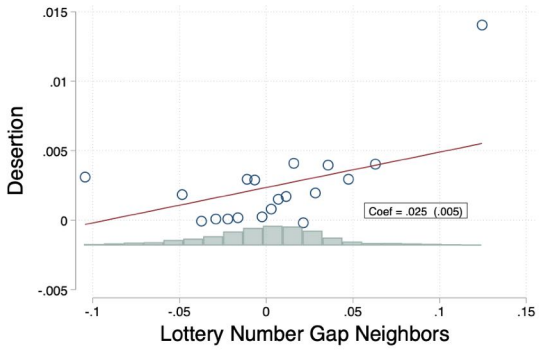
(A) Exemption Rate Gap vs. Lottery Number Gap



(B) Volunteer vs. Lottery Number Gap



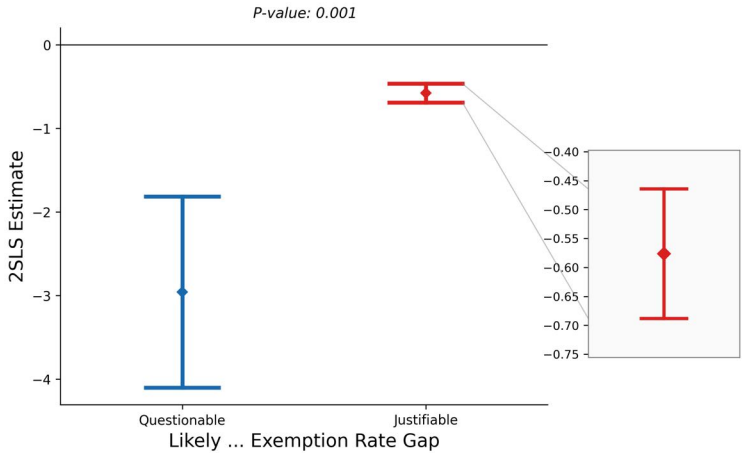
(C) Draft Appeal vs. Lottery Number Gap



(D) Desertion vs. Lottery Number Gap

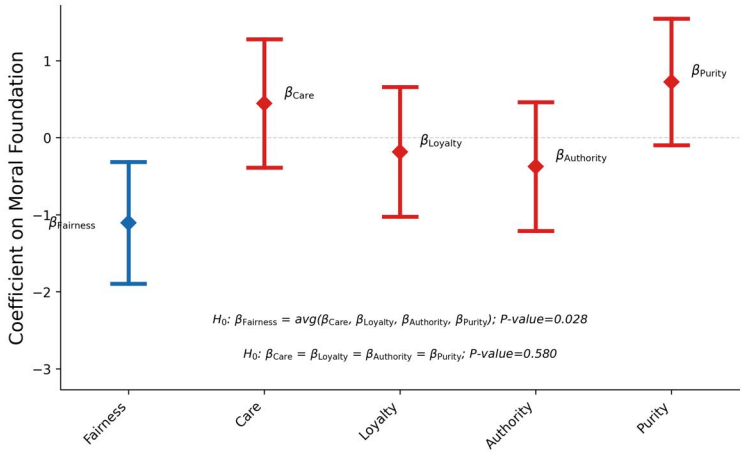
Notes: Binscatter plots. The horizontal axis is the lottery number gap between richer and poorer neighbors. The vertical axis is the exemption rate gap in A, the volunteer indicator in B, the draft appeal filing indicator in C, and the desertion indicator for servicemen in D. Histograms for x-axis variables are also plotted. OLS regression coefficients and their standard errors are reported. I adjust for draft board fixed effects by regressing each variable on the fixed effects and adding the sample mean back to the resulting residuals.

Figure 10: Effects of Likely Questionable vs. Justifiable Exemption Gaps on War Morale



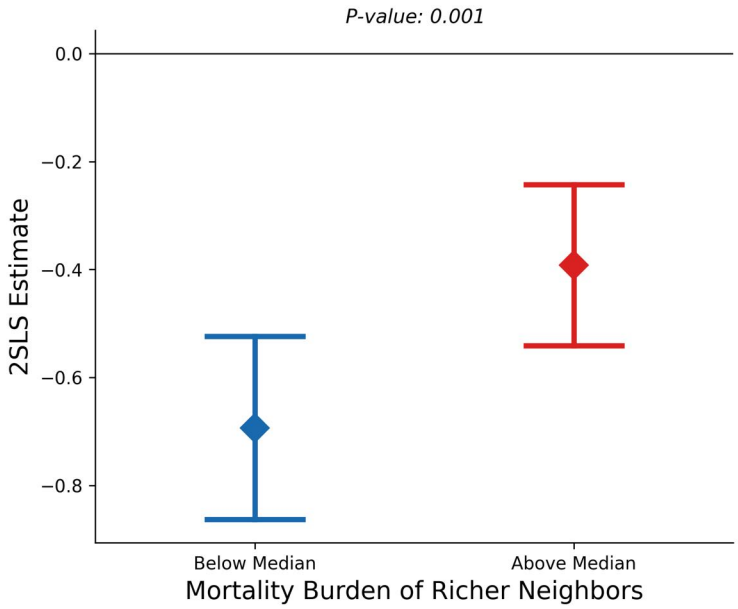
Notes: The same specification as that in Column 3, Panel A of Table 5, but with the overall exemption rate gap replaced by the likely questionable (left) or justifiable (right) exemption rate gap. The figure plots the 2SLS estimates and their 90% confidence intervals. Likely questionable exemptions are those with above-median LASSO residuals among exempted individuals, while likely justifiable exemptions have below-median residuals. The LASSO model predicts exemption probability using demographic and socioeconomic characteristics. The *p*-value tests the null hypothesis that the two coefficients are equal. The inset panel magnifies the likely justifiable estimate to display its confidence interval.

Figure 11: Heterogeneous Treatment Effects by Five Moral Foundations



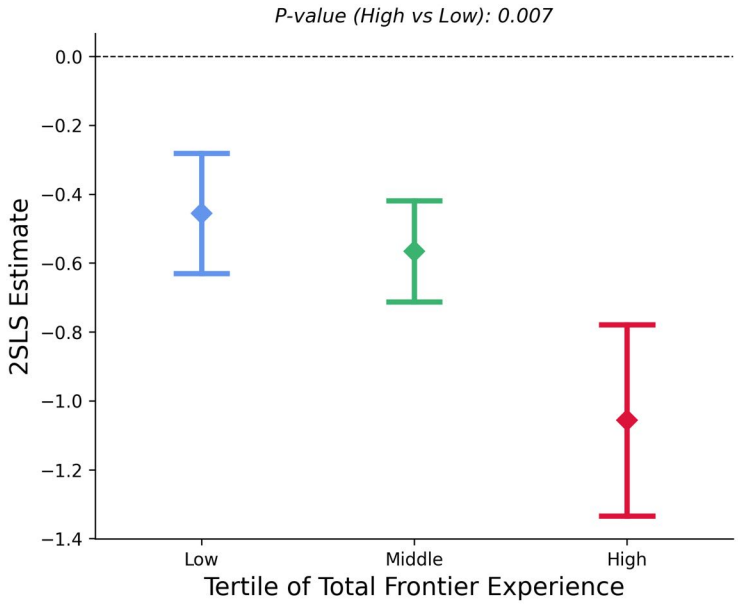
Notes: The figure plots the coefficients from bivariate regressions of board-specific 2SLS estimates on standardized measures of the five moral foundations. In the first step, the effect of the exemption rate gap on the war morale proxy (the outcome in Column 3, Panel A of Table 5) is estimated separately for each draft board. The specification instruments the exemption gap with the lottery number gap. In the second step, these board-specific treatment effects are regressed separately on each standardized moral foundation measure; the diamond markers represent the resulting slope estimates. Error bars represent 90% confidence intervals. The *p*-values displayed correspond to hypothesis tests from a multivariate regression of the board-specific estimates on all five moral foundations simultaneously: (i) a test of equality between the Fairness coefficient and the average of the other four, and (ii) a joint test of equality among the four non-Fairness coefficients. Moral foundation measures are derived from the frequency of foundation-specific words in pre-1940 Kansas newspapers.

Figure 12: Heterogeneous Treatment Effects by Mortality Burden of Richer Neighbors



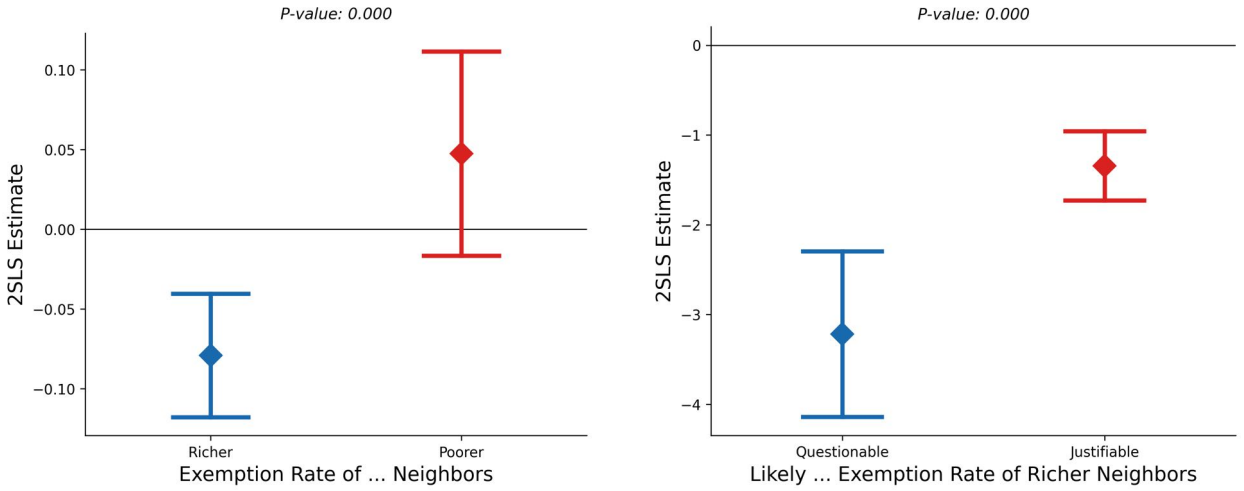
Notes: The figure plots the 2SLS coefficient of the exemption rate gap on war morale from the specification in Column 3, Panel A of Table 5. The sample is split by the median mortality burden of an individual's richer neighbors. Mortality burden is measured using branch-specific death rates (Marine: 5.2%, Army: 3.8%, Navy: 1.9%, Coast Guard: 1.1%; non-service: 0%). Vertical bars represent 90% confidence intervals. Standard errors are clustered at the individual level. The *p*-value tests the null hypothesis that the two coefficients are equal.

Figure 13: Heterogeneous Treatment Effects by Total Frontier Experience



Notes: 2SLS coefficients of the exemption rate gap on war morale from the specification in Column 3, Panel A of Table 5, by the tertile of county-level total frontier experience of Bazzi et al. (2020).

Figure 14: Excluding Labor Supply and Outcome-Based Explanations

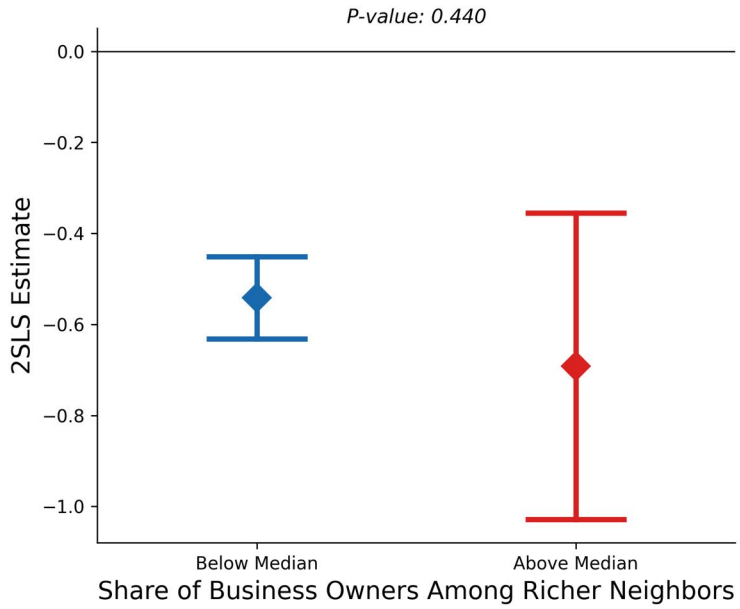


(A) Effects of Exemption Rates of Richer versus Poorer Neighbors

(B) Effects of Likely Questionable versus Justifiable Exemptions

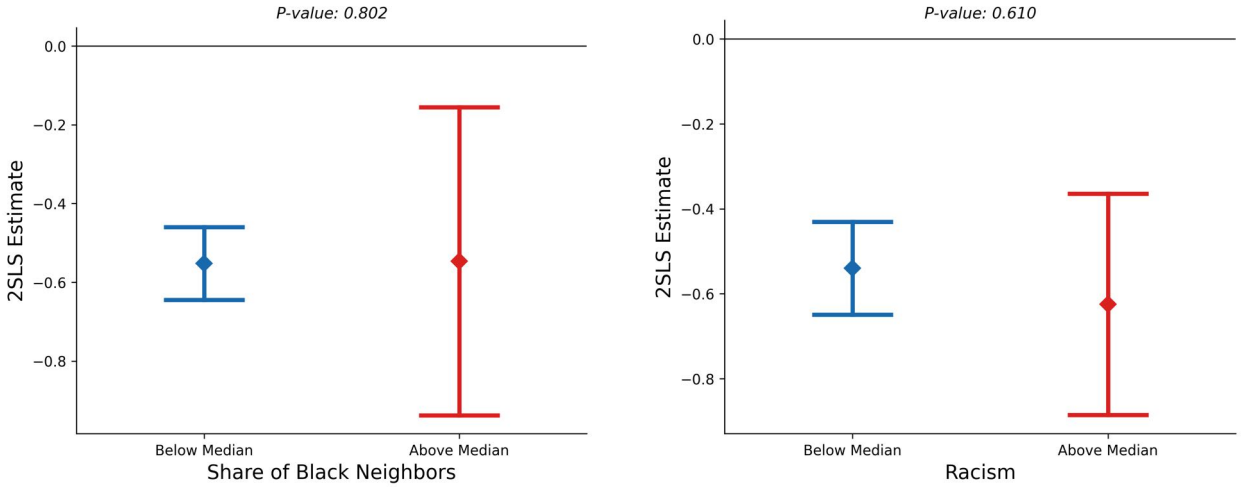
Notes: A: The figure plots 2SLS coefficients for the exemption rates of richer versus poorer neighbors from a single regression on the war morale index used in Column 3, Panel A of Table 5. Each rate is instrumented by the corresponding group's average lottery number. The model includes draft board fixed effects, and standard errors are clustered at the board level. Vertical lines show 90% confidence intervals. The *p*-value tests the null hypothesis that richer and poorer neighbors' exemption rates have equal effects. B: The figure plots 2SLS coefficients from two separate regressions, estimating the effects of richer neighbors' likely questionable versus justifiable exemption rates on the war morale index. In each model, the respective exemption rate is instrumented by the average lottery number of richer neighbors. Both models include draft board fixed effects with standard errors clustered at the board level. Vertical lines show 90% confidence intervals. A likely questionable exemption is one that is statistically unlikely, given the person's observable characteristics; all other exemptions are considered likely justifiable. The *p*-value tests the null hypothesis that likely questionable and justifiable exemptions have equal effects.

