Essays on Political Economy, Industrial Organization, and Public Economics

Essays on Political Economy, Industrial Organization, and Public Economics

A dissertation presented

by

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to

The Department of Economics

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

Economics

Harvard University

Cambridge, Massachusetts

December 2013

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Abstract

The first chapter of this dissertation analyzes voting behavior across multiple elections. The voting literature has largely analyzed voter turnout and voter behavior separately, focusing on individual elections. I present a model of voter turnout and behavior in multiple elections. The assumptions are consistent with individual election preferences and decision is derived from utility maximization. Additionally, I provide necessary moment conditions for identification. The framework is applied to the 2008 California elections. The exit polls made national headlines by linking the historic turnout of African-Americans for Presidential candidate Obama in helping pass Proposition 8. The results show that the African-American turnout and voting share for Proposition 8 was lower than indicated by the exit polls. As a counterfactual, I look at the turnout and outcome of Proposition 8, without the presidential race on the ballot. As predicted, there is lower voter turnout: on par with midterm elections. I also find a lower share of Yes votes on Proposition 8 – enough that the referendum would not have passed.

The second chapter looks at whether policies shift preferences, an important component in policy design. We isolate exogenous variation in abortion jurisprudence using the random assignment of Democratic appointee judges, which strongly increases the probability of a liberal abortion decision. We also document that newspapers report appellate abortion decisions and conduct a field experiment assigning workers to transcribe these news reports. Using both sources of variation, we find that exposure to liberal abortion precedent initially leads to more conservative public opinions, and more liberal public opinions over time.

The third chapter studies payments to physicians by pharmaceutical companies, traditionally a topic of considerable debate. I examine which types of physicians are targeted through payments, and find that physicians with published research are paid by more companies and for larger amounts than non-published physicians. The effect increases in states with existing disclosure laws, consistent with reputation effects. The pharmaceutical company payments are also targeting networks of researchers versus individual specialists. Coauthoring or citing increases the likelihood of being paid by the same company and category. This result is consistent with higher payments under disclosure.

Table of Contents

2 Do Policies Affect Preferences? Evidence from Random Variation in Abortion Jurisprudence (joint with Daniel Chen and Susan Yeh) 41

Acknowledgements

This thesis has been a long journey, and there are many people who have helped me throughout the process. I cannot possibly thank each person, but I gratefully appreciate everyone's help.

First and foremost, I would like to thank my committee. I owe special debt of gratitude to Alberto Alesina. One could not have asked for a better mentor and advisor, and his influence can be seen everywhere in the PhD. Whether it is encouraging and nurturing me as a young researcher, or guiding through all the matters related to PhD and beyond, his advice has always been timely and indispensable. I feel lucky that Alberto's mentorship does not stop with the PhD. I owe many thanks to Ariel Pakes. He has always pushed me to be sensible, yet rigorous in my analysis. Much of how I approach a question and think about economics has come from many interactions with Ariel – both from his lectures and also doing research with him. I thank Ed Glaeser for his guidance and support. I can approach Ed with any question, and it seems no topic in economics is outside his reach. With his piercing insight, Ed gets to the heart of the issue almost immediately, which otherwise would have taken much longer to uncover. I am grateful to Claudia Goldin for her advice and encouragement. Never afraid to ask tough questions, she has helped me to think more carefully about issues and clarify my own research questions. I will miss her discussions around the NBER kitchen table.

I am especially indebted to Daniel Chen. He has been like a committee member to me, and it has been tremendously rewarding to work with him over the years. Always striving for excellence, no question is too big for him to tackle; It still amazes me how he handles so many projects. His enthusiasm and energy towards research is contagious. Chapter 2 of this dissertation is coauthored with him and Susan Yeh, and I look forward to many future joint papers.

Finally, most deeply felt thanks go out to my family. My dad, Levon, and my sisters, Liana and Sona, have been my biggest cheerleaders and fans from day 1: always full of support, even when I did not feel so confident myself. The long distance between us was never felt through all these years. I am most thankful to my wife, Varvara, for her love and tenderness. Varvara has kept me sane during this process – always there with much needed support and a good perspective on what this all amounts to. I will forever cherish her help and encouragement during the final push towards the end, and I am glad to cross the finish line with my *chutik*.

To my family,

and in loving memory of my mother, Jemma, who left too early

Chapter 1

What Led to the Ban on Same-Sex Marriage in California?: Structural Estimation of Voting Data on Proposition 8

1 Introduction

It is the norm in the US, and most places in the world, that multiple elections take place at the same time. A single general election ballot typically includes national, state, and local races, along with referendums and initiatives. The vast literature on voting outcomes has largely taken each election individually in a vacuum, ignoring the presence and possible effect of other elections happening simultaneously. The literature on multiple elections has focused on explaining political phenomena between different elections such as split-ticket voting, either as an equilibrium of strategic considerations (Alesina and Rosenthal 1996) or due to information variation (Degan and Merlo 2011) between races.

Even if voter preferences have no strategic components and voters are perfectly informed of each candidate, multiple simultaneous elections might still affect each outcome simply due to turnout. A voter considers her utility for participating in all elections, and would go to vote only if the utility of participating is larger than the utility of staying home.[1] Thus, even if voting choices between candidates in a given election are due to individual preferences for each candidate, the overall turnout in the election will depend on all the races taking place at that time.

It is well-documented that turnout varies greatly between elections (Blais 2000). The most evident example is that US presidential elections have had historically higher voter turnouts than midterm elections. Voters respond more strongly to presidential races than to Congressional or local elections. (This could be due to various factors, such as the prestige level of the office or simply more campaign advertising). For heterogeneous voters, adding or excluding certain races like a presidential ticket will affect the composition of voters who go to the polls. This will, in turn, impact shares in other races. It may even impact election outcome.

The prior literature has predominantly looked at turnout separately from voting behavior. And the ecological inference on heterogeneous voters has looked at individual election outcomes. I present a model of turnout and voting decisions in multiple elections. When analyzing each election outcome separately, the presence of additional races will not impact other election results. The framework I propose considers the voter's utility of participation as a direct function of choices in all elections and derives turnout endogenously from utility maximization.

I borrow from discrete choice demand estimation theory in a single market (Berry et al. 1995) and apply the setup to voting decisions in a single election. I then extend the model to allow for selection in multiple elections and derive utility of turnout that is consistent with the model's assumptions on voting preferences. To my knowledge, this framework is the first to extend discrete choice literature to multiple elections, and the setup can also be used to analyze simultaneous consumption decisions in multiple markets. I also provide identification condition for estimation, as the usual moment conditions rely on precinct-level variation of

[1] Or net-utility, if explicitly accounting for costs.

candidate characteristics. I propose new moment conditions when the same candidate choices are on each ballot across precincts, as is typically the case during elections. The new moment conditions are once again consistent with the assumptions of the model.

I apply the setup to estimate turnout and outcomes in the 2008 California general elections. The election results in California made national headlines when exit polls singled out the historic turnout of African-American voters for the first-ever Black presidential candidate, Obama, along with their strong preference for passing Proposition 8, which banned same-sex marriage in the state. While the share of the African-American voting bloc was not sufficient to make up the difference of the close election results of Proposition 8, the story nevertheless brought to the forefront the issue of turnout for one election possibly affecting the outcome of other elections on the same ballot.

I estimate the preferences for presidential candidates Obama and McCain and voting Yes or No on Proposition 8 (Prop 8) for each demographic. When compared to exit poll results, African-Americans had a lower turnout and lower share of Yes votes on Prop 8. (Although they came out in large numbers and the majority voted Yes on Prop 8). As a counterfactual, I look at the turnout and election outcome of Prop 8 without the presidential race on the ballot. The structural setup of the model allows for such analysis, and the counterfactual results are consistent with the observed preferences of voting in both elections. I find lower turnout without the presidential election as predicted by the model and on a par with midterm election results. I also find that Proposition 8 would have most likely been defeated.

The intuition of the model predicts that part of the voting population who chose to vote for president and Prop 8, may abstain from voting when the choice is for Proposition 8 only.[2] The intuition for the outcome of Proposition 8 is similar. The estimates indicate

[2] It is clear that voting in two elections will give at least as much utility as voting in one election, since the voter may simply abstain from the second election. Given that a very small percentage actually abstain from the election, the voter gains positive utility from at least one of the choices in each election. This implies that almost all voters will gain a strictly higher utility from voting in two elections versus one.

the demographic groups with a strong preference for one of the presidential candidates: Hispanics and Blacks overwhelmingly voted in favor of Obama. Incidentally, they are also the demographic groups with the highest share of Yes votes on Proposition 8. Eliminating the presidential election from the ballot reduces turnout, but not uniformly across demographics. Voters with the strongest preference for one of the presidential candidate are less likely to vote in this case. In other words, the drop in turnout for Blacks and Hispanics will be higher than for other demographics. Thus, the share of remaining Yes votes will decrease more than the share of No votes. The drop is enough to overturn the election outcome of Prop 8.

The remainder of the paper is organized as follows. Section 2 provides a review of relevant literature on discrete choice models, voting, and the 2008 election. Section 3 presents the data sources and compares them to polling data. Section 4 presents the model, and derives identification. Section 5 presents the main results, and Section 6 concludes.

2 Literature Review

2.1 Related Literature

The extensive literature on voting also includes the choices of voters over multiple elections (Alesina and Rosenthal 1995, and Alesina and Rosenthal 1996). Prior research largely looks at strategic choices across multiple elections. Alesina and Rosenthal (1996), for instance, have shown that interesting dynamics can arise when the ballot choice set goes from one to two elections. Such political normalities as a divided government and a midterm reversion can, in fact, be borne out as stable equilibrium outcomes. I abstract away from strategic considerations and treat a voting choice as a pure consumption choice. This is done in part because voters' choices have an infinitesimal impact on election outcomes, and partly because it is harder to devise a strategic storyline for election choices consisting of a nationwide presidential campaign and a state referendum. A lack of strategic choices still does not preclude considering the elections simultaneously. Voter action in one election may still affect the results in another due to the participation constraint of going to the voting booth. I treat the voting choice as a consumption choice, which naturally leads to analyzing demand literature.

Recent advances in demand estimation have found applications in many markets, or even fields of economics. Pioneered by the seminal work of Berry et al. (1995) (henceforth, BLP), a structural model of discrete choice random coefficients setup is used to study industries as varied as automobiles (BLP), cereals (Nevo 2001), movies (Einav 2007), and TV broadcast (Goolsbee and Petrin 2004). The models are based on individual optimizing decisions, aggregated to obtain market shares. The model's main advantage is the ability to match aggregate market-level (macro-level) data that has total product sales and characteristics with consumer demographics. One does not need to have individual-level (micro-level) data that matches consumer characteristics with their purchases. The models are general enough to produce reasonable markups and substitution patterns.

This paper extends the BLP framework and develops a model that analyzes consumer participation and consumption in multiple markets. Many purchases take place simultaneously in multiple markets, e.g., an individual buying milk and bread in the same shopping trip, making this is a relevant setup for demand analysis. Estimating bread and milk markets separately might produce incorrect results, especially in terms of participation. The consumer considers her choices in both markets and weights the total utility of bread and milk purchases in the decision to go to the store.

I develop a framework that allows for a joint decision to participate in several markets. The decision will be a function of the utilities of all the products, and I show the assumptions to be consistent with the BLP setup. My methodology is unique for two reasons. First, I derive demand estimates using only aggregate, macro-level, data. Prior literature that looks at purchases in multiple markets uses individual-level data, tying consumers to their bundle of products.[3] To my knowledge, my research is the first to rely on only aggregate

[3]See, e.g., the class of AIDS models.

data. Complicating the matter of using only aggregate data is that shares are usually reported for individual products, but not for bundles of products, which are the relevant choices for the consumer. In other words, suppose there are two possible choices for bread – $bread1, bread2$, and two possible choices for milk – $milk1, milk2$. If the consumer purchases one of each, her choice set will be from $(bread1, milk1), (bread1, milk2), (bread2, milk1)$, and $(bread2, milk2)$. The aggregate shares are usually reported not for the combination of each of these four bundles, but for $bread1, bread2, milk1$, and $milk2$ separately. I am able to derive the shares of each possible bundle from the product market shares.

Second, I obtain identification despite no product-level variation. The main identification from logit-type models like BLP comes from product space and product characteristic variations across markets, most notably, price. I show that consumer variation across markets, such as demographic differences, could be used instead to carry out estimation. My methodology once again conforms to the assumptions of the BLP setup.

The BLP approach has been used widely to analyze different markets and forms of competition, using the key methodology – heterogeneous preferences and endogenous prices. These include Nevo (2001), Petrin (2002), and Berry et al. (2004). Applications to other forms of competition extend to advertising (Ackerberg 2001, Anand and Shachar 2011), the expansion of satellite broadcast (Goolsbee and Petrin 2004), geographic distribution (Davis 2006), and the real estate market (Wong 2013), among others.

In the voting literature, ecological inference on election outcomes has a long tradition (King 1997). Analyzing voting data in a discrete choice framework dates back to as early as Poole and Rosenthal (1985). Applying the BLP framework to voter preferences has been used by Rekkas (2007), Gordon and Hartmann (2013), and Martin (2013). In all these cases, campaign spending is used as a substitute for price in the individual utility specification, which provides key moment conditions and identification of the model. In my setup I am able to derive identification despite no campaign spending or any other product characteristic that varies at the market-level.

Hendel (1999) presents a model of multiple markets. However, his model allows for multiple purchases in a given market, e.g., a firm buying multiple PCs or even various brands of PCs. In my setup, the choice is clearly for up to one candidate in each election, and different election races are related primarily by appearing on the same ballot. Perhaps the paper closest in spirit to this one is by Degan and Merlo (2011). The authors develop a structural model of participating in multiple elections, and apply the setup to presidential and Congressional House races on the same ballot. My approach is different from theirs in several key aspects. First, they use individual, micro-level data, whereas my setup can be used with macro-level data. Second, they model the decision to take part in an election as a function of civic duty, which is the same for both elections. Whether to participate in voting, and the difference in voters' preferences for candidates, is due to information (and misinformation) the voter has for each race. By eliminating one of the elections, the outcome in the other election would not change. In my case, the utility from each race depends on observable voter characteristics, such as demographics. The decision to vote is directly determined by all the elections taking place. The utility from turnout is derived as the total utility from choosing a candidate in each election.

2.2 Proposition 8 Background

State amendments and propositions to allow or ban gay marriage were among the newsworthy issues in the 2008 general election. The issue had come to the forefront since the Massachusetts State High Court decision in 2003, and many states subsequently moved to add constitutional amendments through public referendums. In 2008, three additional states – Arizona, California, and Florida – had ballot measures prohibiting same-sex marriage. Probably none received as much media attention as did Proposition 8 in California. As stated on the ballot, voting Yes to Prop 8 would add a provision to the state constitution that "only marriage between a man and a woman is valid or recognized in California".[4] Ear-

[4]http://vig.cdn.sos.ca.gov/2008/general/text-proposed-laws/text-of-proposed-laws.pdf#prop8

lier in 2008, the California State Supreme Court granted permission for same-sex marriages, and by election time, many same-sex marriages had already taken place. Passing Prop 8 would invalidate the State Supreme Court ruling, and the status of already-issued same-sex marriage certificates would be in jeopardy.

