EMPIRICAL ANALYSES OF QUEUES WITH APPLICATIONS TO ELECTIONS AND HEALTHCARE

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Dedicated to my wife Christine, our sweet dog Bella, my Grandma Lillian who passed away during the COVID-19 pandemic, and the rest of my ‘ohana.

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ABSTRACT

EMPIRICAL ANALYSES OF QUEUES WITH APPLICATIONS TO ELECTIONS AND HEALTHCARE

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In Chapter 1, we conduct a dynamic panel data study of voting resource allocation within Florida counties. We find that a 1% increase in the percentage of voters registered as Democrat in a county results in a 2.8% increase in the number of registered voters per poll worker. Furthermore, using a queue simulation, we estimate that a 5% increase in voters registered as Democrat in a county could increase the average wait time to vote from 40 minutes (the estimated average wait time to vote in Florida in 2012) to approximately 100 minutes. Our study recommends that states regulate the number of voters per poll worker or voting machine in polling locations so that wait times are equated across all voters.

In Chapter 2, we perform a differences-in-differences analysis on cross-sectional voter wait time data across the 2006, 2008, 2012, and 2016 Georgia elections. We estimate that polling place closures increased Georgia's average wait time to vote in the 2016 election by 7 minutes or approximately 78% (based on Georgia's average wait time of 16.5 minutes in the 2016 election). This increase in the average wait time to vote suggests that in the 2016 election, Georgia may have idled its spare capacity (e.g., voting machines) following polling place closures. As a result, we suggest that states implement policies that require the redistribution of all functioning voting machines from closed polling places or at least increase transparency in how voting resources are used in elections.

In Chapter 3, we use an instrumental variable estimation and find that patients prefer

waiting for endoscopies in pre-op rather than reception. Additional experiments suggest that pre-op is less favorable due to its intensity (e.g., clinical, emotional). We also find that in transparent, shared waiting areas where patients do not observe the doctor assignment of others, patients may still monitor queue discipline. Finally, patients may be negatively impacted by waits that conclude after a scheduled appointment time but care less about waits that conclude early. This study emphasizes the importance of businesses managing queues where customers wait in multiple locations with different attributes.

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Coauthored with Gérard P. Cachon

1.1. Introduction

Given the importance of voting in a democracy, a considerable amount of attention and study has been applied to how the design of the voting process can influence elections: e.g., gerrymandering of district boundaries (Abramowitz (1983); Cain (1985); Chen and Cottrell (2016); Friedman and Holden (2009)) and voter suppression through voter identification laws or the service provided by local election officials (Hood III and Bullock III (2012); Bentele and O'Brien (2013); White et al. (2015); Hajnal et al. (2017); Stein et al. (2019)). Our study investigates the allocation of voting resources, in particular the number of poll workers. All else being equal, the more registered voters there are per poll worker, the more time voters will experience in the voting process (waiting in queue, checking in, and casting a ballot) (Stein et al. (2019)). Long wait times to vote are not ubiquitous in the United States, but, depending on the election and location, some voters do experience wait times of 30 minutes or more (Ansolabehere and Shaw (2016)), the standard set by a Presidential Commission (Bauer and Ginsberg (2014)). Long waits have been shown to influence choice in non-voting situations, such as blood donation (Gillespie and Hillyer (2002b)), waiting in a call center queue (Mandelbaum and Zeltyn (2013)), and grocery shopping (Lu et al. (2013a)), among others. In the context of voting, theory (Riker and Ordeshook (1968)) and empirical evidence (Alvarez et al. (2008); Cottrell et al. (2017b); Stein et al. (2019)) suggest that long wait times also have the potential to dissuade voters in current and future elections by raising the actual or perceived cost to vote. This raises the possibility that one political party could gain an advantage over another if they are able to allocate more resources towards their likely voters and away from voters who are likely to support the other side.

In our study, we aim to identify whether there is any disparity in poll worker staffing levels

Figure 1: Percent of non-white citizens who voted by state. Data taken from U.S. Census Bureau reports on the voting and registration for states by race (see `https://www.census.gov/topics/public-sector/voting/data/tables.html`). Florida had some counties covered by the Voting Rights Act which was meant to curb discriminatory voting practices in certain areas of the country prior to 2013, so its performance is compared to other states in the South (AL, GA, LA, MS, NC, SC, TX, VA) that had areas covered by the Voting Rights Act

within counties in Florida with respect to race and political party. We focus on Florida for several reasons: (i) it is viewed as an important state in presidential elections; (ii) it experienced well-publicized long polling queues in the 2012 election (e.g., Famighetti et al. (2014)); (iii) it has a checkered past on voting discrimination issues (e.g., Childress (2014); Klas (2016a); Wood (2016); Hawkins (2018)); (iv) unlike most other states, Florida provides county-level data that include racial and political party affiliation; and (v) Florida has lagged behind many of its peer states in terms of minority voter turnout (Fig. 1) and registration (see Fig. 13)

We study election and demographic data from 2004 to 2016 across all 67 counties in Florida. Our empirical strategy is to identify the effect of political party and race within Florida counties across time so as to control for unobserved heterogeneity across counties. To summarize our results, we find no evidence of a disparity in poll worker staffing directly due to race. However, we do find that as the percentage of Democrat party voters increases in a county, the number of registered voters per poll worker also increases, i.e., there are fewer resources per voter. Our estimates indicate a large and meaningful effect size. Thus, changes in the composition of political parties across time can lead to larger disparities in wait times to vote.

1.2. The Voting Process

In Florida, general elections occur every two years with presidential and midterm elections alternating. All voters in a county registered at least 29 days before an election are allowed to cast a ballot in an election. On an early voting day (a practice that was mandated in Florida from 2004) or Election Day, a registered voter who has not voted via an absentee (or "mail-in") ballot may go to an early voting site (for early voting) or her assigned polling place in her precinct (on Election Day) to vote. Based on information from the Florida Division of Elections, the in-person voting process in Florida includes two primary steps: check-in and voting.

At the check-in step poll workers ensure voters are registered to vote using a photo identification with a signature (a practice used in Florida from 1998). If a voter is deemed eligible to vote at the polling place, the voter proceeds to the voting stage. Otherwise, a provisional ballot may be issued and counted later if voter eligibility is verified. Voting can be done via an electronic or a paper ballot. To exit the voting process a voter submits the electronic ballot on a voting machine or processes a paper ballot through an optical scanner.

Queues form at either stage of the voting process whenever the arrival rate of voters exceeds the rate of service. A number of factors contribute to the queue lengths, such as the overall level of service capacity, the variability of demand throughout the day, the complexity of the ballot, and the skill of the poll workers. Although poll workers may be primarily responsible for check-in, they may also play an important role in voting (e.g., via distributing ballots, assisting voters with questions on how to use voting equipment, etc.). Hence, the number of poll workers is a key factor to determine the service experience in elections (Stein et al. (2019)).

Equity in the voting process has received considerable attention. Some states (but not Florida) have laws to ensure there is equality among precincts with respect to voting resources. For example, South Carolina requires (but does not strictly enforce) that precincts not exceed 250 registered voters per voting machine and 500 registered voters per three poll workers for general elections (Famighetti et al. (2014)). Several studies focus on analytical methods to assign voting resources with some form of equity across voters as part of the objective (Allen and Bernshteyn (2006); Yang et al. (2009); Olabisi and Chukwunoso (2012); Yang et al. (2013)).

There are a number of empirical studies on equality in the voting process (Highton (2006); Mebane Jr. (2005); Brady and McNulty (2011); Stewart III (2012); Clinton et al. (2019a); Shepherd et al. (2019)). With respect to Florida, a cross-sectional study of the 2012 general election finds that minorities faced longer wait times and racial disparities existed in the distribution of voting resources across precincts (Famighetti et al. (2014)). Cross-sectional studies are unable to control for unobserved differences across precincts that could influence voter waiting times that are not directly related to race or party affiliation (but may be correlated with race or party). The limited number of panel data studies on voter-reported wait times are unable to control for both race and party affiliation (Pettigrew (2017)). Without data on party affiliation it is not possible to distinguish between a direct racial bias and one that is due to a group's leaning towards an opposition party. For example, non-white voters tend to vote Democrat in the U.S. (Pew Research Center (2016)). Thus, a bias against Democratic voters would affect non-white voters as well as educated young white voters (who lean towards the Democratic party according to Pew Research Center (2015)), whereas a direct racial bias would affect only the former.

1.3. Data and Estimation

We study the 2008 to 2016 elections in Florida but collected data as early as the 2004 election to create lags for certain variables. Our data are from five sources: (1) Election Administration and Voting Survey (EAVS) conducted every two years by the U.S. Election Assistance Commission and collected, typically at the county level, from the 50 States, the District of Columbia, and the U.S. Territories; (2) the Florida Division of Elections publishes voter registration statistics for each of its counties in every election; (3) the U.S. Census Bureau data on annual demographic information on counties; (4) the Verified Voting Foundation data on voting equipment used across counties; and (5) the Federal Reserve Bank of St. Louis data on county-level housing prices. (See Section A.4.1 for more information on the origin of the data.)

$$
\begin{aligned}
logVotersPerPW_{i,t} =& \beta_1 PctDemocrat_{i,t} + \beta_2 PctWhite_{i,t} + \beta_3 logVotersPerPW_{i,t-1} + \\
& \beta_4 logVotersPerPW_{i,t-2} + \beta_5 logAbsentBallotsPerPP_{i,t} + \\
& \beta_6 logEarlyBallotsPerPP_{i,t} + \beta_7 logEDBallotsPerPP_{i,t} + \\
& \beta_8 logProvBallotsPerPP_{i,t} + \beta_9 logPersonPerSqMile_{i,t} + \\
& \beta_{10} PollDiff_{i,t} + \beta_{11} HousePrice_{i,t} + \beta_{12} logMedInc_{i,t} + \\
& \beta_{13} UseDRE_{i,t} + \beta_{14} Pct65Plus_{i,t} + \beta_{15} Presidential_t + \beta_{16} Time_t + \\
& c_i + \varepsilon_{i,t} \qquad (1.1)
\end{aligned}
$$

The proposed regression model is specified in Eq. 1.1. The dependent variable of interest is $logVotersPerPW_{i,t}$ which is the log of the total number of active registered voters per poll worker (across both early voting and Election Day) for county i in election year t (t=3 for election year 2008). According to the Florida Division of Elections, inactive (as opposed to active) registered voters are those who fail "to respond to an address confirmation final notice and there is no voting or voter registration record activity for two subsequent general election cycles." All else equal, a smaller $logVotersPerPW_{i,t}$ should be better for voters, i.e., lead to a shorter time to vote.

Our main regressors of interest are $PctDemocrat_{i,t}$ and $PctWhite_{i,t}$. $PctDemocrat_{i,t}$ is the percentage of active registered voters who identified as Democrat in county i in election year t. In Florida voters have an incentive to keep their political party affiliation up-to-date because only members of a party can vote in the party's primary. Over the time of our study, Florida consistently had a higher number of counties vote Republican in the presidential or gubernatorial elections. $PctWhite_{i,t}$ is the percentage of active registered voters in county i who identified as white in the election year t.

$PctDemocrat_{i,t}$ and $PctWhite_{i,t}$ act as controls for each other because (as discussed) non-white voters tend to vote Democrat in the U.S., but some white voters (younger and more educated) do as well. If some counties become relatively more Democratic while also decreasing their relative percentage of non-white voters, then including only one variable may not be able to identify the effects of interest.

During the time period in our sample, $PctWhite_{i,t}$ is decreasing on average, while $logVotersPerPW_{i,t}$ is on average increasing (see Table 1). All counties had a negative linear trend for $PctDemocrat_{i,t}$ and the average across counties decreased over the time period (see Table 1).

Table 1: Mean values of key variables across Florida's 67 counties within each general election.

Variable	2008	2010	2012	2014	2016
VotersPerPW	178.63	217.85	238.21	277.54	265.94
PctDemocrat	48.61	46.95	44.43	42.74	39.58
PctWhite	79.65	79.36	78.44	77.98	77.12
AbsentBallotsPerPP	270.33	182.56	387.29	314.01	447.01
EarlyBallotsPerPP	419.76	199.37	473.86	261.75	678.69
EDBallotsPerPP	530.26	480.01	620.79	523.51	548.87
ProvBallotsPerPP	4.00	1.87	6.42	1.98	3.90
PollDiff	3.07	3.21	3.37	3.39	4.00
PersonPerSqMile	339.77	338.23	342.46	349.71	360.72
HousePrice	174.30	132.69	119.34	132.22	152.62
MedInc	43959	44269	43876	43908	45205
UseDRE	0.94	0.82	0.78	0.70	0.36
Pct65Plus	17.17	17.56	18.26	19.21	20.32

*Values calculated based on one imputation for the variable

Included in Eq. 1.1 is a number of controls. Poll staffing should depend on forecasted demand in an election, both in the total number of voters (presidential election years have higher demand) and how votes are cast (early voting may require different staffing than Election Day voting). How votes are cast (paper vs. electronic, early vs. on-the-day) may be linked to party or race (e.g., if Democrats prefer early voting). We use two sets of proxies for these forecasts. The first set of proxies for an election's forecasted demand is the staffing used in the previous elections of the same county: we include two lags, $logVotersPerPW_{i,t-1}$ and $logVotersPerPW_{i,t-2}$ as controls. The second set of proxies relate to contemporaneous election turnout. Because staffing is done at the level of a polling place, in all cases we evaluate the turnout proxies as the ratio of the actual ballots in a county to the number of polling places in the county. $logAbsentBallotsPerPP_{i,t}$ is the log of the number of absentee ballots cast per polling place. Absentee ballots (also known as mail-in ballots) should represent a lighter workload per ballot cast for election workers, i.e., a positive β_5. $logEarlyBallotsPerPP_{i,t}$ is the log of the early voting ballots cast per polling place. Given that early voting occurs over multiple days and that voters physically cast a ballot just as they do during Election Day, we expect an increase in the early ballots cast per polling place to decrease $logVotersPerPW$ because more poll workers are needed over multiple days to service early voters, i.e., a negative β_6. $logEDBallotsPerPP_{i,t}$ is the log of the Election Day ballots cast per polling place. We expect increases in Election Day ballots cast per polling place to increase $logVotersPerPW$ because more of the demand for poll workers is concentrated on just one day, i.e., positive β_7. $logProvBallotsPerPP_{i,t}$ is the log of the provisional ballots cast per polling place (with one added in case a county reports zero provisional ballots). Although provisional ballots may not represent a large portion of total votes cast (less than 0.5% of total ballots cast from 2008 to 2016 in our sample), they can require a significant amount of work (Dixon (2012)), i.e., a negative β_8.

Staffing could depend on the difficulty to recruit poll workers (Burden and Milyo (2015)), which is a concern if this is linked to race or political party. In the EAVS, counties rate poll worker recruitment difficulty on a scale from 1 (very difficult) to 5 (very easy) for each election, which we include as the control $PollDiff_{i,t}$ in Eq. 1.1. For this variable there are four out of 67 counties missing data for 2014 and 30 out of 67 counties missing data for 2016. We use multiple imputation to account for the missing data values (see Section A.1.2).

Race and party affiliation is correlated with where a person lives (Parker et al. (2018)). The variability of demand throughout the day may depend on a precinct's degree of urbanization, and that variability may influence staffing. For example, an area with greater morning and evening demand spikes should require more staffing to achieve the same waiting time. We control for any effects of urban versus rural by including in Eq. 1.1 the log of the number of people per square mile in a county, $logPersonPerSqMile_{i,t}$.

We include in Eq. 1.1 the log of the median income of a county, $logMedInc_{i,t}$, and the "All-Transactions House Price Index", $HousePrice_{i,t}$ (normalized at a value of 100 in the year 2000) to control for differences in staffing that could be related to the wealth of a county over time (Spencer and Markovits (2010)).

To control for voter age, which may influence the time a voter needs to cast a ballot (Glenn and Grimes (1968)), we use the U.S. Census estimate of the percentage of the population within each county that is above the age of 65 ($Pct65Plus_{i,t}$).

The method of voting may influence the needed capacity (Spencer and Markovits (2010)). From 2008 to 2016, many Florida counties switched from direct recording electronics (DREs) machines to paper ballots (see Fig. 14). We set $UseDRE_{i,t}$ to 1 if a county used any DRE equipment in an election, otherwise it defaults to 0. The Verified Voting Foundation does not provide data on voting equipment for Florida counties in 2010, so we use multiple imputation to account for this missing year (see Section A.1.2).

To account for variation in interest across elections, we included a dummy variable, $Presidential_t$, to indicate whether the election year was a presidential election. We include a linear time trend in the dependent variable across all counties, $Time_t$ to control for statewide time dependent trends. For example, from Table 1, the total number of active registered voters per poll worker tends to rise across counties during the span of our study. Table 21 provides summary statistics for all key variables across all Florida counties and elections.

The regression in Eq. 1.1 includes county fixed effects, c_i, to control for unobserved heterogeneity across counties that does not vary across time yet influences the staffing level. The presence of fixed effects in Eq. 1.1 along with the lagged dependent variables and regressors raises a concern of endogeneity bias in its estimation (Nickell (1981)). We use Arellano and Bond (1991) dynamic panel data model with first differences and lagged variables as instruments to overcome this issue. We limit the number of lags used as instruments in the model (Bowsher (2002)). To be specific, for the voting resource regressors (*logVotersPerPW*, *logAbsentBallotsPerPP*, *logEarlyBallotsPerPP*, *logEDBallotsPerPP*, *logProvBallotsPerPP*) we use the second and third lags as instruments (corresponding to both a midterm and presidential election). For voter demographic variables (*PctDemocrat*, *PctWhite*), we use the second election lag as an instrument, and for poll worker recruitment difficulty (*PollDiff*), we use the most recent election lag as an instrument. We do not include lags of the other regressors (*logPersonPerSqMile*, *HousePrice*, *MedInc*, *UseDRE*, *Pct65Plus*) because we believe they should be uncorrelated with shocks to the number of voters or poll workers.

The Hansen test (robust to heteroscedasticity) for overidentifying restrictions assumes a null hypothesis that our instruments meet the exogeneity requirement. We do not find evidence that the exogeneity assumption is violated (Table 13). We also address two issues with Arellano-Bond estimation. First, it may perform poorly if instruments are weak, which could occur if changes in county election demographics were fully adjustable from one election to the next, thereby having no relation to past values. We believe, however, that county demographics are somewhat rigid over time. Consistent with that view, we do not find evidence of weakness using F-statistics from the first-stage 2SLS regressions of the first differences of each endogenous variable (pooled across counties and election years) on its lagged instrument(s) (see Table 14). Second, Arellano-Bond estimation requires serially uncorrelated errors, which is supported (Table 13)

1.4. Results

As shown in Fig. 2, our results provide support for a disparity in voting resources due to political party. (See Table 15 for our complete set of estimates.) For a base reference, Fig. 2, *Left* provides the fixed effects estimates from a model without instruments for endogenous regressors. Fig. 2, *Middle* and *Right* provide the estimates from our preferred models, the one-step and the two-step Arellano-Bond procedures, respectively. All three models indicate that the number of voters per poll worker increases as a county's percentage of Democrat voters increases. In particular, based on the two-step Arellano-Bond procedure (Fig. 2, *Right*), a one percent increase in the percentage of Democrat voters is associated with a 2.8% increase in voters per poll worker. This effect appears to be large - as a point of comparison, in a cross-sectional study of voter resource allocation in Florida's 2012 election, a one percent increase in the percentage of white voters is associated with an increase of 0.26% voters per poll worker on Election Day (Famighetti et al. (2014)). (See Section A.1.3 for more details on this benchmark calculation.)

In contrast to prior studies (Pettigrew (2017); Famighetti et al. (2014)), our results do not support the existence of a racial bias: the coefficients on *PctWhite* in both the one-step and two-step Arellano-Bond procedures are not significant. However, those studies are unable to control for political party affiliation. Given that race and political party are correlated, it is possible to conflate racial bias with a political party bias.

We estimate additional models (using the two-step Arellano-Bond procedure) to check the robustness of our results. In particular, we include different time controls, we utilize fewer instruments and lagged variables, we control for voters with no party affiliation, we control for the 2013 Supreme Court decision *Shelby County* v. *Holder* which influenced some of the counties in Florida during our study period, and we substitute our contemporaneous forecast turnout proxies with lagged variables. In all of these models the results are qualitatively similar to our main findings: there is a significant and negative coefficient for *PctDemocrat* and an insignificant effect of *PctWhite* (see Section A.1.4 and Tables 16 to 18 for descriptions of the robustness checks, results, specification tests, and weak instrument tests).

1.5. Simulation

We use a queue simulation tool, developed by Mark Pelczarski, to examine the impact that changing the number of voters per poll worker could have on the wait time to vote (see `https://web.mit.edu/vtp/calc3.htm` for more information on the tool). This tool simulates the wait times voters could experience during a voting day based on queueing

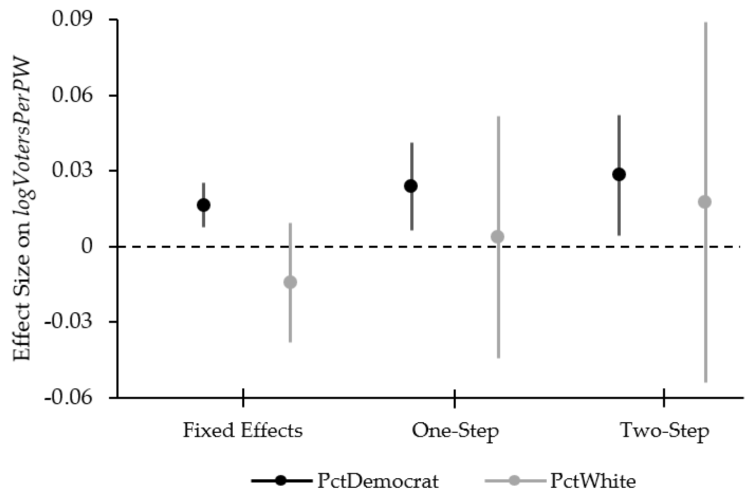

Figure 2: Displayed are coefficients from the regressions that estimate Eq. 1.1. Bars represent the 95% confidence intervals with robust standard errors used for the fixed effects and one-step estimation and Windmeijer corrected standard errors used for the two-step estimation (Windmeijer (2005))

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theory, historical data on polling places, and user-customized inputs on voter demand, voter arrival variability, and polling place capacity. We calibrate the simulation using data from the 2012 election in Florida because data are available on voter resource levels (Famighetti et al. (2014)) and the average voter wait time (Stewart III (2015)).

Table 19 reports the parameters selected for our base simulation. For the 2012 election in Florida, the average reported wait time is 42.3 minutes (Stewart III (2015)) and the average number of ballots cast per polling place on Election Day is 620.8 (EAVS). For our base simulation we selected 675 voters, 2 check-in machines, and 4 voting machines because the simulation tool with those parameters yields an average wait time of 40 minutes and 619 votes cast, similar to the actual results.

The simulation tool uses check-in stations and voting machines as the inputted resources. Our estimates focus on poll worker capacity. To make the linkage between our results and the simulation, we presume that voting resources are assigned proportionally. For example, it makes little sense to have three check-in stations and only one poll worker (or, with the other extreme, 6 poll workers).

