Reversing Reserves: Online Appendix

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Contents

A Data Access

Data files and a survey codebook are available at `https://uasdata.usc.edu/survey/UAS+210`.

B Preregistration

The following pages provide a copy of our preregistration.

ASPREDICTED

Reversing Reserves: UAS Study (#31773)

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1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
Affirmative action policies are often implemented by reserving seats for targeted groups. In a matching procedure, the order of processing of these reserve seats can significantly influence the targeted group's admission rate. Processing these seats first provides a "minimum guarantee," which can have little effect on the outcome if the minimum number of seats guaranteed is lower than would exist without the reserve. In contrast, processing these seats last provides an "over and above guarantee," which assures that some additional seats will be given to the targeted group.

We believe that the importance of processing order is often misunderstood and viewed as counter intuitive. In many settings, letting someone "go first" has advantages, and we believe that this leads individuals to at times support ineffective affirmative action policies in reserve procedures.

In this study, we present simple scenarios designed to reveal subjects' understanding of the importance of processing order.

3) Describe the key dependent variable(s) specifying how they will be measured.
The key dependent variable is an incentivized choice between two options for how reserve seats can be processed. The subject is put in an experimental matching task that governs the bonus earned for the experiment. They are identified as part of a group favored by an affirmative action policy and are given two options for how that policy is implemented. The options involve different numbers of seats reserved, and differ on whether the seats are processed first or last. Each subject faces six of these scenarios.

4) How many and which conditions will participants be assigned to?
The key randomization in the experiment is the number of seats reserved in the scenarios described above. The subject faces 6 scenarios, in which the number of seats reserved in the "process last" option take the values {40, 44, 48, 52, 56, 60}. In each of these scenarios, the number of seats reserved in the "process first" option is randomly drawn according to the following rubric. Given a number of "process last" seats, there are two thresholds of interest for the number of "process first" seats: the threshold where the same number of seats are reserved, and the threshold where the consequences of the policies are identical. For each of these thresholds (T), we uniformly sample seat numbers taking the values of {T-5, T-3, T-1, T+1, T+3, T+5}, as well as for a single point approximately halfway between the two thresholds.

Beyond the randomization of seat numbers, a key condition of interest is the framing of the scenarios. There are two versions of the prompts: one framed as a problem about admissions to a school, and one framed as a problem about the granting of work visas. We include these two conditions because they reflect two key field applications of the reserve policies we study. We do not have ex-ante hypotheses about whether one setting would be more susceptible to suboptimal behavior.

There are also several "conditions" in the sense that they involve randomization, but which are included simply to test for worrying confounds. Within the experiment, the ordering of choice options is randomized. Furthermore, we describe the two groups as the blue and green group, and randomize which group is disadvantaged.

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
We are interested in the rate of optimal choices in these scenarios. We believe that there will be a tendency to select the "process first" option when it is against the subjects' best interests. We additionally believe this is partially driven by a tendency to make the choice purely based on the number of seats reserved without attending to the processing order.

Define a dummy variable (Y) that takes the value of 1 if the subject chooses the "process first" option. Denote the randomly generated number of seats for the "process first" option as S. Define two thresholds: T1) the number of seats in the "process last" option, and T2) the minimum number of seats assigned in the process first option that results in it becoming the optimal choice.

As a primary test of reliance on each threshold, we will compare the mean of Y for the scenarios when the sampled number of "process first" seats is drawn from {T-5, T-3, T-1} as compared to the mean when the number of "process first" seats is drawn from {T+1, T+3, T+5}. We predict that we will see a larger difference in means occurring at the threshold associated with a larger number of reserved seats than we do at the threshold associated with the determination of the optimal choice.

We will explore the robustness of these results using a variety of regression-discontinuity approaches. Our regressions will take the form Y=constant + beta1*(S>=T1) + beta2*(S>=T2) +f(S) +epsilon, and will apply a variety of alternative means of estimating f(S).

As a baseline, we will conduct the analyses above pooling together both the “school” and “visa” version of these scenarios and pooling together the 6 iterations of the scenario presented. We will also reconduct these analyses restricting the data to either the school or visa version, and by restricting the data to each of the 6 iterations a subject faced.

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
Our analyses will be based on all observations flagged as complete by the Understanding America Study.