Proposition 8 passed with 7,001,084 (52.24%) Yes to 6,401,482 (47.76%) No votes. The passage sparked many protests and demonstrations. Its affect on existing same-sex marriages, and even the validity of the entire proposition, was challenged in court. Through successive appeals to higher courts, the case reached the United States Supreme Court. In June 2013, the US Supreme Court declined to take up the case, effectively handing down the lower court ruling overturning Proposition 8. (The court also ruled that all same-sex marriages be federally recognized.) On a nationwide scale, there is also political discussion to pass legislature for broader gay rights (for instance, President Obama's second inaugural speech).[5]

Perhaps more interesting, in the aftermath of the election, was the analysis of the passage itself. Less than 5% of the total vote separated the Yes and No choices. Any one determining factor could have been the deciding factor between the passage of the proposition and its defeat. The only available data immediately after the election were exit polls. Most produced similar results, and as the CNN polls[6] or *The New York Times* polls[7] indicate, for instance, the demographic breakdown of California voters showed that Whites, Hispanics, Asians, and Other races were almost evenly split in favor of, or against, Proposition 8. Across a gender divide, males and females were also almost evenly split in their sentiment for Prop 8. The only glaring exceptions were African-Americans/Blacks, who were an overwhelming 70% in favor of Prop 8. The runaway story from the exit polls was that the Black population turned out in great numbers to vote for the first-ever Black presidential candidate, Barack Obama, and in the process, helped tip the scales in favor of the passage of Prop 8. This

[5]http://www.whitehouse.gov/the-press-office/2013/01/21/inaugural-address-president-barack-obama

[6]http://www.cnn.com/ELECTION/2008/results/polls/#CAI01p1

[7]http://elections.nytimes.com/2008/results/states/exitpolls/california.html

storyline was reported by all the major newspapers, such as *The New York Times*,[8] the *Los Angeles Times*,[9] *The Washington Post*,[10] and other media outlets.[11]

I apply my framework to analyze voting in the 2008 elections in California by looking at the presidential race and the Proposition 8 ballot measure. I look at the relative weight of each demographic on the passage of Proposition 8 by using the official election results for the 2008 general election. This method has several advantages over poll results.[12] Primarily, the polling results have weights applied to match the population average. Such weights may produce distorted results when looking at only a particular demographic. Second, the official election data uses the entire population of the voting count, rather than the poll sample, which may not be representative or could suffer from other types of sample bias. Also, the election results are actual choices made, whereas poll responses are self-reported. Third, the overall official turnout, broken down by precincts, provides a more direct comparison to that of previous years. Finally, the election results also include absentee and overseas ballots, which are not fully represented in exit polls but are, nevertheless, becoming relevant portions of overall electorate counts. According to the official vote count by the Secretary of State of California, 41.64% of all votes were cast by mail.[13]

The main drawback of using election results data is that they are precinct-level (macro-level), whereas poll data is individual-level (micro-level). That is, at the poll level, one knows the demographics of an individual, matched with her voting choices. To compute voting preferences across all races then becomes a matter of straightforward computation. In election results, however, one only knows the overall votes for each election, and the breakdown of possible combinations of election choices is not given. To overcome the lack

[8] http://www.nytimes.com/2008/11/06/us/politics/06marriage.html

[9] http://articles.latimes.com/2008/nov/08/local/me-gayblack8

[10] http://www.washingtonpost.com/wp-dyn/content/article/2008/11/06/AR2008110603880.html

[11] E.g., http://www.slate.com/articles/health_and_science/human_nature/2008/11/original_skin.html

[12] Greiner and Quinn (2013) provides a statistical model to combine election results with poll results.

[13] http://www.sos.ca.gov/elections/sov/2008-general/sov_complete.pdf

of detailed data, I introduce a structural model of voter turnout and voting in multiple elections. Thus, with my model I can potentially explain the behavior of going to vote on a presidential election, and in the process, also voting on Prop 8. At the heart of my structural model is discrete choice estimation.

3 Data

3.1 Census Data

The two main sources of my data are the US Census,[14] and the voting data from the California Statewide Database.[15] The US Census, conducted decennially, provides a detailed description of the population and businesses at a local geographic level. The smallest level of aggregation depends on the choice of variables and is selected such that individual entries cannot be identified from the aggregate data. Race and gender characteristics for the US population are broken down to block level, the finest geographic level possible. Other variables, like income, are provided over a larger geographic area. I am primarily interested in racial characteristics of the population and their impact on elections, and so I collect census data at the block level. Moreover, I restrict the population age to 18 and over, which is the relevant fraction of Americans who can vote.[16] There is no census for the year 2008, so I use the census figures from 2000 and 2010 to extrapolate the population demographics for 2008.

The California Statewide Database is an online redistricting database commissioned by the state itself. It has detailed voting and registration data for all elections since 1992, broken down at the county, district, and precinct levels. Moreover, it provides mapping between census geography units – blocks, and election geography units – precincts. I use the

[14]http://www.census.gov

[15]http://swdb.berkeley.edu

[16]The portion of the population who is ineligible to vote may still be an issue. They may not be US citizens, or haven't registered in time for the elections. I explore alternative baseline populations.

mapping to match race characteristics to voting outcomes across precincts.[17]

Regarding race categories, the 2000 Census differed from the previous ones in that it introduced two or more options as possible choices for the category of race.[18] In addition to mixed race, ethnicity was now primarily divided between Hispanic/Latino and non-Hispanic/Latino. The 2010 Census followed in the same manner, making the distinction between ethnicity and race more explicit. In the 2010 Census, the possible choices for single race are: White, Black or African American, American Indian and Alaska Native, Asian, Native Hawaiian and Other Pacific Islander, and finally, Other. Additionally, it includes all possible combinations of mixed races, starting with any two different races to a maximum of six. The population numbers are reported in two ways. One set of tables is organized along race categories. The other is first broken down by two ethnicities – Hispanic/Latino, and non-Hispanic/Latino – and the non-Hispanic/Latino population is further tabulated by race. Without the ethnicity option and the difference between the first and second methods of reporting, the Hispanic/Latino population identifies themselves primarily as either White, or Other race.[19]

I make use of the second way of reporting census numbers to have a separate category for Hispanics, which is one of the important demographic characteristics in my estimation. The population numbers vary greatly among the six races, as shown in Table 1.1. I combine the individual races into five major, mutually exclusive categories – Hispanic (henceforth, Hispanic); White (henceforth, White); Black or African American (henceforth, Black); Asian or Native Hawaiian and Other Pacific Islander (henceforth, Asian); American Indian and Alaska Native or Other (henceforth, Other).[20] This method is dictated by the OMB Directive

[17] The mapping file does not match with perfect accuracy either with the census blocks or with election precincts. This mismatch is the main source of discrepancy when trying to merge the two datasets; whenever such a discrepancy was too large, I looked for alternate places for datasets, where I could obtain information on missing precincts and blocks.

[18] http://www.census.gov/dmd/www/pdf/d02p.pdf

[19] Only about 2% identify as some other race or a mixed race.

[20] Alternatively, I combine the Native Hawaiian and Other Pacific Islander, American Indian and Alaska Native, and Other race into one, Other race. Another possible alternative is to put American Indian and

15[21] and seems to be the most widely used method of categorization in practice and in the literature (Greiner and Quinn 2013).

I also follow the same directive and combine two or more races into a single race in the following fashion.[22] All the people identified as Hispanic ethnicity, are classified as Hispanic. Within the non-Hispanic population, if one of the races with which a person identifies is Black, then that person is classified as Black. Otherwise, if one of the races a person identifies with is Asian, then that person is classified in the Asian category. Otherwise, if it is a mix of White and Other, I classify them as White. Finally, if a person is a mix of Other races, I put that person in the Other category. This is one possible way of combining multiple races into one, and there are certainly other ways of aggregating mixed races. Since the vast majority of the Census, or about 98%, are either Hispanic or single race, the variations in the breakdown of multiple races into single ones would not have a material impact on the results. In summary, this enables me to place the Census population into one of possible five possible categories: White, Black, Asian, Hispanic, and Other.

3.2 Voting Data

Table 1.1 shows data from the California Statewide database website. The 2008 general election had 8,274,473 votes for the Democratic Presidential candidate (Obama), and 5,011,781 votes for the Republican Presidential candidate (McCain); 7,001,084 Yes votes for Proposition 8, with 6,401,482 No votes. Given the close result for Prop 8, it is not surprising that possible explanations and theories have been topics of intense analysis and scrutiny. These numbers are an exact match with the official count of the California election results as reported by the Secretary of State of California.[23] It is interesting to note that these

Alaska Native into the Hispanic ethnicity, and keep Other race as a stand alone category.

[21] http://www.whitehouse.gov/omb/fedreg_directive_15

[22] Remember that two or more races can come from the combination of the following races: White, Black, Asian, and Other.

[23] http://www.sos.ca.gov/elections/sov/2008-general/sov_complete.pdf

Table 1.1. Summary Statistics

CNN Poll Results for Voter Demographics and Shares of Election Choice

		Proposition 8		*Presidential Election*		
	Population Share	Yes	No	Dem	Rep	Other
White	63%	49%	51%	52%	46%	2%
African-American	10%	70%	30%	94%	5%	1%
Latino	18%	53%	47%	74%	23%	3%
Asian	6%	49%	51%	64%	35%	1%
Other	3%	51%	49%	55%	41%	4%

Total Votes Cast

Proposition 8		
Yes	7,001,084	52.24%
No	6,401,482	47.76%
Presidential Race		
Democrat	8,274,473	61.1%
Republican	5,011,781	37.0%
Other	275,646	1.9%

Population Demographics

	Census 2000	Census 2010	2008	Mean	St Dev	Min	Max
White	12,593,932	16,101,075	11,991,899	52%	27%	0%	97%
Black	1,528,106	1,725,443	1,686,836	6%	10%	0%	93%
Asian	2,793,113	3,648,163	3,804,575	14%	15%	0%	92%
Hispanic	6,915,731	10,517,229	8,618,187	27%	23%	1%	99%
Other	542,033	799,820	431,456	2%	2%	0%	80%
Total	24,621,819	27,958,916	26,532,954				

Notes: The Mean, Standard Deviation, Minimum and Maximum are calculated per precinct.

numbers do not coincide precisely with the initial press release figures printed in *The New York Times*[24] or *USA Today*, for example.[25][26] The press release numbers underreport the outcomes for both candidates and both choices on Proposition 8. The initially reported results were even closer for Prop 8, making discussions about demographics particularly significant. This is primarily due to the fact that numbers reflecting the initial counts are not updated to include all possible remaining ballots (absentee, questionable, and otherwise) that are added to the final tally. The eventual voting count is finalized and certified many weeks after the election, and it is usually a formality as the results are known by this time. (Unless, of course, the close results trigger a recount). Nevertheless, I work with complete election results, which includes all accepted votes.

3.3 Merging

The block-by-block addition of the 2000 Census figures, reported in Table 1.1, matches the official Census count[27] of 24,621,819 people in California who are 18 years of age and over. The 2010 Census[28] summation also matches the official figure of 27,958,916 Californians who are 18 years old and over.

I first merge 2000 and 2010 Census datasets to estimate the population size and demographic breakdown for 2008. I do so by assuming a linear trend of population growth for each block-level demographic group. For each 2000 Census block, I match it with the corresponding 2010 block (or blocks, if there has been any redrawing between censuses), and compute the average of each demographic category for the 2008 population.[29] In this way,

[24]http://elections.nytimes.com/2008/results/states/california.html

[25]http://www.usatoday.com/news/politics/election2008/ca.htm

[26]http://content.usatoday.com/news/politics/election2008/StateDetailResultsByState.aspx?oi=I&rti=G&cn=1&sp=CA

[27]http://www.census.gov/census2000/states/ca.html, http://www.census.gov/census2000/xls/ca_tab_1.xls

[28]http://www.census.gov/2010census/popmap/ipmtext.php?fl=06

[29]If the mapping from a 2000 Census block does not lead to a unique 2010 Census block, I match it to

I can ensure that each block-demographic combination has its own growth rate rather than averaging several growth rates together – for instance, faster growing blocks or demographics with slower growing blocks. In addition, I do not have to worry about computing an unreasonably large population for very fast growing block-demographics since I calculate weighted means of two endpoints as opposed to forecasting into the future.

With my approach, I get a total population of 27,446,193 of California residents for 2008 who are 18 years old and over. The demographic breakdown of the total population is given as: 12,538,434 are White, 1,746,243 are Black, 8,824,512 are Hispanic, 3,886,755 are Asian, and the remaining 450,249 are Other race. Since there is no official census count for 2008 as a comparison, I can instead look at the American Community Survey figures,[30] also conducted by the census. There, the population estimate for 2008 is 27,420,473, which is very close to the figure I obtain. Given that their estimate error is $+/-0.1\%$, it is heartening to see that my estimate falls within their margin of error. I do not use American Community Survey results for the 2008 population as those figures are not broken down to block-level observations, even though they have a very detailed racial breakdown of the population.[31]

I then merge the 2008 population figures with 2008 voting data. As with any merger, I do not get a perfectly accurate matching. I also eliminate small precincts – those with total votes or a total population of less than 100. This ensures that the precinct has a large enough sample size to be treated as a market for discrete choice estimation.[32] I also eliminate precincts where the total vote is larger than the overall population. Finally, I remove the

all the 2010 census blocks that have an area in common with the 2000 block. More specifically, take a 2000 block, with area A. Then take all the 2010 blocks that contain some part of A. If the sum of the areas of all those 2010 blocks is B, and their total population is P, then the relevant 2010 population for the 2000 block will be P*A/B. Thus I assume equal demographic density between neighboring blocks, which provides proportional weight, relative to their areas. With this approach, I ensure correct mapping under various scenarios from 2000 blocks to 2010 blocks, such as 1-to-1, many-to-1, or 1-to-many. The approach then extends naturally to the many-to-many block mappings.

[30] http://www2.census.gov/acs2008_1yr/prod/SelectPopulationProfile/State/California.csv

[31] Including nationalities and ethnicities.

