

Reversing Reserves

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Abstract

Affirmative action policies are often implemented through reserve systems. We contend that the functioning of these systems is counterintuitive, and that the consequent misunderstanding leads individuals to support policies that ineffectively pursue their goals. We present 1,013 participants in the Understanding America Study with incentivized choices between reserve policies that vary in all decision-relevant parameters. Many subjects' choices are rationalized by a *nearly* correct decision rule, with errors driven solely by the incorrect belief that reversing the processing order has no effect. The prevalence of this belief helps to explain otherwise surprising decisions made in field applications of reserve systems.

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Market organizers commonly seek to advantage some group in the course of an assignment procedure. Seats at a school may be granted according to a lottery, but with the desire to admit a comparatively large fraction of local students. Positions at a job may be granted according to a measure of merit, but with easier access granted to members of historically underrepresented groups. Visas may be granted in the order that applications are received, but with some desire to prioritize the allocation of visas to the most educated applicants.

Distributional goals like these are often pursued through the deployment of a **reserve system**. In such a system, some of the objects being allocated are reserved for the group targeted for preferential treatment. When reserve slots are processed, the members of the targeted group with the highest priority receive them. When unreserved slots are processed, members of any group (targeted or otherwise) are admitted in order of their priority.

Despite their prevalence, the theoretical performance of reserve systems has only recently been thoroughly explored. As has become clear in this recent work (see, e.g., Dur et al., 2018; Dur, Pathak and Sönmez, 2019; Pathak, Rees-Jones and Sönmez, 2020), the functioning of these systems is complex. With this complexity comes the possibility that constituencies adopting reserve systems might not fully understand them, opening the possibility of adopting systems that run counter to the constituency’s stated goals. And indeed, as we discuss in Section 2, in several of the large-scale applications of these systems—such as in Boston’s public school system and in the H-1B visa system for U.S. immigration—key stakeholders have come to support policies that are comparatively ineffective at advancing the admission of their own constituency. These stakeholders additionally fail to detect cases when administrators unilaterally change or neglect to specify important features of the system—with the administrator’s decisions at times driven by confusion as well.

In this paper we consider a simple hypothesis: that misguided preferences over reserve policies are largely driven by a specific form of misunderstanding. We are motivated by the belief that the importance of *processing order* in reserve systems is highly counterintuitive. As is shown in Dur et al. (2018), the order in which positions are processed influences the degree of advantage conferred to the target group in a manner of comparable importance to the number of positions that are reserved. By processing reserve seats last, the same degree of affirmative action can be achieved with many fewer positions reserved. Constituencies

that do not appreciate this fact could deploy reserve systems in a manner that significantly blunts the degree of affirmative action achieved by a reserve of a fixed size.

To test this hypothesis, we deployed a preregistered online experiment to 1,013 members of a nationwide survey panel that is approximately representative on a broad range of demographic variables. In this experiment, subjects faced simple scenarios mirroring two high-profile applications of reserve systems: allocation of seats at a high school or allocation of work visas. In the scenarios, subjects are members of a group that will have positions reserved. Subjects face financial incentives to maximize the chance that their admission is attained in a simulation. They then choose how they would like the reserve system to be administered, selecting from pairs of policies that differ in the both the *number* of seats reserved and in the *order* that the reserve seats are processed.

Our experiment was designed to reveal the rate at which subjects adopt several competing decision rules. First, our design identifies the rate at which subjects choose between policies optimally, adopting a decision rule that correctly accounts for both the number of reserve seats and their processing order. Second, our design identifies the rate at which subjects adopt a decision rule that reflects more reserve seats being better but which treats processing order as irrelevant. Third, our design identifies the aggregate rate at which subjects adopt all other decision rules.

Our results illustrate that subjects often miss the importance of processing order. Our primary estimates suggest that 3% (*s.e.* = 2pp) of subjects apply the optimal decision rule; we are unable to reject the hypothesis that the optimal decision rule is never applied. In contrast, we estimate that 40% (*s.e.* = 2pp) of subjects adopt a decision rule that responds to reserve size but treats processing order as irrelevant. The widespread adoption of this decision rule helps explain the frequency of experimental decisions that are not payoff maximizing for subjects.

Beyond documenting the prevalent belief that processing order does not matter, we also document an important correlate of this belief: cognitive ability. Perhaps surprisingly, subjects with higher education, subjects with higher performance on cognitive ability tests external to our survey, and subjects with a higher performance on comprehension tests within our survey all show a greater likelihood of adopting our misguided decision rule of interest. This

contrasts with a common finding in the behavioral market design literature that misreaction to matching-mechanisms' incentives is more prevalent among those of lower cognitive ability (see, e.g., Basteck and Mantovani, 2018; Rees-Jones, 2018; Rees-Jones and Skowronek, 2018; Shorrer and Sóvágó, 2018; Rees-Jones, Shorrer and Tergiman, 2020; Hassidim, Romm and Shorrer, 2020). In this instance, however, the finding may be rationalized by noting that adoption of this decision rule reflects a general understanding of incentives in this procedure. Our decision rule of interest is *almost* sophisticated, missing one subtle component of large ultimate importance.

This paper builds on a long tradition of using lab-experimental methods to test for understanding of matching mechanisms (see, e.g., Chen and Sönmez, 2006; Calsamiglia, Haeringer and Kljin, 2010; Echenique, Wilson and Yariv, 2016; Rees-Jones and Skowronek, 2018).¹ Within this literature, these findings reinforce a growing body of work showing large potential for misunderstanding. While clear and transparent explanation of a matching procedure is often thought to be sufficient for widespread understanding to arise, our results suggest that this is insufficient in reserve systems. These findings mirror similar results showing that misunderstanding of the deferred-acceptance or top-trading-cycles algorithms persists even in settings with substantial training and feedback (Ding and Schotter, 2017; Guillen and Hakimov, 2018; Rees-Jones and Skowronek, 2018). Of course, there remains some possibility that an effective method of training could be discovered, but the pursuit of this method in these related environments has seen limited success.

Two forces lead us to worry that eliminating the problem will be challenging. First, our findings with regard to cognitive ability clearly indicate that this is not analogous to a small, careless mistake that would easily be resolved by more careful thinking. Training people out of this mistake requires teaching them careful consideration of relatively subtle statistical selection problems—a class of problems that remains challenging even for the highly educated. Second, and relatedly, the individuals who run the market may often not understand the importance of these issues, or, worse yet, may be actively incentivized to foster misunderstanding. In such cases, reliance on the internal provision of training and

¹For a recent review of experimental examinations of matching markets, see Hakimov and Kübler (2019). For a recent review of the interaction between market design and behavioral economics, see Chen et al. (2020).

advice will clearly be insufficient to ensure that the final policy adopted efficiently pursues the goals of the populace adopting it. In the conclusion, we further discuss these issues and some potential means to overcome them.

The paper proceeds as follows. In Section 1 we present a brief review of the theory of reserve systems. In Section 2 we describe several field applications in which the complete understanding of reserve systems appears suspect. In Section 3 we formally present our candidate models of decision rules and our econometric strategy for identifying their rate of adoption. In Sections 4 and 5 we describe the design and deployment of our experiment. In Section 6 we present results. In Section 7 we conclude.

1 Theoretical Importance of Reserve Order

In this section, we briefly present existing theoretical results on the functioning of reserve systems. This summary primarily draws upon the work of Dur et al. (2018).

1.1 Decision Environment

Consider a setting in which some number of objects must be allocated. For concreteness, say the objects to be assigned are seats at a school. The school has n seats available, and in the absence of reserve considerations would assign these seats to applicants according to a linear priority order (for example, outcomes of a standardized test or results from a lottery). However, this school wishes to provide some advantage to a particular group of applicants. Call this group the reserve applicants. Call those outside of this group the general-category applicants.

To help advantage the reserve applicants, the school labels n_r of their n seats as reserved seats (with $0 < n_r < n$). The remaining $n - n_r$ seats are open seats.

To determine the assignment of seats at the school, the school fills seats sequentially one at a time. When processing an open seat, the school admits the student with the highest priority among all those not yet admitted. Reserve-category status is not considered. When processing a reserved seat, the school admits the reserve applicant with the highest priority among all those not yet admitted. General-category applicants are ineligible for these seats.

To fully specify the assignment procedure, the sole remaining requirement is to specify the processing order for reserved and open seats. Conceptually, any permutation is possible: one could process one reserved seat, followed by seven open seats, followed by two reserved seats, and so on. In practice, however, these systems are commonly administered in one of two configurations: processing all reserve seats either prior to all open seats or after all open seats. We will restrict attention to these two extremal policies.

1.2 Comparative Statics of Interest

In a system like that just specified, two key comparative statics govern the degree of advantage conferred to the reserve group.

Seat-number comparative static: Hold fixed the priority order and the processing order. Increasing the number of reserved seats weakly increases the number of admitted reserve students.

The seat-number comparative static captures an obvious and intuitive determinant of assignments: saving more seats for a group helps the group. While some may harbor the intuition that this is the *only* relevant comparative static, a second more subtle comparative static follows from the work in Dur et al. (2018).

Processing-order comparative static: Hold fixed the priority order and the number of reserved seats. Switching from processing the reserved seats first to processing the reserved seats last weakly increases the number of admitted reserve students.

Two forces contribute to the result in the processing-order comparative static. The first is a selection effect. When reserved seats are processed last, reserve applicants are admitted in the first-stage processing of open seats at a rate determined by their distribution of priorities relative to general-category applicants. Except for differences in priorities, competition for the open seats is effectively a level playing field between the two groups. In contrast, when reserved seats are processed first, the highest-priority members of the reserve group are removed from the applicant pool before the processing of the open seats. The competition for open seats is therefore between all members of the general category and the comparatively

low-priority members of the reserve group, tilting admissions in favor of the general-category applicants.

The second force driving the processing-order comparative static is a composition effect. To illustrate, notice that when reserve seats are processed last, competition for the open seats is between all general category applicants and all reserve applicants. In contrast, when reserve seats are processed first, competition for open seats is between all general-category applicants and the reserve applicants with n_r group members already removed. In the latter situation, reserve applicants make up a smaller portion of the total applicant pool. As a result, even in the absence of selection effects, admissions are again tilted in favor of the general-category applicants.