We use three different methods to adjust capacity to measure the impact of a change in the percentage of Democrats on the average wait time. The first adjusts the average number of check-in stations in response to changes in *PctDemocrat*, leaving all other parameters constant, and assuming that the average change in check-in stations per polling place is proportional to our estimate for the average change in poll workers per polling place. (See Section A.1.5 for details.) The second method is analogous to the first except now the number of voting stations is reduced. The third method increases the number of voters per polling place holding voting resources constant

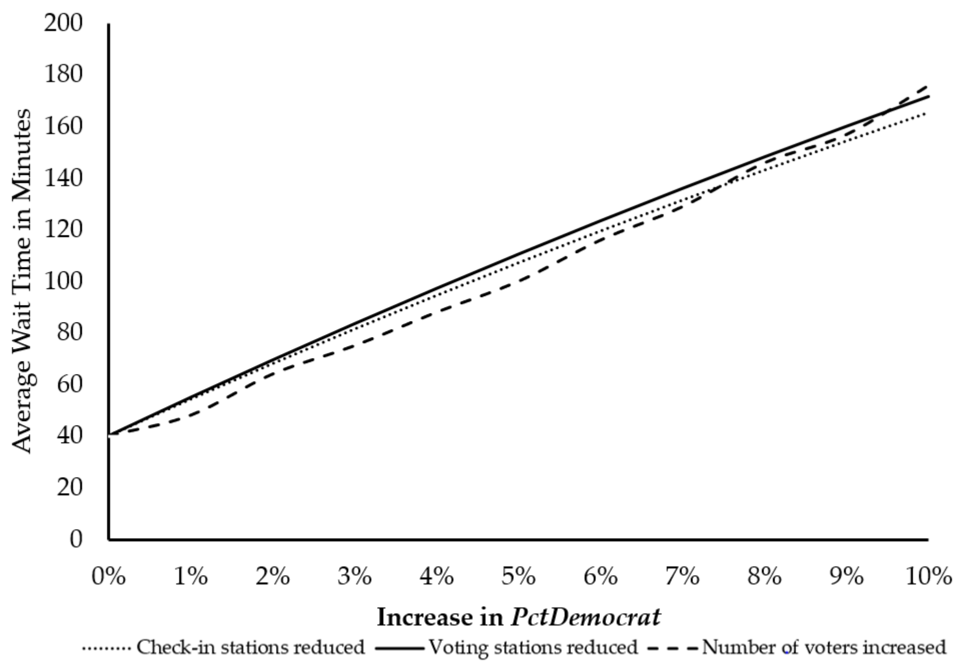


Figure 3: Estimated average wait time in a county resulting from increases in *PctDemocrat* using three different methods for adjusting capacity.

Fig. 3 reports the change in wait times as a function of the increase in *PctDemocrat*. The three methods provide comparable results. For example, a 5% increase in *PctDemocrat* raises the average wait time in a county from an initial 40 minutes to between 100 and 111 minutes. Our wait time estimate seems realistic given that a 5% increase in *PctDemocrat* is 1.30 times the within-county standard deviation from 2008 to 2016 (3.85%), and

Table 2: Wait time (average of the three methods to adjust capacity) in a county resulting from changes in *PctDemocrat* predicted by changes in *PctWhite* (controlling for *Presidential*, *Time*, and county fixed effects between 2008 and 2016).

Δ*PctWhite*	Predicted Δ*PctDemocrat*	Average wait (min)
0%	0%	40
-1%	+1.28%	55.4
-5%	+6.41%	124.0

the resulting average wait times are between 1.06 and 1.23 standard deviations above the average wait time reported by voters in Florida in the 2012 election across the Cooperative Congressional Election Study (Ansolabehere and Schaffner (2013)) and the Survey of the Performance of American Elections (Stewart (2013)).

Our study suggests that there may be no direct effect of race on voting resources and voter wait times. However, because race and political party are correlated, indirect effects of race could exist without controlling for political party. For example, when we regress *PctDemocrat* on *PctWhite*, controlling for *Presidential*, *Time*, and county fixed effects between 2008 and 2016, we find that a 1% decrease in *PctWhite* is associated with a 1.28% increase in *PctDemocrat*. Table 2 suggests that race could appear to drive voter wait times if political party is not observed.

1.6. Conclusion

Ensuring there is no disparity in voting resources among voters of different races or political party is an important endeavor. Ours is the first panel data study of voting resource disparities in elections. Unlike previous studies, we do not find a disparity with respect to race, *per se*. Instead, we provide evidence that as the percentage of Democrat voters in a Florida county increases, the voters in that county experience lower staffing levels (through more voters per poll worker) and longer waits to vote (via our simulation findings). Furthermore, our effect size estimates appear to be meaningful.

Rather than racial animosity, our results are consistent with parties using the allocation of voting resources to their advantage in the hope to dissuade voters from the opposition party from voting: the likelihood of a potential voter actually voting surely is different if the expected wait time is 100 minutes rather than 40 minutes. However, we are not able to rule out all possible explanations for the observed disparity. For example, if non-Democrats experience higher waiting costs per unit of time than Democrats, then a benevolent social planner would choose to bias resources away from Democrats: the unobserved cost of waiting can be equal even if the actual waits differ. Alternatively, if Democrats require less time to complete a ballot (or have lower variance in completion times), then equating waiting times would require different levels of resources per voter. If each poll worker is assigned more hours, as a county becomes more Democratic, then the reduction in poll workers might not imply a reduction in the total work hours dedicated to voting: i.e., fewer workers working more hours could yield a constant total resource level. Finally, unbalanced resources could be socially optimal if Democrats arrive to the polls more consistently throughout the day than non-Democrats. Although these hypotheses are potentially testable, they do not strike us as plausible.

We do emphasize that while our data indicates a resource bias based on political party, this bias does deferentially impact racial groups due to their varying support for the political parties. To the extent that non-white voters lean towards the Democratic party, they are more likely to be adversely affected.

While there are media stories regarding long waits to vote, there has not been reports that precincts have explicitly reduced voting resources. However there is reason to believe that such bias can occur and also be hard to detect. If the overall voting population is growing (as it was during our study period in Florida) but resources were not proportionally added, then Democratic leaning districts could be disadvantaged merely by keeping their resources constant without the need to explicitly have them reduced. They are only reduced relative to the demand they have to serve, which is less visible. Furthermore, voting resource allocations across polling places suffers from consequential integer constraints - the difference between one and two check-in stations can be significant. Thus, the one area that gets the additional resource is given a significant benefit relative to those in which resources are maintained at the status quo.

Given our findings, a practical solution is for a state to regulate the amount of resources in each polling location so as to attempt to equate waiting times across all voters. Unlike Florida, some states, such as South Carolina, have laws which mandate a maximum number of voters per voting resource. Although many laws related to the voting process are controversial (e.g., voter identification laws), laws that ensure democracy is equally served across all citizens should be less contentious.

CHAPTER 2 : Democracy on the Line: Polling Place Closures and the Impact on Wait Times in the 2016 Presidential Election in Georgia

Coauthored with Gérard P. Cachon

2.1. Introduction

In the 2013 *Shelby County* v. *Holder* case, the Supreme Court of the United States decided that Section 4 of the Voting Rights Act was unconstitutional. This provision qualified certain areas of the country with a history of voter discrimination, including the state of Georgia, to gain approval from the U.S. Department of Justice for any changes in the voting process under Section 5 (The Brennan Center for Justice (2019); The United States Department of Justice (2019a,b)). Following the ruling, the office of the Secretary of State for Georgia sent a memo to local election officials in February 2015 to promote the consolidation of polling places (The Leadership Conference Education Fund (2019)).

Research on polling place closures (and changes) has focused on the impact of closures on turnout. A reduction in the number of polling locations increases the average distance voters need to travel to vote. That increases the cost to vote, which theory suggests should reduce turnout (Riker and Ordeshook (1968)). Empirical evidence generally supports a negative impact on voting (Haspel and Knotts (2005); Brady and McNulty (2011); Cantoni (2016); Yoder (2019)). However, there is evidence of mitigating factors: some voters are willing to substitute in-person election day voting with other voting methods, such as absentee ballot or early voting; and the mere notification (generally through postal mail) of a voting location change can prompt some to vote (Clinton et al. (2019b)).

Instead of turnout, our interest is on the impact of polling place closures on the wait time to vote. Research in settings such as healthcare (Camacho et al. (2006); Gillespie and Hillyer (2002a); Batt and Terwiesch (2015)), retail (Davis and Vollmann (1990); Allon et al. (2011)), and transportation (Taylor (1994)) has shown that time waiting is disliked and costly to businesses and customers. Long lines at the polls have the potential to disenfranchise

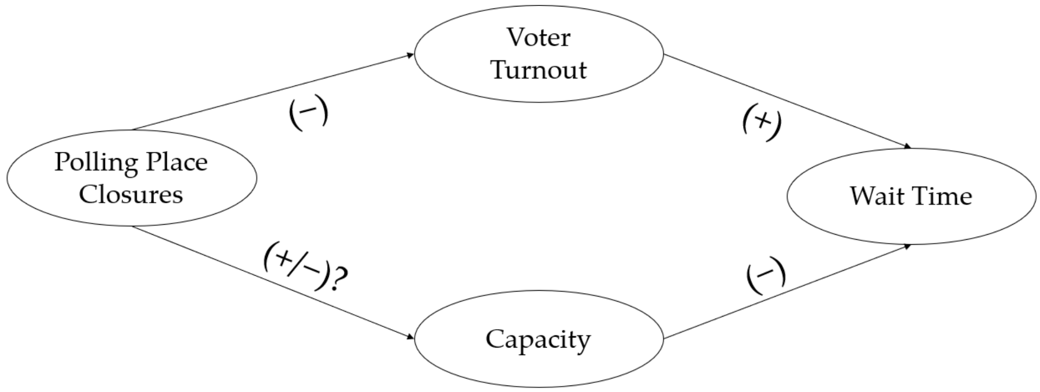


Figure 4: Mechanisms through which polling place closures may impact voter wait times

voters in an election, which can influence the decision to vote: from a survey on the 2008 U.S. election, an estimated 11.1% of voters did not vote because of long lines (Alvarez et al. (2008)). Beyond a contemporaneous effect, waiting could dissuade voters from participating in future elections: in Florida, evidence shows that voters who experienced longer waits in the 2012 election had a lower probability of voting in the next presidential election (Cottrell et al. (2017a)). A number of studies document voter wait time disparities related to factors such as race, political party, and income (Highton (2006); Stewart III (2012); Famighetti et al. (2014); Pettigrew (2017)), and others have linked those differences to unbalanced allocation of voting resources such as poll workers and voting machines (Famighetti et al. (2014); Pettigrew (2017); Cachon and Kaaua (2019)). Our empirical research focuses on how polling place closures in Georgia may have impacted voter wait times in the 2016 election.

From queuing theory (Kleinrock (1975)), the time to vote depends on the characteristics of demand (e.g., turnout, volatility throughout the day, etc.) and capacity (e.g., number of poll workers, voting machines, etc.) Presuming that polling place closures reduce turnout, all else being equal (e.g., capacity), voter waiting times should be reduced, as shown by the top mechanism in Fig.4 - if fewer voters arrive to the polls, then the lower burden on the system should reduce the time each voter waits to vote.

Unlike voter turnout, the impact of polling place closures on voter wait times is ambiguous with respect to capacity, the lower mechanism in Fig.4. A critical question is how county administrators manage the capacity of the closed polling locations. A natural option, as appears to have been done in Texas (Simpson (2016)), is to take the resources from closed locations and redistribute them proportionally to the remaining locations. (See Allen and Bernshteyn (2006); Yang et al. (2009, 2013) for analytical methods to allocate capacity across locations.) That approach, which we refer to as "capacity pooling," continues to utilize all resources, just in fewer locations. Supported by theory (Cachon and Terwiesch (2013)), but contrary to popular opinion in the media (The Leadership Conference Education Fund (2019)), capacity pooling should reduce voter wait time (holding all else constant, such as demand). This occurs because capacity pooling mitigates the primary inefficiency associated with multiple locations, the possibility that resources are idle at some locations while voters are waiting at other locations.

Capacity pooling is not the only option for managing resources. The capacity in closed polling locations could be distributed to a select few of the remaining locations. Voters in those lucky locations would experience an improvement, but voters in the other locations could experience a degradation in service. Due to the non-linearity in the response of wait time to capacity in queuing systems, the overall net effect of this imbalanced redistribution of capacity could be negative (Kleinrock (1975)). The least beneficial of approaches is merely to mothball or cancel the capacity from resources of the closed polling locations, leaving the remaining locations with the same capacity but more voters. A state might take this approach if it has the (real or stated) motivation to reduce the overall budget for conducting an election. In fact, there is some evidence that Georgia indeed took some of its existing capacity out of service in the 2018 general election (Niesse (2018)). Such an approach leads polling closures to cause an increase in voter wait time, as shown in Fig.4.

In sum, our goal is to estimate how polling place closures in Georgia after the *Shelby County* v. *Holder* decision impacted waiting time for voters in Georgia in the 2016 election. Given the existence of multiple mechanisms, we use a difference-in-difference methodology and control for the impact of polling place closures on voter turnout (i.e., the top mechanism in Fig.4). We find that voters in Georgia in the 2016 presidential election experienced a 78% increase in the average wait time to vote, or about 7.2 minutes based on Georgia's average wait time of 16.5 minutes in the 2016 election. This finding is inconsistent with the use of a capacity pooling strategy (redistributing voting resources to remaining locations). Instead, it is consistent with a strategy that combines polling place closures with a reduction in capacity, possibly by mothballing the capacity of the closed locations, thereby imposing on voters greater costs both through the distance needed to travel and the time waiting to vote.

2.2. Methodology and Data

We use a difference-in-difference (DD) methodology to examine the effect of polling place closures, following *Shelby County* v. *Holder*, on wait times in Georgia in the 2016 election. Given that polling place closures were motivated by the office of the Secretary of State of Georgia, we take the entire state of Georgia as our treatment group. It is important to confirm that the state was actually treated with polling place closures between the 2012 and 2016 elections. Georgia reported the number of polling places in the state to the Election Administration and Voting Survey (EAVS) in 2006 and every two years afterwards, with the exception of 2012 (see Table 20). *VICE News* (Arthur and McCann (2018)) conducted a survey of all 159 counties to estimate the magnitude of polling place closures in Georgia and found that among the 84 that reported, the total number of polling places declined by 7.48% between the 2012 and 2016 presidential elections. We view that estimate as a lower bound because of potential reporting bias: counties that closed a substantial number of polling places may be less likely to respond to an informal survey. To provide another measure of closures, we define the imputed number of polling places in 2012 as $Poll\widehat{Places}_{2012}$ and estimate it using Eq.2.1, which accounts for differences in turnout across elections (Fig.5) and expected growth. We find $Poll\widehat{Places}_{2012} = 3,159$, which suggests there was a 13.90% decline in the number of polling places from 2012 to 2016 in Georgia. We conclude that Georgia was indeed treated with polling place closures.

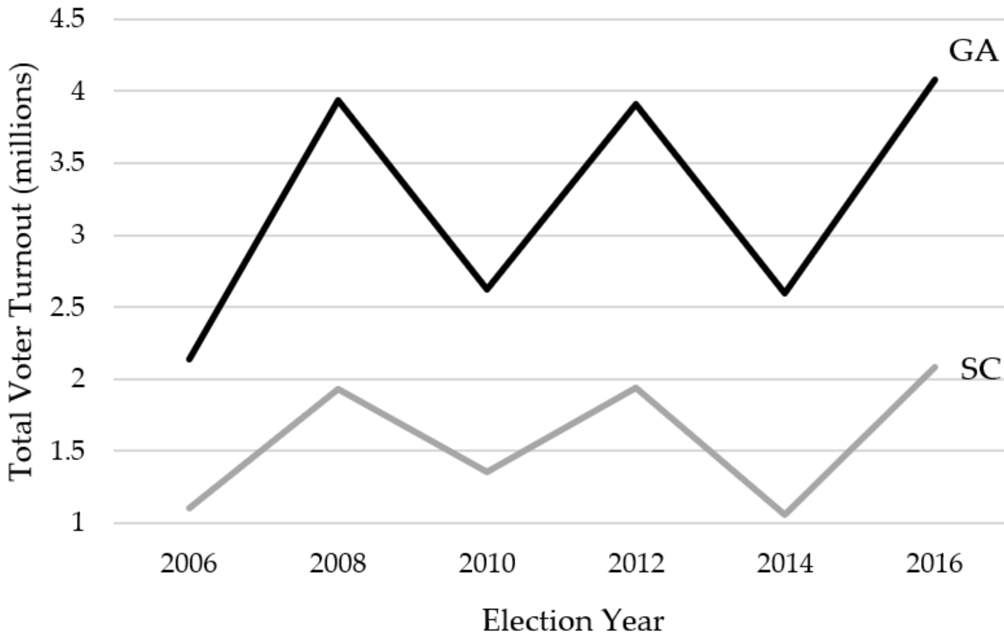


Figure 5: Voter turnout in Georgia and South Carolina by election year

$$\widehat{PollPlaces}_{2012} = \max\{PollPlaces_{2010}, PollPlaces_{2014}\} \times PollPlaces_{2008} / \max\{PollPlaces_{2006}, PollPlaces_{2010}\} \tag{2.1}$$

The DD method requires a control for the treated sample. We propose South Carolina as a control for Georgia in the DD for several reasons. First, Section 5 of the Voting Rights Act fully covered both Georgia and South Carolina prior to 2013. However, while the *Shelby County* v. *Holder* ruling allowed both states to close polling places without Department of Justice approval, state regulations in South Carolina discouraged closures in the state (The Leadership Conference Education Fund (2019)). Consequently, the total number of polling places in South Carolina (see Fig.6) remained relatively unchanged over the period of interest, increasing by just 0.91% between the 2012 and 2016 elections (according to the EAVS). Second, South Carolina is situated adjacent to Georgia in the Southern United

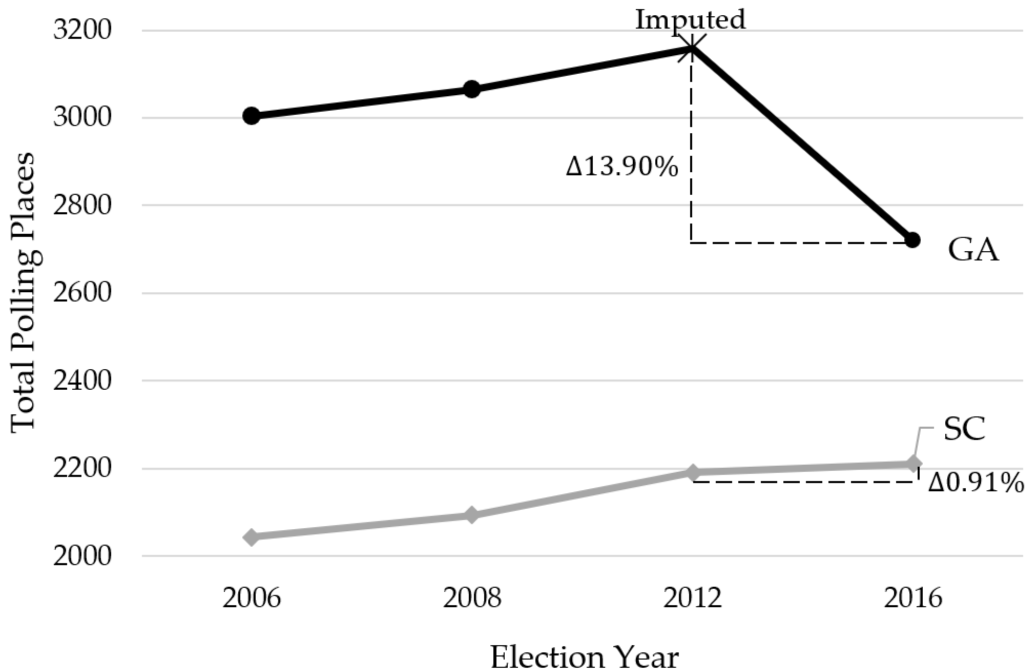


Figure 6: Total polling places in Georgia and South Carolina across the 2006, 2008, 2012, and 2016 elections using $\widehat{PollPlaces}_{2012}$ for Georgia in 2012

States. States close in distance may be more likely to share similar characteristics (e.g., Card and Krueger (1994)). For example, Georgia and South Carolina may have similar voting cultures (as evidenced by each state's complete coverage under the Voting Rights Act prior to 2013) and Election Day weather which can impact voter behavior. Third, at least as early as 2006, according to the *Verified Voting Foundation*, Georgia and South Carolina use, throughout the entire state, direct-recording electronic (DRE) voting machines (rather than paper ballots for example) (See `https://www.verifiedvotingfoundation.org/about-vvf/` for more information on the *Verified Voting Foundation*.) Voting times on DREs can differ substantially from voting times on paper ballots within the same election (Stewart III (2015)), but that is not a concern in our study.

Eq.2.2 provides the specification for the DD estimation. $logWait_i$ is the log of the wait time in minutes reported by voter i in the 2006, 2008, 2012, or 2016 Cooperative Congressional Election Study (CCES) in response to the question *Approximately how long did you wait* *in line to vote.* (Data were not collected for this question in 2010). CCES samples from a 50,000+ pool of adults of which only a fraction vote. CCES is distributed nationwide proportional to the population. Respondents were allowed to choose from the following options: *Not at all*, *Less than 10 minutes*, *10 to 30 minutes*, *31 to 60 minutes*, and *More than 1 hour*. We coded the ranges as: Not at all - 1 minute, Less than 10 minutes - 5 minutes, 10 to 30 minutes - 20 minutes, and 31 to 60 minutes - 45 minutes. Respondents input custom times when the wait was more than one hour so these were manually coded. Because we are analyzing categorical, voter-reported wait times, there is an issue of measurement error, but we do not believe that measurement error in Georgia or South Carolina should be systematically different in either of the states during the elections we study.

$$
\begin{aligned}
logWait_i = &\beta_0 + \beta_1 Treated_i + \beta_2 Election2006_i + \beta_3 Election2008_i + \beta_4 Election2016_i + \\
&\beta_5 (Treated_i \times Election2006_i) + \beta_6 (Treated_i \times Election2008_i) + \\
&\beta_7 (Treated_i \times Election2016_i) + X_i^{'} \mu + \varepsilon_i
\end{aligned}
\tag{2.2}
$$

$Treated_i$ is a dummy variable that equals one if voter i is located in Georgia where polling place closures occurred and zero if located in the control state, South Carolina, where polling place closures were prevented. $Election2006_i$, $Election2008_i$, and $Election2016_i$ are dummy variables that equal one in the specified election year and zero otherwise. *Election2006*, *Election2008*, and the excluded base election year 2012 are the pre-treatment elections while *Election2016* is the post-treatment election. (We ignored the 2010 pre-treatment election due to the data constraint described above.) $X_i^{'}$ is the vector of controls. ε_i is the error term, and we cluster these errors by county since county officials are primarily responsible for managing elections in each state (Georgia counties each have a Board of Elections, and South Carolina counties each have a Board of Voter Registration and Elections). β_7 is

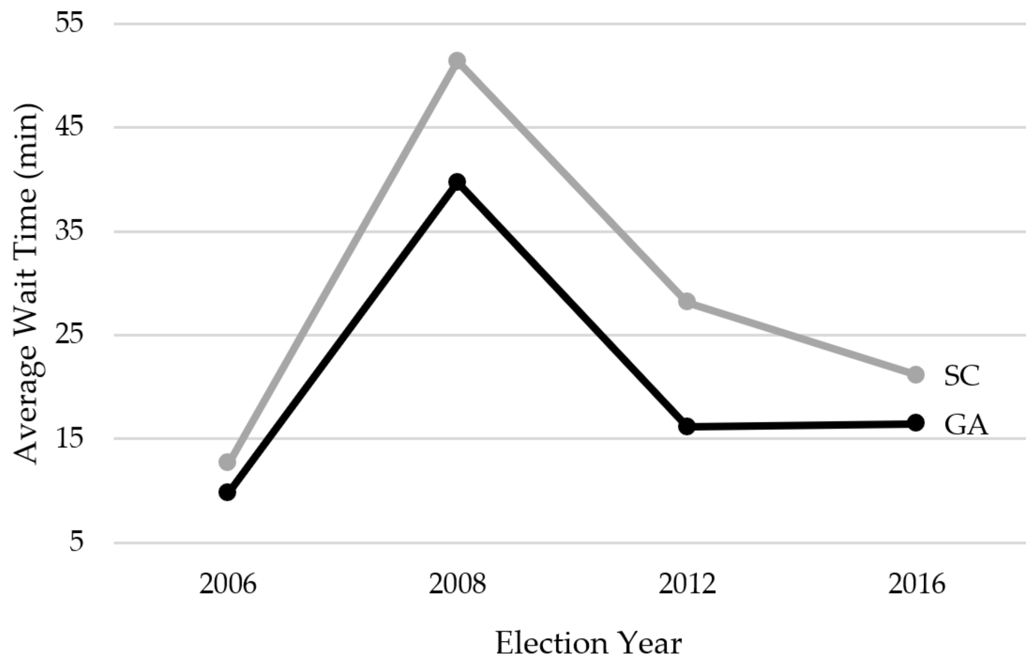


Figure 7: Average wait times (minutes) for Georgia and South Carolina in the 2006, 2008, 2012, and 2016 elections

therefore our DD estimate of the impact polling place closures had on wait times in Georgia in the 2016 election. Section A.4.1 has more information on the origin of the data used in this study.