[32] In theory, the market shares are the total population shares, and if a sample size is used, one needs to account for sample size variance as well.

precincts with less than a 5% or over a 95% participation rate. These constitute only a handful of precincts, and such outlier rates come from precinct grouping and not necessarily from population voting choices. It also ensures that the outside alternative of not going to vote is well-defined in my estimation.[33] After merging the three datasets – 2000 Census, 2010 Census and 2008 voting data, I finish with 26,532,519 people for whom I have block-level demographic and precinct-level voting information. I also account for 18,807 of the 25,423 precincts that have a total of 12,445,659 votes, from which 7,536,220 voted for a Democratic president, 4,499,673 for a Republican; 6,361,321 voted Yes on Prop 8, while 5,797,454 voted No. Table 1.2 shows that there is roughly an equal proportional decrease in the number of votes and races as in my matched sample and I get the same aggregate racial composition and voting shares as in the census numbers and the overall voting tally.[34] It is worthwhile to note that I account for the vast majority of the population and the votes, and since I capture the aggregate demographic and voting variation, my results can be extended to the entire population. Plus, given that I have the detailed breakdown of all block-level data, I can map the results for all Californians and estimate voting outcomes not just on the merged data, but for the entire population.

The summary statistics for the 2008 elections in Table 1.1 show the variation of demographics and voting outcomes. Figure 1.1 plots the relationship between the share of each demographic group in the precinct and the percentage of the vote that the Democratic candidate, Obama, received. Figure 1.2 draws the same picture of voter demographics on the share of Yes votes on Prop 8. A few patterns immediately emerge from the figures. For the White population in both votes, the dispersion grows as the share of the White population increases. The 100% (or nearly 100%) all-White precincts have the most variance on voting choices. This indicates no clear preference for either candidate nor choice for Prop 8 by White

[33] I do not eliminate the tails for the voting choices, as I believe it will omit valuable information about demographic choices.

[34] I am currently investigating the cause of imperfect mapping from blocks to precincts, and will possibly raise the matching percentage between the datasets. However, a higher matching rate will not affect the results.

Table 1.2. Population and Sample Shares

Racial Breakdown	White	Black	Asian	Hispanic	Other	
Overall Population	45.7%	6.4%	14.2%	32.2%	1.6%	
Sample	45.2%	6.4%	14.3%	32.5%	1.6%	
Voting Breakdown	Participation	Obama	McCain	Prop 8 Yes	Prop 8 No	
Overall Population	50.1%	61.0%	37.0%	52.2%	47.8%	
Voting Breakdown between Obama/McCain, Yes/No	Pres. Partic.	Obama	McCain	Prop 8 Partic.	Yes	No
Population	48.4%	62.3%	37.7%	48.8%	52.2%	47.8%
Sample	48.1%	62.6%	37.4%	48.5%	52.3%	47.7%

voters. Such a dispersion can be accounted for by the unobserved consumer characteristic. The model setup will still enable me to have different preferences for minority demographics, which can differ from each other as evident from the figures.

The graphs for the minority demographics show a completely different picture. Figures 1.1 and 1.2 depict a generally favorable preference both for Obama in the presidential election and for a Yes vote on Prop 8. Some other interesting points merit discussion. In the Prop 8 figure, the shares of Yes in largely Black precincts have roughly the same mean as the shares in largely Asian and Hispanic precincts. In the presidential election, there is a clear favorable bias toward Obama by Black, Asian, and Hispanic demographics.[35] The Hispanic and Black populations both approach close to 100% as their precinct shares increase, with the Black population having a smaller variance for moderate to high shares. It is encouraging to have this confirmed in the raw data, as conventional wisdom dictates that Blacks were strongly in favor of Obama.[36] Later in the estimation, however, this causes problems for the discrete choice estimation with no unobserved voter characteristic, as the

[35]To get the overall impact of demographics over preferences, the entire distribution is needed.

[36]Though probably not close to 100%.

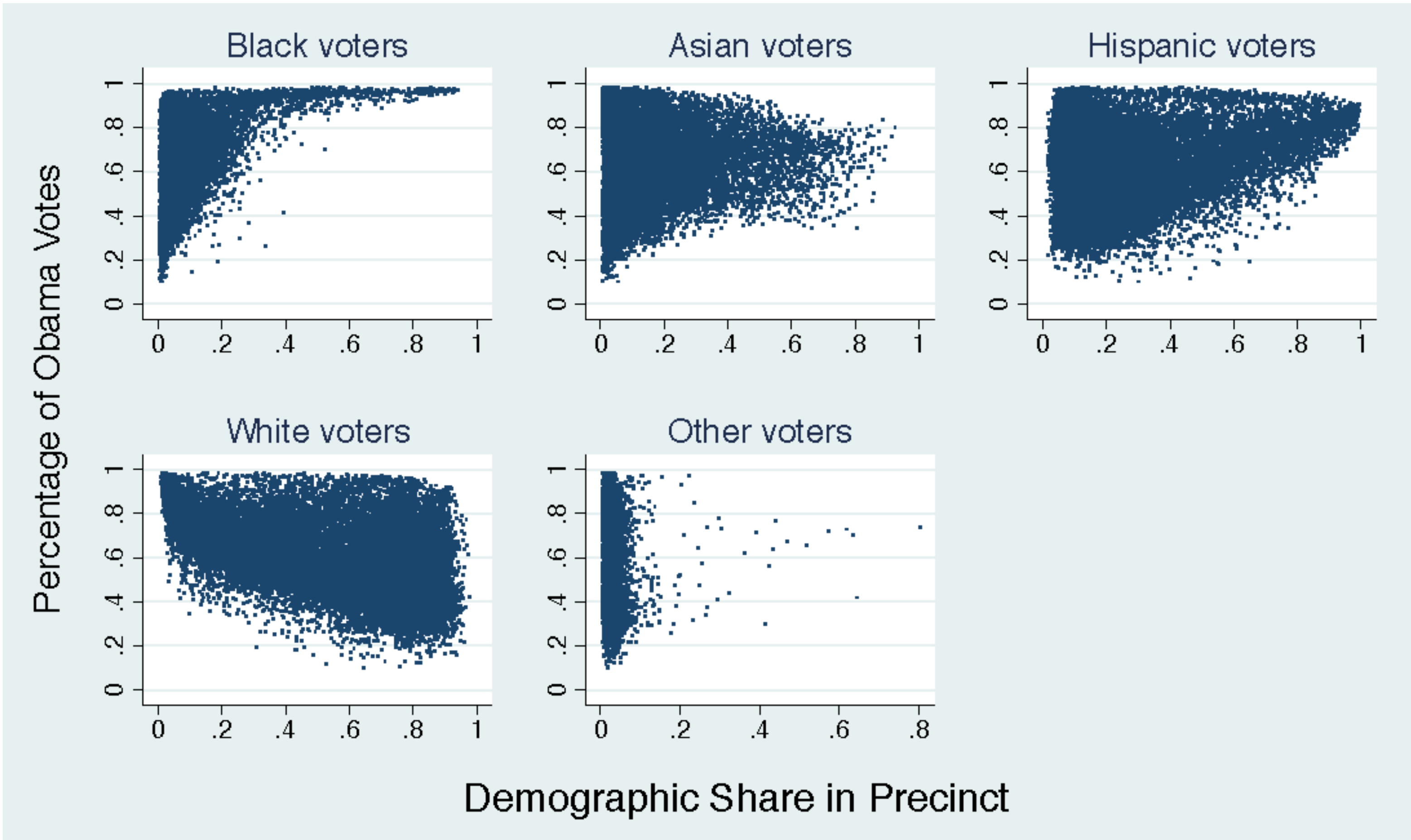

Figure 1.1: Share of Democratic Presidential Candidate on Demographic Shares

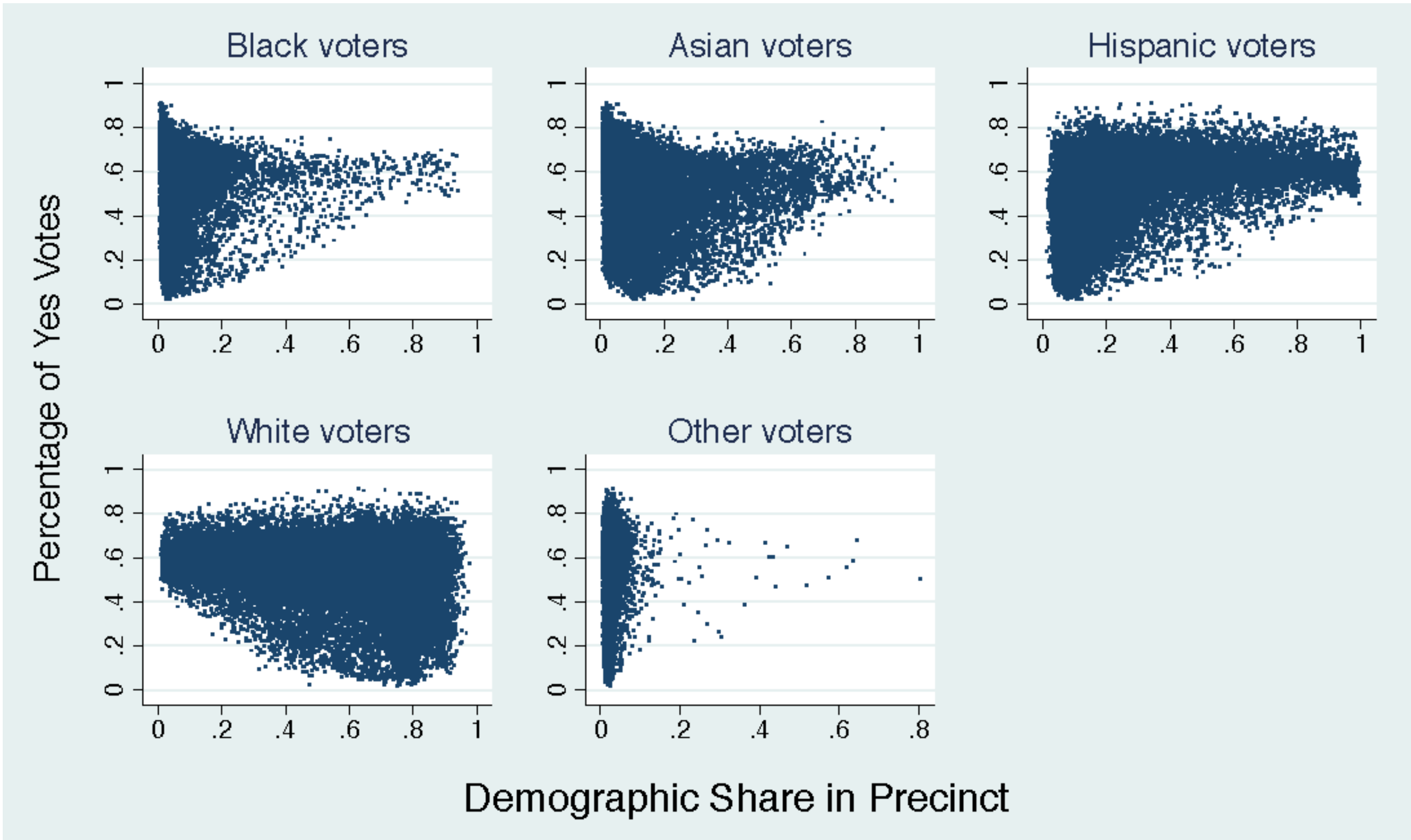

Figure 1.2: Share of Proposition 8 Yes Votes on Demographic Shares

coefficient for Blacks is undefined (or rather approaches positive infinity).[37] For the Other demographic, there is not enough variation at high levels to obtain reasonable preference estimates. In the estimations, I also find very low participation by Other voters. Therefore, I combine the Other demographic with the White demographics, and treat this as the baseline demographic.

Figures 1.3 and 1.4 provide a mechanical intuition of why exit poll results might have provided incorrect results for the subpopulations of different demographics. At the heart of the issue is the question of whether there is reason to believe that the exit polls could provide incorrect predictions for demographic preferences over ballot choices. When the exit poll raw numbers are aggregated, they are weighted to give, on average, correct predictions for the entire population over the elections.[38] As such, by restricting the sample to a subpopulation – such as certain demographics – the outcomes might be strongly weighted in one direction or another, and the results for that subset could be swayed in either direction while being correct for the entire population.[39]

The figures show the plot of the imputed exit poll versus actual voting numbers for each precinct. Figure 1.3 does this for the Going to Vote share, and Figure 1.4 for the share of voting Yes on Proposition 8. For each precinct, I calculate the expected number of total voters and Prop 8 Yes votes based on the exit poll ratios. I then subtract the actual number of votes and Prop 8 Yes votes. The data points are then plotted by first being grouped into 20 equal-sized bins, based on the share of the demographic percentage in a precinct. I also fit a linear model. Looking at the Going to Vote share, it is evident that exit polls overstate the number of Blacks and Hispanics who went to the polls and understate the number of Whites and Asians than actual numbers indicate. The exit polls suggest a higher participation in precincts with large Black or Hispanic populations than the actual turnout. Similarly, they

[37]I have also analyzed the results of the 2004 election and the optimization does not suffer from this problem.

[38]http://www.ncpp.org/drupal57/files/Weighting.pdf

[39]That is partly why the margin of error for the subpopulation is usually larger than for the entire sample, and it is possible for the estimates not to conform to the actual results, even after accounting for the error.

show lower turnout in overwhelmingly White or Asian precincts compared to actual election results. As for Prop 8 votes, there is again overstatement of the Black percentage of votes than is borne out in the election results. As the raw data indicates, the exit poll figures seem to overstate both the Black participation and preference for voting Yes on Prop 8; this is what I find in my own estimation, as well. They also seem to suggest understatement of Hispanic voting preferences on Yes for Prop 8, which is also borne out in the results.

4 Model

4.1 The Benchmark Model

The main obstacle to using discrete choice setup for the voting data is that there is one market in the discrete choice model. On the other hand, if the voter decides to participate, she chooses one candidate or ballot choice in all the elections taking place at the time. Further complicating matters is the fact that voting results are reported by each race (or "market") instead of combinations of races. For instance, suppose there are only two elections on the ballot – for president and governor, and only two candidates in each race – Democrat and Republican. The reported election results tell only how many people have voted for a Democrat in the presidential election, and how many have voted for a Democrat in the gubernatorial election, but not how many have voted all Democrat – (Democrat, Democrat) – in both races.[40]

One approach might be to treat each election race count, along with the outside option numbers, as its own individual market. This, however, does not conform to the individual utility maximization, as presumably the voter makes the choice to go to an election by considering her utility for all the races taking place, and not just from a specific election. Moreover, there is no clear, intuitive way to combine the results to obtain voting counts for say, the (Democrat, Democrat) option that is derived from preferences.

[40]Such cross-tabulation of results is usually found in polls and surveys only.