These two forces result in benefits to the reserve applicants if reserved seats are processed last. Ultimately, the quantitative benefit from choosing this processing order depends on a variety of factors, including the number of members of each applicant group and the nature of the priority ordering used. As we will see in the following section, this impact has been large in several field applications of interest.

2 Motivating Field Environments

In this section we briefly review two of the field environments that motivate our study. In each of these environments a reserve system is used for an assignment procedure, with some evidence that at least some stakeholders appear to harbor misunderstanding of the importance of processing order.

2.1 The Boston Public School Match

In 1999 Boston Public Schools (BPS) abandoned its use of racial and ethnic criteria for school admissions, instead adopting a system that reserves half of each schools' seats for students from the neighborhood surrounding the school, known as the walk-zone.

Leading up to the adoption of the reserve system, different groups of parents, school officials, and involved community members advanced two opposing viewpoints. One viewpoint emphasized the importance of unrestricted school choice. Under this viewpoint, allowing

families to select the school that best suits their needs was critically important. Such a policy would be particularly valuable to families living near a low-performing school, granting them a means of escaping a bad default assignment. An alternative viewpoint emphasized the importance of neighborhood schooling. Under this viewpoint, drawing the student population from the school’s walk-zone benefits the local community and the students themselves. Such a policy would be particularly valuable to families living near a high-performing school, allowing them to avoid intense competition for seats by restricting the admission of non-local students.

Consideration of these two opposing viewpoints led to the reservation of 50 percent of seats for walk-zone students. The remaining seats were open to all. Public accounts of this policy described it as an “uneasy compromise between neighborhood school advocates and those who want choice” (Daley, 1999). And indeed, the superintendent’s memorandum presenting this policy explicitly described his desire to accommodate these two viewpoints, and his belief that the new policy “provides a fair balance” (BPS, 1999).

Ultimately, this reserve system was abandoned in 2013. This abandonment was motivated in part by the discovery that this system only minimally advanced the admission of walk-zone applicants. Because a 50-50 reserve split was incorrectly (but widely) perceived to be an accommodation to both sides, the superintendent advocated for the usage of a new system that would be “honest and transparent” (Johnson, 2013).

The understanding that this system was misleading arose due to the intervention of market designers. In the course of studying this reserve system, Pathak and Sönmez discovered that software code used to determine the final assignment processed all reserved seats before all open seats. By simulating the assignments that would have been achieved with different policies in the preceding years, they found that the 50% reserve resulted in minimal walk-zone advantage relative to a policy with zero seats reserved. These results were delivered in testimony to the Boston School Committee (Pathak and Sönmez, 2013), and minutes of subsequent meetings of the BPS Executive Action Committee acknowledged that the results described would constitute an “unintended consequence that is not in stated policy” (EAC, 2013).

In summary: at the time of the adoption of the reserve system following the 1999 reform,

processing order was neither discussed nor specified in the formal policy documents. With this component unspecified, a programmer’s arbitrary choice of processing order eliminated nearly all benefits meant to be conferred to walk-zone applicants. This elimination appears to have been unrecognized by advocates for walk-zone preferences for more than a decade, and led to rapid reform once it was discovered.

For further details, this reserve system and its history are documented in Dur et al. (2018). The overview above draws on this work.

2.2 U.S. H-1B Visa Assignment

The U.S. H-1B Visa program enables American companies to temporarily employ foreign workers with specialized knowledge. When this program was amended in the H-1B Visa Reform Act of 2004, a reserve system was adopted to help to promote the granting of visas to highly educated applicants. As specified by this legislation, 20,000 visas would be reserved specifically for applicants with qualifying advanced degrees in addition to the 65,000 visas that would be open to all eligible applicants.

While this legislation precisely specifies the number of reserve seats, it does not specify details of processing order. The specification of the number of reserve seats is consistent with legislators understanding the seat-number comparative static described in Section 1. Omitting the specification of processing order is consistent with either a lack of understanding of the processing-order comparative static, or with a desire to leave this dimension unspecified to give the administrators of the H-1B program a means of modifying the degree of advantage given to highly educated applicants (henceforth, “skill bias”) without need for congressional approval.

Consistent with the possibility of underappreciating the importance of processing order, the administration of this reserve system was modified several times in the years after its initial deployment. These changes were at least in part (and potentially entirely) motivated by logistical considerations; the fact that these reforms had large effects on the degree of skill bias was not publicized nor formally acknowledged.

At the time of first adoption of this reserve system, priority was determined by the time of receipt of the visa application. The agency tasked with the enactment of this policy, the

U.S. Customs and Immigration Service (USCIS), initially chose to implement the policy as reserves-first. This decision is perhaps surprising: as is documented in Pathak, Rees-Jones and Sönmez (2020), this version of implementation results in the lowest degree of skill bias of all policies that comply with the legislation. This decision contrasts with the stated intents of the legislation itself, which was explicitly to introduce skill bias into this system.

Despite this initial plan, passage of the relevant act occurred at a time when application processing was already well underway. The reserves-first implementation was therefore considered impossible to administer in the first year of the new regime, and as a result the reserve seats were processed last. This version of implementation results in the highest degree of skill bias of all policies that comply with the legislation (matched only by a later policy adopted in FY2020). This policy was applied for one year only (FY2005), before the reserves-first version was adopted for a window of three years (FY2006-2008).

Over this initial window of the new regime, seats began filling earlier and earlier in the application season. This became a critical concern by FY2008, when all open seats were filled by applications that arrived on the first day that petitions would be considered. This motivated the regime adopted in FY2009 under which arrival time was replaced by lottery numbers as a means of determining priority. In contrast to the other settings considered thus far, a separate priority (i.e., lottery number) was generated for the reserve seats and the seats open to all. This adjustment eliminates the selection effect induced by processing order described in Section 1, but not the composition effect. As such, the USCIS's decision to continue processing advanced-degree applications first preserved a comparatively lower degree of skill bias in this system.

This regime persisted until its recent modification by the Trump administration. In the 2017 *Buy American and Hire American Executive Order*, the administration instructed the USCIS to switch to a reserves-last system for the explicit purpose of maximizing the degree of skill bias. Upon its implementation in FY2020, this restored the degree of skill bias in the reserve system to that achieved in its very first year—the theoretically maximal degree possible of all policies that comply with the legislation. Unlike prior reforms, discussion of this policy in the Federal Register included consideration of the effect of processing order on skill bias, as well as discussion of the policy's legality.

Across this period of 15 years, four different regime changes were put into effect, each influencing the level of skill bias. The reform proposed in 2017 was explicitly enacted for the intent of increasing the share of H-1Bs granted to highly educated applicants; estimates suggest that this reform granted approximately 5,000 more of the fixed 85,000 H-1Bs to advanced-degree applicants (an increase of 16% to the rate of advanced-degree awards granted). While this change is indeed substantial, we note that both of the preceding reforms—enacted without explicit intent to affect skill bias and seemingly motivated by logistical considerations—had even larger effects. The change applied between FY2005 and FY2006 is estimated to have resulted in a reduction of 14,000 annual awards granted to advanced-degree applicants. The change applied between FY2008 and FY2009 is estimated to have resulted in an increase of 9,000 annual awards granted to advanced-degree applicants. Unlike the 2020 reform, the effect of these reforms on skill bias was not contested despite being more pronounced.

Given that changes to immigration policy are often fiercely contested in U.S. politics, we view the lack of discussion and debate of these earlier reforms as suggesting that their importance was not widely understood.

For further details, this reserve system and its history are documented in Pathak, Rees-Jones and Sönmez (2020). The overview above draws on this work.

2.3 Summary

Across these field applications we observe motivated groups of stakeholders supporting or enacting versions of reserve policies that appear in contrast with their stated goals. In each case, we believe the history of these policies supports the idea that confusion regarding the functioning of reserve systems impacted the manner in which they were deployed. Furthermore, these two cases are not alone. There is similar potential for confusion in the deployment of reserve systems for school admissions in Chicago (see Dur, Pathak and Sönmez, 2019) and in New York City (NYCDOE, 2019). And as we will further discuss in Section 7, such worries are also present in the constitutionally mandated reserve systems for school choice and government employment used in India.

While we believe that misunderstanding is widespread in these environments, we note that

in all cases mentioned above that claim is only speculative. This motivates our development of the experimental paradigm in this paper, aimed to directly measure understanding of these systems in a broad swath of the U.S. populace. Should misunderstanding of the importance of processing order be found to be prevalent in this population, it lends credence to the idea that citizens considering immigration policy or parents considering their children’s school assignment may fail to correctly assess these policies.

3 Identifying the Perceived Importance of Processing Order

In this section we present our empirical model for inferring understanding of reserve systems. The experiment that we present in the remainder of the paper was tailored for utilization of this empirical model.

Consider an individual (i) facing an assignment problem like that described in Section 1. This individual is a member of the group that qualifies for reserve seats. He is presented with two potential policies that could be applied to determine admissions: a “reserves-first” (RF) policy with s^{RF} reserve seats, and a “reserves-last” (RL) policy with s^{RL} reserve seats. Beyond seat numbers and processing order, all other features of decision environment are held fixed. The individual’s task is to choose between these two policies.

In this environment, the key objects of interest are individuals’ *choice functions*, denoted by $C : (s^{RF}, s^{RL}) \rightarrow [0, 1]$. Given an assigned number of reserve seats for both the RF and the RL policies ($(s^{RF}, s^{RL}) \in \mathbb{R}_+^2$), a choice function outputs the individual’s probability of indicating a preference for the RF policy. When holding fixed all other elements of the assignment problem, such a function completely characterizes an individual’s observable preferences. At times we will consider a choice function adopted by a specific individual, in which case it will be subscripted by i .