7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
The Understanding America Study will solicit responses until 1000 complete responses are obtained.

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
a) We will conduct exploratory analyses of the relationship between optimal choice in the scenario and the demographic predictor variables available in the Understanding America Study master data. These will take the form of logit regressions, in which the dependent variable is a dummy variable taking the value of 1 when the subjects chose the option that was most advantageous to themselves.

b) We include a set of 4 comprehension questions at the beginning of the study. As a baseline, we will include all subjects who complete the survey, even those that fail the comprehension check. We will explore robustness of results to the exclusion of subjects who fail these questions.

c) In all analyses containing multiple observations from the same subject, we will cluster standard errors at the subject level.

d) The UAS provides sample weights, the use of which improves the representativeness of the sample. For all of our analyses described above, we will conduct both the standard, unweighted version of the analysis as well as the weighted version of the analysis. To the extent that results diverge, we consider the weighted version to be the estimate of primary interest.

C Motivating Field Environments

In this section we briefly review three of the field environments that motivated our pursuit of this 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.

C.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 the computer program 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.

C.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 1.2. 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 manners that do not appear intended to influence skill bias. 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 cases where applications arrived sufficiently quickly. 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 1.3, 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 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.

C.3 Constitutionally Mandated Reserves in India

In India, a reserve for members of historically disadvantaged castes is applied in some school-assignment and government-job allocation procedures.[1] 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"[2] 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 opportunity[3] 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 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 rushed[4], and perhaps as a result the bill did not specify

[1] Formally, the primary groups considered are the "scheduled castes," "scheduled tribes," and "other backwards castes." Each label is precisely defined in law.

[2] See Article 16(4) in the Constitution of India (1949).

[3] As specified in Article 16(1) in the Constitution of India (1949).

[4] A 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

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.[5] 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 memo[6] 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 battle over the constitutionality of this policy.[7]

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.[8] 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.

For for further detail and market-design analysis of the reserve systems described above, see Sönmez and Yenmez (2019*a*,*b*).

C.4 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 three cases are not alone. For example, there is similar potential for confusion

for the bill's speedy passage. See *R.S. Bharathi v. The Union of India* (2019), Madras High Court.

[5]This 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).

[6]Memo available here: https://dopt.gov.in/sites/default/files/ewsf28fT.PDF. Last accessed: 3/24/2020.

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

[8]See, e.g., Ashraf (2019), Dhingra (2019), Mathew (2019), or Mishra (2019).

in the deployment of reserve systems for school admissions in Chicago (see Dur, Pathak and Sönmez, 2020) and in New York City (NYCDOE, 2019).

While we believe that misunderstanding is widespread in these environments, we do emphasize that elements of our discussion above are speculative. Our desire for principled testing of these concerns motivated our development of the experimental paradigm in this paper.

D Illustration of Modeling Approach in a Simple Example

To assist in illustrating the application and interpretation of our empirical strategy from Section 2, we provide a simple example. Because the nature of calculations and interpretation is similar at both thresholds, in this example we will limit attention to assessing the discontinuity at the optimal threshold.

Consider four individuals with known choice functions.

The first individual chooses optimally: $\bar{C}_1(s^{RF}, s^{RL}) = C^*(s^{RF}, s^{RL})$. Inserting individual 1's average choice function into equation 2 yields the estimate $\mathbb{E}[p_i^*] = 1$, correctly identifying that this individual always applies the optimal choice function.

The second individual chooses naïvely: $\bar{C}_2(s^{RF}, s^{RL}) = C^n(s^{RF}, s^{RL})$. Inserting individual 2's average choice function into equation 2 yields the estimate $\mathbb{E}[p_i^*] = 0$, correctly identifying that this individual never applies the optimal choice function.

The third individual chooses nearly optimally, but assesses the threshold $T^*(s^{RL})$ with smoothly-distributed error ϵ_3. This decision-making process may be modeled as a new choice rule: $\bar{C}_3(s^{RF}, s^{RL}) = C^3(s^{RF}, s^{RL})$, where $C^3(s^{RF}, s^{RL}) = \int C^*(s^{RF} - \epsilon_3, s^{RL}) df(\epsilon_3)$. Because ϵ_3 is smoothly distributed, C^3 has no discontinuity at the optimal threshold. As a result, inserting individual 3's average choice function into equation 2 yields the estimate $\mathbb{E}[p_i^*] = 0$.