If South Carolina is a valid control for Georgia in the DD estimation, its wait times, conditional on the regressors in Eq.2.2, should follow a parallel trend from 2006 to 2012 (i.e., before the *Shelby County* v. *Holder* ruling). Unconditional on any other variables, this appears to be visually confirmed in Fig.7. To perform the parallel trend assumption test statistically, however, the estimates for β_5 and β_6 in Eq.2.2 should not be statistically significant, suggesting there is a constant difference (or parallel trend) between Georgia and South Carolina's conditional wait times before treatment.

We include in Eq.2.2 several controls related to three main factors that influence wait times in elections: voter arrival variability, polling place capacity, and the overall demand to vote. (See Table 21 for a numerical summary of all controls).

Urban polling places may have more consistent voter arrivals throughout the day than rural polling places because residents in rural areas may have less access to their polling places throughout the workday. This could cause an arrival pattern in rural areas with larger spikes in demand throughout Election Day. Due to this potential rural versus urban effect, we include the control *logPplPerSqMi*, which is the log of the number of residents per square mile in voter i's county in the voter's election year.

We include several controls related to polling place capacity. In the United States, elections tend to be funded by counties, and as of 2018, the National Conference of State Legislator suggested that Georgia and South Carolina counties primarily funded general elections in the state (see `http://www.ncsl.org/research/elections-and-campaigns/election-costs.aspx`). We have no reason to believe that this funding model was not the same over the duration of our study. Thus, we include $logIncome$, which is the log of the median income in voter i's county and election year. We include $Pct65Plus$, the percentage of residents in voter i's county and election year who are 65 years or older, because counties with higher percentages of older voters may face different capacity constraints (Glenn and Grimes (1968)). Given the evidence that polling place capacity could be affected by the racial or political party composition of voters within various jurisdictions (Famighetti et al. (2014); Pettigrew (2017); Cachon and Kaaua (2019)), we include two related controls. $PctWhite$ is the percentage of white registered voters in voter i's county and election year. Georgia and South Carolina do not ask voters for political party affiliation on their voter registration forms and both states hold open primaries, but we still control for potential political party bias using a proxy for political party affiliation within a county. $PctDem$ is the percentage of voters in voter i's county who voted for the Democratic gubernatorial or presidential candidate in the previous election (relative to the voter i's election year).

The overall demand to vote is driven by two factors: the number of registered voters and the turnout percent of those registered voters. In Eq.2.2, we include controls related to the overall demand to vote. $RegVoters$ is the total number of active registered voters in voter i's county and election year. We also include the squared term of this variable (i.e., $RegVoters^2$) to account for the fact that wait times may increase exponentially at higher levels of voter demand. $Turnout$ is the percentage of voters who turned out to vote in voter i's county and election year. $EDTurnout$ is the turnout specifically on Election Day and is the total number of Election Day ballots cast divided by the total number of active registered voters in voter i's county and election year. $RegVoters$, $Turnout$, and $EDTurnout$ also control for key legislative changes that occurred in Georgia and South Carolina over the time of our study which may have impacted the general demand to vote.

Brennan Center for Justice listed the key voting restrictions states have implemented in the recent past (Weiser and Feldman (2018)), and they are shown verbatim for Georgia and South Carolina in Table 22. In 2006, the state of Georgia attempted to enact a law that would have required voters to present photo identification to vote. This law was blocked for the 2006 election but was upheld in 2007 (see `https://www.brennancenter.org/legal-work/common-causegeorgia-v-billups`). This legislative change may have reduced voter turnout, so $Turnout$, and $EDTurnout$ should control for its effect.

In 2009, Georgia required voter registration applicants to provide documents proving citizenship. This more stringent voter registration requirement could have reduced the pool of registered voters. Therefore, $RegVoters$ should control for this law's effects.

Before the 2010 election, Georgia implemented a law shortening the number of early voting days. This legislative change may have reduced voter turnout in general and shifted more voting demand to Election Day. $Turnout$ and $EDTurnout$ should control for the effect of this change.

In *South Carolina* v. *Holder*, plaintiffs aimed to block a South Carolina law (proposed to the U.S. Department of Justice for approval in 2011) requiring voters to have photo identification to vote. (See `https://www.brennancenter.org/legal-work/south-carolina-v-holder` for more information on *South Carolina* v. *Holder*.) The law was not implemented in the

2012 election and voters were still able to use a non-photo voter registration card in future elections. Although we believe that this law should not have had a significant impact on voter turnout in the 2016 election, we still control for the change in legislation with $Turnout$, and $EDTurnout$.

From 2013 until two months before the 2016 election, Georgia implemented a "no match, no vote" policy and did not process voter registrations for applicants who did not have information on the application matching information in the state's databases (J.E.F. (2018)). During that time, thousands of voter registration applications were not processed which could have reduced the number of registered voters for the 2016 election. $RegVoters$ should control for this policy's effects.

Fig.7 shows that the average wait time for South Carolina decreased from 2012 to 2016, while the wait time for Georgia slightly increased over the same time period. This suggests that polling place closures in Georgia resulted in an increase in wait times in the 2016 election. However, the figure shows unconditional wait times. More convincing evidence is a positive and statistically significant estimate for β_7 in Eq.2.2, which would indicate that the increase in wait times in Georgia is associated with polling place closures. An increase (given the controls for turnout and others), would suggest that Georgia did not implement a capacity pooling strategy, i.e., polling place closures occurred with a reduction in overall capacity, either because the freed capacity was redistributed unevenly, or mothballed, or both.

2.3. Results and Robustness Checks

Using ordinary least squares regression, we estimated the DD specification in Eq.2.2, and the results are displayed in Fig.8 and Table 23. We first note that there is evidence that the parallel trends assumption is satisfied in our regression with insignificant, small coefficients on $(Treated \times Election2006)$ and $(Treated \times Election2008)$. In Fig.8, we see that the difference in $logWait$ for Georgia and South Carolina (i.e., the treatment effect), conditional on all other regressors, looks statistically identical in 2006, 2008, and 2012, suggesting a

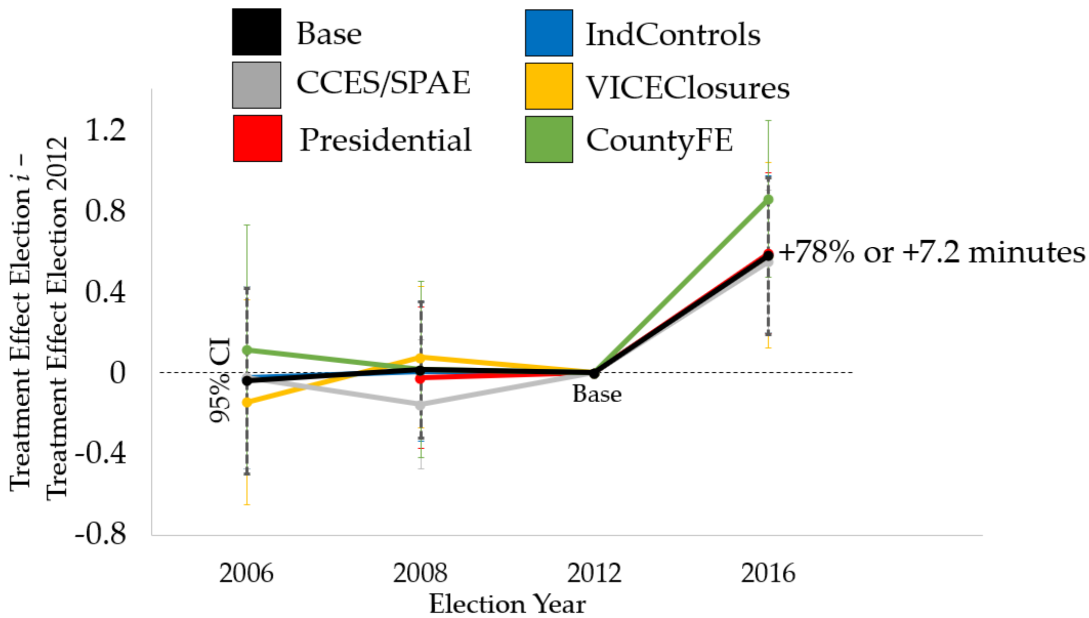


Figure 8: Treatment effect in election i (where $i = 2006, 2008, 2016$) minus the treatment effect in the 2012 election

parallel trend. We therefore assume that South Carolina is a good control for Georgia.

Our results show a statistically significant DD estimate of $\beta_7 = 0.578$, and Fig.8 displays the confidence interval around the treatment effect. According to this estimate, the average wait time in Georgia in 2016 was $1 - e^{0.578} = 78\%$ higher due to polling place closures. From the CCES data, the average wait time in Georgia in 2016 was 16.5 minutes, suggesting that the increase in wait time resulting from polling place closures was about 7.2 minutes $(16.5 - 16.5/1.78)$. In addition, we believe that this result may be a conservative estimate given that we include in our analysis all counties in Georgia rather than focusing only on counties that closed polling places (due to limitations in the data discussed above to identify these counties).

An increase in average wait time in the 2016 election is not consistent with the implementation of a capacity pooling strategy, which would predict a reduction in average wait times. Instead, the increase in the time to vote in Georgia is consistent with administrators who either reallocated the capacity and resources from closed polling locations to a limited set of the maintained locations, or more likely, a reduction in the overall voting resources in the state. Therefore, polling place closures in Georgia in the 2016 election may have not only disenfranchised voters due to the longer average distances needed to travel to vote, but also due to the longer wait times once at the polling places.

We performed five robustness checks to support our results, and they are described below. The results in Fig.8 and Table 23 show that all passed the parallel trends test and had similar results to the base regression's DD estimator.

(1) CCES/SPAE: We combine the CCES wait data with data from the Survey of the Performance of American Elections (SPAE). Each survey requests voters to estimate their wait times using the same question (although the sampling methodology differs). Initiated in 2008, SPAE samples 200 registered voters in each state of which a fraction vote, and it is distributed within states proportional to the population (see `https://dataverse.harvard.edu/dataverse/SPAE`). See Table 24 for the number of wait time observations in the CCES and SPAE surveys in each election.

(2) Presidential: In Eq.2.2, we calculate the DD estimate for a presidential election year (2016) using a midterm election (2006) in the pre-treatment period. Presidential elections tend to have higher turnout relative to midterm elections (see Fig.5), and states have more of an incentive to maximize voter capacity in presidential elections. Therefore, voter wait times across Georgia and South Carolina may follow more uniform pre-treatment trends across presidential elections and provide a more accurate estimate of the treatment effect in the 2016 election. We therefore remove Election2006 and $(Treated \times Election2006)$ from Eq.2.2 and perform our DD estimation on observations from the 2008, 2012, and 2016 elections.

(3) IndControls: In the base regression, all controls are at the county level. We therefore add additional controls to Eq.2.2 specific to each respondent in the CCES survey. The controls are as follows: whether the respondent identified as white ($White$); whether the

respondent identified as male ($Male$); age of the respondent at the time of the Election (Age); whether the respondent identified as a Democrat (Dem); whether the respondent identified as an Independent (Ind); whether the respondent's family income was $100,000 or more ($HighInc$); whether the respondent had some college education but not necessarily a college degree ($College$). See Table 25 for summary statistics on all the individual-level controls.

(4) VICEClosures: Because Georgia did not report any polling place counts for EAVS in 2012, we conduct a robustness check only on those counties which *VICE News* (Arthur and McCann (2018)) found to have closed polling places between 2012 and 2016. We therefore exclude all observations from Georgia counties that did not meet this criteria.

(5) CountyFE: To control for any potential county fixed effects, we add dummy variables for each voter's county to Eq.2.2.

2.4. Discussion and Conclusion

We find that polling place closures substantially increased wait times in Georgia in the 2016 election. In addition, our result suggests that the state did not pursue a capacity pooling strategy and probably had a significant amount of voting resources that were idle or reduced during the election. When this result is coupled with the fact that voters in Georgia may have needed to travel longer distances to vote due to polling place closures, voter disenfranchisement becomes a twofold concern. Moreover, the poor service experience voters may have had in the 2016 election due to polling place closures could have discouraged them from voting in the 2018 election (e.g. Cottrell et al. (2017a))

Evidence suggests that the Georgia Secretary of State's office (which was under Republican leadership at the time) promoted "consolidation" to all local election officials, and counties with specific demographics (e.g., those with more minority voters) were not explicitly targeted. However, polling place closure strategies are likely to be more attractive in poorer counties where election budgets may be tighter and voters may be less likely to protest such changes (Mohai and Saha (2015)). In addition, following polling place closures, poorer counties may find a non-pooling strategy attractive given that a pooling strategy incurs larger administrative costs associated with finding new polling places (that can accommodate more voting machines) and larger labor costs (i.e., more poll workers).

We conducted a county-level analysis (see Section A.4.2 and Table 27) to identify the characteristics of the counties we were able to confirm closed polling locations before the 2016 election (relative to the 2008 presidential election). Racial composition of voters and political party measures do not predict the counties with closures. Instead, we find that a county's average income is the only significant ($p<0.05$) predictor of closures between the two elections (see Table 27). If Democrats tend to be over-represented among lower income voters (Pew Research Center (2016)), then promoting polling place closures without encouraging capacity pooling could help Republicans overall. Furthermore, if Democrats are more sensitive than Republicans to voting costs (Brady and McNulty (2011)) such as wait times at the polls (e.g., due to the lost income associated with voting relative to overall wealth), then Republicans could gain an advantage through longer wait times to vote even if polling place closures occurred randomly across the state. In other words, a party could gain an advantage from a policy that encourages fewer polling places even if such a policy does not focus on areas with a higher concentration of voters from the opposition.

Given our research findings, Georgia policymakers may be interested to determine whether voting resources, such as DREs, were idle in the 2016 election, and if so, why they were idle. Operational transparency can increase trust in government (Buell et al. (2018)), so Georgia policymakers could consider implementing laws promoting transparency in how voting resources are being utilized in elections. Furthermore, Georgia policymakers may also consider implementing legislation requiring the redistribution of all functioning voting machines from closed polling places to reduce the chances of voter disenfranchisement from waiting times at the polls.

CHAPTER 3 : Drivers of Customer Satisfaction in a Tandem Queue: The Role of Buffer Location, Perception of Fairness, and Punctuality

Coauthored with Christian Terwiesch, Santiago Gallino, and Shivan Mehta

3.1. Introduction

When receiving a service, be it in hospitality, travel, healthcare, or banking, customers oftentimes have to wait. Numerous prior studies have empirically established that long wait times lead to poor customer satisfaction (e.g., Davis and Vollmann (1990); Katz et al. (1991); Anderson et al. (2007)) and adverse effects on business (e.g., Camacho et al. (2006); Anderson et al. (2007); Allon et al. (2011); Akşin et al. (2013); Lu et al. (2013b); Batt and Terwiesch (2015); Chan et al. (2017); Berry Jaeker and Tucker (2017)).

A subtle and mostly overlooked point related to waiting is that customers oftentimes experience multiple waiting episodes in different locations as part of one service encounter. In addition, customers often wait commingled with others for an appointment with a server (often privately assigned) for a scheduled appointment. Many types of queues share one or more of these attributes. In a multiple-provider healthcare practice, patients wait for a scheduled appointment with an assigned doctor in a waiting room and then again in an exam room. In a restaurant, guests often place their name on a wait list for a table assignment that can accommodate the party size. Oftentimes guests wait outside the restaurant to receive a ping that the table is ready and then wait inside the restaurant to be seated. In air travel, passengers wait at the gate, during the boarding process, and then again on the tarmac for their scheduled takeoff.

The total waiting time during one overall service encounter is the aggregation of multiple waiting episodes that occur at the various steps that make up for the encounter. This more granular perspective of waiting begs the question to what extent the distribution of the total waiting time over these waiting episodes impact the customer satisfaction. Consider three patients at the multiple-provider healthcare practice. The first patient waits 30 minutes in the waiting room and is then promptly seen by her provider. The second patient waits 15 minutes in the waiting room and then again 15 minutes in an exam room before being seen by his provider. The third patient is immediately moved to the exam room only to wait there for 30 minutes before seeing her provider. All these patients wait for 30 minutes, but which patient will be the most satisfied? Prior research offers conflicting predictions. One might argue that waiting in the first stage might offend a customer the most, as the customer could have delayed the arrival to the scheduled appointment and thus the happiest patient is the third patient who spent no time in the waiting room. In contrast, the peak-end rule, a well-established result in psychology (Redelmeier and Kahneman (1996)) and recently supported in the operations transparency literature (Bray (2018)), emphasizes that customer perception of an experience is dominated more by what happens at the peak and end of the service encounter. The increased wait disutility from things such as anxiety that the patient may experience in the exam room (versus the waiting room) and the fact that the exam room is the end of the waiting experience would both suggest that our first patient would be the most satisfied. But, the peak-end rule also can be interpreted that customers will prefer two small waits over one large wait (keeping all levels of peak frustration low), predicting that the second patient would be the most satisfied. Dividing up one long wait into multiple short waits also provides the customer with a sense of progress (e.g., Soman and Shi (2003)), consistent with the design of many theme parks in which long lines are broken up into smaller lines so that customers always maintain a perception of progress.

How does the distribution of the total waiting time over multiple waiting episodes impact the customer satisfaction? This is the first research question that we aim to study in this paper. Dube-Rioux et al. (1989) tried to address this question experimentally in the context of pre-process, in-process, and post-process waits, but there are a number of challenges that have to be overcome to answer this question observationally which likely explains why it has not yet been addressed. First, a study linking waiting episodes to customer satisfaction requires more micro-level data about the duration of each waiting time episode as well as an overall measure of customer satisfaction. Second, other experiences that can be observed through data might also impact the patient satisfaction during the waiting time and need to be controlled for. And third, omitted data may also impact patient satisfaction during the waiting episode, and so a standard regression of satisfaction on waiting times and observable factors would yield a biased estimate. In this paper, we present an observational study of 560 patients that have been treated in the endoscopy suite of a large academic medical center. After checking in upon arrival, patients wait in the first buffer, a reception area (on average for 35 minutes). They then are moved to the second buffer, a pre-op area, where after having been prepared for their procedure they wait again to be moved into the operating room (on average, they wait for 55 minutes). Following their visit, patients are contacted by an independent agency with the request to report their satisfaction. We develop a two-step estimation procedure that provides an unbiased estimate for the marginal impact of waiting time at each of the two buffer locations. We control for patient experiences that previous research has identified as being detrimental to customer satisfaction such as the degree to which patients are seen with respect to the order in which they arrived.

Consistent with prior work (e.g., Davis and Vollmann (1990); Katz et al. (1991); Taylor (1994); Anderson et al. (2007)), we show that an increase in waiting time reduces customer satisfaction. Specifically, we find that a 50% increase in a patient's total wait time decreases the patient's expected satisfaction score by 0.21 points (on a 5-point scale). More importantly, we show that a 50% increase in a patient's wait time in the second buffer results in a decline in expected satisfaction seven times larger than the decline resulting from a 50% increase in wait time in the first buffer. Furthermore, in an experiment, we find evidence that patients may not necessarily enjoy smaller/equal waits in both buffers and that the peak attribute (rather than the end attribute) of the wait in the second buffer may be driving this result. As a result, managers can focus their efforts to identify the locations of a tandem queue with higher wait disutility and target interventions such as providing wait time fillers or additional staffing resources in these parts of the line.

Returning to our healthcare practice example, suppose that the three patients arrive to the waiting room at 10:00am, 10:00am, and 10:15am respectively. The first and second patients, who arrived before the third patient, may perceive the third patient's immediate movement from the waiting room to the exam room as unfair despite not knowing her provider assignment. We show the importance of perceived fairness when queues are transparent (see Buell and Norton (2011); Buell et al. (2016)). Holding waiting times constant, we find that patients in the first buffer (where patient movements are transparent) experience a significant decrease (increase) in satisfaction if a patient who arrived after (before) them is serviced before (after) them despite patients not observing the doctor assignment of others. (Such fairness has been shown to influence queue abandonment (e.g., Batt and Terwiesch (2015)), but has not yet been linked to satisfaction.) Conversely, the hospital has more flexibility to deviate from FIFO queue discipline in the second buffer (where patient movements are not transparent). In a follow-up experiment, we provide causal evidence that when people perceive cuts in line, satisfaction significantly decreases. Managers can use technology such as paging systems to reduce transparency in areas of the queue where customers may be able to perceive violations in FIFO queue discipline

Finally, in our example, suppose that the three patients have scheduled appointment times of 10:30am, 10:45am, and 10:15am respectively. Although all patients wait for 30 minutes, how might the early start of the second patient affect his satisfaction and how might the late start of patient three affect her satisfaction? Studies have suggested that "post-schedule" waits, or the incremental wait time after the scheduled appointment time, could have a meaningful impact on customer utility (Taylor (1994)). Moreover, prospect theory (Kahneman and Tversky (2013)) suggests that perceived losses could impact satisfaction more than perceived gains. We find evidence that patients may care more about losses of time (waits that conclude after a scheduled appointment time) than gains in time (waits that conclude before a scheduled appointment time). Managers can focus interventions on customers (such as providing delay information) who are likely to experience delays from the scheduled appointment time since these customers are likely to have a particularly painful experience. In addition, managers can consider implementing initiatives that increase the buffer between a customer's arrival time and the scheduled appointment time.

The remainder of this paper is organized as follows. We first describe the clinical setting we study (Section 3.2) followed by a review of the relevant literature and a statement of our hypotheses in Section 3.3. Section 3.4 describes our data and Section 3.5 describes our study of the impact of waiting time and location on patient satisfaction. Section 3.6 provides a number of robustness checks for our observational study. Section 3.7 describes and discusses the methods and results of three experiments that supplement the observational study. Finally, we conclude our paper in Section 3.8.

3.2. Clinical Setting

We examine an ambulatory perioperative process at an endoscopic procedure center at a large, urban teaching hospital from January 2014 to September 2017. Ambulatory patients receiving an endoscopic procedure arrive at a check-in desk approximately one hour prior to their scheduled procedure time. Following check-in, they are seated in reception (the first buffer) where they complete all necessary medical forms with the help of administrative or nurse staff. Reception is an open area where all patients wait . Once a patient arrives at an available pre-op station (the second buffer), they change into a hospital gown and receive an intravenous catheter ("IV"). Once pre-op preparation is complete, patients wait in their individual pre-op station (with privacy curtains surrounding them) until the operating room ("OR"), to which they are assigned, is ready for them. There are a total of four main operating rooms in the center, and the most common procedures performed include colonoscopy and esophagogastroduodenoscopy.

Within a couple weeks of procedure completion, a third-party company mails a satisfaction survey to the patient. The survey provides the patient with the opportunity to provide anonymous feedback to the hospital across multiple dimensions. The surveys are not compulsory, so typically only a fraction of the number of surveys sent to patients are returned to the hospital.

3.3. Related Literature and Hypotheses

Congestions and longer wait times in service settings are often costly to businesses. In healthcare, McCarthy et al. (2000) observed a positive association between waiting times and missed appointments at an outpatient clinic. Camacho et al. (2006) found that longer patient waits lowered the chances of a patient returning to a primary or specialty care clinic. Anderson et al. (2007) observed that longer wait times for primary care significantly decreased patient satisfaction. Batt and Terwiesch (2015) showed that longer perceived waits could lead to patient departures from the ER before receiving service. Chan et al. (2017) found that longer delays for intensive care admissions of patients could result in longer length of stays. Berry Jaeker and Tucker (2017) demonstrated that high congestion in an emergency department can lead to cost and patient severity issues if additional capacity is not brought online during times of high occupancy. In retail, Davis and Vollmann (1990) and Allon et al. (2011) found that customer wait times could negatively impact fast food retailers while Lu et al. (2013b) found that longer queue lengths could reduce grocery purchases. In the transportation industry, Taylor (1994) found that longer delays led customers to have increased feelings of anger and uncertainty. In a banking call center, Akşin et al. (2013) estimated a high waiting cost for high priority customers. Osuna (1985) and Janakiraman et al. (2011) modeled customer utility in the queue as a concave function that decreases in the total wait time for service. As a result of this research, we hypothesize the following in our setting:

Hypothesis 1. Customers who have a longer total wait time before service (time in first buffer plus time in second buffer) are less satisfied.

Given that waiting in queues can be a painful experience for customers, many studies have put forth recommendations on how queues can be managed. Maister (1985); Larson (1987); Katz et al. (1991) suggested that manipulating factors related to the customer's psychology in queue could greatly influence customer satisfaction. Whitt (1999); Osuna (1985); Mowen et al. (1993) found that giving people information on their waits could improve service.