Figure 15: Heterogeneous Treatment Effects by Share of Business Owners Among Richer Neighbors



Notes: The figure plots the 2SLS coefficient of the exemption rate gap on war morale from the specification in Column 3, Panel A of Table 5. The sample is split by the median share of business owners among richer neighbors. Business ownership is identified from 1940 census occupation codes. Vertical bars represent 90% confidence intervals. Standard errors are clustered at the draft board level. The *p*-value tests the null hypothesis that the treatment effect is equal across the two groups.

Figure 16: Excluding the Racism Explanation

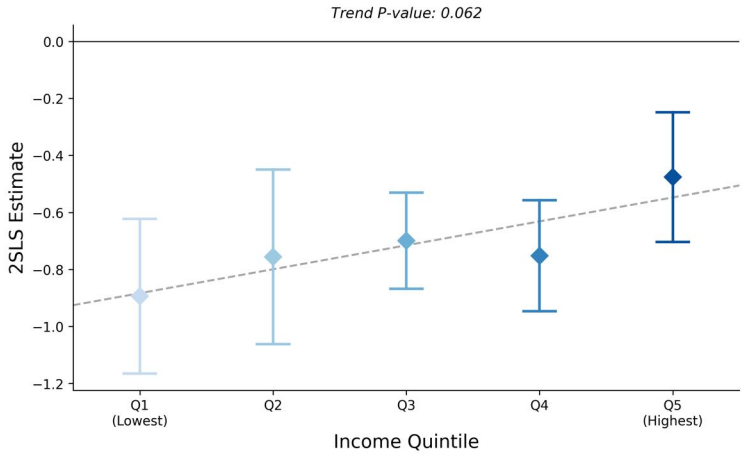


(A) Effects by Share of Black Neighbors

(B) Effects by Racism

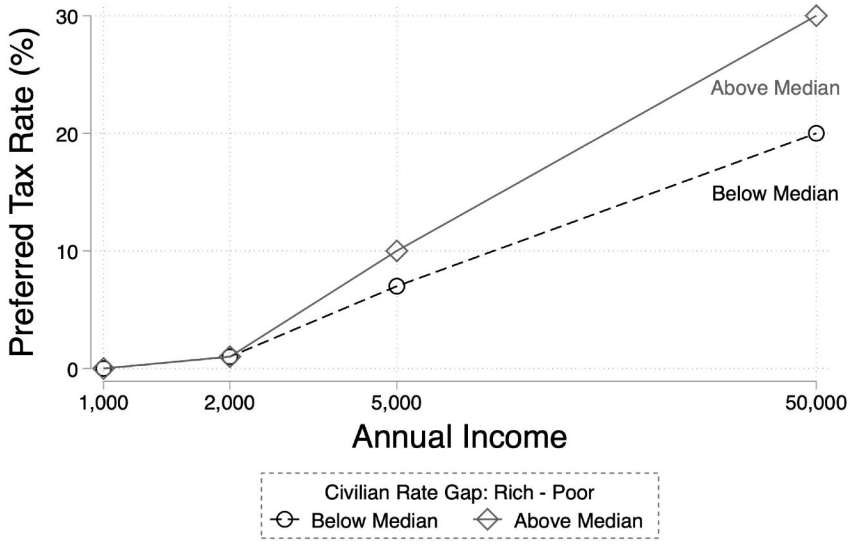
Notes: The figures plot 2SLS coefficients from two separate regressions after splitting the sample by the median share of Black neighbors (A) or the median level of county racism (B). Each regression estimates the effect of the difference in exemption rates on the war morale index used in Column 3, Panel A of Table 5, instrumenting the exemption rate gap with the lottery number gap. Both models include draft board fixed effects with standard errors clustered at the board level. Lines show 90% confidence intervals. The reported *p*-value tests for the equality of the two coefficients and is obtained from a separate, fully interacted model.

Figure 17: Heterogeneous Treatment Effects by Income Quintile



Notes: The figure plots the 2SLS coefficient of the exemption rate gap on war morale from the specification in Column 3, Panel A of Table 5. The sample is split by the income of salient individuals. The dashed line shows the fitted linear trend across quintiles. Diamond markers indicate point estimates; vertical bars represent 90% confidence intervals. The trend p-value tests whether the treatment effect varies linearly with income.

Figure 18: Preferred Income Tax Schedules by Civilian Rate Gap between the Rich and Poor



Notes: Median preferred tax rate at different annual income levels for above- and below-median civilian rate gaps between the rich and poor (the difference in the share of individuals who did not serve in the Army between the top and bottom deciles of the income distribution in a state).

Tables

Table 1: Composition by Adjudication and Exemption

	Civilian	Draftee	Volunteer	Row Total
Adjudicate=1, Exempt=1	120,091	0	8,731	128,822
	(64.4%)	(0.0%)	(4.7%)	(69.1%)
Adjudicate=1, Exempt=0	0	48,075	7,438	55,513
	(0.0%)	(25.8%)	(4.0%)	(29.8%)
Adjudicate=0, Exempt=NA	1,956	0	49	2,005
	(1.0%)	(0.0%)	(0.0%)	(1.1%)
Column Total	122,047	48,075	16,218	186,340
	(65.5%)	(25.8%)	(8.7%)	(100.0%)

Notes: This table shows how each combination of adjudication and exemption corresponds to different military service outcomes in WWII, based on data in Kansas. See Figure 1 for the illustration.

Table 2: Differences in Observables between Linked and Unlinked Individuals

	(1)	(2)	(3)	(4)	(5)	(6)
	Linked		Unlinked		Linked - Unlinked	
	Mean	Standard Deviation	Mean	Standard Deviation	Standardized Difference	*p*-value Equivalence Test
Non-white	0.032	0.177	0.061	0.239	-0.139	1.000
Born in Kansas	0.730	0.444	0.701	0.458	0.064	0.000
Age in 1940	26.435	9.163	26.563	9.002	-0.014	0.000
Lottery number	0.503	0.289	0.497	0.288	0.023	0.000
Latitude	38.439	0.859	38.420	0.864	0.022	0.000
Longitude	-96.849	2.095	-96.907	2.047	0.028	0.000
Height (in)	68.819	2.592	68.755	2.613	0.025	0.000
Weight (lbs)	159.245	21.546	159.000	21.447	0.011	0.000
Surname p50 income ($)	322.086	451.314	344.496	460.158	-0.049	0.000
Share	54.98%		45.02%		100%	

Notes: This table presents the mean and standard deviation for individuals from the draft cards who were successfully linked to the 1940 census (Columns 1-2) and for those who were not (Columns 3-4). Column 5 reports the difference in means, standardized by the pooled sample's standard deviation. Column 6 shows the *p*-value from an equivalence test, defined as the larger of two *p*-values from one-sided *t*-tests under the null hypothesis that the difference is greater than 0.1 or less than -0.1 standard deviations.

Table 3: Summary Statistics

	Mean	p1	p50	p99	Std Dev	N
Volunteer	0.083	0.000	0.000	1.000	0.28	59658
Appeal Filing	0.107	0.000	0.000	1.000	0.31	59658
First PC	-0.013	-0.352	-0.061	0.896	0.28	59658
Desertion	0.002	0.000	0.000	0.000	0.05	21140
Exempt Rate All Neighbors	0.687	0.544	0.685	1.000	0.09	59658
Exempt Rate Richer Neighbors	0.710	0.425	0.702	1.000	0.12	59658
Exempt Rate Poorer Neighbors	0.682	0.477	0.674	1.000	0.12	59658
Exempt Rate Gap Neighbors	0.028	-0.390	0.045	0.351	0.14	59658
Lottery Number Richer Neighbors	0.494	0.118	0.501	0.695	0.07	59658
Lottery Number Poorer Neighbors	0.494	0.122	0.500	0.642	0.06	59658
Lottery Number Gap Neighbors	-0.000	-0.215	0.001	0.217	0.07	59658

Table 4: OLS Results: Effects of the Exemption Rate Gap on Volunteering and Appeal Filing

	Volunteer	Draft Appeal	First PC
	(1)	(2)	(3)
Exempt Rate Gap Neighbors	-0.073	0.867	-0.321
(Richer-Poorer)	(0.011)	(0.033)	(0.015)
	[0.000]	[0.000]	[0.000]
Controls	Yes	Yes	Yes
Draft Board FEs	Yes	Yes	Yes
Mean DV	0.083	0.107	-0.013
R^2	0.0175	0.1461	0.0397
N	59658	59658	59658

Notes: Based on Equation 3. "First PC" is the first principal component of the volunteering indicator and the negative of the draft appeal indicator. Controls are average exemption rate of neighbors, lottery number, average income of neighbors, and income inequality of neighbors. Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets.

Table 5: 2SLS Results: Effects of the Exemption Rate Gap on Volunteering and Appeal Filing

	Volunteer (1)	Draft Appeal (2)	First PC (3)
Panel A: 2SLS			
Exempt Rate Gap Neighbors (Richer-Poorer)	-0.276	1.058	-0.571
	(0.054)	(0.106)	(0.067)
	[0.000]	[0.000]	[0.000]
First Stage:			
Lottery Number Gap Neighbors (Richer-Poorer)	0.334	0.334	0.334
	(0.033)	(0.033)	(0.033)
Kleibergen-Paap *F*	100	100	100
Panel B: Reduced Form			
Lottery Number Gap Neighbors (Richer - Poorer)	-0.092	0.353	-0.191
	(0.015)	(0.049)	(0.022)
	[0.000]	[0.000]	[0.000]
Controls	Yes	Yes	Yes
Draft Board FEs	Yes	Yes	Yes
Mean DV	0.083	0.107	-0.013
N	59658	59658	59658

Notes: "First PC" is the first principal component of the volunteering indicator and the negative of the draft appeal indicator. Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets.

Table 6: Robustness to Alternative Constructions of the Exemption Gap

	2SLS Estimate	SE	*P*-value	Kleibergen-Paap *F*	*N*
(1) Age-adjusted Income	-2.874	0.405	0.000	62	59628
(2) Surname p50 Income	-0.665	0.239	0.000	231	58202
(3) Household Income	-2.594	0.589	0.000	30	59958
(4) Occ. Ranking by p50 Income	-1.303	0.411	0.000	30	56162
(5) Occ. Ranking by Duncan SEI	-0.829	0.170	0.000	192	55603
(6) Home Value	-1.081	0.220	0.000	113	19387
(7) Absolute Income	-0.407	0.206	0.000	12	60020

Notes: The same specification as that in Column 3, Panel A of Table 5. The "Absolute Income" variable is the difference in average exemption rates between an individual's neighbors in the top and bottom 1% of the local income distribution.

Table 7: Effects of the Exemption Rate Gap on Desertion

	Desertion OLS (1)	Desertion 2SLS (2)	Exemption Rate Gap FS (3)	Desertion RF (4)
Exemption Rate Gap Neighbors (Richer - Poorer)	0.0253	0.0866		
	(0.0074)	(0.0302)		
	[0.0000]	[0.0000]		
Lottery Number Gap Neighbors (Richer - Poorer)			0.3110	0.0283
			(0.0125)	(0.0052)
			[0.0000]	[0.0000]
Draft Board FEs	Yes	Yes	Yes	Yes
Mean DV	0.002	0.002	0.033	0.002
Kleibergen-Paap *F*			49	
N	21140	21140	21140	21140

Notes: Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets.

Table 8: Results of the External Validity in Maryland

	Volunteer		Desertion	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)
Civilian Rate Gap Neighbors (Richer-Poorer)	-0.047	-0.067	0.055	0.099
	(0.005)	(0.032)	(0.015)	(0.040)
	[0.000]	[0.029]	[0.000]	[0.001]
First Stage:				
Lottery Number Gap Neighbors (Richer-Poorer)		0.178		0.309
		(0.042)		(0.050)
Kleibergen-Paap *F*		18		38
Draft Board FEs	Yes	Yes	Yes	Yes
Mean DV	0.011	0.011	0.006	0.006
N	52567	52567	12507	12507

Notes: Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets.

Table 9: Effect of Civilian Rate Gap on Draft Fairness Perceptions from Newspapers

	Fairness Index			Civilian Rate Gap	Fairness Index			Civilian Rate Gap
	OLS	2SLS	RF	FS	OLS	2SLS	RF	FS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Explanatory and Instrumental Variables in:							
	Level				Decile			
Civilian Rate Gap	-1.183	-3.620			-0.037	-0.077		
(Rich - Poor)	(0.569)	(0.539)			(0.005)	(0.012)		
	[0.060]	[0.000]			[0.000]	[0.000]		
Lottery Number Gap			-0.522	0.144			-0.037	0.483
(Rich - Poor)			(0.125)	(0.031)			(0.006)	(0.064)
			[0.001]	[0.001]			[0.000]	[0.000]
Kleibergen-Paap *F*		21.04				56.29		
Mean DV	0.063	0.063	0.063	0.036	0.063	0.063	0.063	5.222
N	194	194	194	194	194	194	194	194

Notes: This table reports the relationship between county-level civilian rate gaps and draft fairness perceptions from local newspapers. The dependent variable (except Columns 4 and 8) is the fairness perception, constructed by averaging GPT-4.1 mini classifications of the sentiment of local newspapers published during WWII, where pages are coded as "more fair" = 1, "no change" = 0, and "more unfair" = –1. The Civilian Rate Gap (Rich - Poor) is the difference in civilian rates between the top and bottom income deciles within each county. The Lottery Number Gap (Rich - Poor) is the difference in average draft lottery numbers between the top and bottom income deciles, serving as the instrument for the Civilian Rate Gap. Columns 1-4 use raw levels and Columns 5-8 use within-state decile ranks for both the endogenous variable and the instrument. All regressions include state fixed effects. Standard errors clustered at the state level are reported in parentheses; *p*-values in brackets.