The fourth individual chooses nearly optimally, but assess the threshold $T^*(s^{RL})$ with discretely-distributed error ϵ_4. The error term ϵ_4 has a 25% chance of being 1, a 25% chance of being -1, and a 50% chance of being 0. This decision-making process may be modeled as a mixture of the optimal choice rule and two new choice rules: $\bar{C}_4(s^{RF}, s^{RL}) = .5C^*(s^{RF}, s^{RL}) + .25C^{4a}(s^{RF}, s^{RL}) + .25C^{4b}(s^{RF}, s^{RL})$, where $C^{4a}(s^{RF}, s^{RL}) = C^*(s^{RF}-1, s^{RL})$ and $C^{4b}(s^{RF}, s^{RL}) = C^*(s^{RF}+1, s^{RL})$. Inserting individual 4's average choice function into equation 2 yields the estimate $\mathbb{E}[p_i^*] = .5$, capturing the fact that this individual uses the optimal choice function 50% of the time.

Finally, consider the inference that would arise if this approach were applied to the aggregate choice function arising from these four individuals. The calculations in equation 2 yield the estimate $\mathbb{E}[p_i^*] = 0.375$. This estimate reflects the fact that, in population, one quarter of individuals use the optimal choice function with probably 1, one quarter of individuals use the optimal choice function with probability .5, and the remaining individuals use the optimal choice function with probability 0.

One purpose of this illustration is to demonstrate the interpretation of our estimates when individuals apply choice functions "close to" our two choice functions of interest. In general, our estimates are best understood as the probability mass of decisions made with application of the literal choice function of interest. Subtle variants do not "count" towards this mass. If a reader is interested in more permissive classifications, we note that the estimates that we provide give lower bounds on the rate of usage of any class of choice function that includes the choice function of interest.

E Further Details of the Understanding America Study

E.1 Additional Discussion of Sampling Procedure

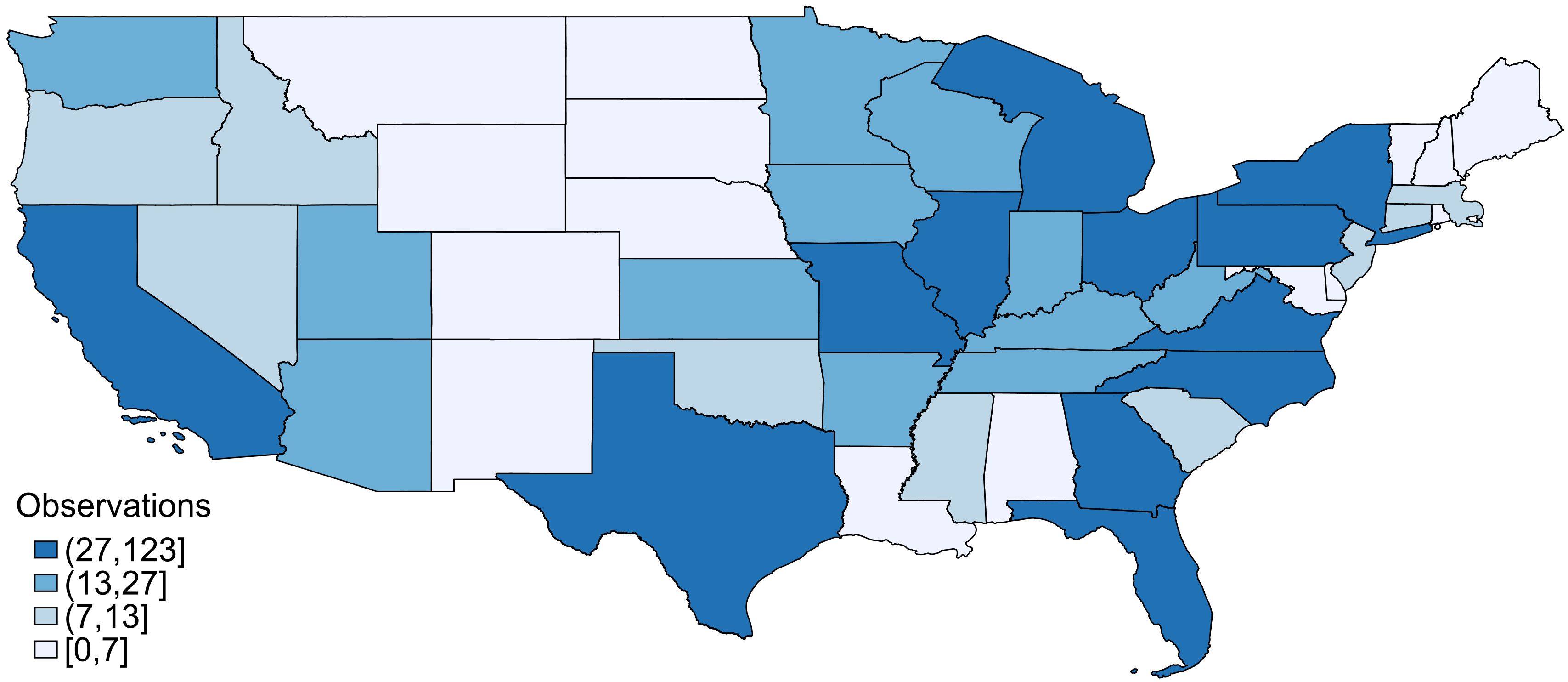
The UAS panel is recruited through address-based sampling. Respondents are targeted for recruitment based on a random draw from postal records. Once targeted for recruitment, substantial efforts to integrate the individual into the panel are pursued. After an initial attempt to recruit a targeted respondent 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 introduces 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 unobservables remains possible. Despite this concern, we note that the procedures described here minimize this worry relative to other commonly-used experimental platforms. Furthermore, we reconstruct our primary analyses making use of sampling weights aimed to correct for these issues in Appendix F.4.