If the choice function were observed, it would provide a direct means of testing an individual’s understanding of the theory described in Section 1. For any given number of RL seats, there exists a threshold number of RF seats ($T^*(s^{RL})$) such that the RF policy will

be most favorable to the individual if and only if its number of reserve seats exceeds the threshold. An individual who correctly analyzes the environment and chooses the policy in his best interest would therefore adopt the choice function

$$C^*(s^{RF}, s^{RL}) = \begin{cases} 1 & \text{if } s^{RF} > T^*(s^{RL}) \\ 0 & \text{if } s^{RF} \leq T^*(s^{RL}) \end{cases} .$$

Adopting this choice function would serve as strong evidence in support of a sophisticated understanding of the decision problem.²

Just as observation of the choice function would allow for the identification of sophistication, it is also useful for identification of the type of misunderstanding that we have posited. Consider next the choice function that would be observed among individuals who understand the seat-number comparative static but who are unaware of the processing-order comparative static. Such individuals adopt the choice function

$$C^n(s^{RF}, s^{RL}) = \begin{cases} 1 & \text{if } s^{RF} > s^{RL} \\ 0 & \text{if } s^{RF} \leq s^{RL} \end{cases} .$$

This choice function dictates choosing the policy that offers more seats, regardless of order. The superscript n denotes the fact this choice function reflects a degree of naïveté in his understanding of incentives.

Given these considerations, we formulate our approach to testing based on the aggregate choice function that would arise from a heterogeneous population of individuals making these decisions. Consider an individual's *average choice function*:

$$\begin{aligned} \bar{C}_i(s^{RF}, s^{RL}) &= p_i^* C^*(s^{RF}, s^{RL}) + \\ & p_i^n C^n(s^{RF}, s^{RL}) + \\ & \sum_k p_i^k C^k(s^{RF}, s^{RL}). \end{aligned}$$

²Note that at the point of indifference ($s^{RF} = T^*(s^{RL})$) any choice probability can be rationally supported. The choice functions written in this section resolve the indeterminacy at the point of indifference arbitrarily. In our experimental design, we intentionally do not present such cases to respondents, motivating our choice to not belabor the details of behavior at this point in this theory.

In this framework, we allow for the individual to probabilistically apply different choice functions at different times. The term p_i^* denotes the individual's probability of using the optimal choice function; p_i^n denotes the probability of using the naïve choice function of interest; the p_i^k terms denote the probabilities of using a set of other arbitrary choice functions. This set of other choice functions is included in the framework for two reasons. First, these other choice functions can capture other reasoned heuristics. Second, their inclusion also provides a means of modeling mistakes. For example, an individual who always *tries* to apply the optimal choice rule but periodically fails to apply it correctly could be modeled as having, e.g., $p_i^* = 0.9$ with the remaining 10% probability weight placed on choice function that assigns a 50-50 chance to each choice regardless of the seats assigned. As another example, an individual who attempts to apply the optimal choice rule but assesses the optimal threshold $T^*(s^{RL})$ with error could be modeled with a choice function that replaces the discontinuity at $T^*(s^{RL})$ with a smooth transition “around” $T^*(s^{RL})$. Because of the inclusion of these alternative choice-functions, the interpretation of p_i^* and p_i^n is the probability that the subject applies the *exact* choice function of interest, as opposed to the choice function with standard notions of error allowed.

To arrive at our final model for estimation, consider the aggregate choice function $\mathcal{C} : (s^{RF}, s^{RL}) \rightarrow [0, 1]$ that would be observed in a population of individuals with heterogeneous average choice functions.

$$\begin{aligned} \mathcal{C}(s^{RF}, s^{RL}) &= \mathbb{E}[\bar{C}_i(s^{RF}, s^{RL}) | s^{RF}, s^{RL}] = \mathbb{E}[p_i^*]C^*(s^{RF}, s^{RL}) + \\ &\quad \mathbb{E}[p_i^n]C^n(s^{RF}, s^{RL}) + \\ &\quad \sum_k \mathbb{E}[p_i^k]C^k(s^{RF}, s^{RL}). \end{aligned}$$

In the equation above, \mathbb{E} is used to denote the expectation taken over all individuals i . In this formulation, the relative weight placed on each choice function is its average rate of use in the population. Under the additional assumption that all auxiliary choice functions are continuous in the neighborhood of the sets of (s^{RF}, s^{RL}) values satisfying $s^{RF} = s^{RL}$ and

$s^{RF} = T^*(s^{RL})$, these average rates of use may be isolated through the following relationships:

$$\lim_{\delta \rightarrow 0} \mathcal{C}(T^*(s^{RL}) + \delta, s^{RL}) - \mathcal{C}(T^*(s^{RL}) - \delta, s^{RL}) = \mathbb{E}[p_i^*] \quad (1)$$

$$\lim_{\delta \rightarrow 0} \mathcal{C}(s^{RF} + \delta, s^{RL}) - \mathcal{C}(s^{RF} - \delta, s^{RL}) = \mathbb{E}[p_i^n]. \quad (2)$$

To help in understanding these equations, consider the case where we hold s^{RL} constant and vary s^{RF} . As s^{RF} crosses the threshold $T^*(s^{RL})$, the optimal choice function dictates that the probability of choosing the reserves-first policy changes discontinuously from zero to one. Note that for the naïve choice function, as well as all auxiliary choice functions (due to the continuity assumption above), no such discontinuity exists. Thus, any discontinuity observed at this point may be attributed to the rate of use of the optimal choice function. Furthermore, the magnitude of the discontinuity will simply be the predicted change in choice probability (known to be one) multiplied by the fraction of individuals applying the optimal choice function ($\mathbb{E}[p_i^*]$). This explains the reasoning behind equation (1) above; equation (2) holds by an analogous argument applied at the point where s^{RF} crosses the threshold s^{RL} .

These equations imply that the average rate of use of these choice functions may be estimated by standard regression-discontinuity techniques applied at the two thresholds of interest. We designed our experiment to apply this empirical strategy.

4 Experimental Design

In this section we present the details of our experiment. Complete text of the experiment, along with details of all data collected, are available in the UAS Experimental Codebook.³

4.1 Overview of Design

The primary purpose of our experiment is to present subjects with incentivized scenarios posing choices between RF and RL policies. In these scenarios, subjects are presented with either a high-school admissions problem or a work-visa allocation problem. Seats are assigned based on a randomly generated priority, but with some number of seats set aside

³Available at <https://uasdata.usc.edu/index.php>, listed as Survey 210.

for the reserve group. The subjects know they are members of the reserve group, and are given a series of choices between an RF and an RL policy with varying reserves. One of their choices is used to determine the final policy that is applied, and if the subject is allocated a school seat or visa as a result of this policy they are given a \$5 bonus payment.

These data allow us to examine the probabilities of choosing the RF policy across a range of (s^{RF}, s^{RL}) values, thus allowing us to deploy the empirical strategy described in Section 3.

On average, our study took 8 minutes to complete. Subjects received a baseline payment of \$5 and an average bonus of \$3.91.

4.2 Walk-through of Survey Content

To concretely illustrate the nature of our experimental task, we present the text associated with the school-choice version of our experimental protocol. The visa version of this protocol is extremely similar, with differences primarily comprised of replacing references to “students” with references to “workers” and references to “seats at a school” with references to “work visas.”

The study began with an overview:

In this study, we are interested in understanding how you think about school admissions policies. Your bonus payment for taking this study will be affected by a simulation of such policies. You will have the opportunity to choose some features of the policy.

Followed by a further elaboration:

To begin, we will explain the type of school admissions policies we will be considering.

Imagine you are applying for a position at an elite high school. Only 100 students will be admitted. The school considers two factors when deciding whom to admit. First, it considers a randomly generated lottery number. Second, it considers group composition.

There are two groups of people, the Blue students and the Green students. Due to their historical underrepresentation, the school particularly values admitting Blue students.

As is illustrated by this text, “Blue” and “Green” labeling dictated group membership. We chose to avoid the usage of more standard racial, gender-based, or income-based group definitions to avoid inviting the subject to rely on beliefs about the desirability of affirmative action for these groups. While the two groups are always labeled Blue and Green, we randomly assign which of these groups is chosen to be favored.

This introduction was followed by an initial presentation of possible reserve policies:

In order to meet its goal of admitting Blue students, the school is considering two policies. In this example, both policies will involve reserving 30 seats for the Blue students. When applying either policy, students will be admitted one at a time.

Admissions will happen in two stages.

In one stage, seats are available to both Blue and Green students. When each seat is assigned, it will be given to the student with the highest lottery number who has not yet been admitted. Color will not be considered.

In the other stage, seats are reserved for Blue students only. When each seat is assigned, it will be given to the Blue student with the highest lottery number who has not yet been admitted.

The policies that the school is considering differ in the order of these stages.

Policy 1: Save the last 30 seats for the Blue students.

- Stage 1: The first 70 seats will be assigned to the 70 students who have the highest lottery numbers, regardless of color.
- Stage 2: The remaining 30 seats will be assigned to the 30 Blue students who have the highest lottery numbers of all Blue students not yet admitted.

Policy 2: Save the first 30 seats for the Blue students.

- Stage 1: The first 30 seats will be assigned to the 30 Blue students who have the highest lottery numbers.
- Stage 2: The remaining 70 seats will be assigned to the 70 students who have the highest lottery numbers of all students not yet admitted, regardless of color.

The assignment of the RF and RL policies to policy 1 and policy 2 was randomized at the subject level. After the initial randomization, these number assignments remained constant throughout the survey.

To test for understanding of the policies presented, this screen contained four comprehension-check questions following the text above. Across these four questions, the subject was asked to consider several students and select who among them would be selected for the first seats assigned by policy 1 and 2 and the last seats assigned by policy 1 and 2. To motivate careful thought, a \$1 reward was given if all four comprehension-check questions were answered correctly. After answers were submitted, a feedback screen reported the correct answer for each question and highlighted where mistakes were made.

At this stage, subjects were introduced to our primary experimental task:

To better understand how you think about these policies, we will now present you with a series of choices. Your choices will affect the bonus you earn in this study.

In each choice, you will face a simulated school admissions process like the one that we have been considering. You must choose between two policies describing different ways of assisting the Blue students. In the simulation, you are one of the Blue students, so you will benefit if you choose the policy that is most favorable for this group.

Across these policies, we will vary both the order in which reserve seats are processed and the number of seats that are reserved.

And, on the following page:

Simulation Details:

All six of the choices you face will have the same basic set-up.

Consider a setting where 200 students are applying to the school. 100 students are Blue and 100 students are Green. You are one of the Blue students.