Soman and Shi (2003) put forward the idea that increasing consumers' perceptions of their own progress could increase their wait tolerances. Carmon et al. (1995) recommended that customer wait times be occupied with "filler" services to reduce the psychological cost of waiting. Pruyn and Smidts (1998) found that the waiting room attractiveness could improve the customer wait experience. Buell and Norton (2011); Buell et al. (2016) suggested that giving customers transparency (or the illusion of transparency) into the waiting process could increase the perceived value of the service received.

A limitation of the studies mentioned above is that they focused on the management of the total wait time before some service of interest began. However, waits in many service operations commonly take place in different pre-process locations (i.e., buffers) before the service of interest begins. A patient may initially wait to see a doctor in a reception area, but after some time, a nurse may take the patient to an exam room to wait before the doctor is ready for the appointment. Thus, the total wait time can be expressed as the sum of the wait time in the pre-process buffer locations (the wait time in the reception area plus the wait time in the exam room). To our knowledge, no one has investigated how waits in different buffer locations can be costly (or not) to businesses and how businesses can better manage queues with multiple pre-process buffers. Dube-Rioux et al. (1989) technically examined customer preferences for waiting in different locations in a restaurant, but their study was based on a survey experiment and not on the actual experience of the customer.

Carmon and Kahneman (1996) ran an experiment to examine the utility of participants in a virtual line throughout the duration of a wait for service. They found that when participants advanced in line, they experienced an increase in their experimentally measured utility. In settings where customers wait in two pre-process buffer locations, we can assume that utility rises when they enter the second pre-process buffer. We could then theoretically have a customer's utility decline at different rates in the first and second pre-process buffers in response to their waiting times in each location. Redelmeier and Kahneman (1996) demonstrated the peak-end rule which suggests that patient utility is most impacted at the peak (or most intensive part) of the experience and the end of the experience. In addition, Bray (2018) suggested that customer satisfaction is impacted most positively when operational transparency is provided later in the wait. Pre-op is not only the second (i.e., later) buffer but patients may also be less comfortable having to wait in a hospital gown with an IV in their hand and patient stress may be higher due to the anxiety-inducing procedure occurring after this buffer (Yung et al. (2002)). Anecdotally, customers in other settings may typically face steeper declines in utility in the second pre-process buffer in response to wait times there. For example, in 2009, the federal government set fines for airlines who made passengers wait too long on the tarmac (see Wald (2009)). Because pre-op is both the later and more clinically/emotionally intensive buffer, we propose the following hypothesis:

Hypothesis 2. Customers will experience steeper declines in satisfaction in response to waiting times in the second buffer.

Many papers describe how customers experience queues. Maister (1985) suggested that fair waits are less painful to customers than unfair waits. We define fair queues as those that adhere to FIFO queue discipline. Often, customers may not wait in a physical line but may wait in a general waiting area commingled with others waiting for service and may, therefore, assess FIFO queue discipline. Milgram et al. (1986); Helweg-Larsen and LoMonaco (2008) show that violations of FIFO queue discipline by others in line can be an unpleasant experience for queue participants. Batt and Terwiesch (2015) found that patients waiting for emergency services in a hospital may renege in response to the arrival of new patients that could potentially jump ahead of them in line. In our study, patients can better assess FIFO queue discipline in the first buffer since the reception waiting room is commingled whereas in the second buffer, pre-op, waiting occurs in individual stations which are partitioned off. We believe patients could assess FIFO queue discipline, and we use heuristics to gauge each patient's experience of FIFO queue discipline: the number of other patients they observe who arrive later (earlier) but advance to the next location in the process earlier (later). These heuristics could be better assessed in the first buffer, and as a result, we propose the set of hypotheses below:

Hypothesis 3A. In a buffer where customer arrivals are observable, a customer's satisfaction decreases (increases) when the number of other customers who arrive later (earlier) to the buffer but advance to the next stage in the process earlier (later) increases.

Hypothesis 3B. In a buffer where customer arrivals are not observable, FIFO queue discipline does not impact customer satisfaction.

Taylor (1994) suggests that "post-schedule" waits, or the incremental wait time after the scheduled appointment time, could have a meaningful impact on customer utility. Therefore, the airline industry, for example, stresses the importance of on-time performance. Given that patients have a scheduled service time and may be cognizant of post-schedule waits (for example by checking the time the procedure is scheduled relative to the approximate time they are wheeled out of pre-op), utility could fall in response to longer post-schedule waits. Similarly, utility could rise in response to service beginning before the scheduled time.

Research shows that people may make mental accounts for time (Rajagopal and Rha (2009)). Thus, when given an estimated wait time, people may create a mental account for how long they are prepared to wait. However, we believe patients may be much more sensitive to perceived losses of time (relative to their budgeted wait time) versus perceived gains in time due to prospect theory (Kahneman and Tversky (2013). The regret of arriving to an appointment too early may impact utility to a larger degree than the joy of knowing one is proceeding ahead of schedule. We therefore hypothesize the following:

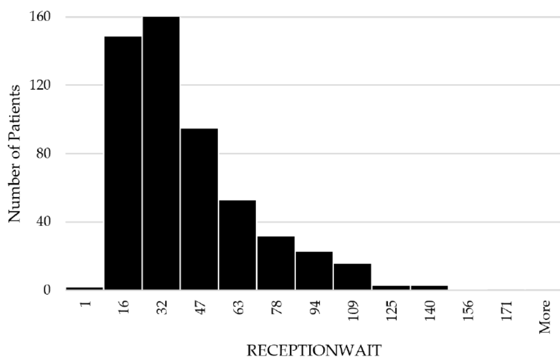
Hypothesis 4. Customer satisfaction is impacted to a smaller (larger) degree by waits that conclude before (after) a scheduled wait time.

3.4. Observational Study - Data Description

From January 2014 to September 2017, 20,881 patients received an endoscopy procedure at the hospital. For each patient, we obtained detailed timestamp information on the process flow: (i) Time the patient entered the facility; (ii) Time the patient entered pre-op; (iii) Time the patient entered the OR; (iv) Time the procedure began; (v) Time the procedure completed; (vi) Time the patient exited the OR. Out of these patients, 583 submitted the patient satisfaction survey used in this analysis. Because we are interested in measuring the satisfaction of patients with respect to their waiting experiences in the queue, $SATISFACTION_i$ is patient i's rating of the following item in the satisfaction survey: *Waiting time before your surgery or procedure began.* This item, in addition to all items in the satisfaction survey, were rated on the following scale: *1 – very poor, 2 – poor, 3 – fair, 4 – good, or 5 – very good*, so higher ratings corresponded to higher satisfaction.

$LOGTOTALWAIT_i$ is the log of the difference (in minutes) between the time patient i started service (i.e., entered the OR) and the time the patient entered the first buffer. This variable allows us to test Hypothesis 1. $LOGBUFFER1WAIT_i$ is the log of the difference (in minutes) between the time patient i entered the second buffer (pre-op) and the time the patient entered the first buffer (reception). $LOGBUFFER2WAIT_i$ is the log of the difference (in minutes) between the time patient i started service and the time the patient entered the second buffer. These two variables allow us to test Hypothesis 2. Figure 9 shows a histogram of each of the non-logged wait times.

Given that we had the timestamps of each patient's movements throughout the day, we reconstructed patient flow through the facility and calculated variables relating each patient's position in the process relative to other patients' positions. (Note that for all patient flow-related calculations, we did not count patients who were missing data for any of the timestamps used to calculate the variable. We also ignored patients for whom the procedure was canceled.) The variable $BUFFER1CUTS_i$ is the number of patients who arrived after patient i in the first buffer but who entered the second buffer before patient



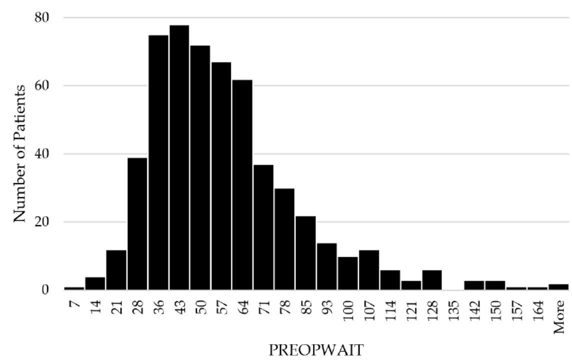


Figure 9: Histograms of the non-logged total wait time in reception ($RECEPTIONWAIT$) and pre-op ($PREOPWAIT$)

i. $BUFFER2CUTS_i$ is the number of patients who arrived after patient i in the second buffer but who entered service before patient i. In addition, we define $BUFFER1ADV_i$ as the number of patients who arrive before patient i in the first buffer but who entered the second buffer after patient i and $BUFFER2ADV_i$ as the number of patients who arrived before patient i in the second buffer but who entered service after patient i. These four variables will allow us to test Hypothesis 3.

To test Hypothesis 4, we tabulated the difference (in minutes) between the time patient i entered service and the time the patient was scheduled for service ($DELAY_i$). Therefore, this variable represents the patient's delay with respect to their scheduled procedure time. We expect that increases in this variable will lead to decreases in satisfaction. We also include the square ($DELAY_i^2$) and cubic ($DELAY_i^3$) terms to test Hypothesis 4 since we anticipate increases in $DELAY$ will impact satisfaction to a greater degree than decreases in the variable.

In our analysis, we aim to control for other factors that may impact a patient's satisfaction and be related to either wait times in reception or pre-op but are not driven by the location of a specific buffer. Businesses often provide customers information on the queue as waits for service become longer (e.g., explanations for delays), and as mentioned above, this practice may increase customer utility. In our setting, any information on longer waits (which could help increase patient satisfaction) is more likely to come about later in the wait in the second buffer (i.e., pre-op). We therefore controlled for patient i's response to the following item in the satisfaction survey: Information nurses gave you on the day of your procedure.

Congestion or lack of training of staff in the first or second buffer may lead to lower staff responsiveness in either location. As a result, a patient who experiences longer waits in a location with less responsive staff may have their satisfaction decline faster in that location. We therefore control for responsiveness of first buffer staff using patient i's response to the following item in the satisfaction survey: Helpfulness of the person at the registration desk. Because patients are primarily dependent on nurses for their non-urgent needs in the second buffer, we control for the responsiveness of pre-op staff using patient i's response to the following in the survey: *Friendliness/courtesy of the nurses.*

The number of patients in the first buffer, a location where patients wait in a common area, may make patients more susceptible to externalities from congestion such as noise from other patients. Lower et al. (2003) found that patients were more satisfied and Yoder et al. (2012) found that patients had better sleep outcomes in quieter hospital environments. The number of other patients who are in reception can vary over the duration a patient is in the location. To control for congestion, we use a proxy based on the number of other patients who arrived before patient i and proceeded into pre-op after patient i arrived.

Patients who are less healthy may require more time in pre-op but may be more uncomfortable and less satisfied waiting in general. Patients who are older, female (see Macintyre et al. (1996)), and of certain races (see Williams (1997)) may face different health issues. We therefore control for the age, gender, and race of each patient.

Olowokure et al. (2006) provided evidence that patients receiving cervical screening tests preferred certain times of day for their appointments. We therefore wanted to control for any potential effect the time of day of a patient's arrival to the endoscopy suite could have on the patient's satisfaction with the wait before the procedure. We therefore controlled for the time of day patient i arrived.

Patients who receive a colonoscopy have to avoid solid food the day before the procedure. Dube-Rioux et al. (1989) showed that when customers are hungry in a restaurant setting, in-process waits can be more painful than pre-process waits. Because in-process waits come after pre-process waits, it is possible that patients receiving a colonoscopy may have steeper declines in satisfaction in the second buffer. We therefore control for whether patient i was receiving a colonoscopy procedure.

Finally, we aim to control for any potential discomfort patients may have encountered relating to the location setting (e.g., air conditioning or heating issues, lack of chairs in reception, uncomfortable beds in pre-op, etc.). With respect to the first buffer, we control for patient i's survey response to: Comfort of the registration waiting area. For the second buffer, we control for patient i's response to the following item in the survey: Comfort of your room or resting area in the Center.

Our final sample size, after eliminating patients surveyed who either had missing or inaccurate timestamp information (e.g., patients who had arrival times in the first buffer recorded as occurring after the time the patient was placed in the second buffer) or who did not have data on one of the survey items was n=560. Table 3 shows a summary of the main variables used in our analysis. We observe that satisfaction scores tend to be really high in our sample (4.377 on average), so even small effect sizes detected in our study can be quite meaningful. Note that we did include in our analysis data from two patients who had timestamps indicating a zero minute wait time in the first buffer. Because we took the log of the wait time in the first buffer, we coded these wait times as equal to 1 minute. We also included data from four patients who did not arrive at the center on the day of their procedure. Because these patients did not have to wait in the first buffer, we recorded the wait times there as equal to 1 minute as well. Finally, we based the wait time in the second buffer of one patient on the time the patient had been recorded to have exited the second buffer rather than the time the patient started service because the service start time seemed erroneously recorded.

Table 3: Summary of regressand and regressors of interest

Statistic	*SATISFACTION*	*TOTALWAIT*	*BUFFER1WAIT*	*BUFFER2WAIT*	*BUFFER1CUTS*	*BUFFER2CUTS*	*BUFFER1ADV*	*BUFFER2ADV*	*DELAY*
Mean	4.377	89.171	34.568	54.604	0.313	0.3	0.338	0.338	16.095
Std. Dev.	0.862	39.312	29.323	25.924	0.861	0.671	0.594	0.6	40.965
Minimum	1	14	1	7	0	0	0	0	-351
Maximum	5	439	357	171	12	6	3	4	159
25%ile	4	62	16	36	0	0	0	0	-4
50%ile	5	83	25	49	0	0	0	0	12
75%ile	5	108	46	66	0	0	1	1	38

3.5. Observational Study - Model and Results

3.5.1. Model

We are interested in examining how different factors impact patient satisfaction through regression analysis. Given the hypotheses and variables described above, we are interested in the following two ordinary least squares ("OLS") regression specifications:

$$\begin{aligned} SATISFACTION_i = &\alpha_1 + \beta_1 LOGTOTALWAIT_i + \omega_1 BUFFER1CUTS_i + \\ &\delta_1 BUFFER2CUTS_i + \zeta_1 BUFFER1ADV_i + \eta_1 BUFFER2ADV_i + \\ &\theta_1 DELAY_i + \iota_1 DELAY_i^2 + \upsilon_1 DELAY_i^3 + X_i^{'}\mu_1 + \varepsilon_{1i} \end{aligned} \tag{3.1}$$

$$\begin{aligned} SATISFACTION_i = &\alpha_2 + \beta_2 LOGBUFFER1WAIT_i + \gamma_2 LOGBUFFER2WAIT_i + \\ &\omega_2 BUFFER1CUTS_i + \delta_2 BUFFER2CUTS_i + \\ &\zeta_2 BUFFER1ADV_i + \eta_2 BUFFER2ADV_i + \theta_2 DELAY_i + \\ &\iota_2 DELAY_i^2 + \upsilon_2 DELAY_i^3 + X_i^{'}\mu_2 + \varepsilon_{2i} \end{aligned} \tag{3.2}$$

where i is the index for each individual patient and X_i is the vector of control variables.

Omitted variable bias is a common issue in any OLS specification. One example of potential omitted variable bias in our setting is that we do not observe the state of health of each patient (e.g., body mass index, blood pressure, etc.) and only control for factors which may be correlated with health (i.e., age, gender, and race). Because patients who are less healthy may require more time in the second buffer (i.e., pre-op) but may be more uncomfortable and less satisfied waiting in general, we have an omitted variable bias in γ_2 of Equation 3.2. We propose the instrumental variable $PATIENTSAHEAD_i$, which is the number of other patients (assigned to the same OR as patient i) who have yet to complete service before patient i when patient i enters the second buffer, to overcome omitted variable bias. The mean and standard deviation of $PATIENTSAHEAD_i$ are 0.852 and 0.711 respectively and Figure 10 shows its distribution. Because $PATIENTSAHEAD_i$ is a function of how the hospital schedules its patients, it should be unrelated to a particular patient's health, thus satisfying the exogeneity requirement of an instrument. However, $PATIENTSAHEAD_i$ should play a key role in how long a patient waits in pre-op since it represents the inventory of patients that must complete their procedures before patient i, thus satisfying the relevance requirement of an instrument. To check the exogeneity requirement of $PATIENTSAHEAD_i$, we run three analysis of variance ("ANOVA") tests examining the difference in means of $PATIENTSAHEAD_i$ conditional on the categories of age, gender, and race, which all may be correlated with health as discussed above. These tests allows us to examine whether health may play a role in the patient scheduling decisions (e.g., if doctors tend to schedule healthier patients in the morning when there is less likely to be more patients ahead in the process). None of the F-statistics came back as significant. For the ANOVA with age, the p-value was 0.247 and for gender, the p-value was 0.938. The ANOVA for race had a p-value of 0.314.

To estimate Equation 3.2, we need to use two-stage least squares ("2SLS") with the following first-stage regression specification:

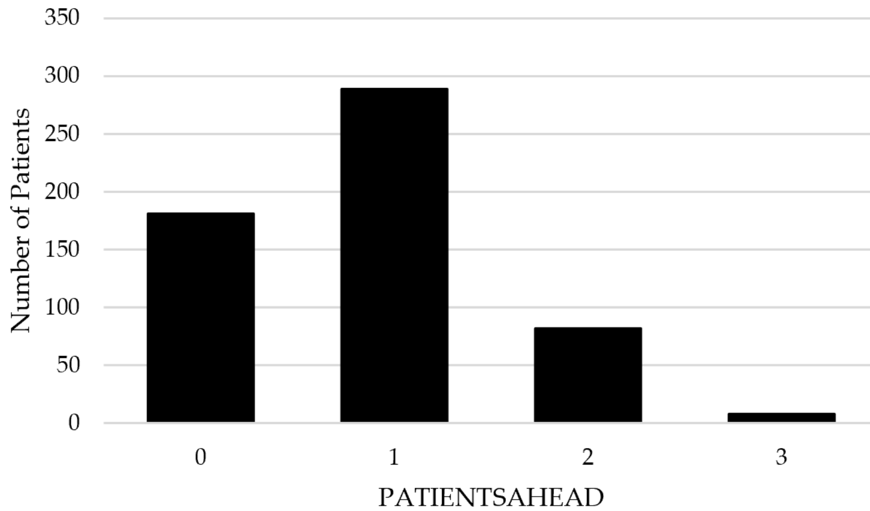


Figure 10: Distribution of $PATIENTSAHEAD$

$$
\begin{aligned}
LOGBUFFER2WAIT_i = & \alpha_3 + \beta_3 LOGBUFFER1WAIT_i + \gamma_3 PATIENTSAHEAD_i + \\
& \omega_3 BUFFER1CUTS_i + \delta_3 BUFFER2CUTS_i + \\
& \zeta_3 BUFFER1ADV_i + \eta_3 BUFFER2ADV_i + \theta_3 DELAY_i + \\
& \iota_3 DELAY_i^2 + \upsilon_3 DELAY_i^3 + X_i^{\prime} \mu_3 + \varepsilon_{3i} \qquad (3.3)
\end{aligned}
$$

Our first-stage regression suggests that the relevance condition of our instrument is met with an F-statistic of 107.5 which far exceeds the rule-of-thumb of 10 (see Staiger and Stock (1997)).

We then estimate the second-stage regression using the predictions generated from Equation 3.3, $LOGBU\widehat{FFE}R2WAIT_i$:

$$
\begin{aligned}
SATISFACTION_i = & \alpha_4 + \beta_4 LOGBUFFER1WAIT_i + \gamma_4 LOGBU\widehat{FFER}2WAIT_i + \\
& \omega_4 BUFFER1CUTS_i + \delta_4 BUFFER2CUTS_i + \\
& \zeta_4 BUFFER1ADV_i + \eta_4 BUFFER2ADV_i + \theta_4 DELAY_i + \\
& \iota_4 DELAY_i^2 + \upsilon_4 DELAY_i^3 + X_i^{'}\mu_4 + \varepsilon_{4i} \qquad (3.4)
\end{aligned}
$$

Note that our model would be misspecified if $PATIENTSAHEAD_i$ impacted the responsiveness of staff in the second stage which may then affect patient satisfaction with the wait before the procedure. To ensure this is not the case in our data, we run an ANOVA which tests the difference of the means of the responsiveness ratings of nurse staff (i.e., patient i's response to the following: Friendliness/courtesy of the nurses) stratified by $PATIENTSAHEAD_i$. The F-statistic was not significant (p-value=0.510). Our model would also be misspecified if $PATIENTSAHEAD_i$ was strongly related to wait times in the first buffer. However, when we perform a 2SLS first-stage regression using $LOGBUFFER1WAIT_i$ as the endogenous regressor (rather than $LOGBUFFER2WAIT_i$), we find that $PATIENTSAHEAD_i$ is a weak instrument (F-statistic of 8.99). Thus, we believe our model is properly specified.

3.5.2. Results

Table 15 shows our regression results for our regressors of interest and Table 28 shows our regression results for our other regressors. Our first regression specification from Equation 3.1, OLS (Total), provides evidence for Hypothesis 1. Specifically, we find that the coefficient on $LOGTOTALWAIT$ is negative and significant, and a 50% increase in a patient's total wait time decreases the patient's expected satisfaction score by 0.17 points (on a 5-point scale). Therefore, a patient's total wait time before service seems to negatively impact their satisfaction.

The regression model, 2SLS, is our main specification stemming from Equations 3.3 and 3.4.

Table 4: Regression results for regressors of interest in the observational study

DV: SATISFACTION	(1) OLS (TOTAL)	(2) OLS (LOCATION)	(3) 2SLS
LOGTOTALWAIT	-0.373***		
	(0.109)		
LOGBUFFER1WAIT		-0.059	-0.082*
		(0.042)	(0.047)
LOGBUFFER2WAIT		-0.212**	-0.522**
		(0.082)	(0.202)
BUFFER1CUTS	-0.095**	-0.129***	-0.125***
	(0.044)	(0.043)	(0.041)
BUFFER2CUTS	-0.067	-0.069	0.000
	(0.076)	(0.080)	(0.090)
BUFFER1ADV	0.135**	0.169***	0.179***
	(0.062)	(0.063)	(0.063)
BUFFER2ADV	-0.008	-0.000	-0.057
	(0.045)	(0.044)	(0.057)
DELAY	-0.003***	-0.004***	-0.002*
	(0.001)	(0.001)	(0.001)
$DELAY^2$	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)
$DELAY^3$	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Observations	560	560	560
R^2	0.454	0.451	0.437
H_0: $\beta_{\widehat{LOGBUFFER2WAIT}}$-$\beta_{\widehat{LOGBUFFER1WAIT}}$=0			
p-value:		0.096*	0.022**

*p<0.1, **p<0.05, ***p<0.01

Robust standard errors reported in parentheses

For our Hypothesis 2 we find evidence that patients do face steeper declines in satisfaction in the second buffer. If a patient's wait time were to increase by 50% in the second buffer, the average satisfaction score would decline by 0.21 points, whereas the same magnitude of increase in the first buffer wait would lead to a 0.03 point decline. We also test the null hypothesis that the second buffer coefficient minus the first buffer coefficient is significantly different from zero, and the result is significant. Wooldridge (1995)'s test for endogeneity with robust standard errors suggests that the second regression specification, OLS (Location), may be endogenous (p-value=0.083). If we did not take into account endogeneity of waits in the second buffer, the effect size for this regressor would have been less than half the size.

Per Hypothesis 3, we observe that the number of patients who overtake a patient in the first buffer (where customer arrivals are observable) does impact a patient's satisfaction whereas in the second buffer (where customer arrivals are not observable) it does not. Each incident in the first buffer reduces the average patient satisfaction score by -0.125. Similarly, for each individual a patient overtakes in the first buffer, the satisfaction score increases by 0.179 on average, whereas in the second buffer, the effect is not significant. Our results indicate that customers may track FIFO queue discipline in locations of the queue where there is no physical line but other customers are more visible.

With respect to Hypothesis 4, our regression result suggests that increases in $DELAY$ result in decreases in patient satisfaction. However, the coefficients on the square and cubic terms of $DELAY$ were not significant. As a result, we were note able to support Hypothesis 4 with our observational study, however we test the hypothesis again using an experiment in Section 3.7.

With respect to our controls, we find that providing patients with information on the day of the procedure may have a significant impact on patient satisfaction. In particular, a one point increase in a patient's satisfaction with respect to whether they were informed leads to a 0.418 increase in the average patient satisfaction score in the 2SLS model. Hospital staff could consider providing patients with information such as the actual number of patients ahead in the process, updates on the expected wait time, or explanations on any delays throughout the waiting experience.