Online Appendix

to

Fairness and Cooperation:
Draft Dodging by the Rich and War Morale in World War II

Guohui Jiang

February 2026

A Appendix: Figures and Tables **I**

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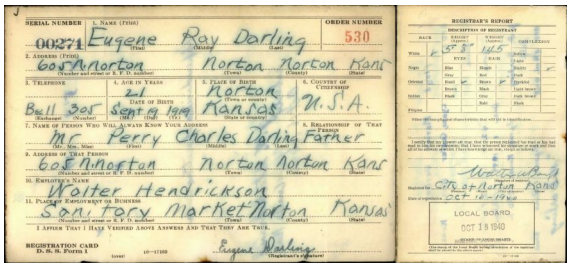
D.4 Summary of Predictions XL

A. Appendix: Figures and Tables

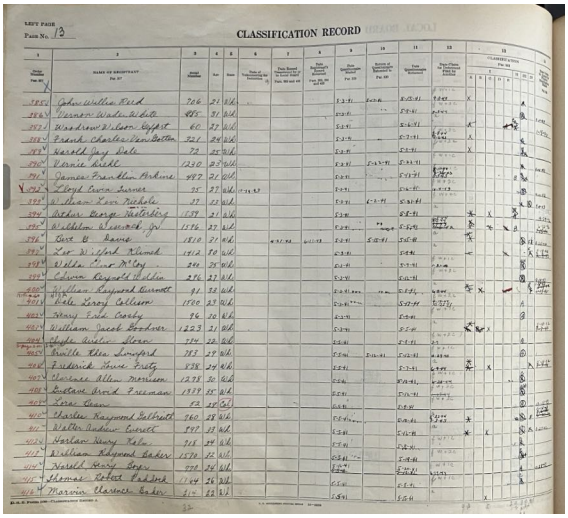
Figure A.1: Data Sources



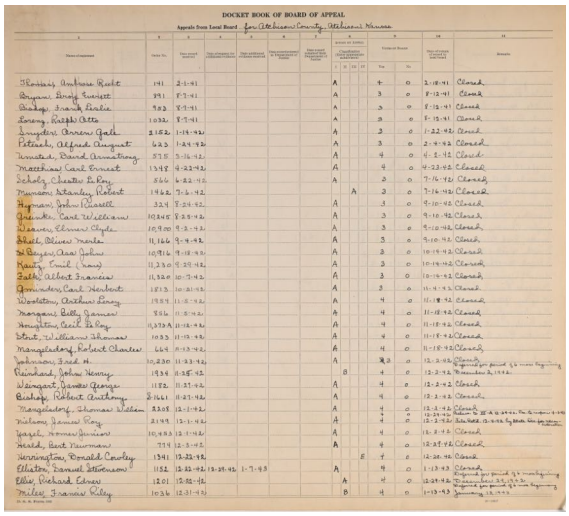
(A) Roosevelt and Stimson at the National Lottery



(B) A Registration Card

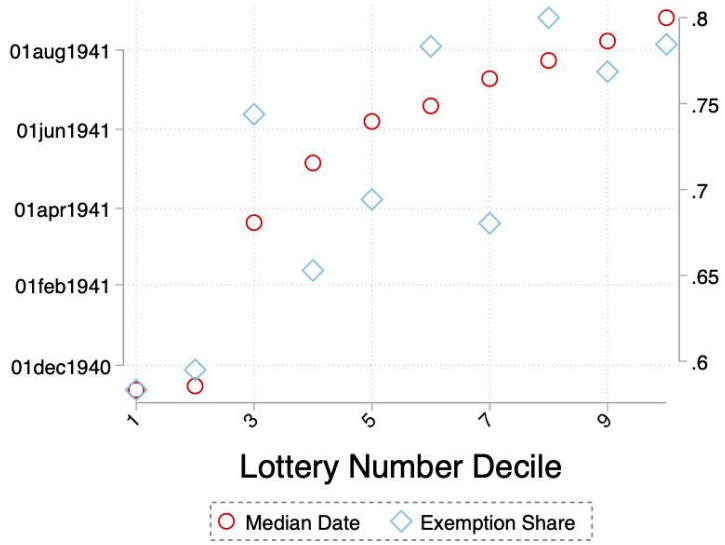


(C) Exemption Records



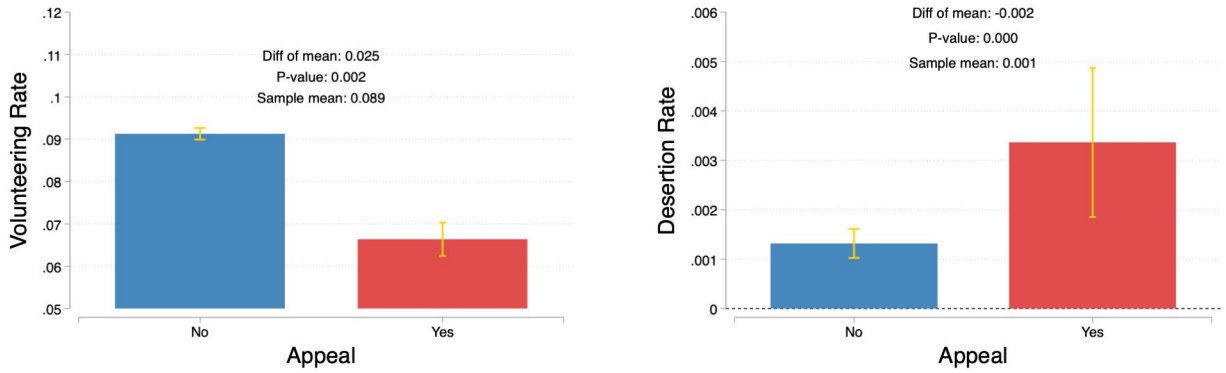
(D) Appeal Records

Figure A.2: Higher lottery numbers were contacted later and received more exemptions



Notes: Binscatter plots of adjudication date (measured by the date the questionnaire was mailed) (left y-axis), exemption rate (right y-axis), and lottery number decile (x-axis) in Pratt, Kansas.

Figure A.3: Draft appeal filing is a valid proxy of war morale

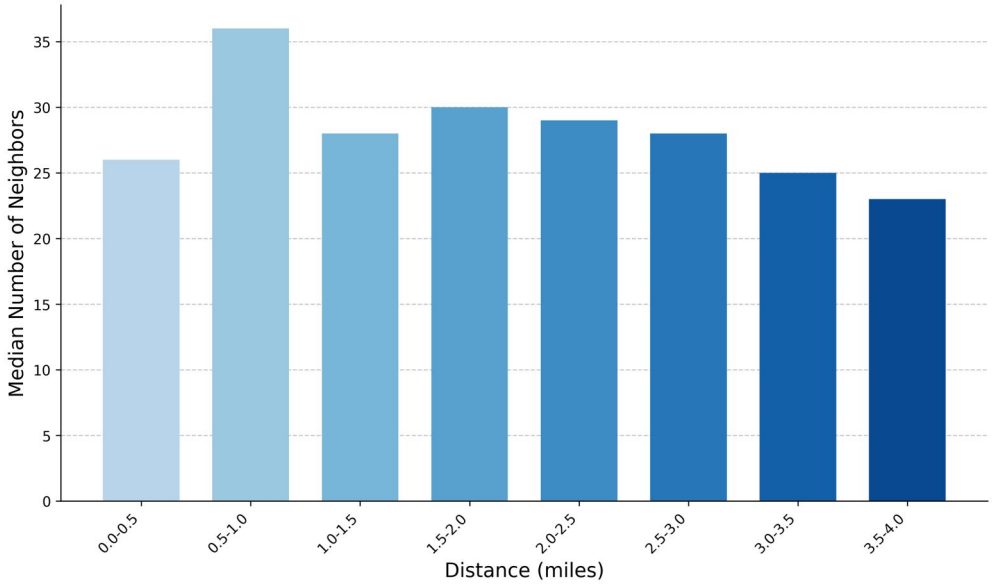


(A) Volunteering by Appeal

(B) Desertion by Appeal

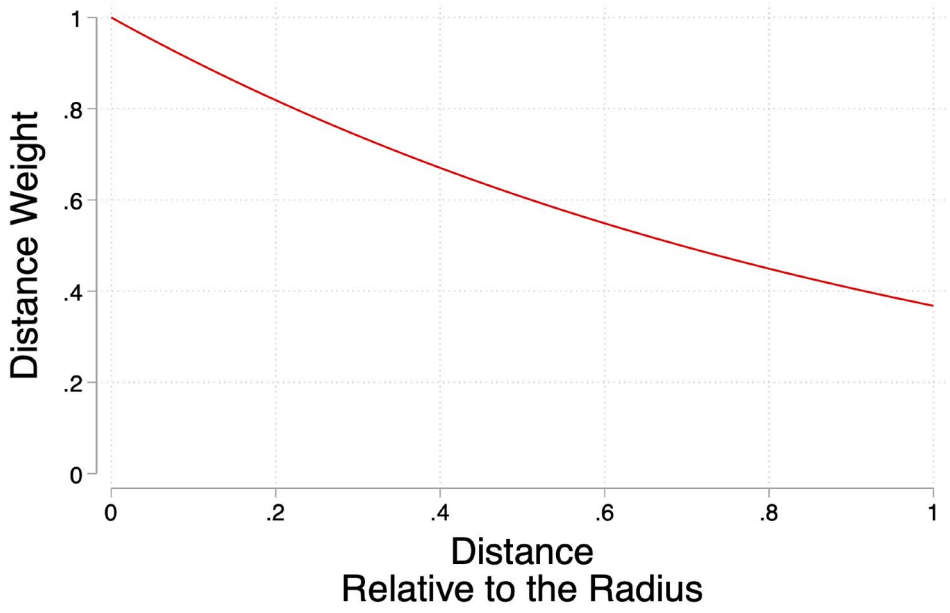
Notes: These figures present bar charts comparing the average volunteering rate (A) or desertion rate (B) between individuals who appealed their draft status ("Yes") and those who did not ("No"). The height of the bars represents the mean level for each group. The vertical error bars indicate the 95% confidence intervals around the mean. I also report the sample average, the difference between two means, and its p-value from the t-test.

Figure A.4: Distribution of Neighbors by Distance



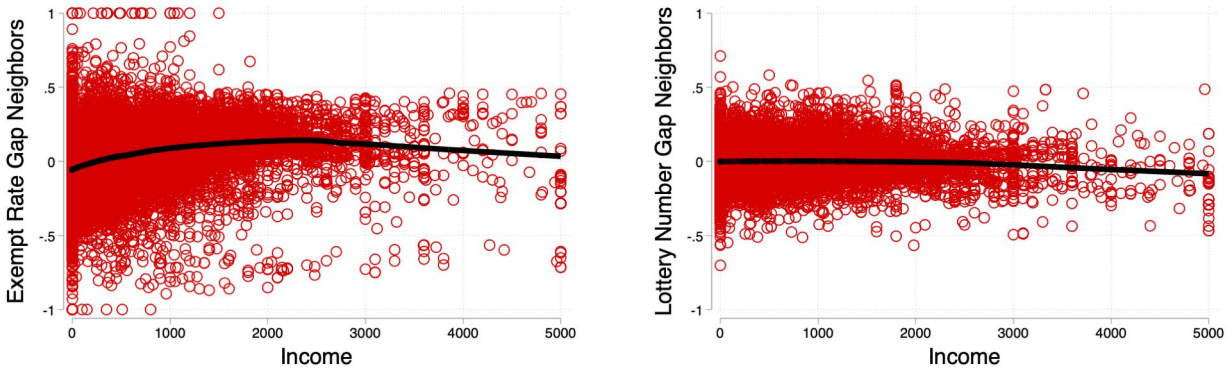
Notes: This figure displays the median number of neighbors within each 0.5-mile distance interval, from 0–0.5 miles to 3.5–4 miles.

Figure A.5: Distance Weight



Notes: This figure illustrates the weight function from Equation 2.

Figure A.6: Exemption Rate and Lottery Number Gaps versus Income



(A) Exemption Rate Gap vs. Income

(B) Lottery Number Gap vs. Income

Notes: A: A scatter plot between an individual's income (x-axis) and the exemption rate gap between the individual's richer and poorer neighbors (y-axis). B: A scatter plot between an individual's income (x-axis) and the lottery number gap between the individual's richer and poorer neighbors (y-axis). The black lines are the local linear smooths.