E.2 Tests for Geographic Selection into Survey Participation

Figure A1 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: $p = 0.28$).

Figure A1: 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.

F Additional Results and Robustness Considerations

F.1 Predictors of Optimal and Naïve Choices

In this subsection we explore cross-group differences in reserve policy choices.

To help assess the predictors of the choice functions of interest, we reconduct 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 functions within each group.

Formally, we estimate regressions of the following form.

$$Y_{ij} = \alpha + \beta^n N_{ij} + \gamma G_i + \delta G_i \times N_{ij} + \epsilon_{ij} \tag{A1}$$

$$Y_{ij} = \alpha + \beta^* O_{ij} + \gamma G_i + \delta G_i \times O_{ij} + \epsilon_{ij} \tag{A2}$$

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.

F.1.1 Predictors of Adopting the Naïve Choice Function

We begin by examining estimates of equation (A1) above, 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 A1. 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 at 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 ($s.e. = 4pp$). We find no statistically significant differences 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 Sóvágó, 2018; Rees-Jones, Shorrer and Tergiman, 2020; Hassidim, Romm and Shorrer, 2021). To

Table A1: 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	0.32	0.33	0.37	0.37	0.28
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
β^n: N_{ij}	0.39	0.35	0.35	0.29	0.28	0.40
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
γ: Group	-0.04	-0.06	-0.08	-0.15	-0.14	0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
δ: Interaction	0.03	0.10	0.09	0.20	0.21	0.00
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(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	0.36	0.31	0.34	0.39
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
β^n: N_{ij}	0.30	0.32	0.37	0.33	0.24
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
γ: High Cog. Perf.	-0.17	-0.16	-0.06	-0.11	-0.20
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
δ: Interaction	0.23	0.20	0.07	0.17	0.31
	(0.04)	(0.04)	(0.04)	(0.04)	(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 with the additional inclusion of 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.

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?").[9] 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.[10] 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 3.

Panel B of Table A1 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.[11]

[9] For 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: c12avgsnsscore.

[10] Whenever 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.

[11] For comparability to the panel A results, panel B presents analyses of discrete above/below median indicators for the cognitive performance measures. Note that the same qualitative results arise from examination of the underlying continuous measures.

F.1.2 Predictors of Adopting the Optimal Choice Function

We next examine estimates of equation (A2), 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 A2 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 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.