We also find that the number of other patients in reception may decrease satisfaction. For each additional patient in the reception waiting area, average patient satisfaction decreases by 0.125 points. This finding suggests that patient externalities, such as noise, may affect patients in commingled areas such as reception. Our results also indicate that patient satisfaction increases with more helpful registration staff, higher levels of comfort in their room/resting area, and during two times of the day at the endoscopy center. For all other controls not discussed, we found no significant effect in our estimation.

3.6. Observational Study - Robustness Tests for Hypothesis 2

Below we discuss four robustness tests for our observational results:

3.6.1. Proportion Effect

Our econometric specification does not take into account the fact that patients may actually care about the proportion of time they spend waiting in each location rather than the location itself. For example, suppose two patients wait 100 minutes before entering the OR. Patient 1 waited 25 minutes in reception and felt increasingly frustrated that the wait in pre-op was much longer than the wait in reception. Patient 2, however, waited 50 minutes in reception and was expecting to wait about the same amount of time in pre-op and thus was not very frustrated with the 50 minute wait. Because we observe longer waits on average in pre-op in our data, this effect could be an alternative explanation for our support of Hypothesis 2. To check whether this alternative explanation is viable in our data, we ran the following OLS regression specification that controls for this effect:

$$
\begin{aligned}
SATISFACTION_i = & \alpha_5 + \beta_5 BUFFER1DIVBUFFER2WAIT_i + \\
& \gamma_5 BUFFER2WAIT_i + \omega_5 BUFFER1CUTS_i + \\
& \delta_5 BUFFER2CUTS_i + \zeta_5 BUFFER1ADV_i + \\
& \eta_5 BUFFER2ADV_i + \theta_5 DELAY_i + \iota_5 DELAY_i^2 + \upsilon_5 DELAY_i^3 + \\
& X_i^{\prime} \mu_5 + \varepsilon_{5i}
\end{aligned} \tag{3.5}
$$

where $BUFFER1WAIT_i$ is patient i's total wait in the second buffer and $BUFFER1DIVBUFFER2WAIT_i$ is patient i's wait in the first buffer divided by the wait in the second buffer. A significant coefficient for β_5 would indicate the presence of the proportion effect in our data since it would suggest that an increase in the disparity between the wait in the first buffer and the wait in the second buffer impacts patient satisfaction. However, we do not find a significant coefficient on β_5 in our regression results (p-value=0.369).

3.6.2. Nonlinear Total Wait Utility

Osuna (1985) suggests that customer utility in response to waiting times decreases and is concave (i.e., decreasing at an increasing rate in later stages). Shimkin and Mandelbaum (2004) and Janakiraman et al. (2011) also suggest that waiting costs may have a nonlinear effect. In our study, a patient may become dissatisfied at an increasing rate in response to the total wait time before service. Because pre-op is the second buffer (i.e., pre-op waits occur later in the process than reception waits), pre-op waits would appear to be more painful if patients are becoming increasingly dissatisfied in response to the total wait time. To check whether this alternative explanation for Hypothesis 2 is viable, we run the following OLS regression:

$$
\begin{aligned}
SATISFACTION_i = &\alpha_6 + \beta_6 TOTALWAIT_i + \gamma_6 TOTALWAIT_i^2 + \\
&\omega_6 BUFFER1CUTS_i + \delta_6 BUFFER2CUTS_i + \\
&\zeta_6 BUFFER1ADV_i + \eta_6 BUFFER2ADV_i + \theta_6 DELAY_i + \\
&\iota_6 DELAY_i^2 + \upsilon_6 DELAY_i^3 + X_i^{'} \mu_6 + \varepsilon_{6i} \qquad (3.6)
\end{aligned}
$$

where $TOTALWAIT_i$ is patient i's total wait time before service. If patients are becoming dissatisfied at an increasing rate in response to the total wait time before service, we would expect γ_6 in our data to have a significant, negative slope. However, after running this regression, we find that γ_6 is not significant (p-value=0.562).

3.6.3. Instrument as a Factor

The distribution of our instrumental variable, $PATIENTSAHEAD$, which is shown in Figure 10 does not look uniform and only takes on four different values. Therefore, we wanted to ensure our 2SLS results were still valid if the instrumental variable were treated as a factor variable rather than a continuous variable. After performing the regression, we find nearly identical results to our original 2SLS specification: a negative coefficient on $LOGBUFFER2WAIT$ ($\hat{\gamma_4}$=-0.495 and p-value=0.007) that is significantly larger (p-value=0.019) than the coefficient on $LOGBUFFER1WAIT$ ($\hat{\beta}_4$=-0.080 and p-value=0.081).

3.6.4. Two-Stage Residual Inclusion

Given that SATISFACTION could be thought of as an ordinal rather than continuous variable, we wanted to ensure that our results still held using an ordinal method. Equation 3.1 can simply be adapted from OLS to ordinal logistic regression using maximum likelihood to test Hypothesis 1. However, for testing the other hypotheses, which require 2SLS, we cannot simply modify Equation 3.4 to become an ordinal logistic regression. Terza et al. (2008) found that the technique of including the first stage predicted values from two-stage least squares (2SLS) into a non-linear second stage regression does not result in consistent estimation of the second stage coefficients on the regressors. Instead, the authors found that including the residuals from the first stage of 2SLS and the original endogenous regressor into a non-linear second stage regression in addition to all the usual exogenous variables resulted in consistent estimation of the coefficients on the regressors in a method known as two-stage residual inclusion ("2SRI"). With respect to implementing 2SRI in our robustness check, we estimate the residuals $\widehat{u}_i$ for each patient resulting from our first-stage 2SLS regression estimation specified in Equation 3.3:

$$\widehat{u}_i = LOGPREOPWAIT_i - LOGPR\widehat{EOP}WAIT_i \tag{3.7}$$

In the second stage, we use the residuals from the first stage and run the following ordinal logistic regression specification:

$$\begin{aligned} SATISFACTION_i = &\alpha_7 + \psi_7 + \rho_7 + \pi_7 + \beta_7 LOGBUFFER1WAIT_i + \\ &\gamma_7 LOGBUFFER2WAIT_i + \omega_7 BUFFER1CUTS_i + \\ &\delta_7 BUFFER2CUTS_i + \zeta_7 BUFFER1ADV_i + \\ &\eta_7 BUFFER2ADV_i + \theta_7 DELAY_i + \iota_7 DELAY_i^2 + \upsilon_7 DELAY_i^3 + \\ &X_i^{'}\mu_7 + \rho_7 \widehat{u}_i + \varepsilon_{7i} \end{aligned} \tag{3.8}$$

We then performed bootstrapping of the observations to create 10,000 resampled data sets to calculate the proper standard errors for hypothesis testing. (Note that 81 of the resampled data sets suffered from collinearity issues and could not be estimated.) The 2SRI results continue to show a significant, negative coefficient on $LOGBUFFER2WAIT$ ($\hat{\gamma}_7$=-1.964 and p-value=0.009) that is significantly larger (p-value=0.034) than the coefficient on $LOGBUFFER1WAIT$ ($\hat{\beta}_7$=-0.435 and p-value=0.065).

3.7. Experiments

To replicate findings in our observational study and support a causal interpretation of our results, we ran three randomized controlled experiments with adult subjects from the Amazon Mechanical Turk platform who completed simulated waiting experiences. In Experiment 1, we disentangle whether patient satisfaction declines quicker in pre-op in response to wait times due to the fact that pre-op is the second/last pre-process buffer or the buffer with higher wait disutility (e.g., clinical and/or emotional intensity). In the experiment, we had each subject wait in a two-part tandem queue to complete two different surveys. All subjects waited for the same amount of time, however we were able to change the proportion of time that subjects waited in the two stages, and while waiting, the subjects listened to different sounds (as a proxy for wait disutility). We find that subject satisfaction declined in response to the disutility of the wait rather than longer wait times in the second stage.

In Experiment 2, we provide causal evidence that customers care about FIFO queue discipline in multi-server settings where server assignment of other customers waiting is unknown and customers can view queue progress of other customers in an open waiting area. In the experiment, each subject waits for the same amount of time for one of three different servers who the subject has chosen to administer a quiz. The subject does not observe the server assignment of others. The subject waits in an open waiting area and can view other subjects entering the area and exiting the area into service. In particular, we find that subject satisfaction declined in response to perceived cuts in line.

In Experiment 3, we provide causal evidence for Hypothesis 4. In the experiment, all subjects wait for the same amount of time to complete a survey. Subjects are given an estimated wait time for a server who will administer a survey. The actual wait times for subjects conclude early, on-time, or after the scheduled wait time. We find that subject satisfaction in the delayed condition was significantly lower than that in the on-time condition. However, subject satisfaction in the early condition was not significantly different from the on-time condition.

3.7.1. Location of Wait vs. Wait Disutility (Experiment 1)

Methods. In a pre-experiment, we recruited twenty nine subjects to rate the pleasantness (i.e., disutility) of six different ambient sounds: a baby crying (*Baby*), alarm clock (*Alarm*), lawnmower (*Lawnmower*), meditation bowls (*Bowls*), a creek (*Creek*), and piano music (*Piano*). To ensure subjects were incentive compatible and actually listened to the sounds, we inserted two spoken words into the ambient sound file, one in a random position prior to the end of the sound file and another at the end of the sound file, and subjects were not informed about the length of each sound file. Subjects understood that they would receive bonus payments for each word correctly identified. The subjects rated each of the ambient sounds from strongly unpleasant (coded as 1) to strongly pleasant (coded as 7). After analyzing the responses of subjects who passed a comprehension check of the experiment and who correctly identified all the spoken words in the six ambient sound files, we found that the alarm clock and baby crying were the most unpleasant sounds (and virtually indistinguishable in terms of unpleasantness), and the piano music was the most pleasant sound (see Table 5). We then defined a high disutility wait as one which a subject would listen to the alarm clock and a low disutility wait as one which a subject would listen to the piano.

For the main experiment, two hundred and seventy three adult subjects were recruited to complete a two-part survey in which they would have to wait to complete each part of the survey. Each subject waited a total of 100 seconds, with the wait for the second part of the survey being 25 seconds (*Short 2nd Wait*), 50 seconds (*Equal Waits*), or 100 seconds (*Long 2nd Wait*). In addition, during both the first wait and second wait, subjects listened either to piano music (*Low Disutility 1st Wait*/*Low Disutility 2nd Wait*) or the alarm clock (*High Disutility 1st Wait*/*High Disutility 2nd Wait*) in the background. All subjects were randomly assigned to one of the twelve conditions (see Table 6). To ensure subjects were incentive compatible and listened to the sounds and paid attention to the wait experience, we used the same technique as in the pre-experiment with the spoken words and only included in our study those subjects who passed a comprehension check of the experiment

Table 5: Results for the pre-experiment measuring sound pleasantness

DV: *Pleasantness* (1-Strongly Unpleasant to 7-Strongly Pleasant)	Linear Regression
Baby (vs. Alarm)	0.000
	(0.133)
Lawnmower (vs. Alarm)	1.000***
	(0.188)
Bowls (vs. Alarm)	3.828***
	(0.220)
Creek (vs. Alarm)	4.069***
	(0.219)
Piano (vs. Alarm)	4.655***
	(0.215)
Constant	1.276***
	(0.109)
Subject fixed effects	Yes
Observations	174
Between R^2	0.020
Within R^2	0.822

*p<0.1, **p<0.05, ***p<0.01
Robust standard errors reported in parentheses

and who correctly identified all the spoken words. After completing the two waits, subjects were asked to rate the overall waiting experience for both parts of the survey from very poor (coded as 1) to very good (coded as 5), a scale consistent with the satisfaction survey used in our observational study. See Section A.8 for more details on Experiment 1.

Table 6: Number of subjects per condition in the location of wait vs. wait disutility experiment

	Low Disutility 1st Wait		High Disutility 1st Wait	
Subjects (273)	Low Disutility 2nd Wait	High Disutility 2nd Wait	Low Disutility 2nd Wait	High Disutility 2nd Wait
Short 2nd Wait	22	27	19	15
Equal Waits	19	20	29	21
Long 2nd Wait	26	25	28	22

Results. We find evidence that the disutility of the wait significantly reduced satisfaction with the overall waiting experience in both the first and second parts of the wait (see Table 7). However, we did not find evidence that equal waits or longer second stage waits significantly impacted overall satisfaction with the wait. *Equal Waits* had a coefficient estimate close to zero, however we acknowledge that the larger, negative coefficient on *Long*

2nd Wait could lack significance due to sample size limitations (see Section A.7 for a power analysis we conducted).

Table 7: Results for the location of wait vs. wait disutility experiment

DV: *Satisfaction* (1-Very Poor to 5-Very Good)	OLS
Equal Waits (vs. Short 2nd Wait)	0.022
	(0.274)
Long 2nd Wait (vs. Short 2nd Wait)	-0.290
	(0.267)
High Disutility 1st Wait (vs. Low Disutility 1st Wait)	-0.663***
	(0.253)
High Disutility 2nd Wait (vs. Low Disutility 2nd Wait)	-1.099***
	(0.251)
Equal Waits x High Disutility 1st Wait	-0.185
	(0.342)
Long 2nd Wait x High Disutility 1st Wait	0.281
	(0.333)
Equal Waits x High Disutility 2nd Wait	-0.059
	(0.346)
Long 2nd Wait x High Disutility 2nd Wait	-0.107
	(0.336)
High Disutility 1st Wait x High Disutility 2nd Wait	0.426
	(0.387)
Equal Waits x High Disutility 1st Wait x High Disutility 2nd Wait	-0.388
	(0.515)
Long 2nd Wait x High Disutility 1st Wait x High Disutility 2nd Wait	-0.138
	(0.528)
Constant	4.136***
	(0.211)
Observations	273
R^2	0.433

*p<0.1, **p<0.05, ***p<0.01
Robust standard errors reported in parentheses

Discussion. Our results help to rule out that patients may prefer waiting in equal time increments. In addition, our results suggest that waits in pre-op may be dissatisfying for patients due to the disutility of the wait rather than the fact that it is the second/last wait. The issue of pre-op being the second/last wait is unavoidable, however the issue of wait disutility (e.g., patient comfort and anxiety) can potentially be mitigated. Studies have found that interventions related to sound/music (Yung et al. (2002); Johnson et al.

(2012)) could help to alleviate patient anxiety. Shell and Buell (2019) found that human contact could alleviate customer anxiety in service settings, so having nurses check in with patients more frequently could be another potential solution to combat the quick decline in satisfaction patients experience in pre-op.

3.7.2. Perceived Fairness (Experiment 2)

Methods. One hundred and seventy subjects were recruited to take a math, logic, or writing quiz that was administered by one of three fictitious servers specializing in one quiz type. The subject chose one quiz type to take and were assigned an animal avatar so that the subject could see themselves waiting in the virtual environment. Subjects were then required to wait for the server in an open waiting area where they could see the animal avatars of other fictitious subjects that were supposedly waiting to take their quizzes with any one of the three servers (see Figure 11 for a visualization of the open waiting area that subjects experience). Each subject waited for 100 seconds. While in the opening waiting area, subjects randomly experienced one of three conditions:

- *FIFO*: When the subject enters, two other avatars are already in the waiting area. Those two avatars depart the waiting area into service, one after the other. The subject then sees the arrival of three additional avatars individually. The subject then enters service. Thus, the subject believes she is the third (out of six) to arrive to the waiting area and the third (out of six) to enter service.

- *Advance in Line*: When the subject enters, two other avatars are already in the waiting area. The subject then sees the arrival of three additional avatars and then enters service before the other two who were present prior to the subject's arrival. Thus, the subject believes she is the third (out of six) to arrive to the waiting area and the first (out of six) to enter service.

- *View Cuts in Line*: When the subject enters, two other avatars are already in the waiting area. Those two avatars depart the waiting area into service, one after the other. The subject then sees the arrival of three additional avatars individually, and two of these avatars enter service before the subject enters into service. Thus, the subject believes she is the third (out of six) to arrive to the waiting area and the fifth (out of six) to enter service.

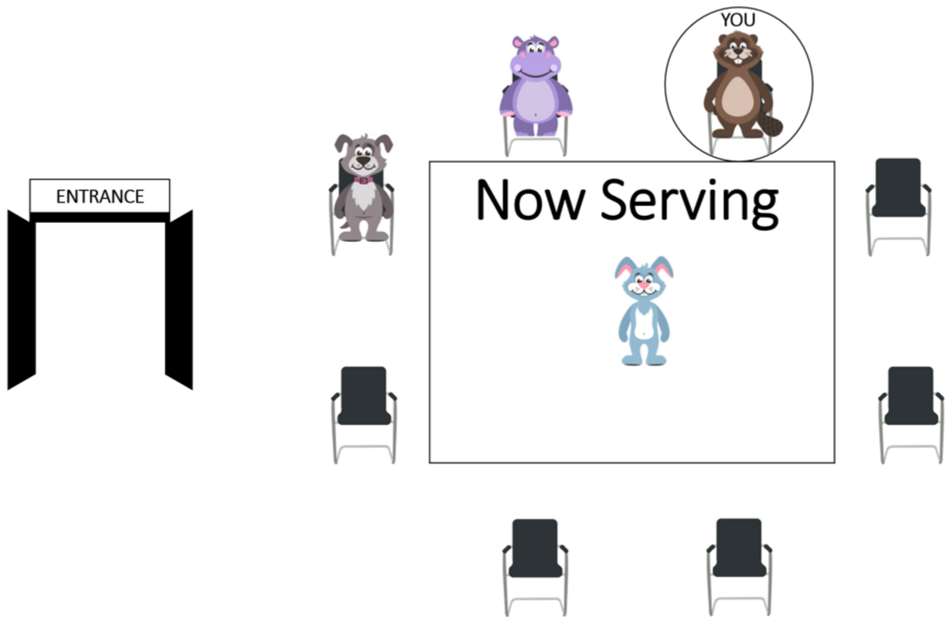


Figure 11: Snapshot of the open waiting area subjects experienced in Experiment 2. New avatars enter through the "ENTRANCE", "You" indicates the avatar of the subject, and when avatars proceed into service, they move from a chair to the "Now Serving" box.

Table 8 shows the number of subjects assigned to each condition. To encourage subjects to pay attention to the movements of the other fictitious subjects in the open waiting area, we provided a bonus to subjects who were able to identify the animal avatar of the individual who entered service immediately before the subject. After the wait was complete, we asked subjects to rate the waiting experience for their server from very poor (coded as 1) to very good (coded as 5). In our analysis, we only included subjects who made a correct identification and who passed a comprehension check of the experiment. See Section A.8 for more details on Experiment 2.

Results. Our results suggest that queue discipline may impact subject satisfaction despite subjects not knowing the server assignment of others. In particular, subjects were negatively impacted when they perceived cuts in line (i.e., others entering the open waiting area

Table 8: Number of subjects per condition in the perceived fairness experiment

	Subjects (170)
FIFO	63
Advance in Line	43
View Cuts in Line	64

after them and departing into service before them). Interestingly, subjects did not have a significant boost in satisfaction when they experienced jumping the line in front of others. See Table 9 for the results.

Table 9: Results for the perceived fairness experiment

DV: *Satisfaction* (1-Very Poor to 5-Very Good)	OLS
Advance in Line (vs. FIFO)	-0.170
	(0.184)
View Cuts in Line (vs. FIFO)	-1.041***
	(0.146)
Constant	3.635***
	(0.107)
Observations	170
R^2	0.236

*p<0.1, **p<0.05, ***p<0.01
Robust standard errors reported in parentheses

Discussion. Unlike our observational study, we find evidence that jumping the line did not significantly impact satisfaction. One possible explanation is that subjects may have positive utility associated with fair outcomes in queues (Ulku et al. (2019)), so the increase in utility from jumping the line could be offset by negative emotions stemming from a lack of fairness in the system. However, it is clear from our observational and experimental studies that people perceive and are impacted by ”cuts” in line even when the server assignment of others is unknown.

We recommend that managers take advantage of the levels of customer transparency that different buffer locations offer to prevent customer dissatisfaction due to queue discipline issues. One solution is to have customers with longer expected wait times stay in locations of the queue that are less transparent since these customers are not only likely to be overtaken in line but also are less likely to overtake others in line. (However, this recommendation would not work well in our clinical setting given that we show satisfaction declines faster in the less transparent second buffer.) Another solution comes from the Department of Radiology in the hospital system we study. Patients are given buzzers which allow them to roam around a large reception area before they check in with registration staff. The buzzer system makes the queue less transparent. Customers are free to roam within a vicinity of the reception area and no longer need to commingle with other customers in line and are thus less likely to observe other customer movements. In addition, customers do not need to wait near the service entrance where customer names are likely to be called for service and customer movements into service are more likely to be observed. As a result of the buzzer system, FIFO adherence becomes less salient, and customers may be less likely to monitor deviations from FIFO.

3.7.3. Scheduled Wait Times (Experiment 3)

Methods. Seven hundred and ninety five subjects were recruited to take a survey which they believed a fictitious server was individually administering for each subject. As a result, subjects believed that they needed to wait for an unknown amount of time for a server. Each subject was randomly assigned to wait 100 or 160 seconds (*100 Seconds/160 Seconds*) for the fictitious server and was given one estimated wait time corresponding to the 100 or 160 second waits respectively:

- 50 or 110 seconds (*Late Finish*)
- 100 or 160 seconds (*On-time Finish*)
- 150 or 210 seconds (*Early Finish*)

Each subject waited in a virtual waiting area by themselves where they could see a timer showing how long they had been waiting and the assigned estimated wait time. In addition, some subjects were randomly assigned to have a bar shown in the virtual waiting area showing progress towards the estimated wait time to increase the salience of the actual wait time versus the estimated wait time (*Bar*/*No Bar*). In total, there were 12 conditions with the number of subjects in each shown in Table 10. To ensure subjects paid attention to the conclusion of the actual wait time relative to the estimated wait time, subjects were paid a bonus for identifying an image that would flash in the virtual area for a few seconds immediately following the end of the subject's actual wait time. (See Figure 12 for a visualization of the open waiting area that subjects experienced.) After the wait was complete, we asked subjects to rate the waiting experience for their server from very poor (coded as 1) to very good (coded as 5). In our analysis, we only included subjects who made a correct identification of the image and who passed a comprehension check of the experiment. See Section A.8 for more details on Experiment 2.

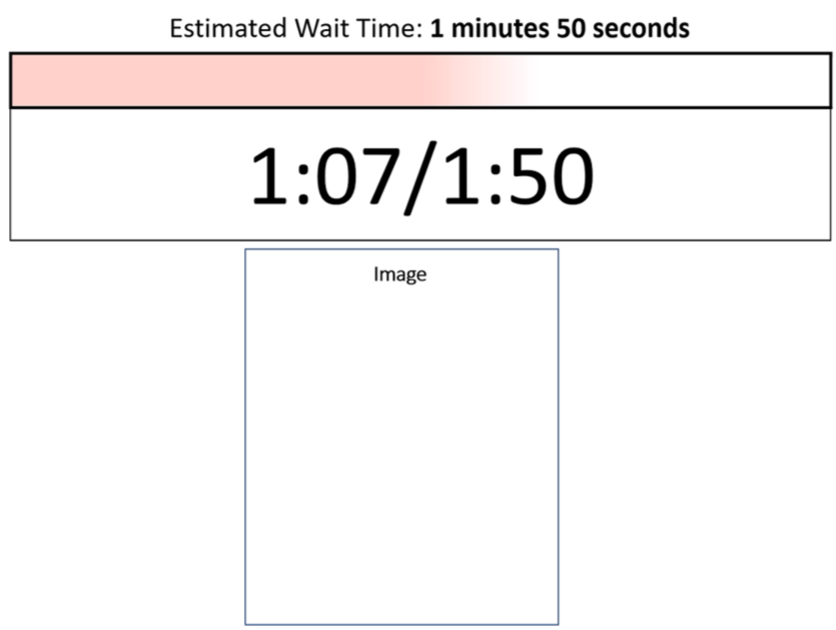


Figure 12: Snapshot of the virtual waiting area subjects experienced in Experiment 3. For experiments with the progress bar, it is shown here in pink. The image to check incentive compatibility of subjects flashed at the end of the wait in the designated box outlined in blue.