As before, only 100 students can be admitted. Admissions decisions are still made based on lottery numbers and on diversity considerations. Lottery numbers will be simulated by assigning each student a random number between 1 and 100. All students' numbers, regardless of color, are randomly drawn from the same uniform distribution, so there are no differences across groups in lottery numbers. If two students have the same lottery number, ties will be broken randomly.

Compensation Details:

One of the six choices you make will be randomly selected to be the choice that “counts.” After you answer all six questions, we will reveal the question that “counts” and simulate the admissions decision in the scenario you chose. If you are admitted based on this simulation, an additional \$5 will be added to your bonus.

Since you do not know which of the six choices will be chosen to “count,” it is in your best interest to answer all six carefully.

Following these screens, subjects faced six screens presenting choices as described above. Each screen took the following format:

Consider the following two ways in which the school could implement its admissions policy.

Policy 1: Save the last (s^{RL}) seats for the Blue students.

- Stage 1: The first $(100-s^{RL})$ seats will be assigned to the $(100-s^{RL})$ students who have the highest lottery numbers, regardless of color.
- Stage 2: The remaining (s^{RL}) seats will be assigned to the (s^{RL}) Blue students who have the highest lottery numbers of all Blue students not yet admitted.

Policy 2: Save the first (s^{RF}) seats for the Blue students.

- Stage 1: The first (s^{RF}) seats will be assigned to the (s^{RF}) Blue students who have the highest lottery numbers.
- Stage 2: The remaining ($100-s^{RF}$) seats will be assigned to the ($100-s^{RF}$) students who have the highest lottery numbers of all students not yet admitted, regardless of color.

As a Blue student, which policy would you prefer?

As described in the prior section, our empirical strategy relies on observing choices between RF and RL policies for a range of (s^{RF}, s^{RL}) tuples. To that end, these values were randomly generated for each choice the subject faced. The six decisions presented six values of s^{RL} , assigned deterministically but in random order: 40, 44, 48, 52, 56, and 60 seats. For each of these scenarios, the required number of seats needed for the RF policy to be optimal was 70, 72, 74, 76, 78, and 80, respectively. For each s^{RL} value, s^{RF} was uniformly sampled from 13 potential values: -5, -3, -1, +1, +3, or +5 seats relative to both the optimal and naïve thresholds, as well as an additional point approximately between the two thresholds. By sampling values in the vicinity of our two thresholds of interest, this design ensures that we are well powered to deploy our proposed regression-discontinuity approach.

Following these choices, one of the six scenarios was randomly selected for simulation as described above. Their chosen policy was implemented, their admissions decision was simulated as specified, and the results of the simulation and the associated payoffs were announced. The study then concluded with a brief elicitation of their degree of interest in the survey and an opportunity for free-response comments on the study.⁴

4.3 Preregistration

Our experiment was preregistered on aspredicted.com. For reference, the preregistration document is included in the Online Appendix. In this document, we specify our exact hypotheses of interest and the details of our regression discontinuity approach. We also

⁴The inclusion of these final two questions is standard practice in the Understanding America Study.

commit to our sample size and exclusion restrictions. While we will also present some exploratory analyses that were not preregistered, we do not deviate from this preregistration in our presentation of primary results.

5 Experimental Deployment and Sample

5.1 The Understanding America Study

We deployed our experiment in the Understanding America Study (UAS).⁵ The UAS is an online panel of American Households recruited for their demographic diversity. The advantage of this panel is its established infrastructure for reaching a broad group of respondents and its substantial efforts to achieve representative sampling. Additionally, by using this panel we can merge data from many other surveys into our analyses, which enables our analysis of the demographic predictors of the behaviors we study.

The UAS panel is recruited through address-based sampling. Respondents are targeted for recruitment based on a random draw from postal records. Once targeted for recruitment, substantial efforts to integrate the individual into the panel are pursued. After an initial attempt to recruit a targeted respondent to the panel, follow-up continues over an approximately six-month period. This follow-up involves attempts to resolve common barriers to survey participation. For example, targeted respondents who do not have internet access are provided with a tablet and broadband internet access so they may participate. Additionally, all UAS materials are available in Spanish to allow for the recruitment of solely Spanish-speaking targeted respondents.

In principle, such a sampling approach can approximate census-level quality in representative sample construction. In practice, however, recruitment of this variety is challenging, and the ultimate panel-entry rate among targeted respondents typically ranges from 10% to 15%. This does introduce the possibility of selection in the sample. However, the UAS's quarterly collection of a very broad set of demographics permits testing for selection on observables, and the construction of sample weights that correct for it. Selection on unob-

⁵For a detailed description of the UAS, see Alattar, Messel and Rogofsky (2018).

servables remains possible. Despite this concern, we note that the procedures described here minimize this worry relative to other commonly-used experimental platforms. Furthermore, we will reconstruct our primary analyses making use of sampling weights aimed to correct for these issues in Section 6.3.1.

5.2 Deployment

Our survey was deployed to the UAS population in December 2019 and January 2020. With the help of UAS personnel, our study was integrated into their online platform and translated into Spanish for the relevant respondents. To achieve our targeted sample size of 1,000 responses, the UAS drew a random subsample of 1,500 respondents from their full panel. These 1,500 respondents received invitations both through the UAS online platform and by mail to take our study, with periodic reminders provided. The survey was closed shortly after the target sample size was attained, ultimately resulting in 1,013 complete observations and a 67% response rate.

5.3 Demographic Properties of Sample

Table 1 summarizes basic demographics of our respondents. As is seen across panels of this table, our sample is demographically diverse. However, due to the selection that occurs in the process of recruitment to online panels, our sample differs from the general U.S. population in several ways. Compared to the general adult population of the U.S., members of our sample are somewhat more likely to be female, married, and U.S. citizens. Our sample also skews to be somewhat older and somewhat more likely to be white.

While there is some evidence of selection on observables influencing the general UAS population, we find little evidence that such effects influence which UAS participants respond to our survey. In the final column of this table, we present formal tests for differences in the demographic variable across respondents who did and did not participate. Only two of the nine tests conducted reach significance at traditional levels. First, participants are slightly less likely to be employed (59.2% vs 66.1%; $p = 0.01$), consistent with the possibility that those not working have more time to complete online studies. Second, participants

who completed our study have a notably different age distribution. On average, those who completed our survey are 3.79 years older than those who did not ($s.e. = 0.90$; $p = 0.00$).

We additionally examine the geographic distribution of respondents. Figure 1 presents the number of observations obtained for respondents residing in each U.S. state. As is observed in the figure, our survey reached a broad populace: the only U.S. state with no representation in our sample is Delaware. Furthermore, we see no evidence of selection by geography: a chi-squared test for differences in state of residency by completion status yields a p-value of 0.24. A similar lack of selection is observed based on place of birth (by country: $p = 0.42$; by state: $p = 0.28$).

6 Experimental Results

6.1 Primary Test of Misguided Policy Choices

In this subsection, we present the preregistered tests of our primary hypothesis: that a substantial fraction of respondents mistakenly believe that processing order does not matter in these assignment mechanisms.

To test this hypothesis, we estimate models of the form

$$Y_{ij} = \alpha + \beta^n N_{ij} + \beta^* O_{ij} + f(s_{ij}^{RF}, s_{ij}^{RL}) + \epsilon_{ij}. \quad (3)$$

Subscripts i and j index the respondent and choice number, respectively. In this model, the dependent variable Y is an indicator for whether the RF policy was chosen in a given binary choice. Variables N_{ij} and O_{ij} provide the value of Y dictated by the naïve or optimal choice function. Formally, $N_{ij} = I(s_{ij}^{RF} > s_{ij}^{RL})$ and $O_{ij} = I(s_{ij}^{RF} > T^*(s_{ij}^{RL}))$, where $I()$ denotes the indicator function taking the value of 1 when the statement in parentheses is true. $(s_{ij}^{RF}, s_{ij}^{RL})$ denote the number of seats assigned to each policy, as before, and $f(s_{ij}^{RF}, s_{ij}^{RL})$ denotes a function meant to control for the number of each type of seats assigned. Across specifications, we will consider a variety of approaches to handling this control, including modeling f as a local polynomial, a cubic spline, or a fifth-order polynomial.

Interpreted in light of our model from Section 3, β^n serves as an estimate of $\mathbb{E}[p_i^n]$ and β^*

serves as an estimate of $\mathbb{E}[p_i^*]$. Despite this interpretation, the model above does not constrain the sign of β^n or β^* to be positive. In principle, this means that these estimates could yield invalid probabilities. We would interpret the detection of a (statistically significant) negative value for these parameters as a rejection of our framework for type estimation.

Table 2 presents our estimates of this model. In columns 1 and 2, we report estimates of this model with the data restricted to s_{ij}^{RF} values that are within 5 seats of the two thresholds. This amounts to a simple difference in means of the rate of choosing the RF policy when s_{ij}^{RF} is immediately above versus immediately below each threshold. Formally, no term controlling for $f(s_{ij}^{RF}, s_{ij}^{RL})$ is included in the regression; instead, the influence of this term assumed to be nearly constant for a sufficiently narrow region of s_{ij}^{RF} values, and the estimation sample is correspondingly restricted to a narrow region near the threshold.

Interpreting the results from column 1, we see that on average, the RF policy is 40 percentage points (*s.e.* = 2*pp*) more likely be chosen when the number of RF seats is just above (versus just below) the number of RL policy seats. This finding is consistent with respondents using the naïve choice function for 40% of decisions.

In contrast, column 2 demonstrates that on average, the RF policy is only 3 percentage points (*s.e.* = 2*pp*) more likely to be chosen when the number of RF seats is just above (versus just below) the threshold from the optimal decision function. This coefficient is statistically distinguishable from zero ($p = 0.03$), but quantitatively suggests that effectively no respondents apply the optimal choice function.

In the remaining columns of the table, we provide a variety of approaches to formally estimating these regression discontinuities through more technical means. All approaches provide similar results. Varying our approach to controlling for $f(s_{ij}^{RF}, s_{ij}^{RL})$ with a local polynomial, a spline, or a high-order polynomial, our estimates of the rate of utilization of the naïve choice functions range from 36 to 37%. Across these specifications, the estimated rate of utilization of the optimal choice function never exceeds 3%, and is generally statistically indistinguishable from zero.