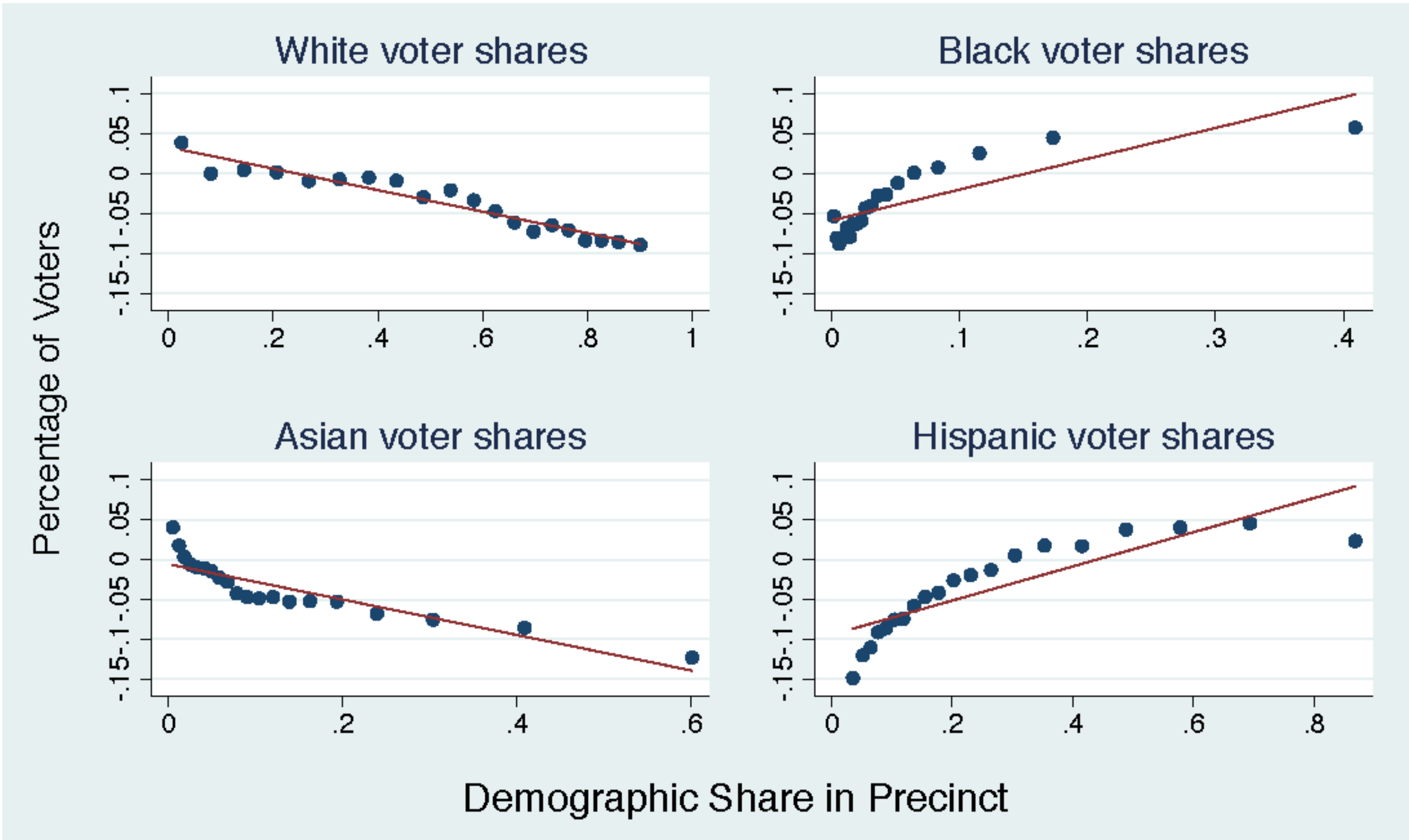

Figure 1.3: Difference in Share of Participation on Demographic Shares

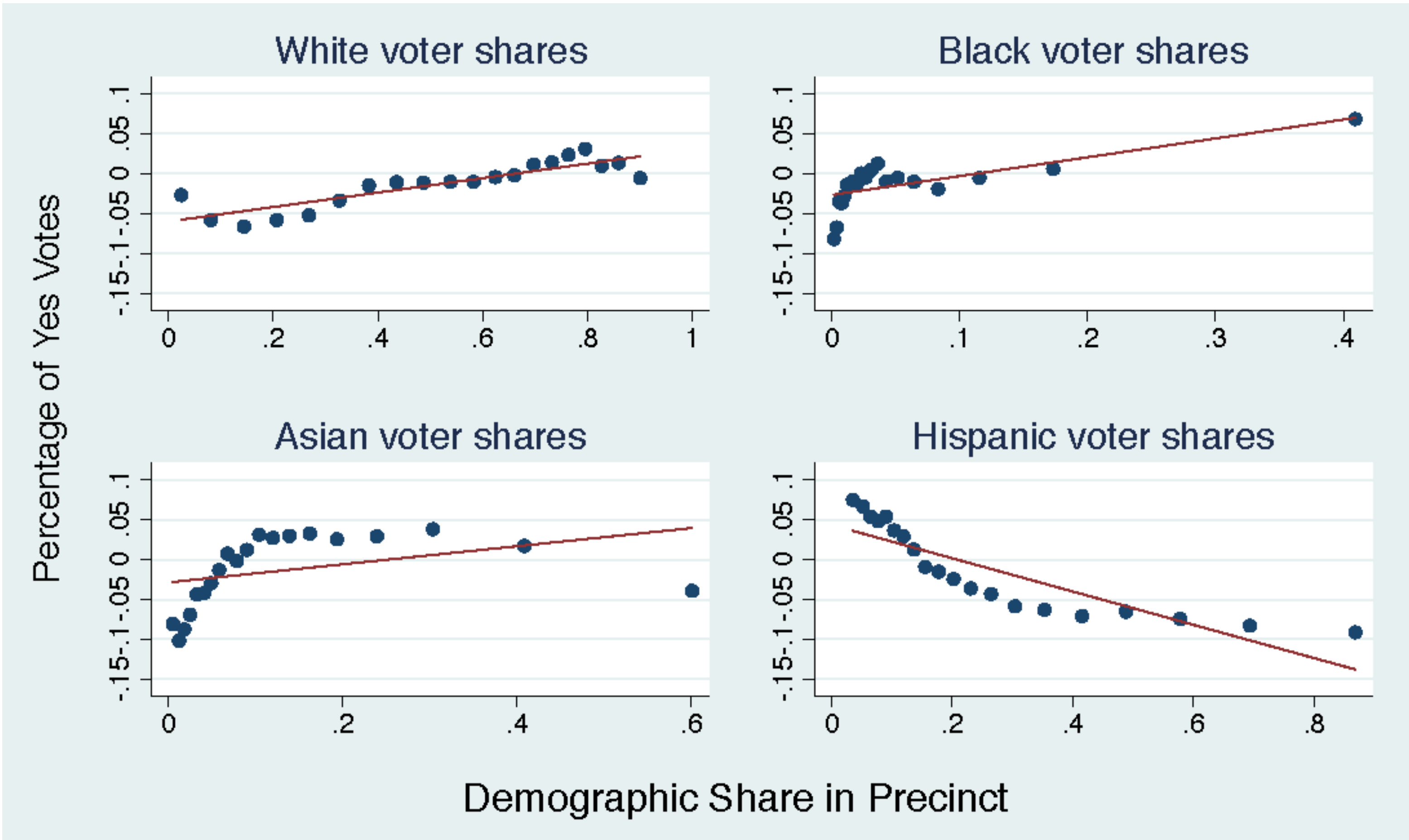

Figure 1.4: Difference in Prop 8 Yes Votes on Demographic Shares

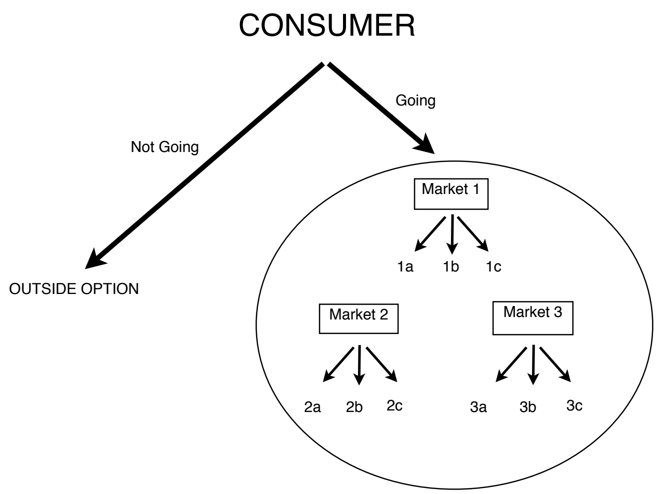

Figure 1.5: The Decision Tree

I present a nested logit specification of voter demand. The framework allows me to model the decision of simultaneously choosing products from multiple markets. Moreover, my analysis remains consistent with individual utility maximization, and does not require additional assumptions or conditions beyond what is imposed by the discrete choice literature.

For the setup of the model, suppose there are $m_1, m_2, ..., m_L$ markets, each offering $J_1, J_2, ..., J_L$ number of products. Without loss of generalization, I assume that the outside option is common to all markets. That is, if the consumer chooses to participate in one of the markets, then she will participate in all markets. Alternatively, I can introduce an outside option to the product space for each individual market and eliminate the aggregate outside option, as then the aggregate outside option will be the joint union of all individual outside options. Figure 1.5 presents the graphical tree representation of the consumer decision.

The consumer i chooses the products $c_{j1}, c_{j2}, ..., c_{jL}$ in each of the markets $m_1, m_2, ..., m_L$, respectively. The number of possible combinations for the bundle of products is $J_1 \times J_2 \times ... \times J_L + 1$ and in the ideal setup, I would have the actual market shares for each combination matched with the model's predicted market shares.[41] However, in the voting data (or in a general macro-level demand data), I only have the shares of the outside good and the goods in each of the markets m_t individually, and not the combination. This gives me a total of $J_1 + J_2 + ... + J_L + 1$ observable market shares. Since in general, $J_1 + J_2 + ... + J_L + 1 << J_1 \times J_2 \times ... \times J_L + 1$, I will not have enough identification to back out $J_1 \times J_2 \times ... \times J_L + 1$ market shares without additional assumptions. I show that the discrete choice framework is sufficient to obtain all the shares of the possible combinations.

My discrete choice logit consists of two loops. On the outer loop, the voter decides whether to go to vote, or not. Conditional on going to the election, I then have the inner loop, where the voter i makes the choice for her preferred candidate for each election. For a given election or market m_t, the inner loop reduces to a standard discrete choice. In particular, let the possible products have mean utilities $\delta_1, ..., \delta_{J_t}$, the individual characteristics of the consumer are $\nu_{i1}, ..., \nu_{ip}$ and the product characteristics of j are $x_{j1}, ..., x_{jq}$. Let k enumerate the relevant consumer and producer characteristic interactions in market m_t from the maximum possible pq pairs. Then, the utility for choosing product j will be:

$$u_{ijt} = \delta_{jt} + \sum_k \alpha_k \nu_{ik} x_{kj} + \varepsilon_{ijt}$$

where ε_{ijt} is i.i.d. Type-I extreme value error term. Note that the mean utility term δ_{jt} includes all product characteristics, including the unobserved product characteristic.

In each market m_t then the consumer chooses the product with the highest utility. Since ε_{ijt} is i.i.d. Type-I, the optimization can be integrated out with a closed form solution for the probability. The probability of choosing good s is:

$$\Pr(jt = s) = \frac{\exp\left(\delta_s + \sum \alpha_k \nu_{ik} x_{ks}\right)}{\sum_{r=1}^{J_t} \exp\left(\delta_r + \sum \alpha_k \nu_{ik} x_{kr}\right)} \tag{1.1}$$

[41] Even with that ideal setup, there can also be an issue of how to specify the utility as a function of the bundle, if the product characteristics enter non-linearly into the mean utility.

These probabilities are then computed for all the products in all the markets. For the integration to get market shares, the standard assumption in the discrete choice literature is used, that ε_{ijt} are independent across products and consumers. I can go one step further and assume that ε_{ijt} are also independent across markets. This assumption is rather innocuous as the error terms for products within a market are more likely to be correlated than the error terms for products across markets.[42] Then, since each market product choice is independent from other markets, the probability of selecting a particular bundle $jt_1 = s_1, ..., jt_L = s_L$ will be a product of individual probabilities:

$$\Pr(jt_1 = s_1, ..., jt_L = s_L) = \frac{\exp(\delta_{s_1} + \sum \alpha_k \nu_{ik} x_{ks_1})}{\sum\limits_{r_1} \exp(\delta_{r_1} + \sum \alpha_k \nu_{ik} x_{kr_1})} \cdot ... \cdot \frac{\exp(\delta_{s_L} + \sum \alpha_z \nu_{iz} x_{zs_L})}{\sum\limits_{r_L} \exp(\delta_{r_L} + \sum \alpha_z \nu_{iz} x_{zr_L})} \quad (1.2)$$

The difference in the notation between k and z allows the possibility that in different markets different consumer-producer characteristic pairs might be relevant. They would also enter the individual utility specification with different magnitudes, specified by α. This probability is computed for each individual and each market combination. The aggregate market share is computed by integrating over all the individuals in the market, which is equivalent to integrating over the distributions of all consumer characteristics. This gives the market shares for each of the $J_1 \times J_2 \times ... \times J_L$ product combinations that represent all possible shares conditional on participating in the market.

4.1.1 The Participation Constraint

The final stage is to bring back the decision to participate or abstain from Figure 1.5, and compute the model predicted probabilities of both decisions. To do this, note that the

[42] If needed, there is an option to add correlation for the error terms across the products, similar to Berry (1994), and Cardell (1997). Another way to interpret the standard discrete choice assumption is that only the product characteristics, along with the interaction of product and consumer characteristics determine the correlation between product choices.

probability shares for the combination of the goods can be rewritten as:

$$\Pr\left(jt_1 = s_1, ..., jt_L = s_L\right) = \frac{\exp\left(\delta_{s_1} + ... + \delta_{s_L} + \sum \alpha_k \nu_{ik} x_{ks_1} + ... + \sum \alpha_z \nu_{iz} x_{zs_L}\right)}{\sum\limits_{r_1 ... r_L} \exp\left(\delta_{r_1} + ... + \delta_{r_L} + \sum \alpha_k \nu_{ik} x_{kr_1} + ... + \sum \alpha_z \nu_{iz} x_{zr_L}\right)} \tag{1.3}$$

This expression can be treated as if being a solution to a discrete choice random coefficients model, with mean utilities of $\delta_{s_1} + ... + \delta_{s_L}$, for the product combination of $\{s_1, ..., s_L\}$, and the bundle's product characteristics being the union of individual product characteristics: $\bigcup\{x_{ks_1}, \ldots, x_{ks_L}\}$. The consumer characteristics set is also the union of all consumer characteristics, if not all consumer attributes enter in all product utilities. Looking at the probabilities this way, the solution in equation (1.3) can be represented as the solution in equation (1.1) to a standard discrete choice optimization problem. The joint selection of individual products in different markets is therefore equivalent to a single discrete choice decision for the entire bundle, with the mean utility of $\delta_{s_1} + ... + \delta_{s_L}$, and product attribute set of $\bigcup\{x_{ks_1}, \ldots, x_{ks_L}\}$.