Figure A.7: Identification Strategy

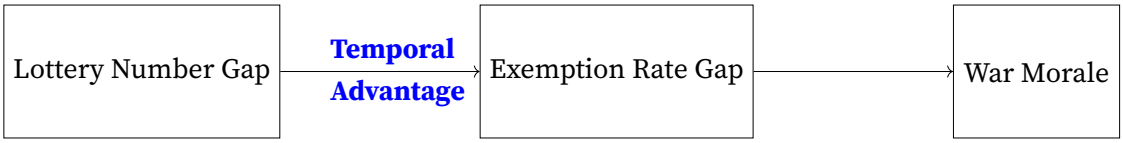
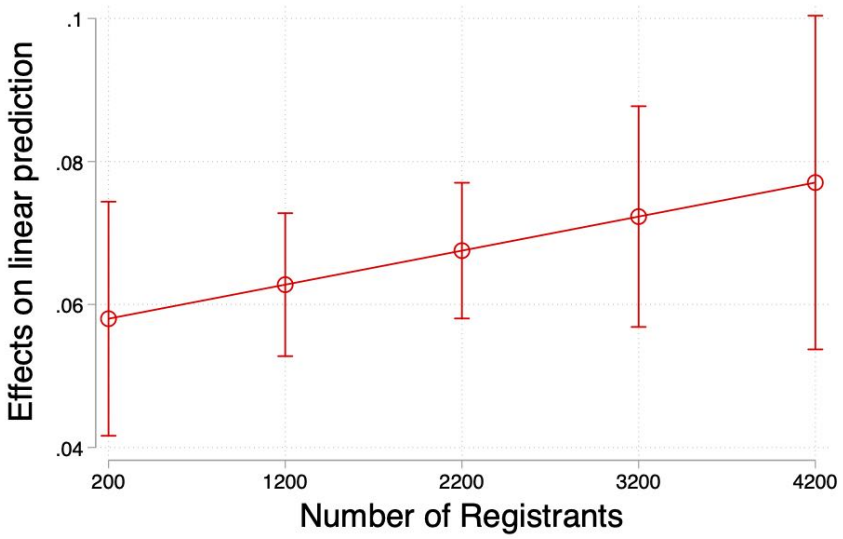
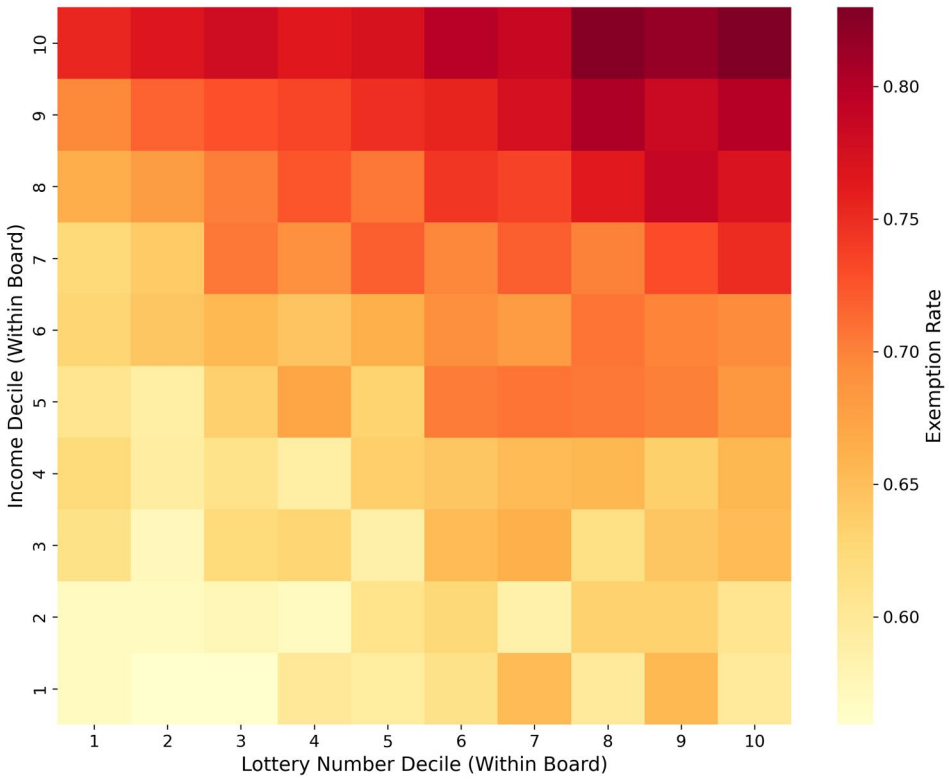


Figure A.8: Average Marginal Effect of Lottery Number on Exemption across Different Board Sizes



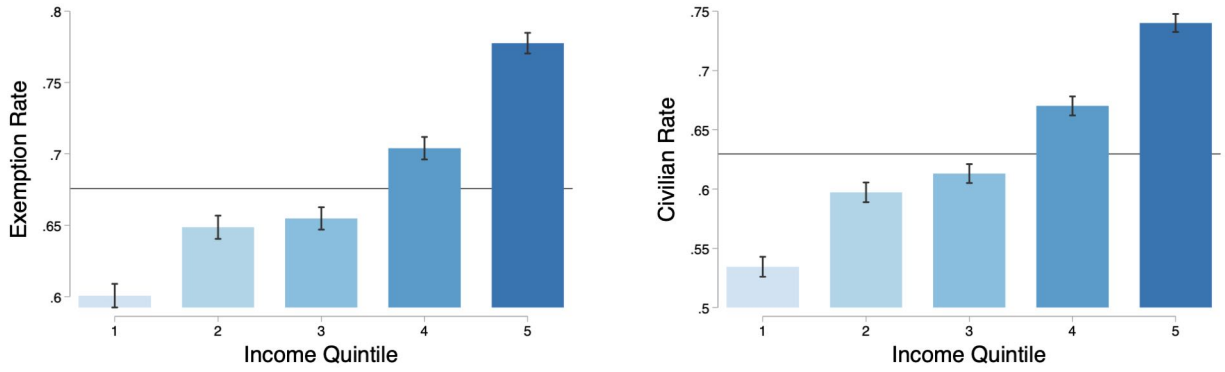
Notes: This figure plots the average marginal effect of the lottery number on the probability of exemption at various levels of the number of registrants per board. Vertical bars represent 90% confidence intervals. Estimates are derived from a linear probability model with standard errors clustered at the board level. Each draft board comprised three members, regardless of the number of registrants in its jurisdiction. Consequently, boards with a larger caseload processed registrants at a slower pace. This implies that for larger boards, a given increase in the lottery number translated into a greater temporal advantage.

Figure A.9: Exemption Rates by Lottery Number and Income



Notes: This figure displays a heatmap of average exemption rates by within-board lottery number decile (horizontal axis) and within-board income decile (vertical axis). Deciles are computed separately within each draft board. Darker colors indicate higher exemption rates.

Figure A.10: Exemption and Civilian Shares by Income



(A) Exemption Shares by Income

(B) Civilian Shares by Income

Notes: These figures present bar charts comparing the average exemption rate (A) or civilian rate (B) across income quintiles. The height of the bars represents the mean level for each group. The vertical error bars indicate the 95% confidence intervals around the mean.

Table A.1: 2SLS Estimates without Baseline Controls

	Volunteer 2SLS (1)	Draft Appeal 2SLS (2)	First PC 2SLS (3)	Desertion 2SLS (4)
Exemption Rate Gap Neighbors (Richer - Poorer)	-0.2769	0.9992	-0.5548	0.0786
	(0.0615)	(0.1160)	(0.0790)	(0.0290)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Mean DV	0.083	0.107	-0.013	0.002
Kleibergen-Paap *F*	89	89	89	46
N	59658	59658	59658	21140
Controls	No	No	No	No
Draft Board FEs	Yes	Yes	Yes	Yes

Notes: 2SLS estimates of the effects of the exemption rate gap between richer and poorer neighbors on war morale proxies under the 4-mile definition of neighbors without baseline controls. "First PC" is the first principal component of the volunteering indicator and the negative of the draft appeal indicator. Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets.

Table A.2: 2SLS Estimates with Unweighted Exemption Rates

	Volunteer 2SLS (1)	Draft Appeal 2SLS (2)	First PC 2SLS (3)	Desertion 2SLS (4)
Exemption Rate Gap Neighbors (Richer - Poorer)	-0.2759	1.0441	-0.5669	0.0718
	(0.0529)	(0.1071)	(0.0648)	(0.0298)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Mean DV	0.083	0.107	-0.013	0.002
Kleibergen-Paap *F*	97	97	97	49
N	59658	59658	59658	21140
Controls	Yes	Yes	Yes	Yes
Draft Board FEs	Yes	Yes	Yes	Yes

Notes: 2SLS estimates of the effects of the unweighted exemption rate gap between richer and poorer neighbors on war morale proxies under the 4-mile definition of neighbors. "First PC" is the first principal component of the volunteering indicator and the negative of the draft appeal indicator. Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets.

Table A.3: 2SLS Estimates under the Radius of 2 Miles

	Volunteer 2SLS (1)	Draft Appeal 2SLS (2)	First PC 2SLS (3)	Desertion 2SLS (4)
Exemption Rate Gap Neighbors (Richer - Poorer)	-0.1907	0.6054	-0.3581	0.1834
	(0.0815)	(0.1573)	(0.0986)	(0.0563)
	[0.0096]	[0.0000]	[0.0000]	[0.0000]
Mean DV	0.085	0.099	-0.009	0.002
Kleibergen-Paap *F*	41	41	41	23
N	56396	56396	56396	20312
Controls	Yes	Yes	Yes	Yes
Draft Board FEs	Yes	Yes	Yes	Yes

Notes: 2SLS estimates of the effects of the exemption rate gap between richer and poorer neighbors on war morale proxies under the 2-mile definition of neighbors. "First PC" is the first principal component of the volunteering indicator and the negative of the draft appeal indicator. Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets.

Table A.4: 2SLS Estimates under the Radius of 3 Miles

	Volunteer 2SLS (1)	Draft Appeal 2SLS (2)	First PC 2SLS (3)	Desertion 2SLS (4)
Exemption Rate Gap Neighbors (Richer - Poorer)	-0.2970	0.9012	-0.5457	0.1076
	(0.0685)	(0.1224)	(0.0807)	(0.0398)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Mean DV	0.084	0.104	-0.012	0.002
Kleibergen-Paap *F*	70	70	70	35
N	58815	58815	58815	20943
Controls	Yes	Yes	Yes	Yes
Draft Board FEs	Yes	Yes	Yes	Yes

Notes: 2SLS estimates of the effects of the exemption rate gap between richer and poorer neighbors on war morale proxies under the 3-mile definition of neighbors. "First PC" is the first principal component of the volunteering indicator and the negative of the draft appeal indicator. Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets.

Table A.5: 2SLS Estimates under the Radius of 5 Miles

	Volunteer 2SLS (1)	Draft Appeal 2SLS (2)	First PC 2SLS (3)	Desertion 2SLS (4)
Exemption Rate Gap Neighbors (Richer - Poorer)	-0.3176	1.3754	-0.7030	0.1454
	(0.0900)	(0.2452)	(0.1336)	(0.0579)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Mean DV	0.082	0.107	-0.014	0.002
Kleibergen-Paap *F*	28	28	28	16
N	60647	60647	60647	21426
Controls	Yes	Yes	Yes	Yes
Draft Board FEs	Yes	Yes	Yes	Yes

Notes: 2SLS estimates of the effects of the exemption rate gap between richer and poorer neighbors on war morale proxies under the 5-mile definition of neighbors. "First PC" is the first principal component of the volunteering indicator and the negative of the draft appeal indicator. Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets.

Table A.6: 2SLS Estimates under the Nearest 100 Neighbors

	Volunteer 2SLS (1)	Draft Appeal 2SLS (2)	First PC 2SLS (3)	Desertion 2SLS (4)
Exemption Rate Gap Neighbors	-0.4617	0.8383	-0.6851	0.0607
(Richer - Poorer)	(0.1768)	(0.2829)	(0.2313)	(0.0382)
	[0.0000]	[0.0000]	[0.0000]	[0.1200]
First Stage:				
Lottery Number Gap Neighbors	0.0884	0.0884	0.0884	0.0825
(Richer - Poorer)	(0.0213)	(0.0213)	(0.0213)	(0.0272)
Kleibergen-Paap *F*	17	17	17	9
Mean DV	0.089	0.082	-0.000	0.001
Mean IDV	0.031	0.031	0.031	0.040
N	61576	61576	61576	21820
Controls	Yes	Yes	Yes	Yes
Draft Board FEs	Yes	Yes	Yes	Yes

Notes: 2SLS estimates of the effects of the exemption rate gap between richer and poorer neighbors on war morale proxies under the closest 100 neighbors. "First PC" is the first principal component of the volunteering indicator and the negative of the draft appeal indicator. Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets.

Table A.7: 2SLS Estimates under the Nearest 50 Neighbors

	Volunteer 2SLS (1)	Draft Appeal 2SLS (2)	First PC 2SLS (3)	Desertion 2SLS (4)
Exemption Rate Gap Neighbors	-0.1500	0.4421	-0.2718	0.0437
(Richer - Poorer)	(0.0822)	(0.1292)	(0.0982)	(0.0210)
	[0.0400]	[0.0000]	[0.0000]	[0.0200]
First Stage:				
Lottery Number Gap Neighbors	0.0929	0.0929	0.0929	0.0977
(Richer - Poorer)	(0.0153)	(0.0153)	(0.0153)	(0.0198)
Kleibergen-Paap *F*	36	36	36	24
Mean DV	0.089	0.082	-0.000	0.001
Mean IDV	0.027	0.027	0.027	0.036
N	59573	59573	59573	21289
Controls	Yes	Yes	Yes	Yes
Draft Board FEs	Yes	Yes	Yes	Yes

Notes: 2SLS estimates of the effects of the exemption rate gap between richer and poorer neighbors on war morale proxies under the closest 50 neighbors. "First PC" is the first principal component of the volunteering indicator and the negative of the draft appeal indicator. Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets.

Table A.8: 2SLS Estimates under the Nearest 10 Neighbors

	Volunteer 2SLS (1)	Draft Appeal 2SLS (2)	First PC 2SLS (3)	Desertion 2SLS (4)
Exemption Rate Gap Neighbors	-0.0328	0.1065	-0.0623	0.0366
(Richer - Poorer)	(0.0494)	(0.0467)	(0.0507)	(0.0131)
	[0.4400]	[0.0000]	[0.0800]	[0.0000]
First Stage:				
Lottery Number Gap Neighbors	0.1125	0.1125	0.1125	0.1141
(Richer - Poorer)	(0.0121)	(0.0121)	(0.0121)	(0.0147)
Kleibergen-Paap *F*	87	87	87	60
Mean DV	0.089	0.082	-0.000	0.001
Mean IDV	0.020	0.020	0.020	0.025
N	40525	40525	40525	14613
Controls	Yes	Yes	Yes	Yes
Draft Board FEs	Yes	Yes	Yes	Yes

Notes: 2SLS estimates of the effects of the exemption rate gap between richer and poorer neighbors on war morale proxies under the closest 10 neighbors. "First PC" is the first principal component of the volunteering indicator and the negative of the draft appeal indicator. Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets.

Table A.9: 2SLS Estimates with Surname Median Income

	Volunteer 2SLS (1)	Draft Appeal 2SLS (2)	First PC 2SLS (3)	Desertion 2SLS (4)
Exemption Rate Gap Neighbors (Richer - Poorer)	-0.0291	0.2346	-0.6652	0.0180
	(0.0412)	(0.0867)	(0.2387)	(0.0369)
	[0.2864]	[0.0000]	[0.0002]	[0.1154]
Mean DV	0.081	0.106	-0.063	0.002
Kleibergen-Paap *F*	231	231	231	99
N	58202	58202	58202	20589
Controls	Yes	Yes	Yes	Yes
Draft Board FEs	Yes	Yes	Yes	Yes

Notes: 2SLS estimates of the effects of the exemption rate gap between richer and poorer neighbors on war morale proxies under the 4-mile definition of neighbors. The relative social status is measured by the surname median income. "First PC" is the first principal component of the volunteering indicator and the negative of the draft appeal indicator. Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets.