Figure 2 helps in visualizing these results. Recall that, for a fixed number of RL seats, the number of RF seats takes values of -5, -3, -1, +1, +3, or +5 seats relative to each of the thresholds of interest. One additional point was sampled between the two thresholds. In

this figure, each dot illustrates the average rate of choosing the RF policy for the the number RF seats illustrated on the x-axis, with the six dots above each point summarizing choices under the six RL seat amounts. The solid line presents a fitted spline analogous to that in column 5 of Table 2. This figure illustrates a stark change in the rate of choosing RF at the naïve threshold of interest. In contrast, there is no apparent discontinuity at the threshold where it should occur among optimizing agents.

In principle, our estimates of the rate of choice-function adoption could differ across the school-choice and visa-allocation versions of our scenarios. In practice, however, the estimated differences are small in magnitude. Appendix Table A1 reproduces Table 2, restricting the data to each of these scenarios in turn. The estimates in these tables typically are within 3 percentage points of the estimates of Table 2,⁶ and the difference never exceeds 6 percentage points. Furthermore, in our primary specifications, we find no statistically significant interaction between the estimated discontinuities and the scenario version ($p = 0.18$ and $p = 0.63$ for the column 1 and 2 analysis, respectively). In short, we find no evidence of differences in choice-rule adoption based on the framing of the scenario.

6.1.1 Summary of Primary Findings

We estimate that a large fraction of respondents (40% in our primary regression) adopt a choice function that reflects an understanding of the seat-number comparative static while reflecting ignorance of the processing-order comparative static. These respondents understand that more seats are better, but do not see the benefits of the reserves-last design.

6.2 Predictors of Optimal and Naïve Choices

In this subsection we explore cross-group differences in policy choices. In contrast to the previous section, which presents pre-registered analyses, most analysis here is exploratory.

To help assess the predictors of the choice functions of interest, we re-conduct the primary analysis of Table 2 while allowing the estimated parameters to vary by group. Interpreted in light of our empirical model, this allows us to infer the rate of use of the two focal choice

⁶More specifically, they are no larger than 3 percentage points for 17 of the 24 estimates.

functions within each group.

Formally, we estimate regressions of the following form.

$$Y_{ij} = \alpha + \beta^n N_{ij} + \gamma G_i + \delta G_i \times N_{ij} + \epsilon_{ij} \quad (4)$$

$$Y_{ij} = \alpha + \beta^n O_{ij} + \gamma G_i + \delta G_i \times O_{ij} + \epsilon_{ij} \quad (5)$$

In these regressions, the term G_i is an indicator variable indicating membership in the relevant group. In groups where classification is not binary, we will split the group into two approximately equal-sized bins. For example, in one regression the group variable will take the value of 1 for male respondents; in another, it will take the value of 1 for respondents of age 50 or greater. The terms $G_i \times N_{ij}$ and $G_i \times O_{ij}$ capture the interaction between this indicator variable and the choice function of interest (which itself is an indicator variable taking the value of 1 when the relevant threshold is surpassed). Except for the terms involving G_i , these regressions are the same as columns 1 and 2 of Table 2. Importantly, we maintain the same sample restriction, estimating the regression only from observations in which the number of RF seats is no more than 5 away from the relevant threshold.

6.2.1 Predictors of Adopting the Naïve Choice Function

We begin by examining estimates of equation (4), capturing differences in the rate of application of the naïve choice function. When interpreting the results of this estimating equation, note that term δ measures the difference in the discontinuity seen at the naïve threshold, and thus estimates the difference in the rate of adoption of the naïve choice function between those in and out of this group. Furthermore, note that in the immediate vicinity of the naïve threshold, the optimal decision is to choose the RL policy. Since a negative value of γ indicates a higher propensity to choose the RL policy, this should be interpreted as indicating on average “better” decisions by this group, holding fixed their rate of adoption of the naïve choice function.

Estimates of these equations are presented in Table 3. In panel A, we split the sample by the demographic groups previously considered in Table 1. We omit only the variables related to race or citizenship status: these classifications yield small subgroups in which our analysis

is substantially less powered. Examining the estimates of the term δ , we find some evidence of cross-group differences in the rate of adopting the naïve choice function. Focusing attention on estimates reaching significance at the 5% α -level, we find that married respondents are 10 percentage points more likely to adopt this choice function ($s.e. = 4pp$); working respondents are 9 percentage points more likely ($s.e. = 4pp$); respondents with an Associate’s degree or above are 20 percentage points more likely ($s.e. = 4pp$); and respondents with annual household income of at least \$50,000 are 21 percentage points more likely ($s.e. = 4pp$). No statistically significant differences are found based on gender or age.

We next examine the estimates of γ , which inform the general decision quality in the region near the naïve threshold among those not adopting the naïve choice function. Again, we find some evidence of variation across the groups considered. Focusing attention on estimates reaching significance at the 5% α -level, we find that married respondents are 6 percentage points more likely to correctly choose the RL policy ($s.e. = 3pp$); working respondents are 8 percentage points more likely ($s.e. = 3pp$); respondents with an Associate’s degree or above are 15 percentage points more likely ($s.e. = 3pp$); and respondents with annual household income of at least \$50,000 are 14 percentage points more likely ($s.e. = 3pp$). Again, we find no statistically significant difference based on gender or age.

The finding that education has comparatively large predictive power for the rate of use of the naïve choice function suggests that the choice function’s adoption may relate to cognitive performance. And indeed, general measures of cognitive performance have been shown to predict mistakes in the use of assignment systems in prior literature (see, e.g., Basteck and Mantovani, 2018; Rees-Jones, 2018; Rees-Jones and Skowronek, 2018; Shorrer and S3v3ag3o, 2018; Rees-Jones, Shorrer and Tergiman, 2020; Hassidim, Romm and Shorrer, 2020). To further explore this hypothesis, we make use of several cognitive performance measures available in the UAS. The first is a measure of numeracy, derived from subjects’ ability to complete a sequence of numbers with one number missing. The second is a measure of verbal abilities, in which subjects must choose the correct completion to an analogy. The third is a measure of vocabulary, in which the subject must name an item that is indicated in a picture. Finally, we analyze a measure of subjective numeracy, constructed from a series of Likert-scale questions directly eliciting self-assessments of mathematical abilities (e.g., “How

good are you at working with fractions?”).⁷ These measures come from independent modules deployed to the UAS sample with broad coverage. Each measure is available for at least 92% of our sample.⁸ In addition to these measures, we analyze one measure internal to our study that is plausibly related to cognitive ability: passing the first-stage comprehension check described in Section 4.2.

Panel B of Table 3 reports analysis of these variables. Across these measures, a consistent picture emerges: higher cognitive performance is associated with a higher rate of adoption of the naïve choice function. These results are statistically significant for all cognitive measures except that measuring the breadth of vocabulary—the measure we believe to be the least related to general logical ability. Furthermore, these differences are large in magnitude: higher ability respondents are estimated to be 17 to 31 percentage points more likely to adopt the naïve decision rule across measures, excluding the measure of breadth of vocabulary. Individuals with high cognitive performance appear to face a pitfall when attempting to choose optimal policies. Note, however, that if this pitfall is avoided, those of high cognitive performance choose comparatively well in this region: estimates of γ reveal that, among those not responding to the threshold, the rate of incorrectly choosing the RF policy is lower.⁹

6.2.2 Predictors of Adopting the Optimal Choice Function

We next examine estimates of equation (5), which measure differences in the rate of application of the optimal choice function. This analysis and its interpretation closely follow that just presented above.

Table 4 shows relatively small differences in the rate of optimal choice function adoption across groups. Interaction effects that are significant at the 5% α -level are only detected by marital status (married respondents are 8 percentage points less likely to adopt the optimal

⁷For complete documentation of these measures, see Moldoff and Becker (2019). We apply the aggregate Wave-12 measures discussed under topics N, V, and A: `n12nsa_score`, `a12vea_score`, and `v12pva_score`. Additionally, the subjective numeracy measure discussed below is documented under Topic C: `c12avgnsnsscore`.

⁸Whenever these data are used, we conduct our analyses on all observations for which these measures are available, consistent with an assumption that these measures are missing at random.

⁹For comparability to the panel A results, panel B presents analyses of a discrete above/below median indicator for the cognitive performance measures. Note that the same qualitative results arise from examination of the underlying continuous measures (see Appendix Tables A2 and A3).

choice function; *s.e.* = 3pp) and by education (respondents with an Associate’s degree or higher are 7 percentage points more likely to adopt the optimal choice function; *s.e.* = 3pp). Despite this difference by education, insignificant and quantitatively small differences are seen for all cognitive measures examined in panel B—i.e., these results do not suggest that more cognitively able respondents are more likely to adopt the optimal choice function. Overall, while some cross-group differences are observed in the baseline rate of choosing the RF policy (as measured by parameter γ), these analyses generally support a much smaller degree of heterogeneity in adoption of the optimal choice rule as compared to the naïve choice rule. This lower degree of heterogeneity is perhaps expected given the lower overall adoption of the optimal choice rule.

6.2.3 Implications for Payoff Maximization

Our results on cross-group differences in choice-function adoption motivate a practical question: how do these differences in inferred perceptions of optimal behavior map into the rate of optimal choice? Since the optimal choice function is estimated to be rarely adopted, the answer to this question ultimately depends on the performance of the naïve choice function as compared to other suboptimal choice functions in use.

In Table 5 we explore this question with particular focus on the differences in outcomes arising due to cognitive performance. This table reports the estimated average marginal effects arising from a series of logit regressions. In these regressions, the dependent variable indicates whether the respondent chose the payoff maximizing option of the two policies presented. The independent variables are the group affiliations considered in the previous two subsections.

In the first three columns we present results from regressions predicting choice of the payoff-maximizing option using our three objective cognitive measures individually. These regressions suggest that individuals with above-median cognitive performance are more likely to choose the payoff-maximizing option. However, effect sizes are modest, with point estimates ranging from 1pp to 3pp. Statistically significant effects are found only for the first two measures. As illustrated in columns 4, 5, and 6, in which all three measures are included simultaneously along with additional controls, the average marginal effect of these variables

either remains stable or declines in magnitude.