To finalize the model, I also need to add the leftmost branch of the decision tree: whether or not to participate in the market. Having established the selection of product bundles as a familiar discrete choice optimization, adding another "product" in the choice set can still be shown as an expression like in equation (1.1). In the discrete choice setup, the maximum of all error terms, net of mean utilities and interaction terms (i.e. the error term that gives highest utility), will also have Type-I extreme value distribution (Cardell 1997). If $\{s_1, ..., s_L\}$ is the maximizing combination, then the decision between choosing that, or not participating at all entails a choice between two terms with the same Type-I distributive error terms. Once again, the solution to this discrete choice setup will have the familiar expression for the probabilities of the shares. A general combination of product probabilities, including the outside option to the choice set amounts to adding the expression $\exp\left(\delta_0\right)$ to the denominator of equation (1.3). Alternatively, I scale all the inner choices of participating by δ_g, called the utility of "going", which will again normalize the outside option's mean utility to 0, as is customary in the literature.

The entire decision tree of the consumer can be computed as a two-stage estimation. First, one would estimate the decision to go or to abstain. This will provide the mean utility of going, δ_g. Then, conditional on going, one would estimate the individual market shares: $\{\delta_{s_1}, ..., \delta_{s_L}\}$. The mean utility of the bundle, $\{s_1, ..., s_L\}$, will then be: $\delta_{s_1} + ... + \delta_{s_L} + \delta_g$, with the product attribute set being the union of all the markets' product attributes. Such a setup provides correct within and participation shares. Moreover, one can do counterfactual analysis if the underlying conditions in the decision tree, or in any of the markets, change.

4.2 Moment Conditions

The typical identification for the logit specification is again problematic in my case. At the macro level with only market shares available, variation in product choices and product characteristics (price, in particular) across markets is used to set up the appropriate moment conditions. This is not applicable in my case. In the California elections I analyze, the same choices are on the ballot in each precinct. In the voting literature the candidate/product characteristic is typically constant across markets, unless it includes region-specific campaign spending (Rekkas 2007, Gordon and Hartmann 2013, and Martin 2013). Even with detailed candidate characteristics, in my voting scenario the product dummies will encompass all other individual characteristics. Therefore, I do not even "open up" the mean utility term δ_{jt}, and instead estimate it in its entirety.[43]

I use the variation in aggregate consumer characteristics across markets to get identification. Taken at face value, the mean utility term in the logit specification is composed of only product characteristics and should be orthogonal to individual consumer characteristics. By aggregating the consumer characteristic over the market, I obtain the market demographic distribution for that characteristic. The orthogonality of the distribution term with the mean utility term still holds from the independence of individual characteristics.

[43]micro-BLP (2004) also takes a similar approach.

This provides the basis for my estimation. That is, if $\delta_{jt} - E_j(\delta_{jt})$ is the mean utility of product j in precinct t, normalized to have mean 0 across all precincts, and w_t is the distribution parameter of consumer characteristics, (e.g., the share of whites in precinct t), then:

$$E\left(\left(\delta_{jt} - E_j\left(\delta_{jt}\right)\right) w_t\right) = 0 \tag{1.4}$$

The orthogonality applies to all products and all consumer demographic components. Since the model assumptions call for complete independence, one may also use higher order terms for distribution components.

The above procedure appears as if the mean utility is treated like an error term. Viewed this way, my moment condition takes the shape of a standard econometric assumption of identification around the error distribution. In fact, such an approach is not far fetched. In traditional logit specification, the mean utility term is split into two components: observed and unobserved product characteristics. The unobserved product characteristic then serves as the error term, around which the moment conditions are built. I treat the entire mean utility, net of mean, as an unobserved product characteristic and form the moment conditions accordingly. One can still add additional information in the δ_{jt} term, such as fixed effects. I do that by including county dummies, which restricts the variation to within-county deviations.

5 Estimation

5.1 Going to Vote

I compare my benchmark model with a parsimonious model that incorporates some of the same qualities as the logit model, and computation is only OLS. The drawback of the parsimonious model is the inability to incorporate unobserved consumer characteristics. Also, it is not micro-founded, and therefore the estimation can be treated as ad hoc. When there is only one racial demographic in the precinct, and no other consumer characteristic including unobserved consumer characteristic, the shares can be expressed in terms of mean-utility as:

$$\ln s_{jt} - \ln(1 - s_{jt}) = \delta_{jt} + \alpha_r$$

where α_r is the utility contribution of race r. For all possible demographies, the more general specification can be written as:

$$\ln s_{jt} - \ln(1 - s_{jt}) = \delta_{jt} + \sum \mathbf{1}_r * \alpha_r$$

where $\mathbf{1}_r$ is indicator for race r. I can then extend it for the fractions of races. That is:

$$\ln s_{jt} - \ln(1 - s_{jt}) = \delta_{jt} + \sum s_{rt}\alpha_{rt} \tag{1.5}$$

It is important to note that this is not a theoretically correct specification, as the individual shares need to be aggregated first before applying the logit transformation rather than doing aggregation over the logit transformation of the shares.

Written this way, the mean utility δ_{jt} once again becomes the error plus the constant term. Table 1.3 presents the specification, OLS, along with the Discrete Choice model. The Discrete Choice model estimation is done two ways: one without unobserved consumer characteristic, and the other – the Full Model – with the unobserved characteristic present. In the OLS, the dependent variable is not (log of) share, but the logistic transformation. I have included county fixed effects in all specifications to restrict the mean utility deviations to be among precincts within a given county. All the coefficients are precisely estimated. This is due in part to having a large number of markets – the precincts. The negative coefficient of all the regressed demographics indicates that Whites have the highest probability of voting. Other minority races are less likely to participate. The positive constant coefficient shows that Whites are more likely to vote than not to vote. This is not surprising, since they

Table 1.3. The Effect of Demographics on Going to Vote

	OLS	*ν = 0*	*OLS*	*ν = 0*	*Full Model*
Black	-0.729**	-0.955**	-0.730**	-0.891**	-0.961**
	(0.0426)	(0.122)	(0.0437)	(0.115)	(0.074)
Asian	-1.637**	-1.694**	-1.470**	-1.538**	-1.6951**
	(0.0293)	(0.051)	(0.0296)	(0.047)	(0.046)
Hispanic	-2.759**	-3.082**	-2.664**	-2.973**	-2.986**
	(0.0193)	(0.049)	(0.0196)	(0.047)	(0.03)
Other	-7.141**	-14.635**			
	(0.222)	(5.42)			
ν					0.067
					(0.0434)
Const	1.355**		1.184**		
	(0.0101)		(0.00879)		
N	18807	18807	18807	18807	18807
R-sq	0.536		0.511		

Notes: Standard errors are in parentheses. + Significant at 10%; * Significant at 5%; ** Significant at 1%.

constitute the majority of the population and roughly 50% of Californians aged 18 and over (75% of the registered voters) voted.[44]

For the remaining demographics, the sum of the constant and the beta coefficient determines their likelihood to vote. The average likelihood of voting doesn't necessarily imply the average ratio of voting share, as the latter would depend on precinct populations and the mean utility terms. In my case, all other demographics are less likely to vote than not to vote. This is perhaps a surprising finding for the Black population, as the media consensus was an historic turnout of Black voters. Even though their turnout is higher than that of Asians, Hispanics, and Others, I find that it is much less than that of White voters. More than half of the Black population living in California, though a lesser percentage of

[44] http://www.sos.ca.gov/elections/sov/2008-general/sov_complete.pdf

registered Black voters, stayed home during the election. For a robustness check, I also compare the Black turnout in 2008 to the turnout in the previous presidential election of 2004.

The Other population has a large negative coefficient, suggesting that this group is not made up of active voting participants. Their low participation translates to a statistically very small contribution to the subsequent analysis of the presidential election and Prop 8. I therefore combine the Other population with the White demographics. The estimates remain roughly identical from this inclusion. I also tried including the Other population with other demographics (e.g., the Black or Hispanic population) and the results again do not change. I include the Other race with the White population as their total population will form the baseline demographics to which the other demographic preferences will be compared. The analysis is still robust despite the fact that, out of all demographics, the Whites are the most likely to vote, and the Other population is the least likely to vote.

The Hispanic population also has a large negative coefficient of participation. This translates to Hispanics having a smaller share in total vote participation than the Asians, even though there are three times more Hispanics than Asians in the population count. This is consistent with many Hispanic residents not being eligible to vote, and it is heartening to see the estimation picking up this effect.[45] It is interesting to note that the R-squared in the OLS regression is almost 54%. Just the demographic component explains over half of the variation in participation to vote. Adding the unobserved consumer characteristic will improve the fit of the model, as is also evident from the figures.

When comparing the imputed probabilities of going to vote across demographics to the poll numbers, as shown in Table 1.4, I find that the polls overestimate the participation of Black and Hispanic voters, and underestimate the participation of White and Asian voters. This is consistent with the intuitive results from Figure 1.3, and also lessens the magnitude

[45]The large negative coefficient of Asian voters conforms with the anecdotal evidence that many remain permanent residents and do not become citizens.

Table 1.4. Estimated Share of Voters by Demographics

	Discrete Choice	*CNN Poll*
White	70%	66%
Black	7%	10%
Asian	12%	6%
Hispanic	11%	18%

of Black voters' impact on Prop 8. To calculate the overall impact, I also need to calculate the preferences over elections ballots, which I turn to next.

5.2 Presidential Election and Prop 8

I next calculate the shares for the two elections – Presidential race and Prop 8. Tables 1.5 and 1.6 report the results. The nested logit specifices the relevant demographic shares as the population of voters, and not the entire population of residents. In the estimation for presidential election with no unobserved consumer characteristic, the preference for Black voters for Democratic candidate, Obama, was problematic. As evident from Figure 1.1, the share of votes for Obama quickly approaches 100% as the share of Black voters increases. The precinct-specific mean utilities, δ_{jt}, are free-form to pick up any precinct-wide excess preferences. However, even such general specification is not enough to account for the strong preference of Black voters for Obama. In the optimization, the coefficient would approach infinity. This is especially problematic for calculation, as in the discrete choice setup, it would involve taking the exponent of a very large number. To partly offset the computations, I instead compute the share of the Republican candidate, McCain. This still produces large negative numbers for the Black population, but allows for the optimization to converge.[46]

[46] The computer handles $e^{-\infty}$ better than e^{∞}.

Table 1.5. The Effect of Demographics on Presidential Candidate Choices

	OLS	$\nu = 0$	*Full Model*
Black	-4.008**	-19.2112	-7.1176**
	(0.0423)	(598835.8)	(0.2949)
Asian	-0.950**	-1.09778**	-0.76015**
	(0.0384)	(0.0294)	(0.0635)
Hispanic	-1.245**	-1.77063**	-.92331**
	(0.0320)	(0.0238)	(0.2695)
ν			-0.3527**
			(0.0956)
Const	-0.0713**		
	(0.00852)		
N	18807	18807	18807
R-sq	0.391		

Notes: The dependent variable is the share of votes for the Republican Candidate (McCain). Standard errors are in parentheses. + Significant at 10%; * Significant at 5%; ** Significant at 1%.

The large numbers are still problematic in calculations for standard errors, as the multinomial standard errors are divided by the square of the shares, thus making the variance for the Black coefficient especially large. The Full Model specification does not suffer from such estimation issues, in part because even with strong preference, the presence of another random variable reduces the choice probability to strictly between 0 and 1.

In the presidential election, the preferences for Black, Asian and Hispanic demographics are strongly for Obama. It is interesting to note that, with the correct specification of demographic shares, I show all races being pro-Obama. This is consistent with the CNN poll findings. The R-squared in the OLS is almost 40%, indicating that demographics alone

Table 1.6. The Effect of Demographics on Proposition 8 Choices

	OLS	ν = *0*	*Full Model*
Black	0.281**	0.32022**	0.8764**
	(0.0409)	(0.022691)	(0.0981)
Asian	-0.0482	-0.03363+	-0.43205**
	(0.0371)	(0.022176)	(0.1472)
Hispanic	1.176**	1.1973**	1.3534**
	(0.0309)	(0.025678)	(0.04438)
ν			0.7085**
			(0.01068)
Const	-0.0567**		
	(0.00823)		
N	18807	18807	18807
R-sq	0.079		

Notes: The dependent variable is the share of Yes votes on Proposition 8. Standard errors are in parentheses. + Significant at 10%; * Significant at 5%; ** Significant at 1%.

Table 1.7. Imputed and Poll Probabilities of Voting Yes on Prop 8

	Discrete Choice	*CNN Poll*
Black	57%	70%
Asian	49.1%	49%
Hispanic	74.4%	53%
White	48.5%	49%

is a strong enough predictor for preference for the presidential candidate.

From Table 1.7, the Prop 8 estimate predictions on the voting preference match almost exactly with the CNN poll estimates for Asian and White demographics. They are both slightly in favor of voting No on Prop 8. For the Black and Hispanic demographics the results, however, vary from the CNN poll. Similar to the poll results, I find strong preference of both demographic groups to vote Yes on Prop 8. The magnitudes however, are different from the polls. I find that the Black voters are less inclined to vote Yes on Prop 8, and the Hispanics are, in turn, more inclined to vote Yes, than is suggested by the poll results. This finding, combined with the fact that more Hispanic voters participated in the election than did Black voters, suggests that they were more pivotal in the passing of Prop 8 than the Black voters.

5.3 Robustness and Counterfactual Analysis

As a robustness check, I also analyze the 2004 voter turnout and presidential elections in California. Table 1.8 reports the results. The coefficients for Black voter turnout are very similar to the 2008 estimates. This shows that Black voter turnout, relative to Whites, was not higher in 2008 compared to the prior presidential election. Of course, that can still be consistent with the fact that more Black voters took part in the 2008 election, if more people in general, including Black voters, participated in the elections in 2008. This can be seen by a larger constant coefficient, implying that more baseline White voters went to vote in 2008. Convergence for discrete choice setup with no unobserved characteristic is no longer an issue for the 2004 results, owing to the fact that 2004 Democratic nominee Kerry was not as popular among Blacks as was Obama. Another interesting finding is the larger support for the Republican nominee by the Hispanic demographics in 2004 than in 2008. This is consistent with the anecdotal evidence that 2004 Republican nominee Bush was more popular among Hispanics than subsequent Republican candidates, McCain and Romney.