Table A.10: 2SLS Estimates with Age

	Volunteer 2SLS (1)	Draft Appeal 2SLS (2)	First PC 2SLS (3)	Desertion 2SLS (4)
Exemption Rate Gap Neighbors	0.1530	0.2180	0.0832	-0.0014
(Older - Younger)	(0.1804)	(0.1710)	(0.1758)	(0.0197)
	[0.9600]	[0.9600]	[0.9800]	[1.0000]
First Stage:				
Lottery Number Gap Neighbors	0.0760	0.0760	0.0760	0.1358
(Older - Younger)	(0.0185)	(0.0185)	(0.0185)	(0.0246)
Kleibergen-Paap *F*	17	17	17	30
Mean DV	0.089	0.082	-0.000	0.001
Mean IDV	0.232	0.232	0.232	0.247
N	144762	144762	144762	51546
Controls	Yes	Yes	Yes	Yes
Draft Board FEs	Yes	Yes	Yes	Yes

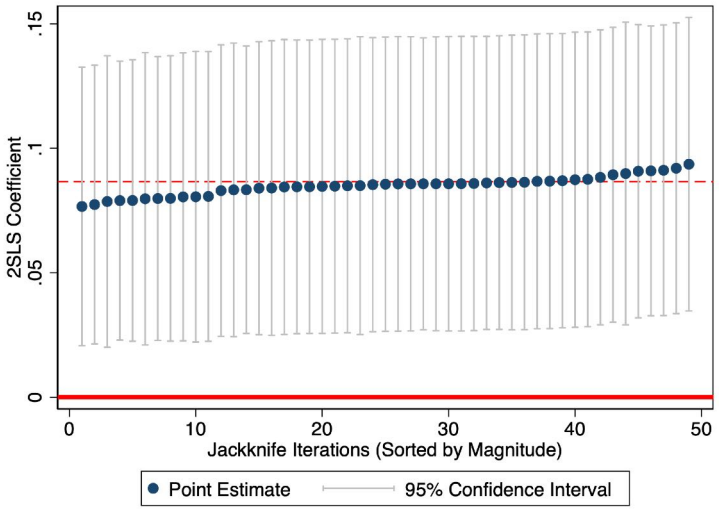
Notes: 2SLS estimates of the effects of the exemption rate gap between older and younger neighbors on war morale proxies under the 4-mile definition of neighbors. "First PC" is the first principal component of the volunteering indicator and the negative of the draft appeal indicator. Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets.

Table A.11: Effects of Draft Exemption Gap by Ancestry

	Volunteer 2SLS (1)	Draft Appeal 2SLS (2)	First PC 2SLS (3)	Desertion 2SLS (4)
Panel A: Same Ancestry				
Exemption Rate Gap Neighbors (Richer - Poorer)	-0.2719	1.0221	-0.5567	0.0898
	(0.0675)	(0.1368)	(0.0862)	(0.0401)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]
N	58,599	58,599	58,599	20,845
Panel B: Different Ancestry				
Exemption Rate Gap Neighbors (Richer - Poorer)	0.1764	0.1172	0.1349	0.0335
	(0.2732)	(0.3055)	(0.2657)	(0.1166)
	[0.2200]	[0.3000]	[0.3400]	[0.6800]
N	46,210	46,210	46,210	17,452
Controls	Yes	Yes	Yes	Yes
Draft Board FEs	Yes	Yes	Yes	Yes

Notes: 2SLS estimates of the effects of the exemption rate gap between richer and poorer neighbors on war morale proxies. "First PC" is the first principal component of the volunteering indicator and the negative of the draft appeal indicator. Standard errors, clustered at the draft board level, are reported in parentheses. *P*-values derived from randomization inference (based on 5,000 permutations, seed 50) are presented in brackets. Panel A focuses on neighbors of the same ancestry, and Panel B focuses on neighbors of different ancestry. Ancestry is measured by the father's birth country.

Figure A.11: Robustness of Desertion Estimates to Influential Observations



Notes: This figure reports the results of a leave-one-out sensitivity analysis designed to test if the main desertion results (Table 7, Column 2) are driven by any single influential observation. The sample includes 49 recorded deserters. I re-estimate the baseline 2SLS specification 49 times, in each iteration excluding a different single deserter from the sample. The resulting coefficients are sorted by magnitude. The blue dots represent the 2SLS point estimates, and the grey vertical bars indicate the 95% confidence intervals. The dashed line represents the baseline full-sample point estimate.

Table A.12: Robustness of Desertion Results: Firth Logit

	Desertion (1)	Desertion (2)	Desertion (3)
Average Marginal Effect:	Exemption Gap (Actual)	Exemption Gap (Predicted by IV)	Lottery Gap (IV)
	0.0145	0.0632	0.0211
	(0.0025)	(0.0129)	(0.0043)
Observations	21140	21140	21140
Rare Events Correction	Yes	Yes	Yes

Notes: This table presents robustness checks for the desertion results using the penalized maximum likelihood estimator from Firth (1993), which corrects for small-sample bias in rare events data (baseline desertion rate $\approx 0.20\%$). The dependent variable in all columns is a binary indicator for desertion. Estimates report Average Marginal Effects (AME) on the probability of desertion. Column (1) reports the baseline association between the neighbor exemption rate gap and desertion. Column (2) employs a two-stage predictor substitution (2SPS) procedure: first regress the exemption gap on the lottery gap, then use the fitted values in the Firth logit (Terza et al., 2008). Column (3) reports the reduced form relationship between the lottery number gap and desertion. All specifications include draft board-level controls (mean deferment rate, mean lottery number, mean income, and Gini coefficient). Standard errors are in parentheses.

Table A.13: Comparisons of Kansas, Maryland, and Other States

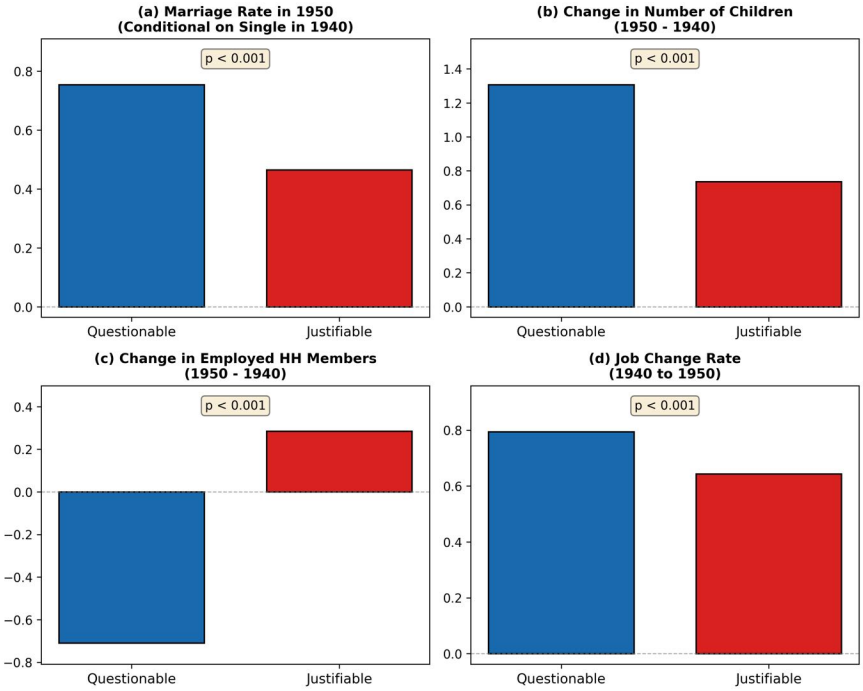
	KS	MD	Other States
	(1)	(2)	(3)
Volunteer Share	0.026	0.014	0.025
Draftee Share	0.038	0.073	0.087
Fairness MFD	0.002	0.002	0.002
Care MFD	0.005	0.005	0.005
Loyalty MFD	0.005	0.004	0.005
Authority MFD	0.006	0.005	0.006
Purity MFD	0.005	0.006	0.005
Urban Share	0.419	0.577	0.424
White Share	0.963	0.837	0.956
Native Share	0.971	0.954	0.936
Median Income	600.000	800.000	720.000
Gini	0.475	0.432	0.447
Population Density	21.934	184.206	49.189
Democratic Vote Share	0.367	0.493	0.441

Notes: This table provides a comparative summary of Kansas (KS), Maryland (MD), and other US states. Column 1: value for KS; Column 2: value for MD; Column 3: median value among other states. The variables are: military service rates, normalized frequencies of words associated with each moral foundation in pre-1940 local newspapers according to the Moral Foundations Dictionary, demographics, economic indicators, population density per square mile, and historical voting patterns.

Table A.14: LASSO Results in Estimating Potentially Questionable Exemptions

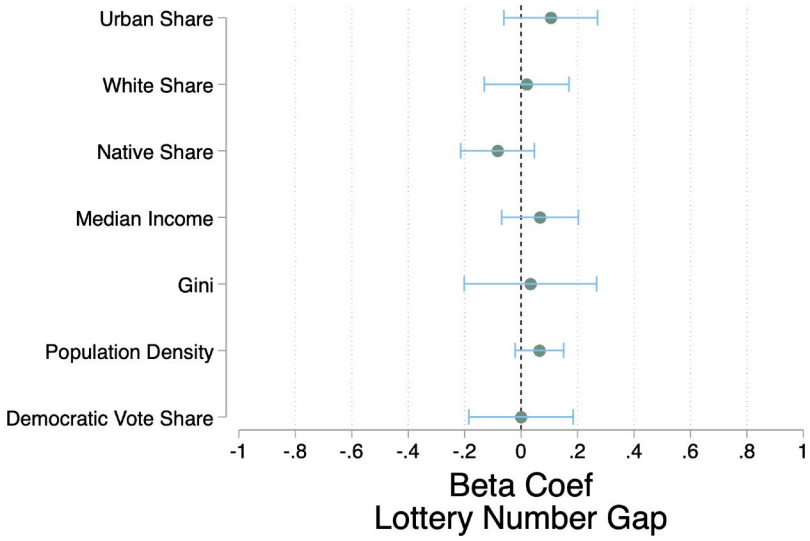
Panel A: Variables with Non-Zero Coefficients	
Variable	Sum of $\|\beta\|$
Birth Year	0.1466
Marital Status	0.0514
Occupation (1950 codes)	0.0303
Number of Children	0.0215
Household Employment	0.0090
Industry (1950 codes)	0.0013
Panel B: Variables Included but Not Selected	
Draft Board, Height, Highest Grade, Weight	
Panel C: Summary Statistics	
LASSO penalty (α)	0.0194
Variable groups included	10
Features after dummy expansion	578
Features with non-zero coefficients	14
Residual cutoff (median)	0.2723
Questionable exemptions	75,839
Justifiable exemptions	52,983

Figure A.12: Validation of Likely Questionable Exemption Classification



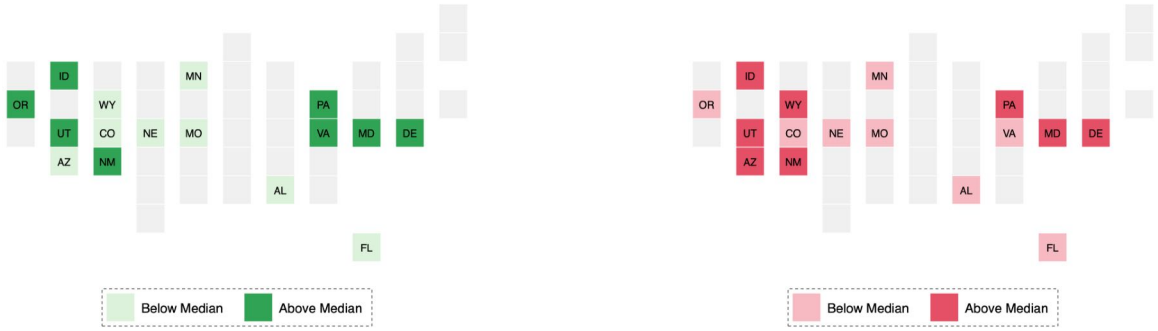
Notes: This figure compares the change in outcomes relevant for draft exemption (1940–1950) between likely questionable and justifiable exemption groups, classified using LASSO residuals (above vs. below median among exempted). Panel (a) shows marriage rates in 1950 among those single in 1940. Panel (b) shows change in number of children between 1940 and 1950. Panel (c) shows change in employed household members. Panel (d) shows job change rates between 1940 and 1950. Sample excludes WWII veterans. *P*-values are from OLS regression with robust standard errors.

Figure A.13: County-Level Balance Test



Notes: beta coefficients of regressions at the county level. The independent variable is the 'Lottery Number Gap: Rich - Poor,' the difference in the average lottery numbers between the top and bottom 10% of the income distribution in a county. The dependent variables are listed on the vertical axis. The sample of counties corresponds to the one used in Table 9. Standard errors are clustered at the state level. 95% confidence intervals are reported.