F.1.3 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 A3 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

Table A2: 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	0.75	0.79	0.76	0.74	0.77
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
β^*: O_{ij}	0.03	0.08	0.04	-0.01	0.01	0.04
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)
γ: Group	0.00	0.07	0.01	0.06	0.09	0.04
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
δ: Interaction	0.00	-0.08	-0.01	0.07	0.05	-0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(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	0.75	0.78	0.75		0.70
	(0.02)	(0.02)	(0.02)	(0.02)		(0.02)
β^*: O_{ij}	0.02	0.03	0.03	0.03		0.04
	(0.02)	(0.02)	(0.02)	(0.02)		(0.02)
γ: High Cog. Perf.	0.10	0.10	0.04	0.10		0.17
	(0.03)	(0.03)	(0.03)	(0.03)		(0.03)
δ: Interaction	0.02	0.00	0.03	0.00		-0.00
	(0.03)	(0.03)	(0.03)	(0.03)		(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 A3: Cross-Group Differences in Rate of Payoff-Maximizing Choice

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Performance:	0.03			0.03	0.03	0.03	
Number Sequences	(0.02)			(0.02)	(0.02)	(0.02)	
High Performance:		0.03		0.02	0.03	0.02	
Analogies		(0.02)		(0.02)	(0.02)	(0.02)	
High Performance:			0.01	0.00	-0.00	-0.00	
Picture Vocab.			(0.02)	(0.02)	(0.02)	(0.02)	
High Performance:					-0.01	-0.02	
Subjective Numeracy					(0.02)	(0.02)	
Passed Comp. Check					-0.01	-0.01	
					(0.02)	(0.02)	
Male						0.02	0.02
						(0.02)	(0.02)
Married						-0.03	-0.03
						(0.02)	(0.02)
Working						0.01	0.00
						(0.02)	(0.02)
High Education						0.03	0.04
						(0.02)	(0.02)
High Income						0.02	0.01
						(0.02)	(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. The variable "Passed Comp. Check" takes the value of 1 for subjects who answered all comprehension check questions correctly. 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.

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 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.

F.1.4 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.

F.2 Confirmatory Evidence from Amazon Mechanical Turk

In this section we report the results of two large-scale pilots on Amazon Mechanical Turk (MTurk). Both pilots examined the "school choice" version of the study. 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 the main text.

F.2.1 MTurk Study 1

Design: The design of MTurk Study 1 closely mirrors the text of the school-choice scenario described in Section 3.2 with two important differences.

First, and most importantly, subjects answered only a single incentivized question. This question offered a choice between an RL policy with 40 seats reserved and an RF policy with 60 seats reserved. Recall that in our scenarios one should expect a 50-50 composition of reserve-qualifying and general-category students among the top 100 students. With near certainty, assigning seats purely based on priority would result in 60 or fewer reserve-category students being admitted. In these cases, the RF policy would result in 60 seats assigned to the reserve group.[12] In contrast, the RL policy assigns 70 seats to the reserve group in expectation: half of the 60 open seats, and all 40 of the reserve seats. In this scenario, the RL policy is payoff maximizing despite having 20 fewer seats reserved for the respondent's group.

Second, this study contained an additional unincentivized question eliciting perceptions of the importance of processing order. Recall that the set-up of the reserve system is communicated to subjects in an initial example presenting two policies: 30 seats reserved first or last. As in the UAS study, this question is followed by 4 comprehension check questions. Unlike the UAS study, after the comprehension check questions, half[13] of respondents are

[12] In the $< 1\%$ of remaining cases, more than 60 reserve-category students are admitted, but the RF policy has no effect on the outcome relative to an assignment procedure with zero reserved seats.

[13] While we were interested in subjects' responses to this question, we worried that asking this question could influence later responses to the incentivized scenario of primary interest. We randomized whether this question was presented in order to collect this data while allowing us to statistically test for this worrying possibility of contamination. We ultimately found no evidence that later answers varied by the presence of

asked to indicate which policy is better for the target group. Three options were presented: the 30-seat RL policy, the 30-seat RF policy, or the option to say that both policies are the same.

The preregistration for this study is available here: https://aspredicted.org/5rn99.pdf.

Deployment: Our study was deployed on MTurk in May, 2019. We targeted a sample for analysis of 500 observations. Pursuing this target, we solicited 639 complete responses to our study, with 508 completing all comprehension questions correctly and being eligible for inclusion in our sample.[14] These 508 observations constitute our sample for analysis.