Would Prop 8 have passed without a presidential election on the same ballot? The setup's main advantages include counterfactual analysis: having a micro-founded framework allows one to estimate differences when the underlying components change. By analyzing going to multiple elections, one can look at alternative consumer behavior when some of the possibilities are eliminated. I look at the voter turnout and the total votes for Proposition 8, without the presidential election on the ticket. The setup would be similar to voting on Prop 8 during a midterm election year. I find that voter turnout is lower – at 40.8%. This is much less than the reported turnout of close to 50%, and is more in line with midterm election turnout. This further signifies the important role that presidential elections play on turnout, and it is encouraging to see reasonable estimates from the counterfactual. It is important to note that such analysis would not be possible with the single election framework, as each election is independent from others, meaning that results will not change with elimination (or addition) of different elections.

More interestingly, I also find that Proposition 8 would gather only 49.3% of the vote. It would, therefore, most likely fail to pass. The results are driven by participation in elections, which is explicitly modeled and accounted for in my setup. Certain minorities, such as Blacks and Hispanics, were in favor of both Obama and Proposition 8. Eliminating the presidential ticket on the ballot would drop the mean aggregate utility from elections below the participation threshold for some of them. If previously, they had a strong preference to vote for Obama and Proposition 8 versus not going to vote, their decision is now reduced to voting only on Proposition 8 versus not going to vote. A share of the population who have participated before would therefore stay home on election night. I find that enough people in favor of Prop 8 abstain from voting, compared to those voting No on Prop 8, which tips the scale against passing Proposition 8.

6 Conclusion

The paper develops a methodology to impute bundles of product choices from only the aggregate market shares. I also provide identification when there is no product variation across markets. I show my estimation is consistent with BLP assumptions of discrete choice analysis. I use the methodology to analyze the voting outcome for 2008 California elections. More specifically, I look at the voting preferences on presidential election and Prop 8 ballots, for each demographic group. I find that the participation of the Black population is largely exaggerated in the media, based on the exit polls, and they were more likely not to go to vote than to vote. Their results are largely consistent with the estimates from the 2004 General Election. There is a very strong preference of the Black voters for presidential candidate Obama, consistent with the poll results. However, I find that the preference of the Black population for choosing Yes on Prop 8 is not as high as suggested by the polls. The Hispanic population is more likely to vote Yes, than the Black population. This, coupled with the fact that more Hispanics voted in the 2008 election that did Blacks, suggests that the role of the Black population in helping pass Prop 8 is largely exaggerated.

Further extensions to this paper would be to expand the model to include possible correlations between different markets. In my setup, I can include a parameter σ that shows the correlation between each of the choices of the nest. The implementation is straightforward and allows an arbitrary level of substitution between choices (Berry 1994, Cardell 1997, Einav 2007). One reason is to see how substitutable election choices are relative to each other and

Table 1.8. Demographics on Going to Vote and Presidential Choice (2004)

	Going to Vote			*Presidential Election*		
	OLS	$\nu = 0$	*Full Model*	*OLS*	$\nu = 0$	*Full Model*
Black	-0.708**	-0.864**	-0.9185**	-1.789**	-4.742**	-6.26**
	(0.0425)	(0.0866)	(0.0606)	(0.0317)	(0.16)	(0.398)
Asian	-1.302**	-1.35**	-1.562**	-1.056**	-1.38**	-1.322**
	(0.033)	(0.045)	(0.0433)	(0.033)	(0.036)	(0.069)
Hispanic	-2.809**	-3.25**	-3.175**	-0.629**	-1.614**	-1.202**
	(0.0213)	(0.055)	(0.041)	(0.0127)	(0.0232)	(0.039)
ν			-0.045*			-0.422**
			(0.018)			(0.0875)
Const	1.022**			1.406**		
	(0.0088)			(0.0055)		
N	18307	18307	18307	18307	18307	18307
R-sq	0.624			0.469		

Notes: Standard errors are in parentheses. + Significant at 10%; * Significant at 5%; ** Significant at 1%.

across elections. In American politics, there is evidence that such strong preferences do not constitute the majority of the electorate. A large number of voters are in fact independent, and election campaigns are mainly targeted to attract those votes. Incorporating correlation can provide an empirical estimation of voter polarization, as my model can be set up to account for the correlation through the σ coefficient.

Perhaps an even more far-reaching analysis would be to extend the voting outcome on gerrymandering. The unique nature of gerrymandering is that voting districts are constructed by the state legislature to conform to certain requirements, such as the Voting Rights Act (1965). The general permission to pattern in any specific shape has resulted in some uniquely shaped districts. Gerrymandering is typically done at the state level, by state legislatures. Voters, however, mostly go to elections to vote for national candidates. My analysis can help answer the question of the extent that gerrymandering is affected by such a setup. Also, how do local candidates optimize their electability through gerrymandering, knowing that voter turnout is largely driven by more national candidates and issues? This is left for future work.

Chapter 2

Do Policies Affect Preferences? Evidence from Random Variation in Abortion Jurisprudence (joint with Daniel Chen and Susan Yeh)

1 Introduction

Do government policies shift policy preferences? This question is important to optimal policy design and central to political economy. Social scientists have long speculated about the role of laws in motivating broader societal change. Yet little population-based causal evidence exists on why people obey the law; whether it is because legal sanctions alone motivate behavioral changes, as in a classical economic framework, or because the law psychologically motivates behavioral changes through moral or expressive messages. To date, behavioral theorists have focused primarily on the expressive effects of public policy,[47] where laws that express societal values draw people's preferences closer to the social norm.[48] Yet an extensive literature, largely anecdotal or qualitative in nature (see, e.g., Roe 1998 and Klarman 2005),

[47] A notable exception is Benabou and Tirole (2011).

[48] See Benabou and Tirole (2011) and the references therein for a theoretical and empirical literature.

has linked policy backlash to almost every policy.[49] Formal theory is ambiguous as to the effect of government policy on individuals' policy preferences. Our analysis provides causal evidence for understanding why some normative arrangements are considered repugnant, and may help in policy design (Mankiw and Weinzierl 2010).

Little empirical work using naturally occurring data has addressed when and why law has expressive or backlash effects, despite their frequent use in justifying one law over another.[50] We define backlash in the policy context as causing people's preferences to shift away from what the law expresses. Our model allows for the possibility for both backlash and expressive effects taking place in society. As is borne out in the data, we find initial backlash effects to appellate decisions, followed by expressive effects. There are two mechanisms through which actions are controlled: external (exogenous) probability, which is determined by laws and establishments, and internal (endogenous) probability, which is determined by the perceptions the agent takes towards actions. The dynamics of such a setup allows for both backlash and expressive effects to be displayed over time.

Persuasive empirical evidence on how policies affect preferences has been limited, partly due to the difficulty of identifying policy shocks that are exogenous. We introduce an instrumental variables approach to these problems and apply our method in the context of abortion regulation.[51] As prima facie evidence of the possibility of backlash, consider that the number of state abortion restrictions have increased over time since the landmark Supreme Court *Roe v. Wade* decision.[52] A time-series or panel analysis is limited because legal decisions are likely endogenous to social changes. We address this issue by focusing on court decisions in US appellate courts, which determine a significant portion of cases that

[49]For a sample, see: voter mobilization (Mann 2010), multiculturalism (Mitchell 2004), environmentalism (Wolf 1995), private infrastructure investments (Lopez et al. 2009), health care (Mechanic 2001), abortion (Pridemore and Freilich 2007), Americans with Disabilities Act (Krieger 2000), globalization (Eckes 2000), Warren Court (Feld 2003).

[50]See Funk (2010) for an exception.

[51]Chen and Yeh (2012) examines the impact of obscenity law on preferences.

[52]http://www.washingtonpost.com/business/economy/the-state-of-roe-v-wade-in-9-charts/2012/01/23/gIQAXo6XLQ_gallery.html?hpid=z2#photo=5

shape the abortion law in the United States. This effective making-of-law occurs because decisions become precedents for decisions in future cases. We isolate an unexpected component of appellate jurisprudence using the random assignment of appellate judges to cases. We demonstrate that the idiosyncratic variation in the proportion of cases with Democratic appointees is a strong predictor of liberal outcomes in abortion cases. We use this random variation to identify the causal impact of policy outcomes on policy preferences.

Our research design can be clarified through the following thought experiment. Consider the Ninth Circuit, a generally more liberal court that includes California, which has a high proportion of judges who are Democratic appointees. From year to year, the proportion of abortion cases that are assigned Democratic appointees varies in a random manner. The idiosyncratic variation is not expected ahead of time since judicial assignments are not revealed to parties until very late and after each litigant's briefs are filed. In years with an unexpectedly high proportion of cases heard by a Democratic appointee, the proportion of abortion cases that will result in liberal precedents is also high. Random variation in the assignment of appellate judges is an attractive instrument for a number of reasons. The random assignment of judges is exogenous and unexpected. It varies in both the cross-section and the time-series, so does not rely on strong assumptions about the comparability of different regions (e.g., circuits) and years. Additionally, the exclusion restriction is likely to hold: The idiosyncratic variation in the proportion of abortion cases with particular judge characteristics is unlikely to directly affect society-wide outcomes except through the appellate precedent alone. The enormous variation in abortion decisions due to the judicial panel composition also makes our empirical design an ideal context to study the effect of policies on preferences. Abortion decisions are decided along partisan lines, are highly emotionally salient, and are likely to affect individuals through more than economic sanctions alone.

We find that Democratic appointee judges are 17 percentage points more likely to vote "pro-choice" in abortion decisions. Using the idiosyncratic variation in judicial panel composition as an instrument, our baseline estimates indicate that one pro-choice abortion decision increases the probability of individuals saying abortion should not be legal by 4 to 10 percentage points. Pro-life decisions increase the likelihood that individuals say abortion should be legal. The effect of pro-life abortion decisions is larger than the effect of pro-choice abortion decisions. In addition, one pro-choice abortion decision increases by 3 percentage points the likelihood of individuals identifying as a strong Republican and reduces by 3 percentage points the likelihood of individuals identifying as an Independent, near Democrat. Party identification shifts to becoming more Democratic after a pro-life decision. We conduct several robustness checks. Public opinions and party identification are not correlated with the idiosyncratic variation in abortion jurisprudence stemming from panel composition before the decision. In addition, as a placebo experiment, liberal jurisprudence in the First Amendment does not affect abortion attitudes.

To examine one mechanism through which appellate decisions affect policy preferences, we document that newspapers subsequently report abortion appellate decisions and conduct a field experiment where 345 data-entry workers are randomly asked to transcribe these newspaper summaries of liberal or conservative abortion decisions. When exposed to liberal abortion decisions, workers become more conservative (and vice versa) on two dimensions of abortion attitudes and the shift is similar in magnitude to the estimates in the population sample.

This paper proceeds as follows. Section 2 provides background on appellate courts and their decision-making process. Section 3 presents a simple theoretical framework for policy preference shifts including backlash and expressive. Section 4 describes the data. Section 5 explains the empirical strategy and threats to the validity of the identification strategy. Section 6 presents the results, showing the robust first-stage relationship between judicial panel composition and abortion decisions, discussing the main instrumental variable results and the results of several robustness tests. Section 7 describes the priming experiment. Section 8 concludes.

2 Background on Appellate Abortion Law

2.1 The Federal Judicial System and Abortion Policy

Federal appellate decisions concerning abortion rights and abortion access can act both as policy changes and as statements of policy and values. To understand policy-making by courts regarding abortion, we describe the system of abortion regulation in the United States, and the crucial role of the US federal court system in abortion policy.

Abortion policy in the United States is represented at several levels. In the seminal 1973 *Roe v. Wade* decision, the US Supreme Court found that constitutional due process rights extend to individual abortions, but any abortion regulations must be balanced with state interests. In the controversial aftermath, states may not completely prohibit abortion but have discretion to regulate it, subject to review by the courts. This discretion has led to much variation in abortion policy across states and localities. Laws on whether a woman can get an abortion can be codified in state statutes and local ordinances, as well as in regulations by government agencies. While there is no single comprehensive federal statute on abortion, a handful of federal laws target specific components of access to abortions.[53] At the state level, statutory provisions can impose various criteria on women seeking abortions as well as on abortion providers.[54] Other state laws address the public funding of abortions; for example, a majority of states disallow the use of state funds for abortions except when the woman's life is in danger or if the pregnancy was the result of incest or rape.[55] At the local level, cities can impose additional ordinances on abortion access and provision. While governments

[53]Among these are Title X, enacted in 1970, which allocates federal funding to family planning services for low income persons but does not directly fund abortions; the Hyde Amendment, enacted in 1976, which bars Medicaid for funding abortions; the Freedom of Access to Clinic Entrances Act of 1994, which made it a federal crime to block individuals' access to clinics; and the Partial Birth Abortion Ban Act of 2003, which bans late-term abortions.

[54]Examples include requiring parental consent or notification for minors (36 states), gestational limits that forbid abortions after a specified period into a pregnancy (38 states), and imposing specific licensing requirements on clinics and physicians.

[55]An overview of state-level abortion laws is available at:
http://www.guttmacher.org/statecenter/spibs/spib_OAL.pdf.

have discretion in enacting their own abortion laws, they must not conflict with laws of a higher level (e.g., federal statutes) and they must meet constitutional requirements, which are determined by the courts. Therefore, the federal appellate courts play a prominent role in determining abortion policy by adjudicating legal challenges against government statutes and deciding whether they are unenforceable.

To illustrate how appellate decisions shape abortion law and to provide background for our empirical methods, we note several key features of the US legal system. First, the US has a common law system where judges both apply the law as well as make the law. This judicial lawmaking occurs as judges' decisions in current cases become precedents that must guide decisions in future cases within the jurisdiction. Second, the federal courts system consists of three levels. Litigation, such as a lawsuit asserting that government-mandated waiting periods for an abortion procedure are unenforceable, begins in the district courts, or the general trial courts. On appeal, cases go to appellate courts, referred to as circuit courts, which examine whether the district court was in error and, importantly, decide issues of new law. (A very small portion of these cases is appealed again to the Supreme Court.)

Appellate law varies by geography. Each of the twelve appellate courts is in charge of a geographic region of the US, called a circuit. Appellate decisions are binding precedents only in the circuit of the court delivering the opinion. That is, the district courts within that circuit, and the circuit court itself, must follow the precedents set by the circuit court's prior decisions; courts in one circuit need not follow precedents from other circuits. In this way, appellate decisions provide geographic variation in laws across the circuits, analogous to variations in legislation across the states.

Finally, judges are randomly assigned to three-judge appellate panels to decide cases. While some judges take a reduced caseload, all judges are randomly assigned by a computer. The judges' identities typically are not revealed to the litigating parties until after they file their briefs. Because a circuit on average has 17 appellate judges in the pool of judges available to be assigned (and some circuits can have over 40 judges), the number of possible combinations of judges and their individual attributes on a panel is very large. Judges' personal attributes, such as gender and political affiliation, can predict their votes on certain types of cases.[56] Moreover, the dynamics of panel decision-making reveal that assigning one judge with a specific attribute can potentially influence the overall decision of the 3-judge panel.[57] Indeed, we establish these voting behaviors for abortion cases, finding that assigning a Democratic appointee increases the probability of a liberal, pro-choice abortion case outcome.