Figure A.14: Civilian Rate Gap and Share of Perceptions of Draft Unfairness

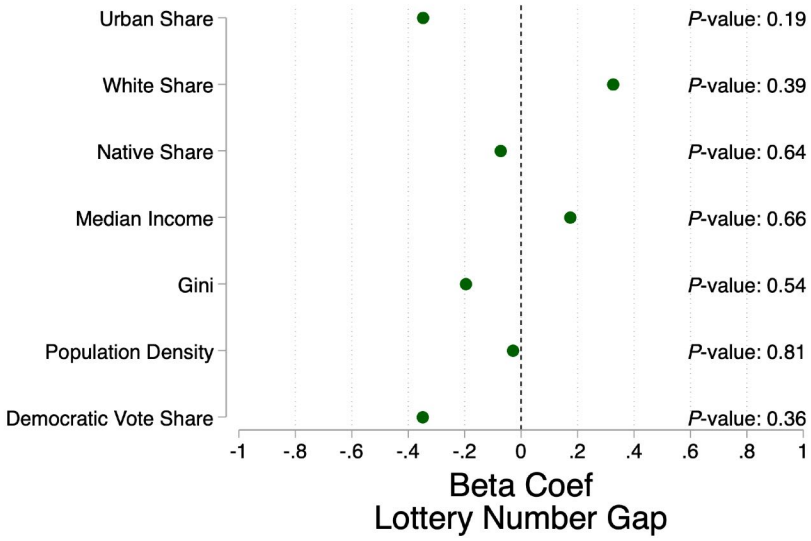


(A) Civilian Rate Gap: Rich - Poor

(B) Share of Perceptions of Draft Unfairness

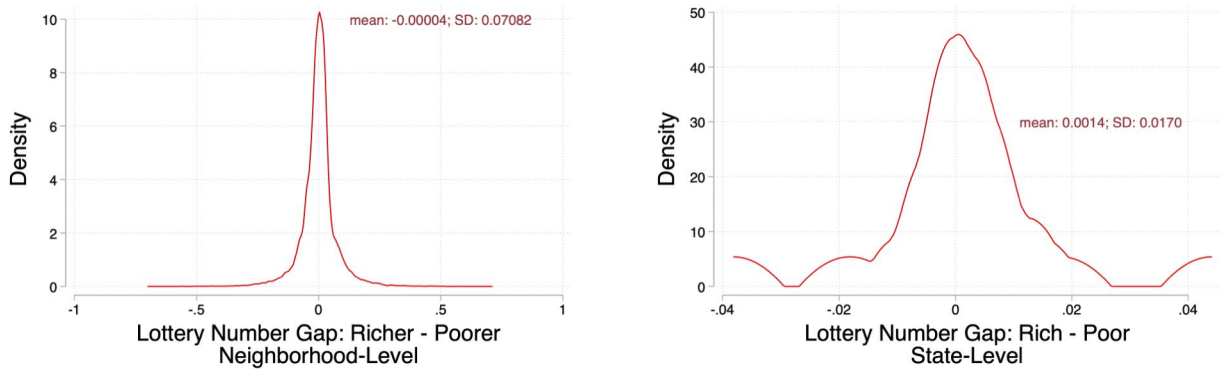
Notes: 16 states where Gallup survey data and individual lottery numbers are available. The 'Civilian Rate Gap: Rich - Poor' is the difference in the share of individuals who did not serve in the U.S. Army between the top and bottom 10% of the income distribution (i.e., 'Rich' and 'Poor'). The 'Share of Perceptions of Draft Unfairness' denotes the proportion of negative responses to the Gallup question: 'Do you think the draft is being handled fairly in your community?' I discretize each continuous variable at its median.

Figure A.15: State-Level Balance Test



Notes: beta coefficients of regressions at the state level. The independent variable is the 'Lottery Number Gap: Rich - Poor,' the difference in the average lottery numbers between the top and bottom 10% of the income distribution in a state. The dependent variables are listed on the vertical axis. *P*-values are from a wild bootstrap procedure to correct for small sample biases (100 replications, seed = 50).

Figure A.16: Density Plots of the Lottery Number Gap



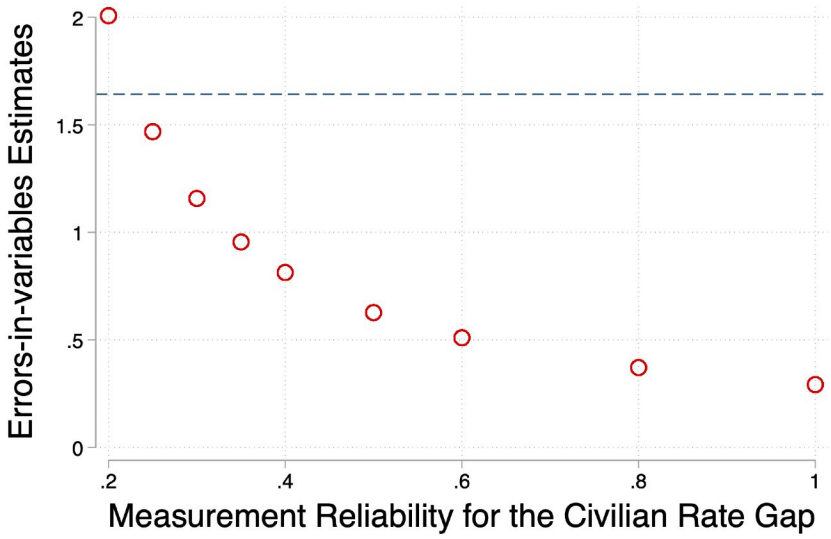
Notes: Density plots of the lottery number gap. Left: the lottery number gap between the richer and poorer neighbors in Kansas. Right: the lottery number gap between the top and bottom 10% of the income distribution in a state. Their mean and standard deviation are also reported.

Table A.15: Effects of Civilian Rate Gap on Draft Unfairness Perceptions from Gallup Surveys

	Perceived Draft Unfairness			Civilian Rate Gap	Perceived Draft Unfairness			Civilian Rate Gap
	OLS	2SLS	RF	FS	OLS	2SLS	RF	FS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Explanatory and Instrumental Variabes in:							
	Level				Decile			
Civilian Rate Gap (Rich - Poor)	0.292	1.642			0.007	0.025		
	(0.175)	(0.970)			(0.004)	(0.014)		
	[0.115]	[0.091]			[0.107]	[0.076]		
Lottery Number Gap (Rich - Poor)			2.225	1.355			0.010	0.415
			(0.742)	(0.748)			(0.005)	(0.169)
			[0.009]	[0.090]			[0.064]	[0.027]
Weak IV Robust p		0.052				0.048		
Kleibergen-Paap F		3.29				6.05		
Mean DV	0.199	0.199	0.199	0.066	0.199	0.199	0.199	4.715
N	1851	1851	1851	1851	1851	1851	1851	1851

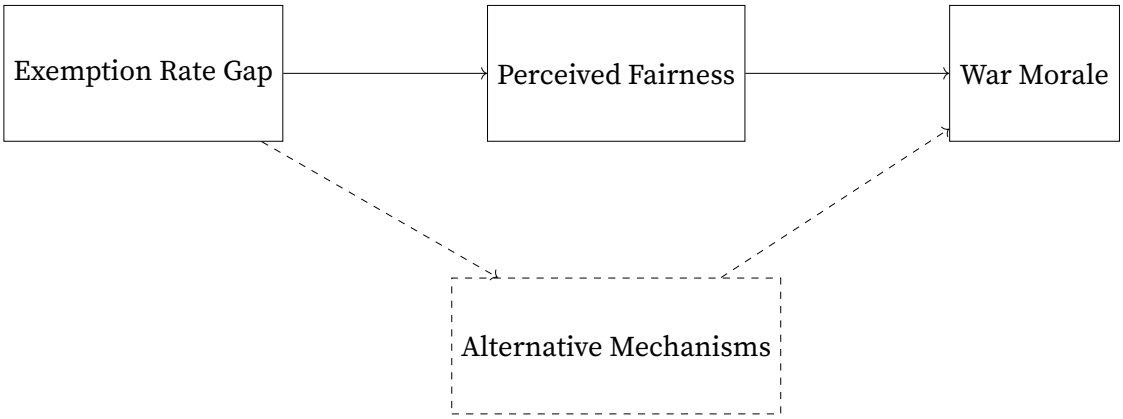
Notes: Respondent-level regressions. Columns 2 and 6 shows the 2SLS estimate. The dependent variable (except Columns 4 and 8) is an indicator for a negative response to the question: 'Do you think the draft is being handled fairly in your community?' In Columns 1-4, the explanatory variable is the civilian rate gap, which is the difference in the share of individuals who did not serve in the US Army between the top and bottom 10 percent of the income distribution in a state (i.e., the rich and poor). The instrumental variable is the lottery number gap: the difference in average lottery numbers between the rich and poor in a state. I employ a matching strategy to increase the precision of the 2SLS estimate. First, I use one-to-one nearest neighbor propensity score matching to create a sample where respondents are balanced on race, gender, education, urban status, income, birth year, and survey date between states with above- and below-median values of the instrument. Second, I run regressions on this matched sample, controlling for these same characteristics. Columns 5-8 use decile ranks for both the endogenous variable and the instrument. Columns 1 and 5 report the OLS estimate, Columns 3 and 7 present the reduced form estimate, and Columns 4 and 8 show the first stage. Standard errors, clustered at the state level, are in parentheses; corresponding p-values are in brackets. I also report the p-value of the 2SLS estimate on the civilian rate gap based on the weak-instrument-robust Anderson-Rubin test.

Figure A.17: Error in Variables Estimates



Notes: The figure plots estimates of the impact of the civilian rate gap on perceived unfairness (y-axis) in Table A.15 against different levels of assumed measurement reliability for the gap variable (x-axis). The point at 100% reliability corresponds to the baseline OLS estimate, which assumes no measurement error. The dashed, horizontal blue line corresponds to the 2SLS estimate, which corrects for measurement error using an instrument.

Figure A.18: Illustration of Mechanisms



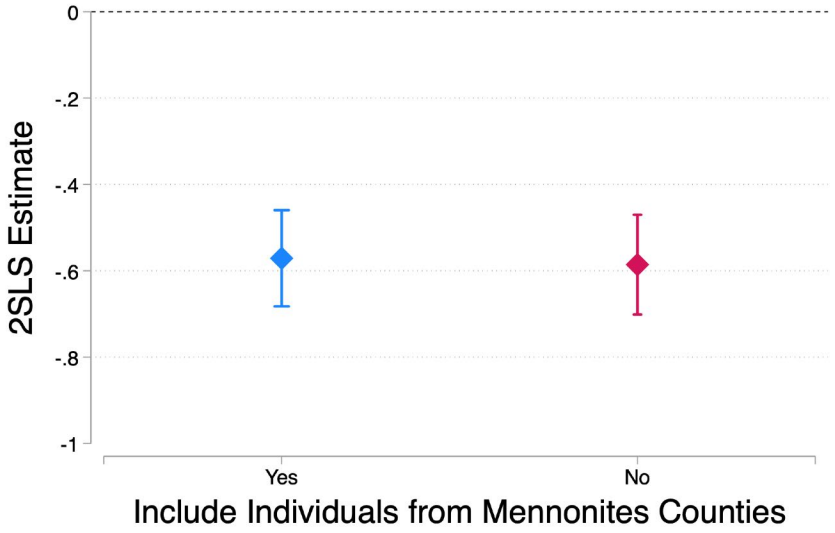
Notes: Alternative mechanisms include those discussed in Section 5.4.

Figure A.19: Mennonites in Kansas



Notes: A map on Mennonite congregations and institutions in Kansas. Source: Mennonite Library and Archives, Bethel College, North Newton, Kansas.

Figure A.20: Treatment Effects by Excluding Counties with Concentrated Mennonite Populations



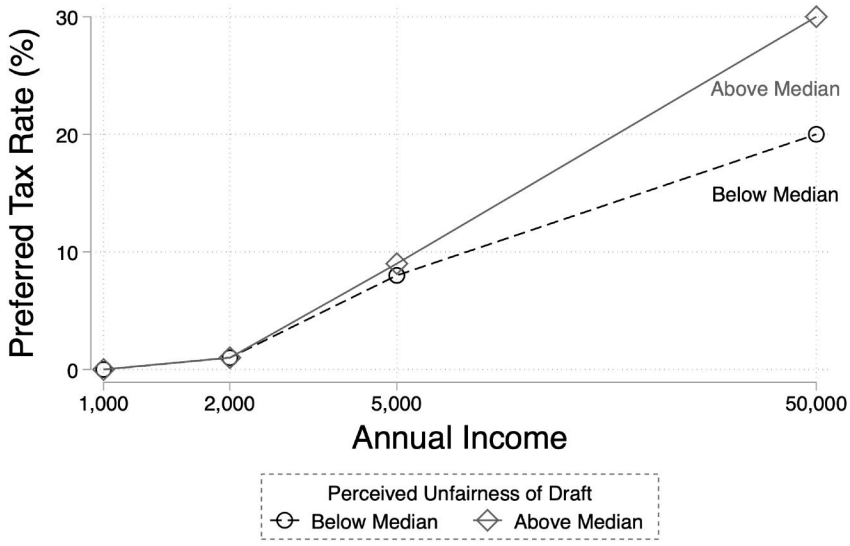
Notes: 2SLS coefficients of the exemption rate gap on war morale from the specification in Column 3, Panel A of Table 5, by excluding counties with concentrated Mennonite populations (Butler, Harvey, Marion, McPherson, and Reno).

Table A.16: Heterogeneous Treatment Effects by Share of Population Born in the Axis Powers

	First PC of Volunteering and -Draft Appeal		
	2SLS	2SLS	2SLS
	(1)	(2)	(3)
Exemption Rate Gap Neighbors	-0.624	-0.577	-0.589
(Richer - Poorer)	(0.096)	(0.064)	(0.094)
Exemption Rate Gap Neighbors $\times$ X	5.903	-20.902	0.156
(Richer - Poorer)	(11.475)	(12.227)	(11.199)
X is the share of population born in	Germany	Italy	Axis
N	59,658	59,658	59,658

Notes: 2SLS coefficients of the exemption rate gap and its interaction with the county-level share of population born in Germany, Italy, or the Axis powers on war morale from the specification in Column 3, Panel A of Table 5. Standard errors are in parenthesis.

Figure A.21: Preferred Income Tax Schedules by the Share of Perceptions of Draft Unfairness



Notes: Median preferred tax rate at different annual income levels by the above- or below-median share of individuals perceiving the draft system as unfair in a state.

Table A.17: Effects of Civilian Rate Gap on Preferred Income Tax Schedules

	Preferred Tax Rate for a Family of Four with an Annual Income of ...				Tax Progressivity Index
	$1,000	$2,000	$5,000	$50,000	Overall
	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)
Civilian Rate Gap	-3.787	-9.385	60.139	237.327	0.005
(Rich - Poor)	(3.233)	(7.428)	(46.117)	(137.121)	(0.003)
	[0.426]	[0.436]	[0.123]	[0.045]	[0.042]
Mean DV	0.640	3.164	10.946	30.631	0.001
Kleibergen-Paap *F*	3.96	3.96	3.96	3.96	3.96
N of States	13	13	13	13	13
N	317	317	317	317	317

Notes: Respondent-level 2SLS regressions. The dependent variables are the preferred average tax rates for different income levels (Columns 1-4) and the index of tax progressivity (i.e., the average percentage point change in the tax rate for each additional dollar of income between an income of $1,000 and $50,000) (Column 5). The explanatory variable is the civilian rate gap, which is the difference in the share of individuals who did not serve in the US Army between the top and bottom 10 percent of the income distribution in a state (i.e., the rich and poor). The instrumental variable is the lottery number gap: the difference in average lottery numbers between the rich and poor in a state. I employ a matching strategy to increase the precision of the 2SLS estimate. First, I use one-to-one nearest neighbor propensity score matching to create a sample where respondents are balanced on race, gender, education, urban status, income, birth year, phone ownership, and car ownership between states with above- and below-median values of the instrument. Second, I run 2SLS regressions on this matched sample, controlling for these same characteristics. Standard errors, clustered at the state level, are in parentheses. *P*-values, reported in brackets, are based on weak-instrument-robust Anderson-Rubin tests.