Results. Our results are shown in Table 11. First, our results show that subjects are less satisfied waiting for 160 seconds versus 100 seconds as one would expect longer waits to be more painful than shorter waits. Second, we found that the progress bar did not significantly impact satisfaction although we acknowledge that the coefficient could lack significance due

Table 10: Number of subjects per condition in the schedule time experiment

	100 Seconds		160 Seconds	
Subjects (795)	No Bar	Bar	No Bar	Bar
On-time Finish	59	64	64	70
Early Finish	69	67	71	63
Late Finish	70	64	68	66

to sample size limitations (see Section A.7 for a power analysis we conducted). Finally, we observe that subjects had a significant decline in satisfaction for late finishes versus on-time finishes. Early finishes (versus on-time finishes) were not significantly different with respect to satisfaction.

Discussion. We were able to find support for Hypothesis 4. Subject satisfaction was negatively impacted by wait times that concluded after an estimated wait time (versus an on-time finish). In addition, early finishes did not improve satisfaction relative to on-time finishes. Thus, our results provide evidence that prospect theory may be applicable to how customers in queues with scheduled appointment times experience exceeding and not exceeding their budgeted wait times.

We find in our observational study that providing customers with more information about their waits (such as anticipated delays) can help to improve customer satisfaction. Information on anticipated delays can help to increase the amount of time that customers budget for their wait, thus helping to reduce the impact of a perceived loss of time. In addition, service providers could consider building in reasonable buffers into the expected wait time communicated to customers. For example, suppose the hospital that we study estimates that one hour is the average amount of time needed to get a patient from reception to the operating room with a standard deviation of 7.5 minutes (approximately Normally distributed). The hospital could consider asking patients to arrive one hour and fifteen minutes prior to the scheduled procedure time and only have about five percent of patients receive the procedure after the scheduled procedure time (versus 50 percent if the hospital asks patients to arrive one hour prior to the scheduled procedure time). Of course, this strategy would come at a

Table 11: Results for the schedule time experiment

DV: *Satisfaction* (1-Very Poor to 5-Very Good)	OLS
Early Finish (vs. On-time Finish)	-0.069
	(0.143)
Late Finish (vs. On-time Finish)	-0.553***
	(0.149)
160 Seconds (vs. 100 Seconds)	-0.476***
	(0.146)
Bar (vs. No Bar)	-0.142
	(0.155)
Early Finish x 160 Seconds	0.246
	(0.211)
Late Finish x 160 Seconds	0.187
	(0.203)
Early Finish x Bar	0.209
	(0.208)
Late Finish x Bar	-0.208
	(0.228)
160 Seconds x Bar	0.304
	(0.219)
Early Finish x 160 Seconds x Bar	-0.329
	(0.300)
Late Finish x 160 Seconds x Bar	0.360
	(0.314)
Constant	3.814***
	(0.100)
Observations	795
R^2	0.101

*p<0.1, **p<0.05, ***p<0.01
Robust standard errors reported in parentheses

cost of having patients arrive earlier to the hospital, but the overall impact on satisfaction could increase through greatly reducing the number of patients who experience perceived losses of time.

3.8. Conclusion

In our study, we connect customer satisfaction data with timestamps of the customer's movement through a service process with two buffer locations. In order to identify whether customers experience faster declines in one of the buffer locations in our observational data, we use a 2SLS approach to overcome endogeneity with the wait time in the second buffer location (pre-op). Our novel instrument, $PATIENTSAHEAD$, relies on appointment scheduling of patients as an exogenous shock to wait times in the second buffer thus addressing bias in our regression estimation. We provide empirical evidence that customers care about buffer location: we show that customers in the second buffer (pre-op) experience faster declines in satisfaction. In an experiment, we infer that patients may be less happy in the second buffer due to the clinical and/or emotional intensity patients may experience in that buffer.

We also find evidence in our observational study that queue discipline affects customer satisfaction in shared, transparent waiting areas where the server assignment of other customers is unobserved. Our experimental data suggests that customers are unhappy when they observe other customers entering a waiting area after them but entering service before them, despite not know who will serve these other customers.

Finally, our results suggest that customers may be negatively impacted by waits that conclude after a scheduled appointment time. However, customers may not have an equivalent increase in satisfaction when their actual waits conclude before a scheduled appointment time.

Our study demonstrates that when businesses manage queues in which customers wait in multiple locations, managers should first consider evaluating the relative disutility of waiting in each location. Resources should then be focused on areas of the queue with higher disutility. For example, music and television are classic examples of wait time fillers that can help to mitigate wait disutility, and newer technology such as iPads could also achieve the desired effect.

In addition, our results suggest that managers should pay attention to queues where customers wait in transparent, open areas with others. In these situations, customers are likely to be disappointed if they perceive others cutting in line even if they are not necessarily waiting for the same server. To decrease transparency in these queues, businesses can consider investing in technology such as customer paging apps which work with customer phones and do not restrict customers to staying in the transparent waiting area.

Finally, our study suggests that managers should try to mitigate the negative impact of delays from scheduled appointment times on customer satisfaction. In particular, initiatives that aim to prevent customers from experiencing a delay from schedule may be worthy of consideration (e.g., initiatives that create a larger buffer time between a customer's arrival and their scheduled appointment time). In addition, managers may consider interventions for customers who experience delays from schedule while in queue such as providing them with waiting information.

APPENDIX

A.1. Serving Democracy: Supporting Information Text

A.1.1. Data Sources

logVotersPerPW

Florida Division of Elections provides information on the number of active registered voters by county for each election (see `https://dos.myflorida.com/elections/data-statistics/voter-registration-statistics/bookclosing/`). Election Administration Voting Survey provides county-level information on the number of poll workers by county from question D3 or equivalent (see `https://www.eac.gov/research-and-data/election-administration-voting-survey/`).

PctWhite/PctDemocrat/PctNoParty

Florida Division of Elections provides information on the number of active registered voters by county for each election (see `https://dos.myflorida.com/elections/data-statistics/voter-registration-statistics/bookclosing/`). Florida Division of Elections provides information on the racial and political party composition of active registered voters by county for each election (see `https://dos.myflorida.com/elections/data-statistics/voter-registration-statistics/bookclosing/`).

logAbsentBallotsPerPP

Election Administration Voting Survey provides county-level information on the number of Uniformed and Overseas Citizens Absentee Voters Act (UOCAVA) voters who voted absentee, Federal Write-in Absentee Ballots (FWAB), and domestic civilian absentee voters from question F1 or equivalent (see `https://www.eac.gov/research-and-data/election-administration-voting-survey/`). Election Administration Voting Survey provides county-level information on the number of polling places in an election from question D2 or equivalent (see `https://www.eac.gov/research-and-data/election-administration-voting-survey/`).

logEarlyBallotsPerPP

Election Administration Voting Survey provides county-level information on the number of ballots cast at an early vote center from question F1 or equivalent (see `https://www.eac.gov/research-and-data/election-administration-voting-survey/`). Election Administration Voting Survey provides county-level information on the number of polling places in an election from question D2 or equivalent (see `https://www.eac.gov/research-and-data/election-administration-voting-survey/`).

logEDBallotsPerPP

Election Administration Voting Survey provides county-level information on the number of ballots cast on Election Day from question F1 or equivalent (see `https://www.eac.gov/research-and-data/election-administration-voting-survey/`). Election Administration Voting Survey provides county-level information on the number of polling places in an election from question D2 or equivalent (see `https://www.eac.gov/research-and-data/election-administration-voting-survey/`).

logProvBallotsPerPP

Election Administration Voting Survey provides county-level information on the number of provisional ballots cast in an election from question E1 or equivalent (see `https://www.eac.gov/research-and-data/election-administration-voting-survey/`). Election Administration Voting Survey provides county-level information on the number of polling places in an election from question D2 or equivalent (see `https://www.eac.gov/research-and-data/election-administration-voting-survey/`).

PollDiff

Election Administration Voting Survey provides county-level information on poll worker recruitment difficulty in an election from question D5 or equivalent (see `https://www.eac.gov/research-and-data/election-administration-voting-survey/`).

HousePrice

Federal Reserve Bank of St. Louis collects county-level house price data through its "All-Transactions House Price Index" in the FRED database (see `https://fred.stlouisfed.`

org/). The All-Transactions House Price Index was only available for 56 out of 67 counties. For the 11 counties missing data, we imputed the index based on the average of the housing indexes from adjacent counties.

logMedInc

U.S. Census Bureau collects county-level information on income in the past 12 months and publishes its 5-year estimates in report ID S1901 (see `https://www.census.gov/programs-surveys/acs/`). The U.S. Census only reports median income for all counties in Florida from 2009 and onward. We use the 2009 median incomes as a proxy for 2008.

logPeoplePerSqMile

Florida Office of Economic and Demographic Research publishes county-level information on population estimates used for calculating revenue sharing (see `http://www.edr.state.fl.us/Content/population-demographics/data/index-floridaproducts.cfm`). U.S. Census Bureau published information on the total square mileage in a county in 2010 (see `https://www.census.gov/quickfacts/fact/note/US/LND110210`).

UseDRE

Verified Voting Foundation publishes polling place equipment information by county (see `https://www.verifiedvotingfoundation.org/about-vvf/`). See below for how we impute missing data for the 2010 election.

Pct65Plus

U.S. Census Bureau collects county-level information on age and publishes its 5-year estimates in report ID S0101 (see `https://www.census.gov/programs-surveys/acs/`)

A.1.2. Multiple Imputation Methodology

Predicting PollDiff

We use the following as predictors of *PollDiff* in counties: total active registered voters, male-to-female ratio, percentage of the population with a Bachelor's degree or higher, median age, median income, percentage Democrats, urban sprawl (people per square mile), political activeness (Election Day turnout percent) (Kimball et al. (2009); Burden and Milyo (2015)). We also use past values of *PollDiff* within a county as a predictor. Gender and age data is from the U.S. Census Bureau report ID S0101 (see `https://www.census.gov/programs-surveys/acs/`). Educational attainment data is from the U.S. Census Bureau report ID S1501 (see `https://www.census.gov/programs-surveys/acs/`). Election Day turnout data is calculated from the number of ballots cast on Election Day divided by the total number of active registered voters.

Predicting UseDRE

If a county used (or did not use) DREs during both the 2008 and 2012 elections, then we assume they would have used (or not used) DREs during the 2010 election. There are 56 counties for which we infer their 2010 usage in that manner. For the remaining 11 counties, we perform multiple imputation in which we randomly assigned each county to either use DREs or not use DREs across 10 imputations.

Imputing Results

We impute the regression coefficients and standard errors per Rubin (2004) with adjusted degrees of freedom suggested in Barnard and Rubin (1999) and 10 imputations based on guidance from White et al. (2011).

A.1.3. Famighetti et al. (2014) Poll Worker Result Calculation with Percentage of White Voters

Famighetti et al. (2014) do not explicitly examine the relationship between the percentage of white registered voters and the number of Election Day eligible voters per poll worker in their study (i.e., they look at black, Latino, and other minority registered voters). However, using their data and regression methodology, we were able to examine this relationship by regressing the number of Election Day eligible voters per poll worker on the percentage of white registered voters in each precinct and included county fixed effects. To convert this regression estimate into a percentage (0.26%, as reported in our results), we divided the regression estimate by the mean number of Election Day eligible voters per poll worker in the data.

A.1.4. Robustness Check Descriptions

TimeFE

We replace the variable *Presidential* and the linear time trend with time fixed effects and include exogenous dummy variables for election years 2010 through 2016 (*Election2010*, *Election2012*, *Election2014*, *Election2016*).

NoLag

We remove both lags of the dependent variable.

PctNoParty

We include the control *PctNoParty*, the percentage of active registered voters who had no political party affiliation.

Shelby

We include an exogenous variable, *ShelbyCounties* which equals 1 in the 2014 and 2016 elections and zero otherwise for those counties that were effected by the 2013 Supreme Court decision *Shelby County* v. *Holder*.

AltProxy

Instead of the contemporaneous forecast turnout proxies, we use the second lag of each variable (to match the previous election of the same type, midterm or presidential) and we eliminate the first lag of *VotersPerPW* because it is not significant in our main results.

ParsInstr

We examine a more parsimonious instrument model in which for all endogenous regressors, we use only the most recent election lag or lags as instruments that correct the first difference for endogeneity and pass our 2SLS F-test.

A.1.5. Average Waiting Time Simulation

We use three different methods to measure the impact of a change in the percentage of Democrats on the average wait time. The first adjusts the average number of check-in stations in response to changes in *PctDemocrat*, leaving all other parameters constant. Let Q be the initial number of check-in stations. Assuming that the average change in check-in stations per polling place is proportional to the average change in poll workers per polling place, the average number of check-in stations after the change is

$$Q/e^{\hat{\beta}_1 P}$$

where $\hat{\beta}_1$ is our estimate for the change in voters per poll worker ($\hat{\beta}_1 = 0.028$), and P is the percentage change in Democrats in a county. To overcome the integer constraint on the number of voting resources (e.g., it is not possible to reduce the number of check-in stations across all polling locations by 0.1, but it is possible to reduce the number of check-in-stations in 10% of polling locations), we presume that the fraction ρ of polling places has $Q - \delta$ check-in stations and the remaining fraction, $1 - \rho$, has $Q - \delta + 1$ stations, where

$$\delta = Q - \left\lfloor \frac{Q}{e^{\hat{\beta}_1 P}} \right\rfloor$$

and

$$\rho = Q\left(1 - \frac{1}{e^{\hat{\beta}_1 P}}\right) + 1 - \delta$$

For most changes $\delta = 1$, but it is possible that a larger change in voting resources is needed. With our first measure we let W_1 be the new average waiting time due to a P percent change in Democrats

$$W_1 = \rho\Omega(675, 2 - \delta, 4) + (1 - \rho)\,\Omega(675, 2 - \delta + 1, 4)$$

where $\Omega(v, c, s)$ is the simulated average wait time for a polling place with v voters, c check-in stations, and s voting stations.

The second method is analogous to the first except now the number of voting stations is reduced:

$$W_2 = \rho\Omega(675, 2, 4 - \delta) + (1 - \rho)\,\Omega(675, 2, 4 - \delta + 1)$$

The third method increases the number of voters per polling place holding voting resources constant:

$$W_3 = \Omega\left(675e^{\hat{\beta}_1 P}, 2, 4\right)$$

A.2. Serving Democracy: Additional Figures

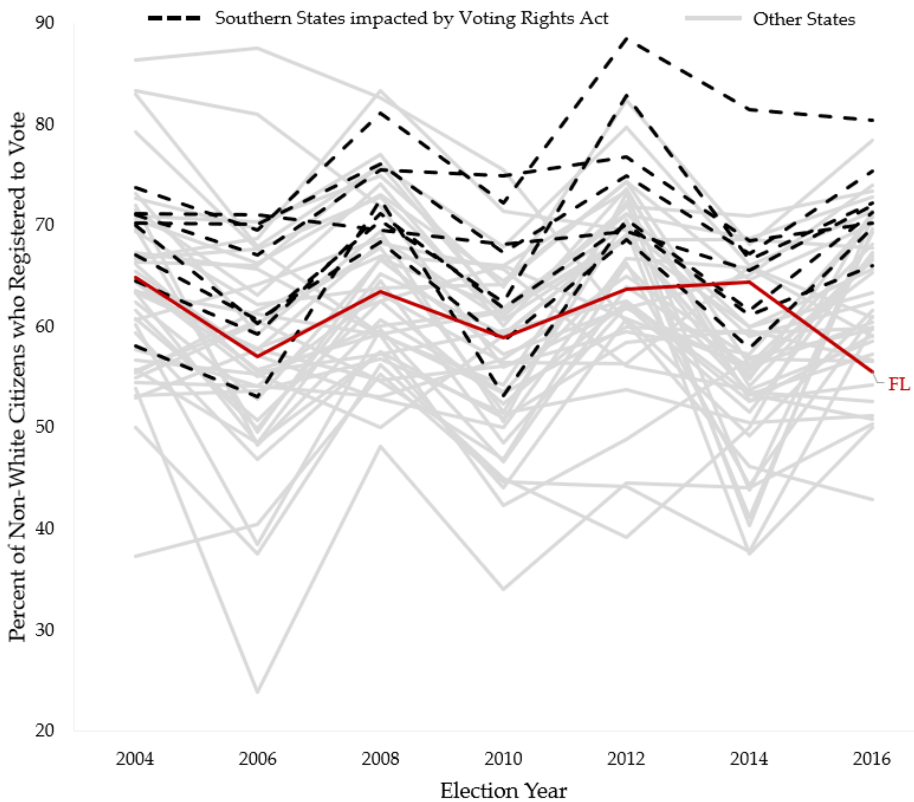


Figure 13: Percent of non-white citizens who registered to vote by state. Data taken from U.S. Census Bureau reports on the voting and registration for states by race (see `https://www.census.gov/topics/public-sector/voting/data/tables.html`).

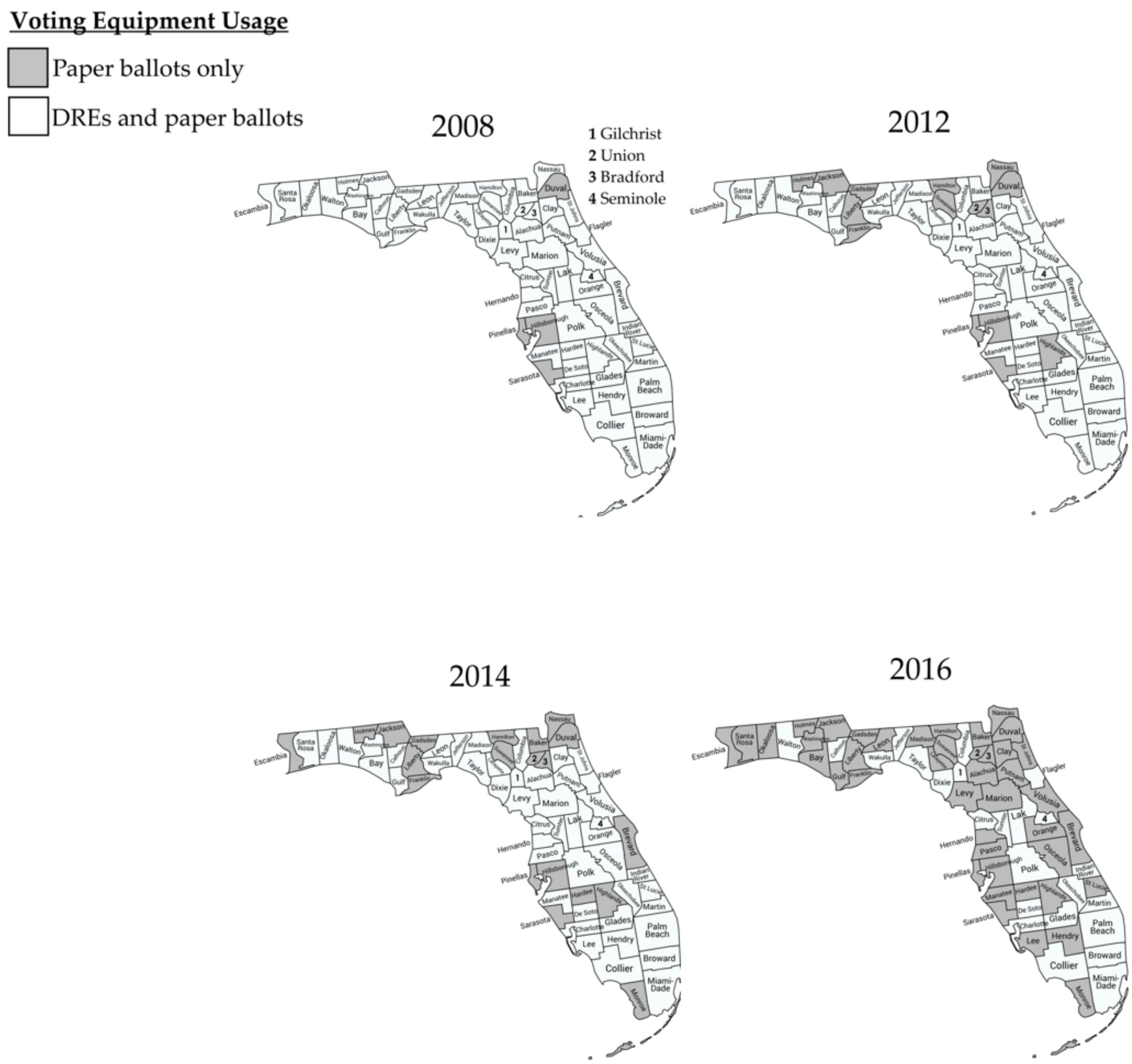


Figure 14: Change in Florida's voting equipment usage over time courtesy of the Verified Voting Foundation (figure created courtesy of `https://mapchart.net/`)

A.3. Serving Democracy: Additional Tables

Table 12: Summary statistics for key variables across Florida's 67 counties and the 2008 to 2016 general elections

Statistic	Voters Per PW	Pct Dem ocrat	Pct White	Absent Ballots PerPP	Early Ballots PerPP	ED Ballots PerPP	Prov Ballots PerPP	Poll Diff*	Person PerSq Mile	House Price	Med Inc	Use DRE*	Pct 65Plus
Mean	235.64	44.46	78.51	320.24	406.69	540.69	3.64	3.41	346.18	142.23	44243	0.72	18.51
Std. Dev.	93.11	14.60	14.09	229.48	295.14	223.86	4.02	1.05	531.99	23.53	7648	0.45	6.84
Minimum	79.70	19.42	18.37	30.88	16.00	142.69	0.00	1	9.76	97.55	29642	0	7.90
Maximum	581.74	88.38	95.80	1251.83	1591.49	1162.39	32.09	5	3486.37	214.61	69523	1	53.10
25th%ile	168.58	33.20	72.42	148.80	187.94	342.29	1.00	3	46.12	124.44	37778	0	13.80
50th%ile	220.01	42.28	82.84	257.12	315.44	543.90	2.22	3	166.57	137.22	43787	1	17.10
75th%ile	280.40	54.22	87.29	428.34	549.50	707.63	5.24	4	413.12	159.29	49135	1	21.70

*Calculated based on one of the ten imputations for the variable

Table 13: Main regression specification checks

Model Specification Checks (Note: $\triangle\hat{\varepsilon_{i,t}} = \triangle\hat{\varepsilon_{i,t}} - \triangle\varepsilon_{i,\hat{t-1}}$)	1-Step	2-Step
Number of instruments:	59	
H_0: No correlation between $\triangle\hat{\varepsilon_{i,t}}$ and $\triangle\varepsilon_{i,\hat{t-1}}$ | Max p-value among imputations:	0.008	0.014
H_0: No correlation between $\triangle\hat{\varepsilon_{i,t}}$ and $\triangle\varepsilon_{i,\hat{t-2}}$ | Min p-value among imputations:	0.876	0.813
H_0: No correlation between $\triangle\hat{\varepsilon_{i,t}}$ and $\triangle\varepsilon_{i,\hat{t-3}}$ | Min p-value among imputations:	0.728	0.766
Hansen test of overidentifying restrictions | Min p-value among imputations:	0.143	

Table 14: F-statistics resulting from the first-stage regression of the first differences of the covariate at time t (pooled across county and election years) on the lagged instrument candidate(s) for all endogenous covariates.

General Election Years: 2008 to 2016

First difference of covariate at time t	Lags used as Instruments	F-Statistic
PctDemocrat	t-2	59.92
PctWhite	t-2	52.00
logVotersPerPW$_{t-1}$	t-2/t-3	41.72
logVotersPerPW$_{t-2}$	t-2/t-3	33.77*
logAbsentBallotsPerPP	t-2/t-3	371.49
logEarlyBallotsPerPP	t-2/t-3	538.32
logEDBallotsPerPP	t-2/t-3	23.15
logProvBallotsPerPP	t-2/t-3	59.80
PollDiff† minimum among all imputations	t-1	73.53

*The first difference of logVotersPerPW$_{t-2}$ (i.e., logVotersPerPW$_{t-2}$ – logVotersPerPW$_{t-3}$) is fully identified by the two lags used as instruments so we report the minimum F-statistic resulting from the first-stage, pooled county regression of the first difference of the second lag on each individual lagged instrument candidate.

†We conducted the F-test for all 10 imputed cases for PollDiff and report the minimum F-statistic.