Together, these features of the federal court system are important in constructing a natural experiment with random variation in abortion precedents across regions of the US and over time. Circuit court decisions form abortion policy by setting legal precedents that become the law of the circuit and by affirming or invalidating government regulations. In abortion cases, the bulk of constitutional challenges concern the validity of statutes, ordinances, and regulations implemented by governments. Thus, circuit court abortion decisions, which we find to be linked to the political ideology of randomly assigned judges (see Section 6), can directly affect codified policies on abortion rights while setting legal precedent for future abortion decisions.

2.2 The Communication of Social Norms with Abortion Decisions

Beyond serving as actual law, circuit court decisions can simply reveal positions on highly sensitive issues, which can motivate backlash or support. Ruling that a local ordinance is in violation of constitutional rights can in itself be an announcement of a value judgment about the acceptable scope of abortion rights. Are people aware of appellate abortion decisions? Studies have linked major, controversial Supreme Court decisions, such as *Roe v. Wade*, with subsequent changes in public opinions about abortion (Franklin and Kosaki 1989) and have suggested that the media, as well as other factors, can predict people's awareness of

[56] Boyd et al. (2010); Chang and Schoar (2008); Sunstein et al. (2004); Peresie (2005).

[57] Farhang and Wawro (2004); Fischman (2007)

these decisions (Hoekstra 2000).

Exploring the media channel, we examine how appellate abortion decisions are communicated to the public by using a national sample of newspapers and collecting their mentions of appellate decisions over time. Hoekstra (2000) suggests that local media are more likely to report on cases in their community and that local residents are more likely to be aware of those cases than cases in other jurisdictions. We therefore select the major newspaper for the city in which each circuit court resides.[58] Figure 2.1 plots the number of appellate decisions on abortion and the number of news articles on abortion decisions for 1979-2004.[59] Controlling for circuit and year fixed effects, we find a positive relationship between the number of abortion decisions and the number of newspaper mentions; and the relationship between the number of pro-life decisions and newspaper mentions is statistically significant at the 5% level.

3 Theory

The theoretical framework is intended to assist in understanding when laws have expressive effects as opposed to backlash effects. Scholars in a wide range of areas have made arguments for or against certain policies on the basis of their expressive or backlash effects but without a clear framework for assessing the likelihood of their occurrence.[60] We present a model of

[58]These newspapers are: the Boston Globe, New York Times, Philadelphia Inquirer, Richmond Times Dispatch, Times-Picayune, Cincinnati Post, Chicago Tribune, St. Louis Post-Dispatch, San Francisco Chronicle, Denver Post, Atlanta Journal and Constitution, and Washington Post. We collected data from 1979 to 2010 from NewsBank using the search term: "abortion in All Text and appellate or circuit in All Text and judgment or "court ruling" in All Text not "Supreme Court" in All Text not state near10 appellate in All Text"

[59]Not every newspaper is available for every year. In our model, we include circuit and year fixed effects. In the figure, we divide the number of newspaper articles by the proportion of newspapers available. For example, if in 1980, only half of the typical newspaper coverage is available because of data limitations, we divide by 0.5. This allows us to compare graphically the number of appellate decisions and news articles about abortion cases over time.

[60]For a sample of backlash claims, see: voter mobilization (Mann 2010), multiculturalism (Mitchell 2004), environmentalism (Wolf 1995), private infrastructure investments (Lopez et al. 2009), health care (Mechanic 2001), abortion (Pridemore and Freilich 2007), Americans with Disabilities Act (Krieger 2000), globalization (Eckes 2000), Warren Court (Feld 2003).

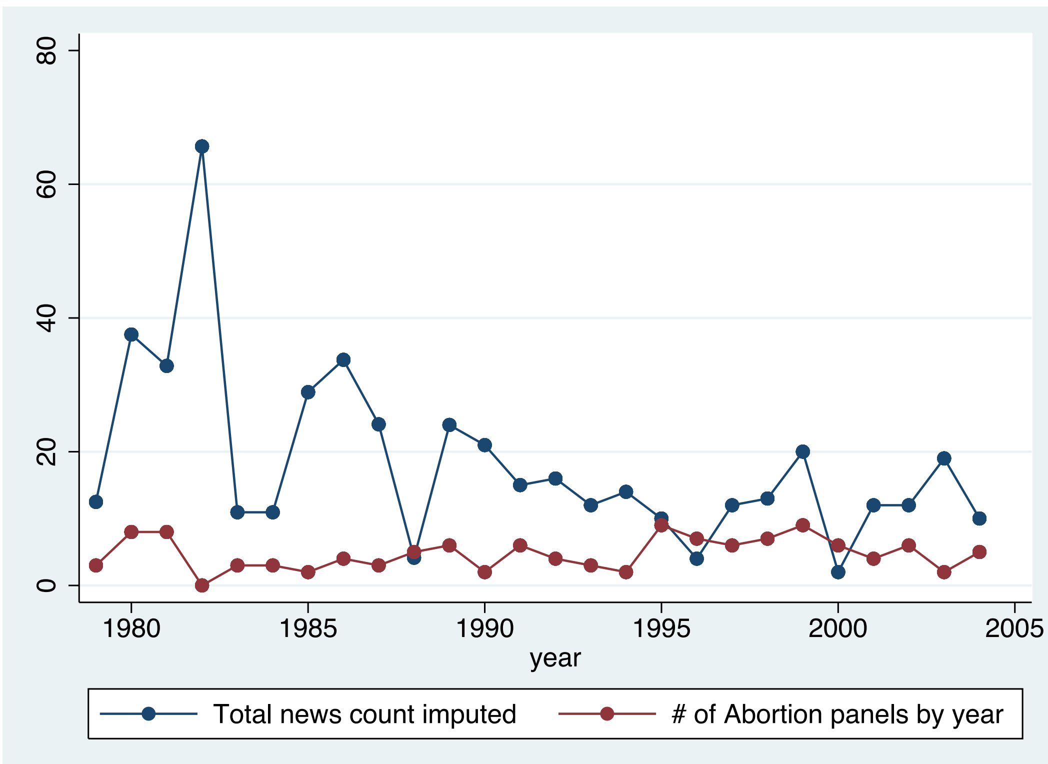

Figure 2.1: Total Number of News Reports and Abortion Cases Across All Circuits

initial backlash and subsequent expressive effects of attitudes. At the heart of the model is the interplay of external factors (laws) and internal factors (perceptions) that come together to minimize the probability of certain actions happening. Changing of laws affects the optimal level of perceptions, creating a backlash effect. Over time, as the distribution of the population with respect to the action changes, the backlash effect can change into an expressive effect. The model also does not depend on the observability of actions (or their perceptions), as it might be difficult to accurately observe abortions in the society.

For the setup we start with a representative agent. The agent may face an abortion opportunity at date $t = 1$. If the person does not undertake abortion, the utility is normalized to 0. The representative agent's net (expected) utility of having an abortion is negative, $-u_a < 0$, relative to the status quo. (Some people may receive positive utility from abortion, but overall, in the representative agent framework, it is a safe assumption that the average expected utility will be negative, an outcome that agents would like to avoid). Analogously, we assume that once the person has had an abortion there will be no subsequent changes in utility (or utility expectation) from more abortion undertakings. We can then normalize the utility of those that have already had an abortion to 0, as well. The share of those not having previously had an abortion is s in the society. (Since only women can have abortions, we assume that the choices by men in the society can also lead to abortion outcomes, which will result in negative utility, on average, for the men as well.)

The probability of the abortion depends on two factors: the outside (exogenous) factor q, and the internal (endogenous) factor p. q measures the overall state of the society, and depends on, among other things, the laws and the establishments currently in place in the society. The internal factor p depends on the actions the person takes at date $t = 0$ to avoid abortion at $t = 1$. There are convex costs $c(p)$ to avoid abortion: $c' > 0, c'' > 0$. For our setup, we generalize and call such actions "negative perceptions" towards abortion. (Or alternatively, there is a 1:1 translation of actions into perceptions). That is, the higher the person's negative attitude towards abortion, the lower the (individual) probability of facing those actions, and vice versa.

The overall probability of abortion is given by: e^{q-p}. Higher external factors increase abortion probability and larger negative perceptions decrease it. If the agent has already had an abortion, the positive costs ensure that they will not undertake any positive level of negative perceptions: $p = 0$. Thus, the overall amount of negative perceptions towards abortion in the society will be sp.

For the agents not having faced abortion previously, the equilibrium level of abortion will be determined by:

$$\max_p \{ \left(e^{q-p} \right) (-u_a) - c(p) \}$$

We can normalize the costs $c(p)$ by u_a, and, by slight abuse of notation, rewrite the new costs again as $c(p)$, to simplify the equation to be:

$$\max_p \{ -e^{q-p} - c(p) \}$$

The FOC yields:

$$e^{q-p^*} = c'(p^*)$$

Or,

$$q = p^* + \ln c'(p^*)$$

The right-hand side of the equation is monotone increasing in p, which implies $\frac{\partial p^*}{\partial q} > 0$. Also, from the implicit function theorem, we have:

$$1 = \frac{\partial p^*}{\partial q} + \frac{c''(p^*)}{c'(p^*)} \frac{\partial p^*}{\partial q} > \frac{\partial p^*}{\partial q}$$

Thus, $0 < \frac{\partial p^*}{\partial q} < 1$.

This gives the initial backlash effect in the society. Namely, passing of pro-choice law, represented by increase in q, will raise the overall exogenous probability of abortions taking place in the society at date $t = 0$. This implies that a rise in q will also raise the overall level of negative perceptions sp^*, which is done to partially offset the increase in q.

To look at subsequent expressive effect, first note that even without any law, in the steady state equilibrium, there will be some positive ratio of abortion taking place in the society. To assume net 0 steady state change in abortions, let $s_0 = e^{q-p_0^*}$, and assume the overall level probability of abortion next period will be $e^{q-p} - s_0$, where p_0^* is the initial equilibrium level of negative perceptions. This lump-sum value s_0 will not affect marginal conditions, and in equilibrium, in expectation, there will be 0 abortions.

In general, when the law changes, the share of society in period $t = 1$ with no abortions will equal $s - \left(e^{q-p^*} - s_0\right) = s + s_0 - e^{q-p^*}$. Thus, the overall level of negative perception in the economy will equal: $\left(s + s_0 - e^{q-p^*}\right) p^*$. The partial with respect to q will equal:

$$(s + s_0) \frac{\partial p^*}{\partial q} - e^{q-p^*} \frac{\partial p^*}{\partial q} - p^* e^{q-p^*} \left(1 - \frac{\partial p^*}{\partial q}\right)$$

From the bounds on $\frac{\partial p^*}{\partial q}$ we know that the last two expressions are negative, whereas the first one is positive. Moreover, from the First Order Condition, we know that $e^{q-p^*} = c'(p^*)$. Therefore, a sufficient (though not necessary) condition for the expression in the equation to be negative is for $c'(p^*) \geq s + s_0$. That is, for sufficiently large marginal costs, the equilibrium level of overall negative perceptions in the society will be decrease at $t = 1$, meaning that the society turns from backlash to expressive towards the law. This happens due to the fact that even though the average level of negative perceptions increases, the number of such people in the society decreases. For large-enough marginal costs, it is possible for the product to decrease in equilibrium.

The intuition lies in the strength of costs to change perceptions, relative to other factors in the economy, like laws. If the costs are relatively low to change perceptions, then any change in law could be internalized, to a greater extent, through perception. This will create persistent backlash effect. However, if marginal costs are large enough, the change in law will have a sizable impact on the actual number of people getting versus not-getting an abortion. It is intuitive to think of laws that may initially be unpopular, to develop expressive effects through increasing the share of the population to do those actions that the law condones.

4 Data

4.1 Judicial Biographies

We compile our data from three main sources. We use federal appellate-level abortion decisions originally coded by Sunstein et al. (2006), with corrections by Kastellec (2013). We match each judge who adjudicated the cases with judge data from the Federal Appeals and District Court Attribute Data assembled by Zuk, Barrow, and Gryski[61] as well as from the Federal Judicial Center's biographies of judges.[62] We measure preference shifts using data on political attitudes and abortion opinions from the General Social Survey (GSS).

Our set of abortion decisions consists of 143 published opinions on abortion that were decided between January 1, 1971 and June 30, 2004, at the federal appellate level.[63] The cases are limited to those decided on constitutional grounds. These largely consist of challenges to state statutes, local ordinances, or other government policies regulating abortion access. Examples include parental notification or consent requirements for minors seeking abortions,[64] prohibitions on state funding for abortions,[65] and "partial-birth" abortion

[61]http://www.cas.sc.edu/poli/juri/attributes.htm

[62]http://www.fjc.gov/history/home.nsf

[63]Sunstein, Schkade, and Ellman (2006) obtain these cases from a broader Lexis search using the terms "core-terms (abortion) and date aft 1960 and constitutional" and "abortion and constitution!"

[64]See, e.g., Akron Center for Reproductive Health, Inc. v. City of Akron, 651 F.2d 1198 (6th Cir., 1981); Manning v. Hunt, 119 F.3d 254 (4th Cir., 1997); Planned Parenthood Of Northern New England v. Heed, 390 F.3d 53 (1st Cir., 2004).

[65]See, e.g., D R v. Mitchell, 645 F.2d 852 (10th Cir., 1981); State of New York v. Sullivan, 889 F.2d 401 (2nd Cir., 1989)

bans.[66] A small portion of the cases represents challenges to restrictions on anti-abortion protesting.[67] Appendix Table A gives a rough summary of the challenges to statutes and policies that reached the Supreme Court. A total of 117 circuit-years of the 408 circuit-years in our time period experienced at least one abortion decision.

Each decision was coded as either "pro-choice," favoring abortion rights and stronger protections from anti-abortion protest methods, or "pro-life." In this paper, we sometimes refer to pro-choice decisions as "liberal" and pro-life decisions as "conservative." Among the years with any abortion appellate decisions, 58% of the panel decisions were pro-choice, with 80% of these pro-choice decisions being unanimous. Of the pro-life decisions, 65% were unanimous. Figure 2.2 plots the frequency of pro-choice decisions and pro-life decisions nationwide in appellate courts by year.

Each appellate case was decided by a panel of three randomly assigned federal judges.[68] A key feature of our identification strategy relies on judicial pool characteristics, where we observe judge characteristics to predict votes and case decisions. We match each judge to her or his individual biographical attributes from Zuk, Barrow, and Gryski's Appeals Court Attribute Data and District Court Attribute Data, as well as biographical data from the Federal Judicial Center for judges appointed after 2000. The data include a judge's vital statistics, education, religion, race, political affiliation and other variables. For a number of specifications, we use the Federal Court Management Statistics to construct a measure of the annual circuit workload, or the number of federal appeals terminated within each circuit by year.[69]

We obtain outcome measures of individuals' abortion views and political ideology

[66]See, e.g., Carhart v. Stenberg, 192 F.3d 1142 (8th Cir., 1999); Rhode Island Medical Society v. Whitehouse, 239 F.3d 104 (1st Cir., 2001).