Table A.18: Unfairness of War Mobilization and Trust

	Approval Roosevelt	Trust in Govt for War	Interpersonal Trust
	OLS	OLS	OLS
	(1)	(2)	(3)
Unfairness from the Rich	0.003	-0.050	-0.061
	(0.018)	(0.014)	(0.020)
Controls	Yes	Yes	Yes
Mean DV	0.859	0.812	0.754
N	2079	2079	2079

Notes: The results are based on Gallup Survey 813, conducted on March 26, 1942. The key regressor is an indicator of whether the respondent believed that rich people were taking unfair advantage of the war. The regression was run on three different outcome variables. Column 1: The outcome is an indicator of whether the respondent approved of the way President Roosevelt was handling his job. Column 2: The outcome is an indicator of whether the respondent thought the government was doing a good job dealing with national defense and war-related problems. Column 3: The outcome is an indicator of whether the respondent believed most people could be trusted. The models include controls for meritocratic views (i.e., whether success depends mostly on ability, luck, or connections), parents' countries of origin, church membership, having voted for Roosevelt, car ownership, phone ownership, age, income class, urban status, education, gender, race, and the interview location. Standard errors, clustered at the state level, are reported in parentheses.

B. Appendix: Data Linkage and Sample Construction

I link the Registration Cards to the complete count 1940 Census using the matching algorithm developed by Abramitzky et al. (2021). The Registration Cards comprise the universe of men of draft age in Kansas. The 1940 Census covers all individuals residing in the US. I rely on the following variables available in both data sets: first name, last name, birth year, birth state, race, and county of residence in 1940.

The matching process proceeds in two steps. First, I match individuals across the two data sets based on first name, last name, birth state, race, and birth year.[1] This step successfully links 47.68% of the Registration Cards to the census. Second, I match the remaining unmatched entries using first name, last name, county of residence, race, and birth year. This step recovers an additional 7.30% of the sample.

In total, I link 54.98% of the entries in the Registration Cards to the 1940 Census. Table 2 compares summary statistics for the matched and unmatched samples. The two groups are comparable across eight out of nine observable characteristics. However, matched individuals are more likely to be white.

It is important to note that off-the-shelf crosswalks, such as those provided by the Census Tree or the Multigenerational Longitudinal Panel (MLP), are not applicable in this context. Those resources link individuals across census years (e.g., 1930 to 1940), whereas this study requires linking the 1940 Census to a newly digitized dataset of draft cards.

C. Appendix: Newspaper Data and LLM Classifications

I construct the newspaper dataset by first collecting page identifiers from newspapers.com for the period from November 1, 1940, to August 15, 1945.[2] I restrict the search using the keyword query "draft OR deferment OR exemption." Because the website limits queries to three keywords, I select "draft" to capture the broadest set of relevant content. I include "deferment" and "exemption" to identify reports regarding the delay or avoidance of service specifically, which often raise questions of systemic fairness. I conduct the query county by county.

I then use these page identifiers to access the OCR text for each page. My scrapers visit "www.newspapers.com/newspage/{ID}/" by replacing {ID} with a specific identifier, such as the page 978496586, and download the full text for the identified pages without a premium subscription. Eventually, I create a dataset comprising 11,281 pages across 195

[1] I allow for a discrepancy of up to one year in birth year.

[2] Geographical coverage in *Chronicling America* is limited for the 1940s.

counties where lottery numbers are also available in Chen (2025) (58 pages per county).

With these data, I use GPT-4.1 mini, a large language model (LLM) provided by OpenAI, to classify sentiment: specifically, whether a given page would lead a reader to perceive the military draft as fair or unfair. This classification approach involves two important methodological choices. First, I do not attempt to identify the personal intent of the author. Instead, I treat newspapers as instruments of *persuasion* (DellaVigna and Gentzkow, 2010), focusing on how a reader might update their perception of fairness after receiving the information. For example, a factual report on draft avoidance by the rich may lead a reader to conclude that the system is unfair, even if the author did not intend to condemn the behavior. Second, I utilize the "theory of mind" capabilities of modern LLMs to predict reader reactions. Strachan et al. (2024), Kosinski (2024), and Chen et al. (2025) show that frontier LLMs can make educated guesses about what is going on in someone else's head, allowing them to understand and predict behavior across different contexts. In my implementation, I instruct the LLM to role-play as a 20-year-old American man at the time of publication—a member of the group deciding whether to volunteer for the military, file a draft appeal, or exert effort on the battlefield—and predict his response to the content.

I use the following prompt for this classification:

> You are a 20-year-old American man in {`publication_year`}. Before reading this article, you had no strong opinion about whether the draft system is fair.
>
> Now, after reading the article below, answer this question: "Does this article make you more likely to think the military draft is being handled fairly or unfairly?"
>
> Choose ONE:
>
> - "MORE FAIR": The article makes me lean toward thinking the draft is fair.
> - "MORE UNFAIR": The article makes me lean toward thinking the draft is unfair.
> - "NO CHANGE": The article didn't shift my view either way.
>
> Respond in JSON format:
>
> {{
> "answer": "[MORE FAIR / MORE UNFAIR / NO CHANGE]",
> "confidence": [1-5],
> "reasoning": "In one sentence, what in the article influenced your view?",
> "evidence": "Exact short quote (5–15 words) that best supports your answer"
> }}
>
> [Article Content]

```
{article_text}
```

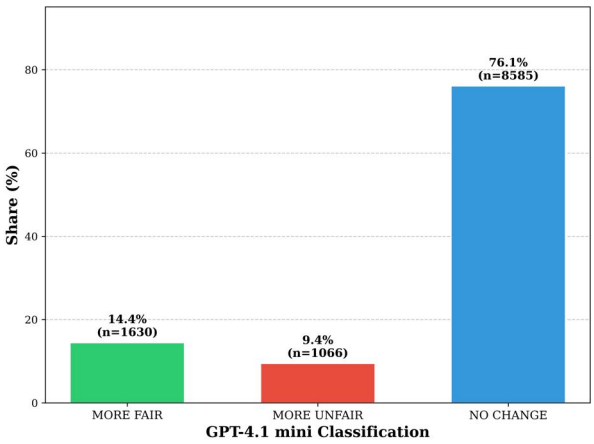
I update the `publication_year` and `article_text` variables for each query. I explicitly ask the LLM to adopt a blank-slate perspective by assuming "no strong opinion" beforehand. This prevents the model from relying on pre-existing historical biases and ensures that the classification reflects the marginal impact of the specific text provided.

The model provides four variables: the directional answer, a confidence score, a reasoning statement, and supporting evidence. Requiring a reasoning statement and an exact short quote from the text is a crucial step for grounding the model's output. This process forces the LLM to perform a deeper semantic analysis and ensures the classification is anchored in the provided article text rather than hallucinatory patterns (see Lagakos et al. (2025) for current practices on using LLMs to improve classification reliability).

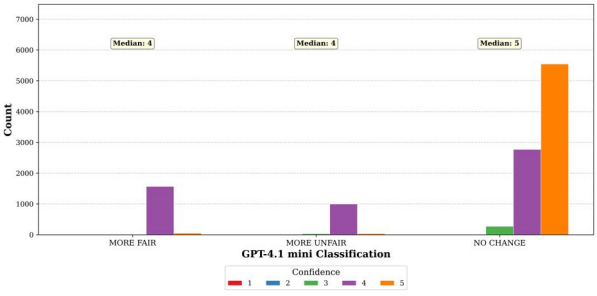
C.1 GPT Classifications

I find that GPT classifies 14.4% of the articles as "more fair," 76.1% as "no change," and 9.4% as "more unfair" (see Figure C.1A). The model generally assigns high confidence scores to its predictions. For the "more fair" and "no change" categories, the median confidence score is 4. For the "more unfair" category, the median score is 5 (see Figure C.1B).

Figure C.1: GPT Classification



(A) Distribution of LLM Classifications Across Newspaper Articles



(B) Distribution of LLM Confidence Scores by Classification Category

Notes: Panel A: This figure displays the share of newspaper articles in each classification category as determined by GPT-4.1 mini. Panel B: This figure shows the distribution of GPT-4.1 mini's self-reported confidence scores (1–5 scale, where 5 indicates highest confidence) for each classification category.

C.2 Examples

Table C.1 presents three examples from the dataset that GPT classifies as more unfair, more fair, or no change. Each entry in the first column contains a link to the original page on newspapers.com. For each example, I also report GPT's confidence score, its reasoning, and the short quote that best supports the classification.

Table C.1: Examples of GPT-4.1 Mini Classification of Newspaper Articles

Newspage	Answer	Conf.	Reasoning	Evidence
978496586	MORE UNFAIR	4	The article details multiple instances of draft deferments and preferential treatment due to political influence, highlighting unfairness in the system.	"long series of draft deferments for the young men who did so much flag-waving on Capitol Hill... Congressman May of Kentucky went over the head of the local draft board and got him deferred"
538114807	MORE FAIR	4	The article highlights a draft board enforcing induction equally, denying deferment even for a well-connected and brilliant young man, indicating fairness in the draft process.	"It was concerned with getting him into the army, just like any other man's son"
554540495	NO CHANGE	5	The article primarily discusses draft administrative details and potential policy changes without expressing fairness or unfairness.	"Draft officials said they had already put fathers on the floor of the barrel...but this step by itself would have little effect."

Notes: This table presents representative examples of articles classified by GPT-4.1 mini into three categories: "MORE UNFAIR," "MORE FAIR," and "NO CHANGE."

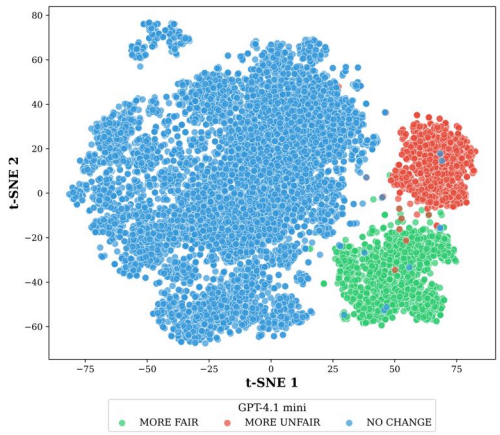
C.3 Validation of GPT Classifications

I validate the GPT classification using three methods. First, I evaluate whether the reasoning provided by the LLM is consistent with its classification. Second, I compare the results with classifications from Claude Haiku 4.5, an LLM developed by Anthropic. Third, I compare the model outputs with manual classifications performed by three human annotators. The second and third exercises rely on a sample of 30 pages, comprising 10 pages from each GPT category: "more fair," "more unfair," and "no change."

C.3.1 Consistency of GPT Classification and Reasoning

I assess whether the reasoning provided by GPT is informative of its classification or merely symbolic output unrelated to the decision. To do so, I convert the GPT reasoning text of each page into an embedding using the NLI-MPNet-base-v2 sentence transformer model. I then employ t-SNE to reduce the dimensionality of these embeddings to a two-dimensional space. Figure C.2 plots the projected embeddings for each GPT reasoning text, colored by GPT classification. The clustering patterns indicate that pages within the same GPT category tend to share semantically similar GPT reasoning texts. This supports the internal consistency of the LLM's classification logic.

Figure C.2: t-SNE Visualization of LLM Reasoning Text Embeddings by Classification



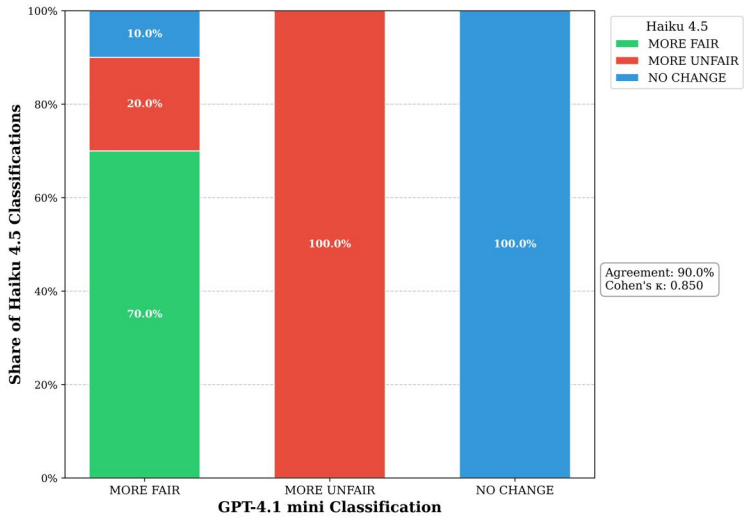
Notes: This figure displays a two-dimensional t-SNE projection of sentence embeddings derived from GPT-4.1 mini's reasoning text for each classified page. Embeddings are computed using the NLI-MPNet-base-v2 sentence transformer model, which is trained on natural language inference tasks to capture semantic relationships. Each point represents one page, colored by its classification by GPT: MORE FAIR (green), MORE UNFAIR (red), and NO CHANGE (blue). Perplexity = 30; random seed = 1234.

C.3.2 Cross-Validation with Claude Haiku 4.5

Figure C.3 compares the classifications from Claude Haiku 4.5 against the categories assigned by GPT. Among the 10 pages GPT identifies as "more fair," Claude agrees with the label in 70% of cases. For the 20 pages GPT classifies as "more unfair" or "no change," the two models reach identical conclusions for all. Overall, GPT and Claude agree on 90% of the classifications in this subsample.

To quantify this agreement beyond what might occur by chance, I calculate the Cohen's kappa coefficient (κ). This statistic ranges from 0 to 1, where 1 indicates perfect agreement and 0 represents agreement no better than random guessing. For these data, the coefficient is 0.85. This value indicates substantial agreement between the two models and suggests that the classification results are robust if I use a different LLM architecture.

Figure C.3: Cross-Validation of LLM Classifications: GPT-4.1 mini vs. Claude Haiku 4.5



Notes: This figure shows the cross-validation of newspaper article classifications between GPT-4.1 mini and Claude Haiku 4.5. Each bar represents the share of Haiku classifications conditional on GPT's classification. A random sample of 10 articles per GPT classification category (N=10 each for MORE FAIR, MORE UNFAIR, and NO CHANGE) was classified by Haiku using identical prompts.