Table 15: Main regression results
General Election Years: 2008 to 2016 | Counties: 67

DV: logVotersPerPW	FE†	1-Step†	2-Step‡
PctDemocrat	0.017***	0.024***	0.028**
	(0.004)	(0.009)	(0.012)
PctWhite	-0.014	0.004	0.018
	(0.012)	(0.024)	(0.036)
logVotersPerPW$_{t-1}$	-0.033	-0.006	0.016
	(0.061)	(0.055)	(0.071)
logVotersPerPW$_{t-2}$	0.094	0.128*	0.150
	(0.058)	(0.073)	(0.108)
logAbsentBallotsPerPP	0.174***	0.205**	0.181**
	(0.061)	(0.089)	(0.086)
logEarlyBallotsPerPP	0.047	0.056	0.020
	(0.058)	(0.094)	(0.105)
logEDBallotsPerPP	0.238***	0.368***	0.385***
	(0.088)	(0.123)	(0.143)
logProvBallotstPerPP	-0.013	-0.068**	-0.071***
	(0.017)	(0.028)	(0.023)
PollDiff	-0.002	-0.023	-0.011
	(0.012)	(0.020)	(0.023)
logPersonPerSqMile	-0.275	-0.454	-0.419
	(0.381)	(0.425)	(0.554)
HousePrice	-0.001*	-0.001*	-0.001*
	(0.001)	(0.001)	(0.001)
logMedInc	-0.188	-0.042	-0.015
	(0.269)	(0.304)	(0.360)
UseDRE	0.006	-0.028	-0.028
	(0.035)	(0.042)	(0.042)
Pct65Plus	-0.004	-0.004	-0.007
	(0.006)	(0.007)	(0.009)
Presidential	-0.170***	-0.136**	-0.077
	(0.044)	(0.061)	(0.066)
Time trend	0.089***	0.107**	0.133**
	(0.022)	(0.042)	(0.063)
Constant	5.997*		
	(3.280)		

*p<0.1, **p<0.05, ***p<0.01

†Robust standard errors reported in parentheses

‡Windmeijer (2005) corrected standard errors reported in parentheses

Table 16: Robustness check results

General Election Years: 2008 to 2016 | Counties: 67

DV: logVotersPerPW	(1)TimeFE‡	(2)NoLag‡	(3)PctNoParty‡	(4)Shelby‡	(5)AltProxy‡	(6)ParsInstr‡
PctDemocrat	0.029**	0.030**	0.027**	0.029***	0.038***	0.035***
	(0.012)	(0.012)	(0.011)	(0.011)	(0.010)	(0.012)
PctWhite	0.024	0.021	0.017	0.023	0.013	0.032
	(0.035)	(0.035)	(0.027)	(0.035)	(0.028)	(0.036)
PctNoParty			0.035			
			(0.050)			
ShelbyCounties				0.098		
				(0.090)		
logVotersPerPW$_{t-2}$					0.098	
					(0.063)	
logAbsentBallotsPerPP$_{t-2}$					-0.002	
					(0.053)	
logEarlyBallotsPerPP$_{t-2}$					-0.029	
					(0.042)	
logEDBallotsPerPP$_{t-2}$					-0.005	
					(0.089)	
logProvBallotsPerPP$_{t-2}$					-0.024	
					(0.016)	
Election2010	0.244**					
	(0.108)					
Election2012	0.202					
	(0.141)					
Election2014	0.446***					
	(0.167)					
Election2016	0.553**					
	(0.232)					
logVotersPerPW lags	t-1/t-2	No	t-1/t-2	t-1/t-2	t-2	t-1/t-2
Presidential + Time trend	No	Yes	Yes	Yes	Yes	Yes
Other standard covariates	Yes	Yes	Yes	Yes	Yes	Yes

*p<0.1, **p<0.05, ***p<0.01

‡ Windmeijer (2005) corrected standard errors reported in parentheses

Table 17: Robustness specification checks

Model Specification Checks (Note: $\triangle\hat{\varepsilon_{i,t}} = \triangle\hat{\varepsilon_{i,t}} - \triangle\varepsilon_{i,\hat{t-1}}$)	TimeFE	NoLag	PctNoParty	Shelby	AltProxy	ParsInstr
Number of instruments:	61	51	67	60	59	43
H_0: No correlation between $\triangle\hat{\varepsilon_{i,t}}$ and $\triangle\varepsilon_{i,\hat{t-1}}$ | Max p-value among imputations:	0.011	0.006	0.017	0.015	0.013	0.042
H_0: No correlation between $\triangle\hat{\varepsilon_{i,t}}$ and $\triangle\varepsilon_{i,\hat{t-2}}$ | Min p-value among imputations:	0.565	0.5	0.656	0.805	0.544	0.762
H_0: No correlation between $\triangle\hat{\varepsilon_{i,t}}$ and $\triangle\varepsilon_{i,\hat{t-3}}$ | Min p-value among imputations:	0.873	0.818	0.734	0.808	0.22	0.646
Hansen test of overidentifying restrictions | Min p-value among imputations:	0.207	0.254	0.186	0.157	0.159	0.178

Table 18: F-statistics resulting from the first-stage regression of the first differences of the covariate at time t (pooled across county and election years) on the lagged instrument candidate(s) for the PctNoParty covariate and for the endogenous covariates with new instruments in the ParsInstr robustness check

General Election Years: 2008 to 2016

First difference of covariate at time t	Lags used as Instruments	F-Statistic
PctNoParty Robustness Check		
PctNoParty	t-2/t-3	13.22
ParsInstr Robustness Check		
logVotersPerPW$_{t-1}$	t-2	18.81
logVotersPerPW$_{t-2}$	t-2	96.97
logAbsentBallotsPerPP	t-2	72.57
logEarlyBallotsPerPP	t-2	167.26
logProvBallotsPerPP	t-2	19.38

Table 19: Base simulation inputs

Simulation parameter	Selection/Value
Polling hours (from Florida's Division of Elections website)	7am to 7pm
Arrival pattern	composite
Voting technology	paper ballot + optical scanner
Time to check in (minutes) Stewart III (2015)	2
Time to complete a ballot (minutes) Stewart III (2015)	5
Time to scan ballot (minutes) Stewart III (2015)	0.5
% voters unable to check-in (observed % of provisional ballots)	1%
% of voters arriving before polls open (default for simultation)	1.20%
Same day voter registration	no
Expected number of voters	675
Number of check-in stations	2
Number of voting stations	4

A.4. Democracy on the Line: Supporting Information Text

A.4.1. Data Sources

logWait

Cooperative Congressional Election Study (CCES) publishes its survey results in each election (see `https://cces.gov.harvard.edu/`). Note, there was one respondent in the 2008 CCES survey who reported a wait time greater than an hour but then wrote "got early in the morning at 6am" when specifying his wait time. We excluded this observation from the analysis. Survey of the Performance of the Performance of American Elections (SPAE) publishes its survey results in each election (see `https://dataverse.harvard.edu/dataverse/SPAE`). One respondent in the 2008 SPAE survey reported a wait time greater than an hour but wrote "I got to there an hour before they opened but once the polls opened I waited und" when specifying his wait time. We excluded this observation from the analysis.

PctWhite

Georgia's Secretary of State publishes information by county on the number of white registered voters and the number of total registered voters in each election (see `http://sos.ga.gov/index.php/Elections/voter_turn_out_by_demographics`). South Carolina Election Commission provided voter registration tallies by race as of the following dates: January 1, 2006; October 25, 2008; October 26, 2012; and October 28, 2016.

PctDem

Georgia's Secretary of State publishes information on current and past election results (see `http://sos.ga.gov/index.php/Elections/current_and_past_elections_results`). South Carolina Election Commission publishes information on current and past election results (see `https://www.scvotes.org/election-returns-primaries-and-general-elections-statewide`).

RegVoters

Election Administration Voting Survey (EAVS) provides county-level information on the number of active registered voters from question A3 or equivalent (see `https://www.eac.`

gov/research-and-data/election-administration-voting-survey/).

Turnout, Voters

Georgia's Secretary of State publishes information by county on the number of registered voters and the number who voted in each election (see http://sos.ga.gov/index.php/Elections/voter_turn_out_by_demographics). South Carolina Election Commission publishes information by county on the number of registered voters and the number who voted in each election (see https://www.scvotes.org/data/voter-history.html).

EDTurnout

EAVS provides county-level information on the number of ballots cast on Election Day from question F1 or equivalent (see https://www.eac.gov/research-and-data/election-administration-voting-survey/). (Note, for Georgia, the 2016 EAVS survey data for section F listed McDuffie County and McIntosh County twice and did not list Meriwether County or Miller County. However, the responses to the questions in the survey for the second instances of McDuffie and McIntosh counties are different from the first instances. Based on data from other years in the survey, we determined that the second instance of McDuffie County was Meriwether County and the second instance of McIntosh County was Miller County.) EAVS also provides county-level information on the number of active registered voters from question A3 or equivalent (see https://www.eac.gov/research-and-data/election-administration-voting-survey/).

logPplPerSqMile

U.S. Census Bureau publishes information by county on the intercensal estimates of the resident population (see https://www.census.gov/data/tables/time-series/demo/popest/intercensal-2000-2010-counties.html for 2000 to 2010 estimates and report ID PEPANNRES at https://www.census.gov/programs-surveys/acs/ for 2010 to 2016 estimates). U.S. Census Bureau also publishes information on the total square mileage in a county in 2010 (see https://www.census.gov/quickfacts/fact/note/US/LND110210).

logMedInc

U.S. Census Bureau collects county-level information on median household income in its Small Area Income and Poverty Estimates (see `https://www.census.gov/programs-surveys/saipe.html`).

Pct65Plus

U.S. Census Bureau collects county-level information on age and publishes its estimates in report ID S0101 (see `https://www.census.gov/programs-surveys/acs/`). For all counties which an estimate was not available in 2006, the 2008 estimate was used.

PollPlaces

EAVS provides county-level information on the number of polling places in an election from question D2 or equivalent (see `https://www.eac.gov/research-and-data/election-administration-voting-survey/`).

White, Male, Age, Dem, Ind, HighInc, College

CCES provides demographic information on survey respondents (see `https://cces.gov.harvard.edu/`).

PctDriveAlone

U.S. Census Bureau collects county-level information on the percent of the population that drives to work alone and publishes its estimates in report ID S0801 (see `https://www.census.gov/programs-surveys/acs/`). Data was not available in 2008, so 2009 data was used as a proxy.

A.4.2. County-Level Analysis of Predictors of Polling Place Closures

To analyze the predictors of polling place closures in Georgia, we first defined a closure as occurring in a county if the number of polling places declined in the 2016 presidential election relative to the 2008 presidential election according to the EAVS data. We created a dependent variable, *Closure*, which equaled 1 if county i experienced closures in the 2016 election (relative to 2008) and 0 otherwise. See Fig.15 for all counties that experienced closures according to our definition.

We then ran a cross-sectional logistic regression (see Eq.A.1) using predictors of closures in 2016 (relative to 2008) specified in Table 26 based on data election managers could reasonably observe prior to the 2016 election. Note that we introduce a new variable, *PctDriveAlone*, as a predictor which is the percentage of the population which drives to work alone. We believe that public transportation accessibility could play a role in whether counties close polling places. The results of our analysis can be seen in Table 27.

$$\begin{aligned} Closure_i = &\gamma_0 + \gamma_1 logMedInc16_i + \gamma_2 Pct\Delta MedInc0816_i + \gamma_3 PctWhite12_i + \\ &\gamma_4 Pct\Delta PctWhite0812_i + \gamma_5 PctDem12_i + \gamma_6 Pct\Delta PctDem0812_i + \\ &\gamma_7 logRegVoters16_i + \gamma_8 Pct\Delta RegVoters0816_i + \gamma_9 Turnout12_i + \\ &\gamma_{10} Pct\Delta Turnout0812_i + \gamma_{11} EDTurnout12_i + \gamma_{12} Pct\Delta EDTurnout0812_i + \\ &\gamma_{13} logPplPerSqMile_i + \gamma_{14} Pct\Delta PplPerSqMile0816_i + \gamma_{15} Pct65Plus16_i + \\ &\gamma_{16} Pct\Delta Pct65Plus0816_i + \gamma_{17} PctDriveAlone16_i + \\ &\gamma_{18} Pct\Delta PctDriveAlone0916_i + \epsilon_i \end{aligned} \tag{A.1}$$

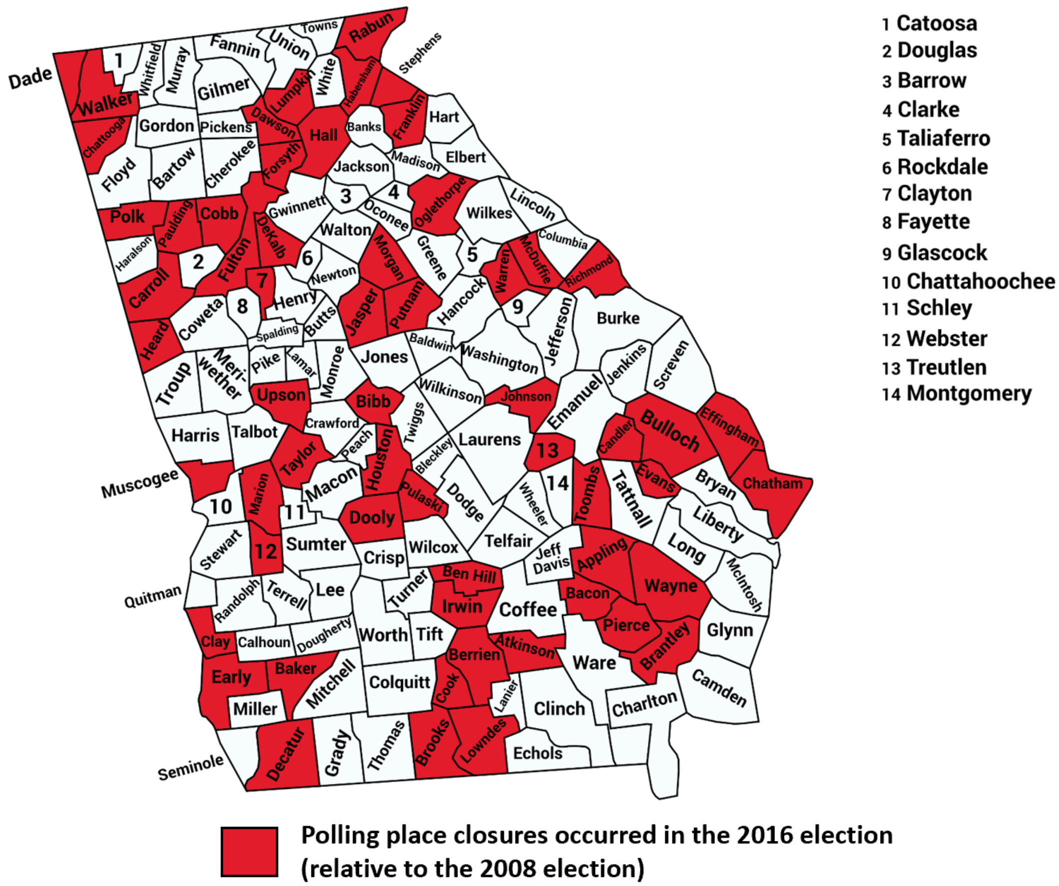


Figure 15: Where polling place closures occurred in Georgia counties in the 2016 election (relative to the 2008 election) (figure created courtesy of `https://mapchart.net/`)

A.5. Democracy on the Line: Additional Figures

A.6. Democracy on the Line: Additional Tables

Table 20: Total number of polling places in Georgia and South Carolina for the 2006 to 2016 elections as reported in the EAVS

Election Year	Polling Places GA	Polling Places SC
2006	3003	2044
2008	3064	2094
2010	2831	2191
2012	n.a.	2191
2014	3096	1929
2016	2720	2211

Table 21: Summary of the dependent variable and controls across elections 2006, 2008, 2012, and 2016

Statistic	Wait	PctWhite	PctDem	RegVoters	Turnout	EDTurnout	PplPerSqMi	Income	Pct65Plus
Mean	21.78	64.01	42.68	185610	68.25	40.51	798.05	51306	11.91
Std. Dev.	34.18	18.15	15.97	171402	10.44	10.87	830.67	11878	3.58
Minimum	1	12.69	12.21	1743	34.77	14.27	9.67	25633	2.60
Maximum	360	99.44	82.70	595979	85.48	68.90	2753.89	101804	33.10
25th%ile	5	48.41	31.76	49861	65.84	32.74	172.26	41543	9.10
50th%ile	5	68.23	39.37	112696	71.66	39.49	403.74	50427	11.50
75th%ile	20	77.90	53.16	316917	75.05	46.63	1694.83	58167	13.90

Table 22: Voting restrictions implemented in Georgia and South Carolina in the recent past according to Brennan Center for Justice (Weiser and Feldman (2018))

State	Voting Restrictions
GA	"No match, no vote" limit on access to voter registration (2017 law)
	Reduced early voting period (2010 law)
	Documentary proof of citizenship to register (2009 law)
	Strict voter ID requirement (2006 law)
SC	Voter ID requirement (2011 law, mitigated after lawsuit)

Table 23: Regression results

DV: logWait	Base	CCES/SPAE	Presidential	IndControls	VICEClosures	CountyFE
Treated	-0.280	-0.332	-0.195	-0.286	-0.179	0.038
	(0.221)	(0.214)	(0.218)	(0.218)	(0.245)	(0.373)
Election2006	-1.206***	-1.268***		-1.255***	-1.527***	-0.232
	(0.322)	(0.292)		(0.323)	(0.407)	(0.459)
Election2008	0.970***	1.006***	1.030***	0.956***	1.109***	0.983***
	(0.145)	(0.140)	(0.146)	(0.145)	(0.152)	(0.207)
Election2016	-0.423**	-0.403**	-0.434**	-0.452**	-0.534***	-0.738***
	(0.178)	(0.157)	(0.184)	(0.178)	(0.178)	(0.204)
Treated x Election2006	-0.039	-0.022		-0.023	-0.144	0.116
	(0.232)	(0.229)		(0.229)	(0.256)	(0.313)
Treated x Election2008	0.016	-0.154	-0.023	0.008	0.078	0.018
	(0.171)	(0.162)	(0.178)	(0.175)	(0.177)	(0.221)
Treated x Election2016	0.578***	0.552***	0.593***	0.588***	0.583**	0.862***
	(0.197)	(0.178)	(0.203)	(0.198)	(0.231)	(0.197)
County-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual-level controls	No	No	No	Yes	No	No
County fixed effects	No	No	No	No	No	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.145	0.149	0.164	0.150	0.152	0.210
Number of observations	5091	6235	4195	5091	4459	5091

*p<0.1, **p<0.05, ***p<0.01

Standard errors clustered by county in parentheses

Table 24: Number of logWait observations by state and election year for the CCES and SPAE surveys

State	Survey	2006	2008*	2010†	2012	2014‡	2016
Georgia	CCES	617	541	0	1093	n.a.	1201
	SPAE	n.a.	344	0	170	n.a.	163
South Carolina	CCES	279	279	0	559	n.a.	522
	SPAE	n.a.	156	0	160	n.a.	161

*In 2008, SPAE sampled an additional 200 registered voters in Georgia by phone
†CCES did not collect wait time observations in 2010 and SPAE was not conducted
‡2014 election was not applicable to our study

Table 25: Summary of the individual-level controls across elections 2006, 2008, 2012, and 2016

Statistic	White	Male	Age	Dem	Ind	HighInc	College
Mean	0.72	0.47	51.28	0.31	0.28	0.18	0.73
Std. Dev.	0.45	0.5	15.19	0.46	0.45	0.39	0.44
Minimum	0	0	18	0	0	0	0
Maximum	1	1	92	1	1	1	1

Table 26: Predictors of polling place closures between 2008 and 2016

Predictors	**Description**
logMedInc16	logMedInc in 2016
PctΔMedInc0816	Percentage change in non-logged MedInc between 2008 and 2016
PctWhite12	PctWhite in 2012
PctΔPctWhite0812	Percentage change in PctWhite between 2008 and 2012
PctDem12	Contemporaneous values of PctDem in 2012
PctΔPctDem0812	Percentage change in contemporaneous values of PctDem between 2008 and 2012
logRegVoters16	Log of RegVoters in 2016
PctΔRegVoters0816	Percentage change in RegVoters between 2008 and 2016
Turnout12	Turnout in 2012
PctΔTurnout0812	Percentage change in Turnout between 2008 and 2012
EDTurnout12	EDTurnout in 2012
PctΔEDTurnout0812	Percentage change in EDTurnout between 2008 and 2012
logPplPerSqMile16	logPplPerSqMile in 2016
PctΔPplPerSqMile0816	Percentage change in non-logged PplPerSqMile between 2008 and 2016
Pct65Plus16	Pct65Plus in 2016
PctΔPct65Plus0816	Percentage change in Pct65Plus between 2008 and 2016
PctDriveAlone16	PctDriveAlone in 2016
PctΔPctDriveAlone0916	Percentage change in PctDriveAlone between 2009 and 2016

Table 27: County-level analysis of predictors of polling place closures in the 2016 presidential election (relative to the 2008 presidential election)

DV: Closure	Estimates
logMedInc16	-4.307**
	(2.052)
PctΔMedInc0816	-0.006
	(0.032)
PctWhite12	0.063
	(0.057)
PctΔPctWhite0812	0.054
	(0.089)
PctDem12	0.041
	(0.057)
PctΔPctDem0812	0.002
	(0.046)
logRegVoters16	0.237
	(0.473)
PctΔRegVoters0816	0.007
	(0.019)
Turnout12	0.128*
	(0.073)
PctΔTurnout0812	-0.010
	(0.052)
EDTurnout12	-0.009
	(0.029)
PctΔEDTurnout0812	-0.022
	(0.015)
logPplPerSqMile16	0.416
	(0.490)
PctΔPplPerSqMile0816	-0.001
	(0.042)
Pct65Plus16	-0.121
	(0.080)
PctΔPct65Plus0816	0.010
	(0.018)
PctDriveAlone16	0.011
	(0.049)
PctΔPctDriveAlone0916	-0.021
	(0.026)
Constant	Yes
Pseudo R^2	0.083
Number of observations	159

*p<0.1, **p<0.05, ***p<0.01
Standard errors in parentheses

A.7. Drivers of Customer Satisfaction in a Tandem Queue: Power Analysis

A.7.1. Location of Wait vs. Wait Disutility

The coefficient on *Long 2nd Wait* was not shown to be significant in Table 7. In a power analysis, we set *Long 2nd Wait* as the treatment group and *Short 2nd Wait* as the control. We use the following parameters for a two-tailed test with zero covariates and 184 observations across the *Long 2nd Wait* and *Short 2nd Wait* conditions (55% assigned to treatment):

- Alpha level (α)=0.05
- Power (1-β)=0.80

We find a minimum detectable effect size (MDES) of 0.417 standard deviation units of the outcome. Using the observed standard deviation of the outcome across the two conditions (0.996), we estimate the MDES in satisfaction units to be 0.416. With the observed effect size on *Long 2nd Wait* being -0.290 (in satisfaction units), it is possible that the sample size was not sufficiently large to detect a significant effect in our study. We estimate needing roughly 1,250 subjects to detect an effect size of 0.290 satisfaction units.

A.7.2. Scheduled Wait Times

The coefficient on *Bar* was not shown to be significant in Table 11. In a power analysis, we set *Bar* as the treatment group and *No Bar* as the control. We use the following parameters for a two-tailed test with zero covariates and 795 observations across the *Bar* and *No Bar* conditions (50% assigned to treatment):

- Alpha level (α)=0.05
- Power (1-β)=0.80

We find a minimum detectable effect size (MDES) of 0.199 standard deviation units of the outcome. Using the observed standard deviation of the outcome across the two conditions (0.920), we estimate the MDES in satisfaction units to be 0.183. With the observed effect size on *Bar* being -0.142 (in satisfaction units), it is possible that the sample size was not sufficiently large to detect a significant effect in our study. We estimate needing roughly 2,500 subjects to detect an effect size of 0.142 satisfaction units.

A.8. Drivers of Customer Satisfaction in a Tandem Queue: Materials for the Experiments

A.8.1. Sounds Pre-Experiment Materials

Instruction Text Highlights

In this experiment, we are researching customer experiences in service settings. The experiment should take about 15 minutes of your time and you will be compensated $0.80 plus any bonuses you accrue (up to $1.20). An overview of the experiment and instructions will follow. The tasks will solicit information on your experience in the experiment and also demographic information on yourself (e.g., gender, race, age range, education level, income level, employment status, and state of residence). No other identifying information will be obtained. All information will be stored in a secure, password-protected data storage facility.