Overall, these results illustrate a consequence of conflicting findings from the prior sections. On the one hand, cognitive performance predicts adoption of the naïve choice function—a behavior that pushes respondents to make suboptimal choices in some circumstances. On the other hand, conditional on not responding to the threshold associated with the naïve choice function, cognitive performance predicts better choices in the vicinity of the naïve threshold. The results presented here show the the benefits of wisdom inherent in this latter finding are mostly offset by the costs of the naïveté in the former. Adopting a choice function that is *nearly* optimal—failing to attend only to the processing-order comparative static—offsets the comparatively high rate of payoff-maximizing choices that would be realized in the absence of this pitfall.

Finally, column 7 of this table presents results using only our demographic variables to predict choices. Again, cross-group heterogeneity is shown to be quite modest.

6.2.4 Summary

Taken together, these findings demonstrate that misunderstanding of the importance of processing order in reserve systems is a prevalent, cross-group phenomenon. Across a wide range of demographic variables available, some variation in decision rules exists; however, adoption of the naïve choice function remains common among all groups studied. Indeed, the subjects who traditionally would be expected to be the most likely to avoid this pitfall—the highly educated, the comparatively rich, the cognitively able, and those who pass our internal comprehension checks—are those that are most susceptible to it in our data.

6.3 Robustness Considerations

6.3.1 Sample Weights

As emphasized in Section 5, the UAS follows a variety of good practices to target representative sampling, but some selection into the survey panel remains. To help assess the importance of this issue to our primary estimates, we reproduce all main analyses with the inclusion of sampling weights (see Appendix Section B.1). These weights, constructed by

the UAS, account for both the adaptive sampling procedure used in recruitment as well as any differences in attrition seen across measured demographics (for complete details, see Angrisani et al. (2019)). In these analysis, the reweighting has very modest effects on our estimates, and all qualitative findings remain—a reassuring finding, albeit one that is to be expected given the small differences between our sample and the general population.

6.3.2 Importance of Stake Size

In our experiment, the financial reward for admission in the simulation is a \$5 bonus payment.¹⁰ We believe that this is lower than the material rewards to the real-world assignment of a desirable school seat or a work visa. While our incentives are in line with standard practices in the experimental market design literature, a reader may reasonably question whether the quality of the decisions would respond to increases in the financial consequences of suboptimal choice.

Whether the misunderstanding of assignment procedures observed in the lab predicts mistakes in the field is a topic of ongoing debate. Several studies support the possibility: for example, Shorrer and Sóvágó (2018) find that financially consequential mistakes are observed in the Hungarian college-admissions match, and Rees-Jones and Skowronek (2018) find that medical students show misunderstanding of the deferred acceptance algorithm immediately after their participation in the high-stakes medical residency match that uses it. In contrast, Artemov, Che and He (2020) find that the mistakes made in the Australian college-admissions match are often payoff irrelevant, suggesting a more minimal role for the field-importance of misunderstanding.

Despite this ongoing debate, we believe that the applications that motivate our study are less susceptible to criticism of mismatched stakes size than the environments considered above. Note that in most studies of suboptimal behavior in assignment mechanisms, the object of interest is the preferences that the individual submits to the assignment system. In the field, incorrect submission of these preference can easily lead to consequences much

¹⁰Furthermore, the return to making an optimal choice is less than \$5, since the optimal choice does not guarantee admission and the suboptimal choice does not rule it out. Across all scenarios presented in our study, the average difference in the probability of assignment across the two policies was 13 percentage points, translating to an increase in the expected value of an optimal choice of 63 cents.

larger than can be feasibly recreated in the lab. In contrast, our study concerns policy preferences: i.e., how individuals would like a reserve system to be implemented. In the field, the implementation of these systems rarely directly responds to an individual’s preference. Instead, that preference can determine which candidates or administrators the individual supports or how the individual votes—both behaviors with much lower (and potentially zero) payoff consequences. In short, we believe that the intuitions we elicit in our experiment are comparatively likely to be the same intuitions that a parent would call upon in a school-board meeting when discussing a school-choice policy, or the same intuitions that a citizen would call upon when considering the wisdom of H-1B policy. Of course, further study would be needed to formally validate this belief, however plausible it may be.

6.3.3 Confirmatory Evidence from Amazon Mechanical Turk

Prior to the deployment of our study, we ran two large-scale pilots on Amazon Mechanical Turk (MTurk). Both pilots examined the “school choice” version of the study, as presented above. The first pilot assessed the rate of optimal choice in a single scenario with non-randomized seat numbers. The second pilot was nearly identical to the study deployed in the UAS, with the exception of excluding the visa version of the scenarios. Across these two pilots, we find extremely similar qualitative and quantitative results as reported in this paper. Due to the higher incentives offered in our UAS study, along with the comparatively high quality of the UAS panel’s recruitment procedures, we believe the results derived from this sample are the most credible. However, we view the fact that closely analogous results arise on MTurk reassuring, serving as a replication among a different sample. For documentation of these pilots, see the Online Appendix.

7 Discussion

In this paper, we have examined the general understanding of reserve systems held by the U.S. populace. Among participants in the Understanding America Study, we found that a very small fraction adopted the choice function that reflects a fully sophisticated understanding of these systems. In contrast, a plurality of respondents—40% in our leading specification—

adopt a *nearly* sophisticated choice function, demonstrating general understanding of the decision environment but a lack of understanding of the importance of processing order. In contrast to many other environments, we do not find that this misguided behavior is tempered by education or cognitive ability, but rather that it is primarily driven by the educated and cognitively able.

Given the rapid proliferation of reserve systems—used to enact affirmative action policies in a wide variety of settings—the tendency for misunderstanding that we document is unfortunate. We believe this misunderstanding serves as a primary explanation for several surprising elements of the history of the school-choice reserve system in Boston and the H-1B visa allocation system in the United States. Furthermore, we believe the potential for this misunderstanding to influence policy (or policy’s reception by the public) extends well beyond these two examples. Indeed, when reserve systems are deployed, this type of misunderstanding may be the rule and not the exception.

When facing such a situation, a well-meaning market organizer may benefit from taking steps to help his constituency clearly assess the consequences of potential implementations of a reserve policy. One potential solution that we view as promising is to have stakeholders vote on policies with transparent forecasts of their degree of affirmative action provided. Even the mere requirement to provide such a forecast imposes discipline on the process: a forecast cannot be made without specifying processing order, eliminating the possibility that this component will be left undefined. Furthermore, when the degree of affirmative action is made transparent, we speculate that failures to pursue one’s own best interests would be reduced. As a concrete illustration, we believe that proponents of neighborhood schooling in Boston would have been substantially less supportive of the original 50-50 reserve system if it had been presented alongside forecasts showing its lack of advancement of walk-zone students.

Of course, information interventions like these are only possible when market organizers actively and intelligently attempt to improve their constituents’ understanding. If market organizers themselves do not understand reserve systems, these steps will not be taken. As was illustrated in the case of H-1B policy, it’s not obvious that administrators are always aware of these issues. However, as this literature continues to evolve and as market design-

ers continue their interactions with market organizers, we believe that the probability that market organizers are informed will be higher. We hope that papers like this one will help.

Even in cases where market organizers are informed, however, the assumption that they will be motivated to debias the populace is a strong one. When policy makers benefit from misunderstanding, we believe there is relatively little to stop them from using it to their advantage. This may mark one of the most potentially costly implications of the behavior we have documented.

While our interpretation is only speculative, we believe that a version of this story played out in recent reassessments of reserve policy in India.¹¹ In India, a reserve for members of historically disadvantaged castes is applied in some school-assignment and government-job allocation procedures.¹² The implementation of these reserves was considered in the landmark Supreme Court case *Indra Sawhney and others v. Union of India (1992)*. In this case, the court interpreted constitutional support for the “the reservation of appointments or posts in favor of any backward class of citizens”¹³ to specify that a reserves-last policy should apply, providing these groups with the most effective policy for achieving affirmative action. It also specified that other reserves promoting equality of opportunity¹⁴ should be implemented as reserves-first, granting them a lower degree of affirmative action for the same number of seats. We view this court case as a rare demonstration of clear understanding of the use of reserve order as a policy lever.

In the lead-up to the 2019 election, this reserve system became the topic of public debate and criticism. Many economically disadvantaged Indians do not come from a historically disadvantaged caste. Based on their economic disadvantage, it seemed unreasonable to many that their admission was deprioritized relative to more affluent members of historically disadvantaged castes. In response to these concerns, incumbent President Modi widely publicized his pursuit of a 10% reserve for the “economically weaker sections” (EWS). Partially motivated by a desire to pass this policy before the spring election, the One Hundred and Third Amendment of the Constitution of India went from its first presentation in the lower house

¹¹For extensive market-design analysis of these systems see Sönmez and Yenmez (2019*a,b*).

¹²Formally, the primary groups considered are the “scheduled castes,” “scheduled tribes,” and “other backwards castes.” Each label is precisely defined in law.

¹³See Article 16(4) in the Constitution of India (1949).

¹⁴As specified in Article 16(1) in the Constitution of India (1949).

of parliament to its final passage in the upper house of parliament over a period of two days in January, 2019. The EWS reserve policy took effect four days later.

Despite its public support, this amendment and its passage received substantial criticism. The process of passing the bill was rushed¹⁵, and perhaps as a result the bill did not specify the order in which this reserve would be processed. This omission is important, since one reading of *Indra Sawhney (1992)* suggests that this policy would be implemented as reserves-first.¹⁶ Similar to the application in Boston Public Schools, a reserves-first policy would not be effective in these markets (Pathak and Sönmez, 2019). In a memo¹⁷ following shortly after passage of the amendment, the administration clarified that this should be internally implemented as a reserves-last policy, leading to an immediate and ongoing battle over the constitutionality of this policy.¹⁸

The passage of this bill was perceived by some as a politically motivated attempt to woo economically disadvantaged voters who did not qualify for the existing reserves.¹⁹ If these portrayals are accurate—which we cannot guarantee, but do view as plausible—they belay a shrewd reliance on misunderstanding of processing order. Given the unique legal precedents in India, we believe that the likelihood that an EWS preference would be implemented as a reserves-first policy would be known to informed politicians, as would be the lesser efficacy of these policies. The results of this paper—perhaps already intuited by politicians—suggest that at least some potential voters would be unaware of these nuances, instead only seeing this policy as a step to help voters like them. Modi ultimately did win reelection, and while this issue was widely publicized, we cannot say definitively if misunderstanding of processing order played any role. However, we view the possibility as worrying.