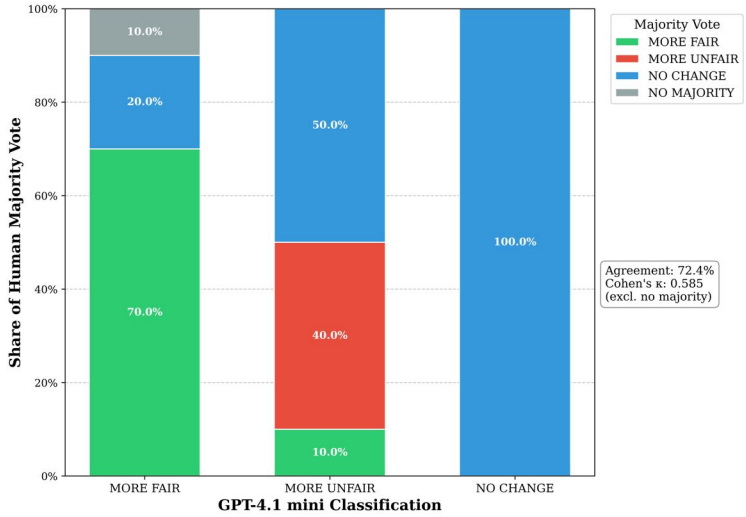
C.3.3 Cross-Validation with Human Coding

To validate the GPT classification against human coding, I recruited three human annotators. I provided them with the same instructions given to GPT without telling them GPT classifications. I used the identical set of 30 pages from the Haiku cross-validation task (10 pages for each GPT classification).

Figure C.4 compares GPT outputs with the majority vote of human classifications. First, the human majority and GPT agree on 72.4% of pages, and the Cohen's kappa coefficient is 0.585.

Second, conditional on GPT identifying a directional change ("more unfair" or "more fair"), the human majority rarely selects the opposite direction. Specifically, for pages classified as "more unfair" by GPT, the human majority identifies 90% of them as either "more unfair" (40%) or "no change" (50%). Similarly, for pages classified as "more fair" by GPT, the human majority categorizes them as either "no change" (20%), "more fair" (70%), or yields no majority vote (10%).

Figure C.4: Cross-Validation of GPT Classifications with Human Annotators



Notes: This figure shows the cross-validation of newspaper article classifications between GPT-4.1 mini and human majority voting from three independent annotators. Each bar represents the share of human classifications conditional on GPT's classification. A random sample of 10 articles per GPT classification category (N=10 each for MORE FAIR, MORE UNFAIR, and NO CHANGE) was classified by three annotators using identical instructions for GPT.

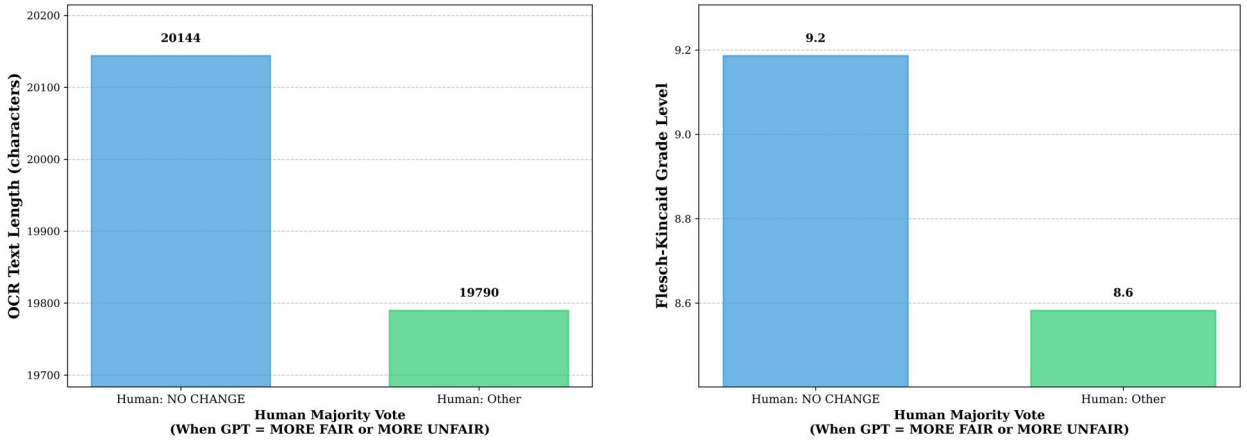
Third, in nearly all cases of disagreement, the human majority selects "no change," whereas GPT selects a directional category. While the validation task has one-third of "no change" pages by GPT, the human majority assigns 57% of the pages to this category.

Why do human annotators favor the "no change" classification? High textual complexity may lead human coders to select the neutral option, a cognitive constraint that does not affect the LLM.

To test the hypothesis, I compute the total character count for each page's OCR text. I then compare the text length between cases where the human majority voted "NO CHANGE" and those with other outcomes, conditional on GPT classifying the article as "MORE FAIR" or "MORE UNFAIR." Pages where the human majority voted "NO CHANGE" have 354 more characters on average than cases where the vote was directional or split (see Figure C.5A). This difference extends beyond overall length to sentence length and word complexity. In Figure C.5B, I compare the Flesch-Kincaid grade level—a readability score indicating the US school grade required to understand a text, calculated from sentence length and

word complexity. The average Flesch-Kincaid grade level for pages where the human majority voted "NO CHANGE" is 9.2, compared to 8.6 for cases where the vote was directional or split. Although the differences in character count and Flesch-Kincaid grade level are statistically insignificant due to the small sample size, the pattern is consistent with the explanation that increased textual complexity may drive human annotators toward the neutral option.

Figure C.5: Textual Complexity and Human Classification Decisions



(A) Text Length

(B) Flesch-Kincaid Grade Level

Notes: Panel A: This figure compares OCR text length measured by character count between articles where human majority voted "NO CHANGE" versus other outcomes (MORE FAIR, MORE UNFAIR, or NO MAJORITY). Sample restricted to articles where GPT-4.1 mini classified as MORE FAIR or MORE UNFAIR (n=20). Bar plots show the average level. Panel B: This figure compares Flesch-Kincaid grade level (a measure of text readability where higher values indicate more difficult text).

D. Appendix: Theoretical Framework

I develop a model of an individual's war contribution decision, focusing on how this choice is influenced by the military service and exemption patterns of his neighbors. This framework aims to disentangle several potential economic and psycho-social mechanisms.

D.1 The Agent's Decision

Consider an individual i who must decide whether to contribute to the war effort ($s_i = 1$) or not ($s_i = 0$). The agent's social environment is characterized by the actions of his richer ($g = r$) and poorer ($g = p$) neighbors. For each group g, a fraction e_g receives an exemption from service, while the remaining fraction $s_g = 1 - e_g$ serves. Exemptions can be either seemingly justifiable ($t = j$) or questionable ($t = q$), such that the total exemption rate for a group is $e_g = e_g^j + e_g^q$.

A key variable of interest is the difference in exemption rates between richer and poorer neighbors, which I define as $\Delta e = e_r - e_p$.[3]

The utility for individual i from choosing action s_i is given by:

$$U_i(s_i) = w_i(s_i) - c_i s_i + m_i s_i + \epsilon_{i,s_i} \tag{6}$$

where $w_i(s_i)$ is the monetary wage. I normalize the military wage to zero, $w_i(1) = 0$, and denote the civilian wage as $w_i(0) = w$. The term c_i represents the private cost of military service. The term m_i captures the net psycho-social utility derived from service. Finally, ϵ_{i,s_i} is an idiosyncratic preference shock associated with the choice.

An individual chooses to contribute if the utility from doing so exceeds that of not contributing, $U_i(1) > U_i(0)$. This choice is determined by the condition $m_i - c_i - w > \tilde{\epsilon}_i$, where $\tilde{\epsilon}_i \equiv \epsilon_{i,0} - \epsilon_{i,1}$. I define the deterministic component as the latent variable V_i^*:

$$V_i^* = m_i - c_i - w \tag{7}$$

The probability of $s_i = 1$, $\mathbb{P}(s_i = 1)$, is an increasing function of V_i^*.

My analysis decomposes V_i^*, particularly how the economic and psycho-social incentives shape an individual's propensity to contribute.

[3] Similarly, I can define $\Delta e^t = e_r^t - e_p^t$ for an exemption $t \in \{j, q\}$, so $\Delta e = \Delta e^j + \Delta e^q$.

D.2 Economic Incentives

Economic Incentive 1 (Labor Supply Effect)**.** *Assuming poorer neighbors supply unskilled labor, their higher service rate (s_p) contracts the local labor supply. This raises the equilibrium civilian wage (w), increasing the opportunity cost of service for an unskilled individual i. Per Equation 7, a higher w lowers the net utility from service, V_i^*, thus reducing the probability that $s_i = 1$. This channel implies that $\frac{\partial \mathbb{P}(s_i=1)}{\partial s_p} < 0$ and $\frac{\partial \mathbb{P}(s_i=1)}{\partial s_r} = \frac{\partial \mathbb{P}(s_i=1)}{\partial e_r} = 0$.*

Economic Incentive 2 (Labor Demand Effect)**.** *Assuming richer neighbors are employers, their higher service rate (s_r) contracts the local labor demand. This lowers the equilibrium civilian wage (w), reducing the opportunity cost of service for an employee i. Per Equation 7, a lower w raises the net utility from service, V_i^*, thus increasing the probability that $s_i = 1$. This channel implies that $\frac{\partial \mathbb{P}(s_i=1)}{\partial s_r} > 0$, $\frac{\partial \mathbb{P}(s_i=1)}{\partial e_r^q} = \frac{\partial \mathbb{P}(s_i=1)}{\partial e_r^j} < 0$, and $\frac{\partial \mathbb{P}(s_i=1)}{\partial s_p} = \frac{\partial \mathbb{P}(s_i=1)}{\partial e_p} = 0$.*

D.3 Psycho-Social Incentives

Psycho-Social Incentive 1 (Perceived Unfairness)**.** *Perceived unfairness in the conscription system can affect an individual's psycho-social utility from service. I model this by assuming utility is decreasing in the exemption of the rich, e_r, with a greater sensitivity to questionable exemptions (e_r^q). This fairness component of psycho-social utility is given by $m_i^{fairness} = -\theta_i(\tau e_r^q + e_r^j)$, where $\theta_i \geq 0$ is the agent's preference for fairness and $\tau > 1$. A larger exemption of the rich lowers the overall psycho-social utility, m_i. Per Equation 7, a decrease in m_i lowers the net utility from service, V_i^*, thus reducing the probability that $s_i = 1$. This channel implies that $\frac{\partial \mathbb{P}(s_i=1)}{\partial e_r} < 0$, $|\frac{\partial \mathbb{P}(s_i=1)}{\partial e_r^j}| < |\frac{\partial \mathbb{P}(s_i=1)}{\partial e_r^q}|$, and $\frac{\partial \mathbb{P}(s_i=1)}{\partial e_p} = 0$.*

Psycho-Social Incentive 2 (Leadership Effect)**.** *An individual's decision may be influenced by emulating the behavior of richer neighbors, who are perceived as community leaders. This effect on psycho-social utility is based on the observed service rate of the richer group (s_r) and is modeled as $m_i^{leadership} = \lambda_i s_r$, where $\lambda_i \geq 0$ reflects the agent's propensity for emulation. A higher service rate among the rich provides a positive signal, increasing an agent's overall psycho-social utility, m_i. Per Equation 7, an increase in m_i raises the net utility from service, V_i^*, thereby increasing the probability that $s_i = 1$. This channel implies that $\frac{\partial \mathbb{P}(s_i=1)}{\partial s_r} > 0$, $\frac{\partial \mathbb{P}(s_i=1)}{\partial e_r} < 0$, $|\frac{\partial \mathbb{P}(s_i=1)}{\partial e_r^j}| = |\frac{\partial \mathbb{P}(s_i=1)}{\partial e_r^q}|$, and $\frac{\partial \mathbb{P}(s_i=1)}{\partial s_p} = \frac{\partial \mathbb{P}(s_i=1)}{\partial e_p} = 0$.*

Psycho-Social Incentive 3 (Racism)**.** *Racial prejudice can negatively affect an agent's psycho-social utility from contributing to the war effort. If an agent harbors animus towards minority groups who are disproportionately represented among poorer neighbors, his motivation to serve*

may decrease as their service rate (s_p) rises. I model this as a component of psycho-social utility, $m_i^{racism} = -\delta_i s_p$, where $\delta_i \geq 0$ captures the agent's level of prejudice. A higher service rate among the poorer group decreases the agent's overall psycho-social utility, m_i. Per Equation 7, a decrease in m_i lowers the net utility from service, V_i^*, thus reducing the probability that $s_i = 1$. This channel implies that $\frac{\partial \mathbb{P}(s_i=1)}{\partial s_p} < 0$, $\frac{\partial \mathbb{P}(s_i=1)}{\partial e_p} > 0$, and $\frac{\partial \mathbb{P}(s_i=1)}{\partial s_r} = \frac{\partial \mathbb{P}(s_i=1)}{\partial e_r} = 0$.

D.4 Summary of Predictions

My theoretical framework yields a clear set of falsifiable predictions for each of the five mechanisms, as summarized in Table D.1. The table shows the predicted effect of a marginal increase in each type of neighbor exemption on an individual's probability of serving, $\mathbb{P}(s_i = 1)$. A key insight is that while three channels—Perceived Unfairness, Leadership, and Labor Demand—predict that exemptions among the rich decrease an individual's propensity to serve, only *Perceived Unfairness* predicts a stronger negative reaction to questionable exemptions (e_r^q) than to justifiable ones (e_r^j). This differential effect provides the primary empirical test for my central mechanism. Furthermore, the model distinguishes these channels from the Labor Supply and Racism mechanisms, which are exclusively sensitive to the actions of poorer neighbors.

Table D.1: Predicted Effects of Exemptions on Probability of Serving ($\mathbb{P}(s_i = 1)$)

	Richer Neighbor Exemptions		**Poorer Neighbor Exemptions**	
Mechanism	Justifiable (e_r^j)	Questionable (e_r^q)	Justifiable (e_p^j)	Questionable (e_p^q)
Psycho-Social:				
Perceived Unfairness	–	$-\tau$ (Stronger)	0	0
Leadership Effect	–	–	0	0
Racism	0	0	+	+
Economic:				
Labor Demand Effect	–	–	0	0
Labor Supply Effect	0	0	+	+

Notes: $-\tau$ (where $\tau>1$) denotes a negative effect whose absolute value is larger than the baseline.