You may abandon the experiment at any time, however in order to receive any payment for your participation in the experiment, you must complete all tasks according to the directions provided. Therefore, please ensure you follow all directions in the experiment carefully. If certain tasks are not completed according to the directions, your participation in the experiment may be terminated.

Please note that in some research studies, the investigators cannot tell you exactly what the study is about before you participate in the study. We will describe the tasks in the study in a general way, but we can't explain the real purpose of the study until after you complete these tasks. When you are done, we will explain why we are doing this study, what we are looking at, and any other information you should know about this study. You will also be able to ask any questions you might have about the study's purpose and the tasks you did. Though we may not be able to explain the real purpose of the study until after you complete the tasks, there are no additional risks to those that have been described in this consent form.

In this experiment, you will listen to six sound files. Each file will contain ambient sound along with two spoken words. The two spoken words will be inserted at random points in the sound file. When the sound file concludes, we will ask you to rate the pleasantness or unpleasantness of each sound. We will also ask you to correctly identify the spoken words following the conclusion of each sound file, so feel free to write the words down while you wait. Finally, we will also ask you for demographic information as mentioned in the disclaimer.

To receive the base payment ($0.80) for the experiment, all tasks must be completed. To receive any bonus payments, spoken words must be correctly identified. For each spoken word correctly identified, you will receive a bonus of $0.10. Thus, if all spoken words are correctly identified, you will receive a bonus of $1.20 and a total payment of $2.00 upon completion of the experiment.

Please note that once you are in the listening portion of the experiment, sound files will play automatically and you WILL NOT be able to stop, rewind, or fast-forward the sound file. So please proceed to the next portion of the experiment to make sure your headphones or speakers are set up properly for listening.

You will need to use speakers or headphones to listen to sounds. Before starting the experiment, ensure your speakers or headphones are on and the volume is at a comfortable level. Press the play button below to hear the word ”testing” at the volume level we will be using throughout the experiment. Adjust the volume on your device so that the sound level is comfortable and the word is audible. Once the sound is complete and you have something handy to write down the spoken words (if desired), click on the → button to begin the experiment.

Comprehension Check Questions

Correct answers in **bold**.

- Each sound file will be composed of ambient sound and two spoken words randomly inserted into the sound file. (**True**/False)

- To remember the spoken words in each file, I can write them down. (**True**/False)
- For each spoken word I correctly identify in the audio files, I will receive a bonus payment of $0.10. Thus, if I complete all tasks in the experiment and correctly identify all the spoken words, I will receive a total payment of $2.00. (**True**/False)
- Once you are in the listening portion of the experiment, sound files will NOT play automatically. (True/**False**)
- You will be able to pause each sound file once the sound file begins playing. (True/**False**)
- You will not be able to rewind or fast-forward the sound file once the sound file begins playing. (**True**/False)

Incentive Compatibility Questions

Correct answers in **bold**. Each subject had the same two spoken words in each of the six sound files.

- What was the first spoken word in the [Baby] sound file? (**university**, zebra, statue, state)
- What was the second spoken word in the [Baby] sound file? (giraffe, book, picture, **apple**)
- What was the first spoken word in the [Alarm] sound file? (iceberg, clock, bell, **communication**)
- What was the second spoken word in the [Alarm] sound file? (**winter**, spring, summer, fall)
- What was the first spoken word in the [Lawnmower] sound file? (elephant, **vocabu-**

lary, river, stone)

- What was the second spoken word in the [Lawnmower] sound file? (**breakfast**, lunch, dinner, dessert)
- What was the first spoken word in the [Bells] sound file? (operation, frame, envelope, **America**)
- What was the second spoken word in the [Bells] sound file? (theory, economics, **photograph**, treadmill)
- What was the first spoken word in the [Creek] sound file? (probability, **information**, tale, tree)
- What was the second spoken word in the [Creek] sound file? (air conditioner, **library**, machine, guava)
- What was the first spoken word in the [Piano] sound file? (angel, **conversation**, approach, data)
- What was the second spoken word in the [Piano] sound file? (**exercise**, hat, orange, figurine)

Satisfaction Question

- How would you rate the pleasantness of the ambient sound? (Strongly unpleasant, Unpleasant, Somewhat unpleasant, Neither unpleasant or pleasant, Somewhat pleasant, Pleasant, Strongly pleasant)

A.8.2. Experiment 1 Materials

Instruction Text Highlights

In this experiment, we are researching customer experiences in service settings and conducting a two part demographic survey. The experiment should take about 10 minutes of your time and you will be compensated $0.50 plus any bonuses you accrue (up to $1.00).

An overview of the experiment and instructions will follow. The tasks will solicit demographic information on yourself (e.g., gender, race, age range, education level, income level, employment status, and state of residence) in addition to information on your experience in the experiment. No other identifying information will be obtained. All information will be stored in a secure, password-protected data storage facility.

You may abandon the experiment at any time, however in order to receive any payment for your participation in the experiment, you must complete all tasks according to the directions provided. Therefore, please ensure you follow all directions in the experiment carefully. If certain tasks are not completed according to the directions, your participation in the experiment may be terminated.

Please note that in some research studies, the investigators cannot tell you exactly what the study is about before you participate in the study. We will describe the tasks in the study in a general way, but we can't explain the real purpose of the study until after you complete these tasks. When you are done, we will explain why we are doing this study, what we are looking at, and any other information you should know about this study. You will also be able to ask any questions you might have about the study's purpose and the tasks you did. Though we may not be able to explain the real purpose of the study until after you complete the tasks, there are no additional risks to those that have been described in this consent form.

In this experiment, you will wait to complete the first part of a demographic survey and then wait again to complete the second part of a demographic survey. For each of the two waits, you will listen to ambient sound along with two words that will be spoken. The two words will be randomly spoken during your wait to ensure you focus on completing the experiment in a timely manner. When your wait concludes, we will ask you to correctly identify the spoken words, so feel free to write the words down while you wait.

To receive the base payment ($0.50) for the experiment, all tasks must be completed. To receive any bonus payments, spoken words must be correctly identified. For each spoken word correctly identified, you will receive a bonus of \$0.25. Thus, if all spoken words are correctly identified, you will receive a bonus of \$1.00 and a total payment of \$1.50 upon completion of the experiment. Because you will be listening to ambient sound and spoken words during your two waits, please proceed to the next portion of the experiment to make sure your headphones or speakers are set up properly for listening.

You will need to use speakers or headphones to listen to sounds. Before starting the experiment, ensure your speakers or headphones are on and the volume is at a comfortable level. Press the play button below to hear the word ”testing” at the volume level we will be using throughout the experiment. Adjust the volume on your device so that the sound level is comfortable and the word is audible.

Once the sound check is complete and you have something handy to write down the spoken words (if desired), click on the → button to begin the experiment.

Comprehension Check Questions

Correct answers in **bold**.

- In this experiment, I will wait to take the first part of a demographic survey and then I will wait again to take the second part of a demographic survey. (**True**/False)
- While waiting in each part of this experiment, I will listen to ambient sound and two words which will be spoken at random times during my wait. (**True**/False)
- For each spoken word I correctly identify in the audio files, I will receive a bonus payment of \$0.25. Thus, if I complete all tasks in the experiment and correctly identify all the spoken words, I will receive a total payment of \$1.50. (**True**/False)
- While I am waiting to take either part of the demographic survey, I cannot write down the spoken words that I hear in order to identify them once my wait concludes. (True/**False**)

- I will be able to predict the times at which I will hear the spoken words while waiting. (True/**False**)
- If I do not correctly identify any of the spoken words in the experiment but I complete all the tasks, I will receive a payment of $0.50. (**True**/False)

Incentive Compatibility Questions

Correct answers in **bold**. Each subject had the same two spoken words in the first and second sound files.

- What was the first spoken word in the [first] sound file? (zebra, statue, **information**, state)
- What was the second spoken word in the [first] sound file? (giraffe, **library**, apple, picture)
- What was the first spoken word in the [second] sound file? (**conversation**, clock, iceberg, book)
- What was the second spoken word in the [second] sound file? (university, winter, bell, **exercise**)

Satisfaction Question

- How would you rate your overall waiting experience for both parts of the demographic survey? (Very Poor, Poor, Fair, Good, Very Good)

A.8.3. Experiment 2 Materials

Instruction Text Highlights

In this experiment, we are researching customer experiences in service settings. The experiment should take about 10 minutes of your time and you will be compensated $0.50 plus any bonus you accrue (up to $1.00). An overview of the experiment and instructions will follow. The tasks will solicit information on your experience in the experiment and also demographic information on yourself (e.g, gender, race, age range, education level, income level, employment status, and state of residence). No other identifying information will be obtained. All information will be stored in a secure, password-protected data storage facility.

You may abandon the experiment at any time, however in order to receive any payment for your participation in the experiment, you must complete all tasks according to the directions provided. Therefore, please ensure you follow all directions in the experiment carefully. If certain tasks are not completed according to the directions, your participation in the experiment may be terminated.

Please note that in some research studies, the investigators cannot tell you exactly what the study is about before you participate in the study. We will describe the tasks in the study in a general way, but we can't explain the real purpose of the study until after you complete these tasks. When you are done, we will explain why we are doing this study, what we are looking at, and any other information you should know about this study. You will also be able to ask any questions you might have about the study's purpose and the tasks you did. Though we may not be able to explain the real purpose of the study until after you complete the tasks, there are no additional risks to those that have been described in this consent form.

In this experiment, you must complete a 1-question multiple choice quiz with one of three quiz administrators. The three quiz administrators are assigned to one of three areas: math, logic, and writing. Administrator 1 is responsible for administering the math quiz. Administrator 2 is responsible for administering the logic quiz. Administrator 3 is responsible for administering the writing quiz. You are responsible for choosing the type of quiz you would like to complete. The administrator in each area will be responsible for generating the quiz question you will receive.

All participants in the experiment, including yourself, will first choose a specific quiz to take. You will then be assigned an available animal avatar (e.g., dog, cat, elephant, etc.) so that you will be able to see yourself waiting along with all other participants (regardless of the quiz/server they have chosen). All administrators will only process their participants in the order that they arrive, and when it is your turn with your administrator, your avatar will appear in the "NOW SERVING" box.

Before you can proceed to your administrator to take your quiz, you will be asked to identify the animal avatar of the individual who was served immediately before you (if no one was before you, this option will be present), so pay attention to who is being served before you. For example, if a dog avatar proceeded into service immediately before you, we will ask you to identify the dog avatar. If you correctly answer this question, you will receive a $1.00 bonus payment bringing your total payment in the experiment to $1.50 if all tasks are completed. Thus, feel free to write down information such as the names of the animal avatars that proceed into service before you (e.g., tiger was first, lion was second, etc.).

After answering the question regarding the animal avatar that proceeded into service before you, the server will take a moment to decide the question to administer on your quiz, and then you will be taken to the quiz question in another screen. You will click on the answer to the question you believe is correct, however you will not receive a bonus for a correct quiz answer. Once the quiz is finished, you must answer some short questions about your demographics.

Comprehension Check Questions

Correct answers in **bold**.

- Each of the three quiz administrators will be assigned to only one subject area: math, logic, or writing; and you will be asked to choose only one type of quiz to complete with one of the administrators. (**True**/False)

- My quiz administrator will not necessarily process his/her participants in the order that they arrive. (True/**False**)

- Before proceeding into the waiting area for my quiz administrator, I will be assigned an animal avatar that will allow me to see myself waiting for my administrator with other participants (who may or may not be waiting for the same administrator) in the experiment. (**True**/False)
- All experiment participants that you may see in the waiting area must be assigned to the same quiz administrator. (True/**False**)
- When my wait is over and I am ready to proceed to a quiz administrator, I will be asked to identify the animal avatar of the participant who was called for their quiz immediately before me. (**True**/False)
- I may not write down any information regarding the animal avatars that proceeded to their quiz administrator before me. (True/**False**)
- If I correctly identify the animal avatar of the participant who was called for their quiz immediately before me, I will receive a bonus payment of $1.00, and upon completion of the experiment, I will receive a total payment of $1.50. (**True**/False)
- You will receive an additional bonus payment if you answer the math, logic, or writing quiz question correctly. (True/**False**)

Incentive Compatibility Questions

Correct answer in **bold**.

- What was the animal avatar that proceeded to service immediately before you [FIFO Condition]? (Dog, Rabbit, Hippopotamus, **Elephant**, Cat, None of these)
- What was the animal avatar that proceeded to service immediately before you [Advance in Line Condition]? (Dog, Rabbit, Hippopotamus, Elephant, Cat, **None of these**)
- What was the animal avatar that proceeded to service immediately before you [View

Cuts in Line Condition]? (**Dog**, Rabbit, Hippopotamus, Elephant, Cat, None of these)

Satisfaction Question

- How would you rate your waiting experience for your administrator? (Very poor, Poor, Fair, Good, Very good)

A.8.4. Experiment 3 Materials (without Progress Bar

Instruction Text Highlights

In this experiment, we are researching customer experiences in service settings. The experiment should take about 10 minutes of your time and you will be compensated $0.50 plus any bonus you accrue (up to $1.00). An overview of the experiment and instructions will follow. The tasks will solicit information on your experience in the experiment and also demographic information on yourself (e.g, gender, race, age range, education level, income level, employment status, and state of residence). No other identifying information will be obtained. All information will be stored in a secure, password-protected data storage facility.

You may abandon the experiment at any time, however in order to receive any payment for your participation in the experiment, you must complete all tasks according to the directions provided. Therefore, please ensure you follow all directions in the experiment carefully. If certain tasks are not completed according to the directions, your participation in the experiment may be terminated.

Please note that in some research studies, the investigators cannot tell you exactly what the study is about before you participate in the study. We will describe the tasks in the study in a general way, but we can't explain the real purpose of the study until after you complete these tasks. When you are done, we will explain why we are doing this study, what we are looking at, and any other information you should know about this study. You will also be able to ask any questions you might have about the study's purpose and the tasks you did. Though we may not be able to explain the real purpose of the study until after you complete the tasks, there are no additional risks to those that have been described in this consent form.

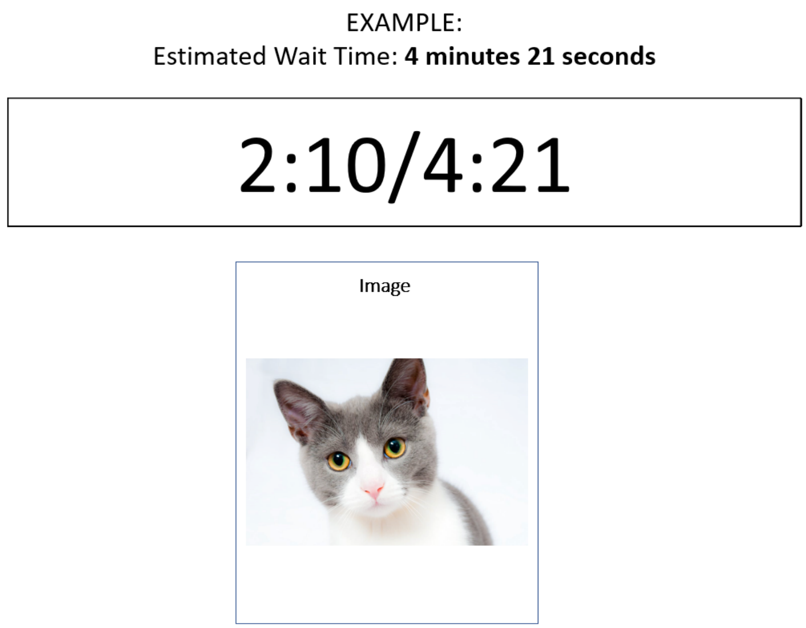
In this experiment, you must wait to complete a customized demographic survey. Because each survey is customized for a participant by a surveyor, you will be given a scheduled waiting time since the surveyor may also be processing other participants in the experiment.

Because we want the surveyor to process as many experiment participants as possible, you must pay attention in the waiting area. Your actual waiting time may be complete before or after the scheduled time, so you must pay attention to when your wait ends. To help you keep track of your actual wait time and scheduled wait time, a wait timer will be provided like the one below. Your wait will end when your wait timer stops. To help you pay attention in the waiting area, we will flash an image in a designated image area for a brief time period immediately after your wait is complete (see the example below). Please feel free to record what this image is. After clicking proceed, you will be asked to identify the image correctly. If you identify the image correctly, you will receive a $1.00 bonus payment bringing your total payment in the experiment to $1.50 if all tasks are completed. Please click the → button when you are ready to begin the experiment.

Comprehension Check Questions

Correct answers in **bold**.

- In this experiment, you will receive a scheduled waiting time for the surveyor. (**True**/False)
- Your actual waiting time for the surveyor will never be longer or shorter than the scheduled waiting time. (True/**False**)
- Your actual waiting time will end when the wait timer stops. (**True**/False)
- When my wait is over and I am ready to proceed to the surveyor, I will be asked to



identify the image that flashes briefly immediately following the end of my wait time. (**True**/False)

- I may not write down any information regarding the image I must identify following the end of my wait time. (True/**False**)

- If I correctly identify the image following the end of my wait time, I will receive a bonus payment of $1.00, and upon completion of the experiment, I will receive a total payment of $1.50. (**True**/False)

Incentive Compatibility Question

Correct answer in **bold**.

- What was the image that flashed for five seconds when your wait was complete? (Dog, Rabbit, Hippopotamus, **Elephant**, Cat, None of these)

Satisfaction Question

- How would you rate your waiting experience for your surveyor? (Very poor, Poor, Fair, Good, Very good)

A.8.5. Experiment 3 Materials (with Progress Bar

Instruction Text Highlights

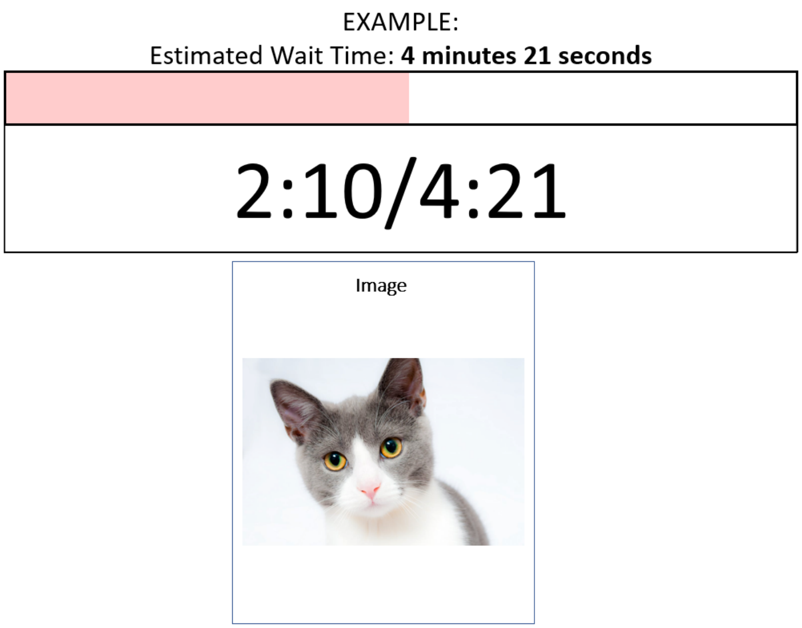
In this experiment, we are researching customer experiences in service settings. The experiment should take about 10 minutes of your time and you will be compensated $0.50 plus any bonus you accrue (up to $1.00). An overview of the experiment and instructions will follow. The tasks will solicit information on your experience in the experiment and also demographic information on yourself (e.g, gender, race, age range, education level, income level, employment status, and state of residence). No other identifying information will be obtained. All information will be stored in a secure, password-protected data storage facility.

You may abandon the experiment at any time, however in order to receive any payment for your participation in the experiment, you must complete all tasks according to the directions provided. Therefore, please ensure you follow all directions in the experiment carefully. If certain tasks are not completed according to the directions, your participation in the experiment may be terminated.

Please note that in some research studies, the investigators cannot tell you exactly what the study is about before you participate in the study. We will describe the tasks in the study in a general way, but we can't explain the real purpose of the study until after you complete these tasks. When you are done, we will explain why we are doing this study, what we are looking at, and any other information you should know about this study. You will also be able to ask any questions you might have about the study's purpose and the tasks you did. Though we may not be able to explain the real purpose of the study until after you complete the tasks, there are no additional risks to those that have been described in this consent form.

In this experiment, you must wait to complete a customized demographic survey. Because each survey is customized for a participant by a surveyor, you will be given a scheduled waiting time since the surveyor may also be processing other participants in the experiment.

Because we want the surveyor to process as many experiment participants as possible, you must pay attention in the waiting area. Your actual waiting time may be complete before or after the scheduled time, so you must pay attention to when your wait ends. To help you keep track of your actual wait time and scheduled wait time, a wait timer and pink progress bar (showing your progress towards your scheduled wait time) will be provided like the one below. Your wait will end when your wait timer stops (not necessarily when the pink progress bar stops). To help you pay attention in the waiting area, we will flash an image in a designated image area for a brief time period immediately after your wait is complete (see the example below). Please feel free to record what this image is. After clicking proceed, you will be asked to identify the image correctly. If you identify the image correctly, you will receive a $1.00 bonus payment bringing your total payment in the experiment to $1.50 if all tasks are completed. Please click the → button when you are ready to begin to experiment.



Comprehension Check Questions

Correct answers in **bold**.

- In this experiment, you will receive a scheduled waiting time for the surveyor.

(**True**/False)

- Your actual waiting time for the surveyor will never be longer or shorter than the scheduled waiting time. (True/**False**)
- Your actual waiting time will end when the wait timer stops (not necessarily when the pink progress bar stops). (**True**/False)
- When my wait is over and I am ready to proceed to the surveyor, I will be asked to identify the image that flashes briefly immediately following the end of my wait time. (**True**/False)
- I may not write down any information regarding the image I must identify following the end of my wait time. (True/**False**)
- If I correctly identify the image following the end of my wait time, I will receive a bonus payment of $1.00, and upon completion of the experiment, I will receive a total payment of $1.50. (**True**/False)

Incentive Compatibility Question

Correct answer in **bold**.

- What was the image that flashed for five seconds when your wait was complete? (Dog, Rabbit, Hippopotamus, **Elephant**, Cat, None of these)

Satisfaction Question

- How would you rate your waiting experience for your surveyor? (Very poor, Poor, Fair, Good, Very good)

A.9. Drivers of Customer Satisfaction in a Tandem Queue: Additional Tables

Table 28: Regression results for other regressors in the observational study

DV: SATISFACTION	(1) OLS (TOTAL)	(2) OLS (LOCATION)	(3) 2SLS
Others in reception	-0.095**	-0.122***	-0.125***
	(0.039)	(0.039)	(0.038)
Information on procedure day	0.405***	0.415***	0.418***
	(0.107)	(0.106)	(0.104)
Helpful registration staff	0.098	0.103*	0.105*
	(0.061)	(0.061)	(0.058)
Friendliness of nurses	0.069	0.069	0.071
	(0.125)	(0.125)	(0.122)
Comfort of registration area	0.148	0.141*	0.134
	(0.082)	(0.082)	(0.082)
Comfort in resting area	0.278***	0.277***	0.277***
	(0.074)	(0.075)	(0.073)
Colonoscopy (Base: Not colonoscopy)	-0.063	-0.075	-0.087
	(0.071)	(0.071)	(0.071)
Male (Base: Female)	0.079	0.075	0.081
	(0.058)	(0.058)	(0.058)
Age (Base: 18-35 years)			
36-53 years	-0.004	-0.015	-0.025
	(0.122)	(0.123)	(0.121)
54-71 years	0.070	0.063	0.058
	(0.114)	(0.116)	(0.114)
72+ years	0.010	0.003	-0.006
	(0.130)	(0.132)	(0.131)
Race (Base: Other/Unknown)			
Black	0.051	0.043	0.054
	(0.093)	(0.096)	(0.095)
Hispanic	0.039	0.015	-0.006
	(0.244)	(0.255)	(0.261)
White	-0.048	-0.059	-0.044
	(0.090)	(0.092)	(0.090)
Time of day (Base: Before 10am)			
10am to 11:59am	-0.212***	-0.226***	-0.205**
	(0.081)	(0.081)	(0.082)
12pm to 1:59pm	-0.146*	-0.163**	-0.144*
	(0.077)	(0.077)	(0.077)
After 2pm	-0.098	-0.101	-0.121
	(0.131)	(0.132)	(0.130)
Constant	1.572**	0.979*	2.223**
	(0.625)	(0.552)	(0.906)

*p<0.1, **p<0.05, ***p<0.01

Robust standard errors reported in parentheses

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