Summary of Results: Examining the incentivized choice between a 40-seat RL policy and a 60-seat RF policy, we found that only 34% of respondents chose the payoff-maximizing RL policy. Additionally, in the unincentivized question asking which processing order most benefits the target group, 42% of respondents correctly indicated the RL policy, 24% of respondents incorrectly indicated the RF policy, and 33% of respondents incorrectly stated that both processing orders have the same effect. Similar to our results in the main text, these results support the idea that many subjects fail to pursue payoff-maximizing choices regarding the design of a reserve system, and that a plurality of respondents believe that processing order does not matter (partially explaining the first result).

F.2.2 MTurk Study 2

Design: Unlike MTurk Study 1, which presented a single scenario, Study 2 presents 6 scenarios with seat numbers sampled in a similar manner as in the UAS study, with the same preregistered main analyses. While this study is nearly identical to that in the UAS, two differences are noteworthy.

this question.

[14]In the UAS study, in accordance with our preregistration, we include subjects in the main analysis regardless of whether they completed their comprehension check correctly. In the MTurk studies, we followed a different (but also preregistered) strategy of excluding subjects who failed the comprehension checks. Excluding these subjects is a common practice on MTurk, meant to help screen for "unserious" respondents and for bots. This is not a concern in the UAS, which is what motivated our decision to no longer exclude these respondents (particularly given that such exclusions leads to a selection problem that undoes some of the benefits of the UAS's representative sampling).

First, recall that the number of seats for the RF policy in the UAS was 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. This resulted in most data being sampled from the regions "near" the thresholds of interest, with only a single point sampled in between these two regions. In MTurk Study 2, the number of seats in the RF policy was sampled uniformly from all odd intergers between $T^n - 5$ and $T^* + 5$. This covers all values sampled in the UAS study, but with more possibilities sampled in the region that's not local to either threshold. Because our test critically relies on regression discontinuities at the thresholds, the new sampling structure used in the UAS was adopted to preserve power with a smaller sample size.

Second, we exclude data from one of the six scenarios due to a typo that appeared in its text.[15] Since we are interested in studying confusion that arises from intuitions about reserve policies, it is a confound if confusion could be explained by imprecise text.

The preregistration for this study is available here: https://aspredicted.org/tq7u8.pdf.

Deployment: Our study was deployed on MTurk in May, 2019. We targeted a sample for analysis of 2,000 observations. Pursuing this target, we solicited 2,625 complete responses to our study, with 2,054 completing all comprehension questions correctly and being eligible for inclusion in our sample. These 2,054 observations constitute our sample for analysis.

Summary of Results: Appendix Table A4 reproduces the main analyses of Table 2 conducted in the MTurk data. Across all columns, the estimated rate of use of the naïve choice function is higher in the MTurk data, but differences never exceed 8 percentage points. These results similarly lead to the conclusion that a substantial fraction of subjects exhibit a nearly sophisticated understanding of optimal behavior, with errors driven solely by an incorrect belief that processing order is irrelevant. In contrast to the results in the UAS, these estimates suggest that a statistically significant proportion of respondents applied the optimal choice function. However, this difference may be driven by the higher precision of estimates in the larger MTurk sample. The estimated rate of use of the optimal choice function is

[15] In this question, at one point where the two stages of the reserve policy are explained, the number of seats processed in the first and second stage are permuted for one of the policies.

Table A4: Estimates of Choice Functions Governing Policy Preferences: MTurk Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β^n: N_{ij}	0.47		0.40		0.43	0.42	0.44	0.41
	(0.02)		(0.04)		(0.03)	(0.04)	(0.02)	(0.03)
β^*: O_{ij}		0.06		0.06	0.05	0.04	0.02	0.04
		(0.01)		(0.03)	(0.02)	(0.03)	(0.02)	(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 × f	No	No	No	No	No	Yes	No	Yes
Respondents	1782	1795	1013	1013	2054	2054	2054	2054
N	3368	3505	10270	10270	10270	10270	10270	10270
R^2	0.225	0.007			0.213	0.215	0.213	0.213

Notes: This table reports regressions of an indicator for choosing RF policy on controls for the number of seats reserved. All analyses are identical to those in Table 2, but use the MTurk Study 2 sample instead of the UAS sample. Standard errors, clustered by respondent, are reported in parentheses.