Our hope is that our work ultimately fosters transparent implementation of reserve systems in settings with potential for either manipulation or confusion.

¹⁵A recent court case notes that copies of the bill were not furnished to members of parliament with sufficient time for review, and that the parliamentary session was unexpectedly extended by one day to allow for the bill’s speedy passage. See *R.S. Bharathi v. The Union of India* (2019), Madras High Court.

¹⁶This could be justified both by its potential classification as an equal opportunity provision, and by the fact that adding an additional 10% of seats to the reserves-last group would exceed the mandated 50% maximum on reserves. For public support of this opinion, see Khemka (2019).

¹⁷Memo available here: <https://dopt.gov.in/sites/default/files/ewsf28fT.PDF>. Last accessed: 3/24/2020.

¹⁸See, e.g., *Youth for Equality v. Union of India* (2019).

¹⁹See, e.g., Ashraf (2019), Dhingra (2019), Mathew (2019), or Mishra (2019).

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Table 1: Demographic Information and Sample Selection

	(1)	(2)	(3)	(4)
	Survey Completion Status			Test for Difference
	Complete	Incomplete	All Recruits	
<i>Basic Demographics</i>				
Female	56.2	57.2	56.5	$p = 0.71$
Married	58.4	59.1	58.7	$p = 0.80$
Working	59.2	66.1	61.4	$p = 0.01$
U.S. Citizen	97.9	97.9	97.9	$p = 0.98$
Hispanic or Latino	10.8	13.6	11.7	$p = 0.12$
<i>Race</i>				
White Only	82.2	77.1	80.5	
Black Only	9.0	10.1	9.4	
Am. Indian or Alaska Native Only	1.3	2.3	1.6	$p = 0.11$
Asian Only	2.8	2.7	2.7	
Hawaiian/Pacific Islander Only	0.6	0.5	0.5	
Multiple Races Indicated	4.3	7.2	5.2	
<i>Education</i>				
< 12th grade	5.4	4.5	5.1	
High school grad.	19.3	19.9	19.5	
Some college	20.0	25.1	21.7	$p = 0.30$
Assoc. degree	13.6	13.3	13.5	
Bachelor's degree	24.7	21.6	23.7	
Master's degree +	16.9	15.6	16.5	
<i>Household Income</i>				
< \$10,000	6.7	7.2	6.9	
\$10,000 - \$24,999	12.9	13.8	13.2	
\$25,000 - \$49,999	21.5	21.9	21.6	$p = 0.87$
\$50,000 - \$74,999	18.2	17.8	18.1	
\$75,000 - \$99,999	14.3	12.0	13.6	
\geq \$100,000	26.4	27.3	26.7	
<i>Age</i>				
18-29	10.5	15.7	12.2	
30-39	19.3	21.3	19.9	
40-49	17.1	22.5	18.9	$p = 0.00$
50-59	19.3	17.4	18.6	
60 +	33.9	23.1	30.4	

Notes: This table presents summary statistics characterizing the demographic features of our sample. With the exception of p-values, all numbers presented are the percentage of respondents with a given row's classification. The first panel characterizes a series of binary demographic variables, and the panels that follow present tabulations of individual categorical variables. The first column presents results for subjects included in our primary analyses. To help assess selection into our study, the second and third columns present results for the subjects who were contacted but did not complete the study and all contacted subjects, respectively. The final column provides p-values for chi-squared tests for differences in the distribution of the categorical variable by survey completion status.

Table 2: Estimates of Choice Functions Governing Policy Preferences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\beta^n: N_{ij}$	0.40 (0.02)		0.39 (0.04)		0.36 (0.02)	0.37 (0.04)	0.36 (0.02)	0.36 (0.04)
$\beta^*: O_{ij}$		0.03 (0.02)		0.02 (0.03)	-0.01 (0.02)	0.03 (0.03)	-0.01 (0.02)	0.03 (0.03)
Control for s^{RF} (f)	Sample	Restriction	Local Poly		Cubic Spline		5th-order Poly	
s^{RL} Fixed Effects		No	No		Yes		Yes	
s^{RL} FEs \times f	No	No	No	No	No	Yes	No	Yes
Respondents	990	991	1013	1013	1013	1013	1013	1013
N	2865	2709	6078	6078	6078	6078	6078	6078
R ²	0.155	0.001			0.169	0.174	0.169	0.174

Notes: This table reports regressions of an indicator for choosing the RF policy on controls for the number of seats reserved. Variables N_{ij} and O_{ij} provide the value that Y should take if the respondent adopts the naïve or optimal choice function defined in Section 3. Across columns, we present a variety of approaches to estimating the model $Y_{ij} = \alpha + \beta^n N_{ij} + \beta^* O_{ij} + f(s_{ij}^{RF}, s_{ij}^{RL}) + \epsilon_{ij}$, varying the means of controlling for the number of seats assigned to the RF and RL policies through term $f(s_{ij}^{RF}, s_{ij}^{RL})$. In columns 1 and 2, we attempt to control for this term by assuming that it is approximately constant in a small enough window. Each column restricts the data to observations in which the number of seats in the RF policy is within 5 seats of those provided in the RL policy. In columns 3 and 4, we present estimates arising from local polynomial regressions, applying a rectangular kernel with a bandwidth of 3. In column 5, $f(s_{ij}^{RF}, s_{ij}^{RL})$ is approximated with a cubic spline over s_{ij}^{RF} combined with fixed effects for the six possible values of s_{ij}^{RL} . In column 6, the spline is interacted with the fixed effects, effectively allowing for s_{ij}^{RL} -value-specific splines over s_{ij}^{RF} . Columns 7 and 8 follow the same format as 5 and 6, replacing the usage of splines for approximation with the usage of a 5th order polynomial. Standard errors, clustered by respondent, are reported in parentheses.

Table 3: Cross-Group Differences in Naïve-Choice-Function Adoption

Panel A: Demographic Groups						
Group Indicates:	(1) Male	(2) Married	(3) Working	(4) High Education	(5) High Income	(6) High Age
α : Constant	0.30 (0.02)	0.32 (0.02)	0.33 (0.02)	0.37 (0.02)	0.37 (0.02)	0.28 (0.02)
β^n : N_{ij}	0.39 (0.03)	0.35 (0.03)	0.35 (0.03)	0.29 (0.03)	0.28 (0.03)	0.40 (0.03)
γ : Group	-0.04 (0.03)	-0.06 (0.03)	-0.08 (0.03)	-0.15 (0.03)	-0.14 (0.03)	0.02 (0.03)
δ : Interaction	0.03 (0.04)	0.10 (0.04)	0.09 (0.04)	0.20 (0.04)	0.21 (0.04)	0.00 (0.04)
Respondents	990	990	990	990	988	989
N	2865	2865	2865	2865	2859	2863
R ²	0.164	0.165	0.166	0.176	0.174	0.163

Panel B: Cognitive Performance Measures					
Cog. Measure:	(1) Number Sequence	(2) Analogies	(3) Picture Vocab.	(4) Subjective Numeracy	(5) Comp. Check
α : Constant	0.37 (0.02)	0.36 (0.02)	0.31 (0.02)	0.34 (0.02)	0.39 (0.02)
β^n : N_{ij}	0.30 (0.03)	0.32 (0.03)	0.37 (0.02)	0.33 (0.03)	0.24 (0.03)
γ : High Cog. Perf.	-0.17 (0.03)	-0.16 (0.03)	-0.06 (0.03)	-0.11 (0.03)	-0.20 (0.03)
δ : Interaction	0.23 (0.04)	0.20 (0.04)	0.07 (0.04)	0.17 (0.04)	0.31 (0.04)
Respondents	968	943	956	914	990
N	2811	2724	2772	2640	2865
R ²	0.178	0.176	0.161	0.170	0.190

Notes: This table reports regressions analogous to that in column 1 of Table 2, but additionally including a control for group affiliation and an interaction with the estimated discontinuity. High education indicates that the respondent completed an Associate's degree or higher. High income indicates that the respondent's household income is \$50,000 per year or more. High age indicates that the respondent is 50 years old or higher. In panel B, we present similar analyses based on splitting the sample by tests of cognitive performance. Standard errors, clustered by respondent, are reported in parentheses.

Table 4: Cross-Group Differences in Optimal-Choice-Function Adoption

Panel A: Demographic Groups						
Group Indicates:	(1) Male	(2) Married	(3) Working	(4) High Education	(5) High Income	(6) High Age
α : Constant	0.79 (0.02)	0.75 (0.02)	0.79 (0.02)	0.76 (0.02)	0.74 (0.02)	0.77 (0.02)
β^* : O_{ij}	0.03 (0.02)	0.08 (0.03)	0.04 (0.03)	-0.01 (0.02)	0.01 (0.03)	0.04 (0.02)
γ : Group	0.00 (0.03)	0.07 (0.03)	0.01 (0.03)	0.06 (0.03)	0.09 (0.03)	0.04 (0.03)
δ : Interaction	0.00 (0.03)	-0.08 (0.03)	-0.01 (0.03)	0.07 (0.03)	0.05 (0.03)	-0.02 (0.03)
Respondents	991	991	991	991	989	990
N	2709	2709	2709	2709	2703	2705
R ²	0.002	0.006	0.002	0.019	0.021	0.003
Panel B: Cognitive Performance Measures						
Cog. Measure:	(1) Number Sequence	(2) Analogies	(3) Picture Vocab.	(4) Subjective Numeracy	(5) Comp. Check	
α : Constant	0.75 (0.02)	0.75 (0.02)	0.78 (0.02)	0.75 (0.02)	0.70 (0.02)	
β^* : O_{ij}	0.02 (0.02)	0.03 (0.02)	0.03 (0.02)	0.03 (0.02)	0.04 (0.02)	
γ : High Cog. Perf.	0.10 (0.03)	0.10 (0.03)	0.04 (0.03)	0.10 (0.03)	0.17 (0.03)	
δ : Interaction	0.02 (0.03)	0.00 (0.03)	0.03 (0.03)	0.00 (0.03)	-0.00 (0.03)	
Respondents	969	943	957	916	991	
N	2642	2579	2614	2510	2709	
R ²	0.023	0.017	0.007	0.018	0.051	