6% in the preferred analysis of column 2—i.e., our finding that applying the optimal choice function is very rare continues to hold.

F.2.3 Explanation of Change in Sampling Policy

In both MTurk studies, subjects who failed to correctly complete the comprehension check after the initial instructions were not permitted to continue the survey and thus are not included in our data. Requiring the successful completion of a comprehension check is a common technique applied on MTurk. It helps screen inattentive participants, "survey farmers," and bots from the study, which both improves data quality and limits study costs. While excluding these subjects has some advantages, it does potentially result in some dimensions of selection in the final sample—for example, excluding real and attentive participants who otherwise struggle with comprehension of the task at hand. While we continue to believe this is on net a desirable policy in an MTurk study, we chose not to continue imposing this selection criteria in the UAS study for three reasons. First, a major advantage of the UAS is the representative nature of its sample, which this policy would disrupt. Second, the screening and validation of panelists conducted by the UAS makes concern about bots and survey farmers unnecessary. And third, examining how decision-rule adoption varies by comprehension (as we do in Appendix F.1) is potentially interesting in cases where indications of low comprehension do not create worries of invalid data (as we would worry on MTurk). This line of reasoning led to our decision to change our inclusion criteria across our preregistrations.

F.3 Scenario-Specific Results

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 A5 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,[16] 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.

F.4 Sample Weights

As emphasized in Appendix E.1, 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 our main analyses with the inclusion of sampling weights (see Appendix Table A6). 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.

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

Table A5: Estimates of Choice Functions Governing Policy Preferences (by Scenario)

Panel A: School-Choice Scenario

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β^n: N_{ij}	0.43		0.38		0.40	0.40	0.40	0.38
	(0.03)		(0.06)		(0.03)	(0.05)	(0.03)	(0.06)
β^*: O_{ij}		0.04		0.08	0.01	0.09	0.00	0.07
		(0.02)		(0.05)	(0.03)	(0.04)	(0.03)	(0.05)
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 × f	No	No	No	No	No	Yes	No	Yes
Respondents	498	498	511	511	511	511	511	511
N	1437	1358	3066	3066	3066	3066	3066	3066
R^2	0.185	0.003			0.220	0.226	0.220	0.225

Panel B: Visa-Allocation Scenario

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β^n: N_{ij}	0.38		0.40		0.33	0.35	0.33	0.35
	(0.03)		(0.06)		(0.03)	(0.05)	(0.03)	(0.05)
β^*: O_{ij}		0.03		-0.04	-0.03	-0.03	-0.02	-0.01
		(0.02)		(0.05)	(0.03)	(0.04)	(0.03)	(0.05)
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 × f	No	No	No	No	No	Yes	No	Yes
Respondents	492	493	502	502	502	502	502	502
N	1428	1351	3012	3012	3012	3012	3012	3012
R^2	0.142	0.001			0.179	0.182	0.180	0.183

Notes: This table reports regressions of an indicator for choosing the RF policy on controls for the number of seats reserved. Each panel reproduces Table 2, restricting the data to one of the two scenarios. The top panel presents results for the school-choice scenario, and the bottom panel presents results for the visa-allocation scenario. Standard errors, clustered by respondent, are reported in parentheses.

Table A6: Estimates of Choice Functions Governing Policy Preferences (with Weights)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β^n: N_{ij}	0.39		0.37		0.34	0.38	0.34	0.36
	(0.03)		(0.06)		(0.03)	(0.05)	(0.03)	(0.05)
β^*: O_{ij}		0.03		-0.02	-0.01	-0.02	0.00	-0.01
		(0.02)		(0.05)	(0.03)	(0.05)	(0.03)	(0.05)
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 × 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. All analyses are identical to those in Table 2, modified only to weight observations according to the procedures described in Section 6.3.1. Standard errors, clustered by respondent, are reported in parentheses.

F.5 Importance of Stake Size

In our experiment, the financial reward for admission in the simulation is a \$5 bonus payment.[17] 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

[17]Furthermore, 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. This difference translates to an increase in the expected value of an optimal choice of 63 cents.

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 Austrialian 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 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.

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