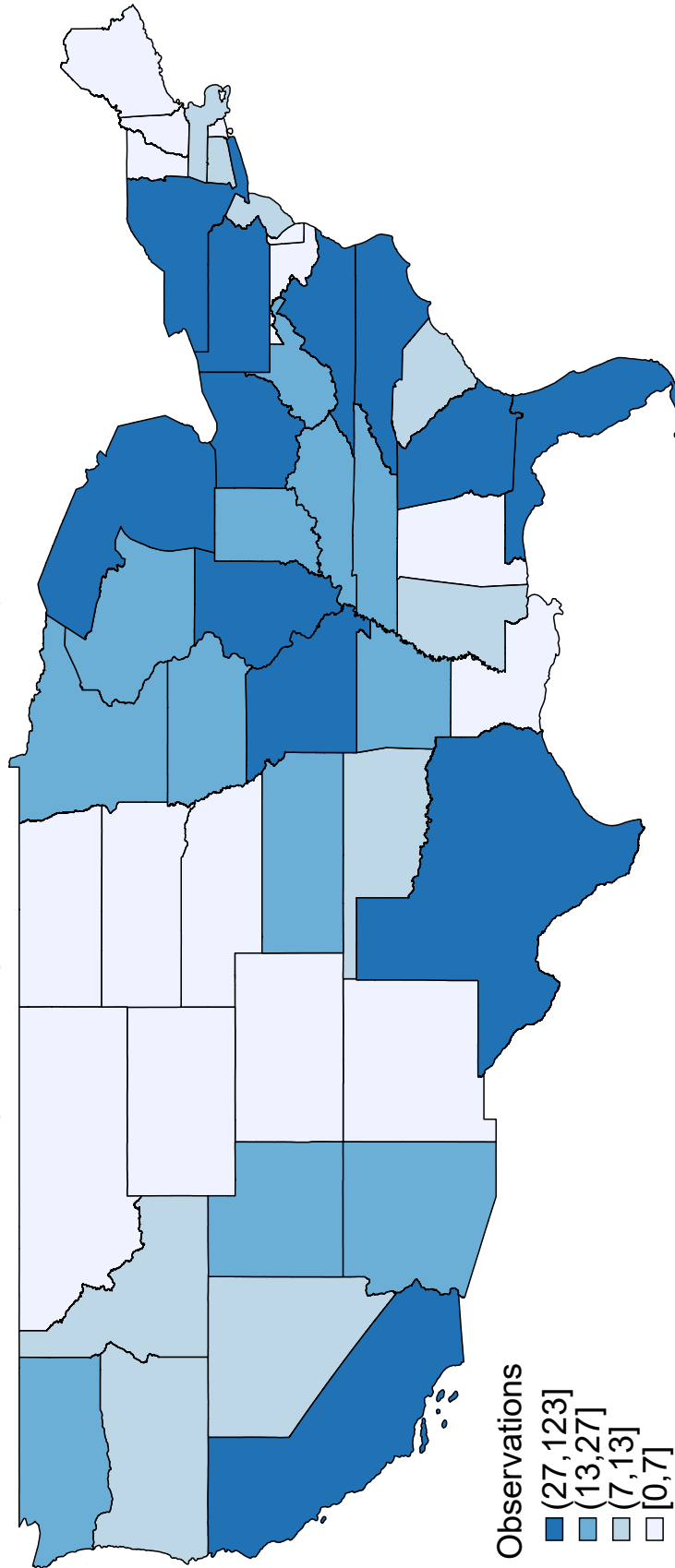
Notes: This table reports regressions analogous to that in column 2 of Table 2, but additionally including a control for group affiliation and an interaction with the estimated discontinuity. High education indicates that the respondent completed an Associate's degree or higher. High income indicates that the respondent's household income is \$50,000 per year or more. High age indicates that the respondent is 50 years old or higher. In panel B, we present similar analyses based on splitting the sample by tests of cognitive performance. Standard errors, clustered by respondent, are reported in parentheses.

Table 5: Cross-Group Differences in Rate of Payoff-Maximizing Choice

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Performance: Number Sequences	0.03 (0.02)			0.03 (0.02)	0.03 (0.02)	0.03 (0.02)	
High Performance: Analogies		0.03 (0.02)		0.02 (0.02)	0.03 (0.02)	0.02 (0.02)	
High Performance: Picture Vocab.			0.01 (0.02)	0.00 (0.02)	-0.00 (0.02)	-0.00 (0.02)	
High Performance: Subjective Numeracy					-0.01 (0.02)	-0.02 (0.02)	
Passed Comp. Check					-0.01 (0.02)	-0.01 (0.02)	
Male						0.02 (0.02)	0.02 (0.02)
Married						-0.03 (0.02)	-0.03 (0.02)
Working						0.01 (0.02)	0.00 (0.02)
High Education						0.03 (0.02)	0.04 (0.02)
High Income						0.02 (0.02)	0.01 (0.02)
High Age						-0.01 (0.02)	-0.02 (0.02)
Respondents	991	964	979	964	921	917	1009
N	5946	5784	5874	5784	5526	5502	6054

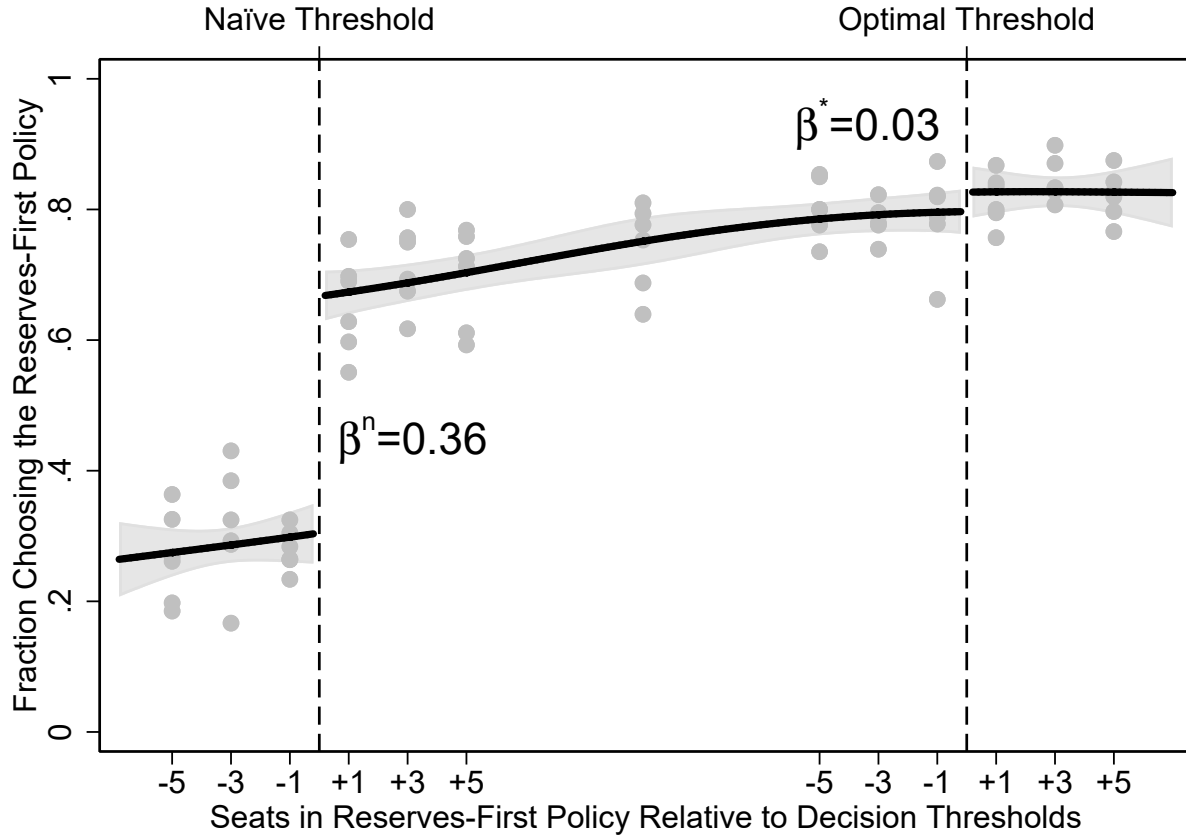
Notes: This table reports average marginal effects of logit regressions predicting the choice of the payoff maximizing policy with cognitive performance and demographic measures. The “high performance” measures are indicator variables indicating above-median performance on the cognitive measure of interest. High education indicates that the respondent completed an Associate’s degree or higher. High income indicates that the respondent’s household income is \$50,000 per year or more. High age indicates that the respondent is 50 years old or higher. All other variables are indicators of their respective title. Standard errors are reported in parentheses, and are calculated by applying the delta-method to the clustered (by respondent) standard errors of the logit coefficient estimates.

Figure 1: Geographic Distribution of Survey Respondents



Notes: This figure presents the number of respondents in our sample who reside in each state. Four respondents are omitted: 1 from Alaska, 1 from Hawaii, and 2 with unknown states of residence. The ranges of values indicated in the legend are split to form quartiles.

Figure 2: Illustration of Regression Discontinuity Estimates



Notes: This figure illustrates the discontinuities in choice probabilities that occur at the thresholds of interest. In our experiment, subjects faced six scenarios containing choices between reserves-first and reserves-last policies. The scenarios always contained the same six reserves-last policies. In each scenario, the number of seats in the reserves-first policy was randomly drawn from 13 values spanning the the x-axis, defined by their position relative to two thresholds. Vertical dashed lines demarcate these thresholds: the point where the number of reserves-first seats comes to exceed the number of reserves-last seats (the naïve threshold), and the point where the number of reserves-first seats comes to exceed the amount needed to make choosing the reserves-first policy optimal (the optimal threshold). The six dots above each point on the x-axis illustrate the average rate of choosing the reserves-first policy across the six scenarios. As seen in this figure, subjects' average propensity to choose the reserves-first policy increases substantially when the naïve threshold is exceeded, but does not change substantially when the optimal threshold is exceeded. The plotted line is a fitted cubic spline over these points, with its associated 95% confidence interval. Reported in the figure are the formal estimates of the discontinuity at these two points arising from this spline, which closely matches the results from Table 2.