[67]See, e.g., Cheffer v. Reno, 55 F.3d 1517 (11th Cir., 1995); US v. Gregg, 226 F.3d 253 (3rd Cir., 2000).

[68]Most are federal appellate-level judges, though some are district court judges who sit within the case's circuit.

[69]http://www.uscourts.gov/fcmstat/index.html

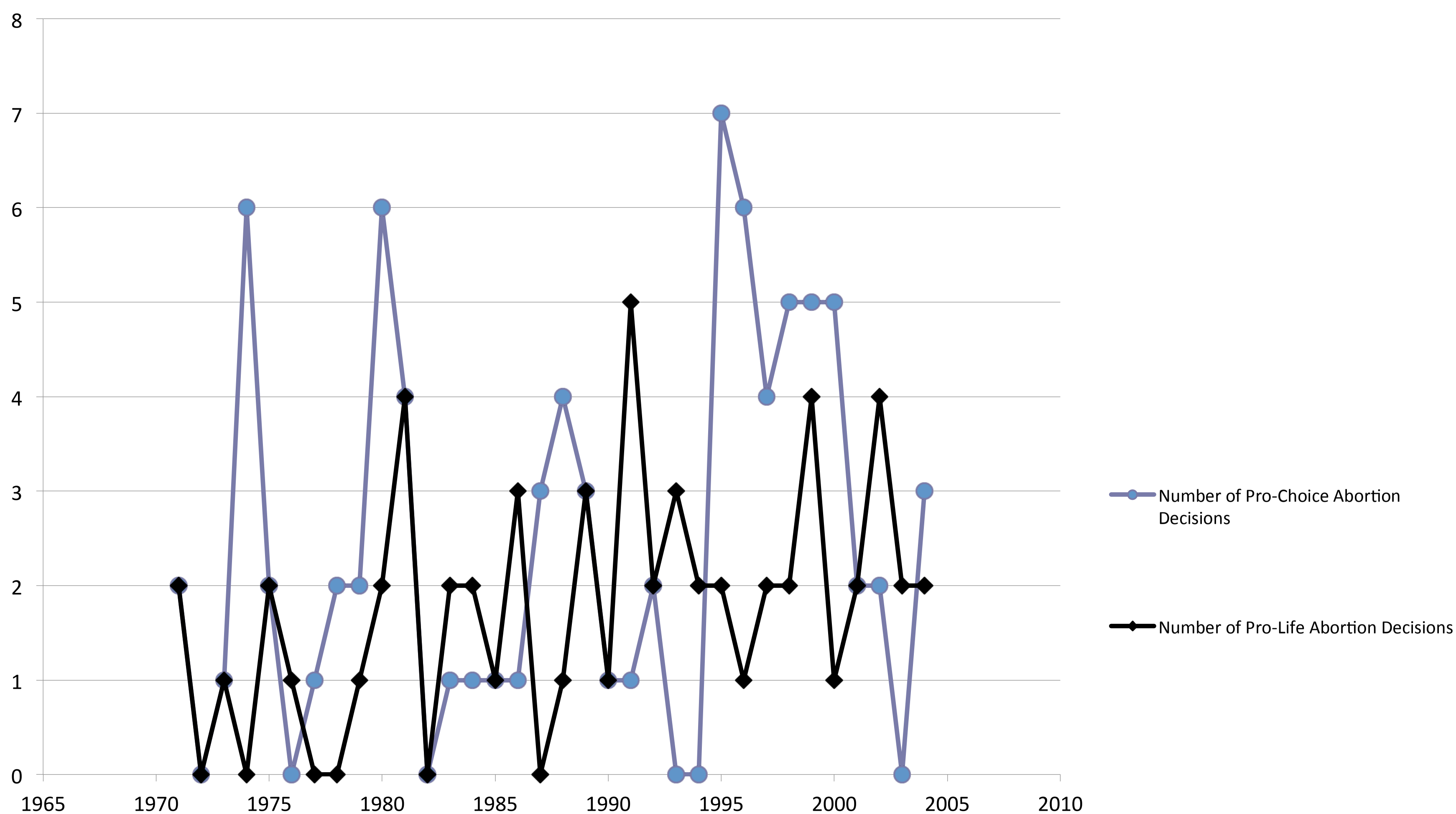

Figure 2.2: Appellate Abortion Decisions, 1971-2004

from the General Social Survey (GSS).[70] The GSS is an individual-level survey that was conducted annually from 1973 to 1994 (except for 1979, 1981, and 1992), and biannually from 1994 to 2006. For each year, the GSS randomly selects a cross-sectional sample of residents of the United States who are at least 18 years old. The GSS provides responses from around 1500 respondents for each survey year between 1973 and 1992, and around 2900 respondents per survey year from 1994 to 2006. We shift the survey responses by one year because people can be surveyed at any time during the year. We use GSS survey weights in our regressions.

4.2 Summary Statistics

Appendix Table B shows the summary statistics. Means of appellate court characteristics are shown for the judicial pool at the circuit and year level. Of the 408 circuit-years between 1971 and 2004, 117 circuit-years experienced at least one appellate abortion decision. On average, a circuit-year has 16.8 active (appellate) judges, 0.35 appellate abortion decisions, and 0.203 pro-choice decisions. Thus, the majority of abortion cases had pro-choice outcomes. Of the GSS respondents experiencing an abortion decision and surveyed on their abortion views, around 80% believe that a woman should be able to obtain a legal abortion if her health is seriously endangered by the pregnancy, while only 40% believe so if the woman wants an abortion for any reason. On self-identified political affiliation, 48% lean towards being a Democrat, while 36% lean towards being a Republican.

5 Empirical Strategy

We first present a basic specification of the effects of appellate abortion laws on political preferences. This naïve OLS model controls for various sources of biases arising from time and place. However, it can be susceptible to reverse causality as well as omitted variable

[70] http://publicdata.norc.org:41000/gssbeta/index.html

biases arising from outside trends. Indeed, constituents can influence the types of policies in their jurisdictions to satisfy their preferences.[71] Later, we present our identification strategy, which overcomes the endogeneity of policy and preferences. We exploit exogenous variation from a natural experiment where liberal abortion decisions vary randomly across circuits and over time due to the random assignment of judges to appellate panels.

5.1 Basic Specification

Our basic specification models the changes in abortion precedent at the circuit-year level and their relationship to individual political preferences as:

$$Y_{ict} = \beta_0 + \beta_1 Law_{ct} + \beta_2 \mathbf{1}[M_{ct} > 0] + \beta_3 C_c + \beta_4 T_t + \beta_5 C_c * Year + \beta_6 X_{ict} + \beta_7 W_{ct} + \varepsilon_{ict} \quad (2.1)$$

The dependent variable, Y_{ict}, is a measure of the preferences of individual i in circuit c and year t. These include value judgments about abortion rights and political ideology. The main coefficient of interest is β_1 on Law_{ct}, where Law_{ct} is the measure of liberal, pro-choice abortion decisions issued in circuit c and year t. We construct this as the percentage of abortion decisions that are liberal (pro-choice). This captures the net effect of liberal decisions given that conservative decisions may also occur. In alternate specifications, we measure the law as the raw number of liberal decisions, and then as the raw number of conservative (pro-life) decisions. With these, we test whether a higher quantity of liberal abortion decisions would produce backlash; the number of conservative decisions also serves as a robustness check.

The presence of the case variable, $\mathbf{1}[M_{ct} > 0]$, allows for the possibility that pro-choice and pro-life decisions might have effects of different magnitudes on the dependent variable. It also enables us to construct the decision variable for circuits and years with multiple legal

[71] See Besley and Case (2000).

decisions. The presence of the case, however is endogenous and is likely to be correlated with the error term. We instrument for $\mathbf{1}[M_{ct} > 0]$ with the random assignment of district court judges to their cases. Recall from Section 2.1 that appellate cases appear only on appeal from the district court. Thus random variation in the district courts serves to hold the fitted values of $\mathbf{1}[M_{ct} > 0]$ to vary in a manner that no longer threatens the moment condition in our specification. One district court judge is randomly assigned per case (Bird 1975).[72] Figure 2.3 displays the boundaries of each district court with dashed lines. Whether the district court cases were disproportionately assigned to certain types of judges will be uncorrelated with treatment (the random assignment of appellate judges) but may affect the likelihood of subsequent appeal. As a general matter, district judges could affect the likelihood of appeal, for example, if some district judges are less likely to be reversed and this lower reversal rate discourages litigating parties from pursuing an appeal. The correlation between district judge demographic characteristics and their reversal rates has been previously documented (Barondes 2010; Steinbuch 2009; Haire, Songer and Lindquist 2003).

Observed differences in policy preferences might arise from regional traditions rather than from the laws themselves. For example, church attendance may be more ingrained in the culture of a Southern circuit, so people there may express more conservative attitudes than people on the West Coast. We address potential biases arising across time and space with controls: C_c is the vector of circuit fixed effects, which absorb circuit-level unobservables; T_t is the vector of year fixed effects, which control for year-specific unobservables that equally affect all circuits; and C_c*$Year$ are the circuit-specific time trends to allow different circuits to be on different trajectories with respect to outcomes. We also include state fixed effects to address state-specific characteristics; these would capture state statutes and state court decisions. X_{ict} is the vector of observable individual characteristics such as age and gender. Because political attitudes may be correlated by space so that ε_{ict} is not i.i.d., we cluster

[72]We use only cases decided by district court judges and exclude recommendations by magistrate judges because litigants cannot directly appeal a magistrate judge's recommendation (28 U.S.C. § 636(c)(1)).

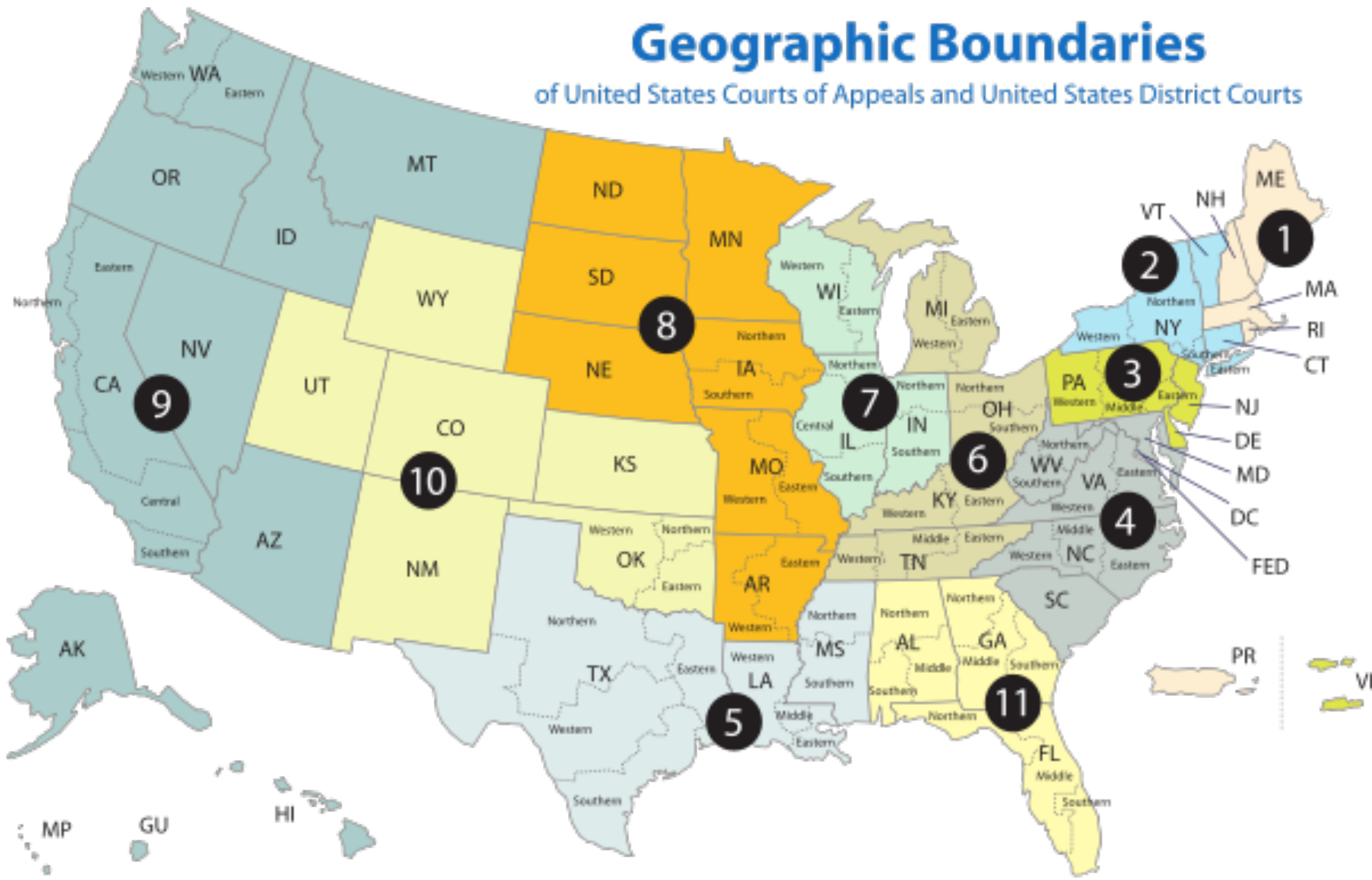

Figure 2.3: US Circuit and District Court Boundaries

standard errors at the circuit level. Finally, W_{ct} represents judicial pool controls, such as the circuit-specific docket size or the total number of abortion cases. The particular variables included in the judicial pool controls depend on specification, which we discuss in the next sub-section.

Are estimates from the model plausible? One critique is that decisions in one circuit may influence another circuit towards the same direction. Second, appellate case selection may be correlated with trends in the lower courts; for example, more liberal appellate decisions can occur when the trial courts are extremely conservative.[73] These behaviors, however, would merely contribute measurement error, attenuating the magnitudes toward zero or generating imprecision. A third critique concerns residential sorting: People who are pro-choice may choose to locate in jurisdictions with more liberal political attitudes. Our circuit fixed effects and controls for time trends within circuits could address this. A fourth critique is that litigants engage in forum-shopping. Forum-shopping, however, is addressed by controlling for the total number of abortion cases.

5.2 Remarks

Before moving on to extensions of the basic model, we make two remarks. First, the exclusion restriction is likely to hold, and we will thus be able to interpret the 2SLS estimates as the causal impact of abortion precedent. Here, the identity of judges sitting on abortion panels is not likely directly to affect population outcomes that are of interest except through the appellate precedent alone. In ongoing research, we find that markets do not respond to the judge announcement, even in a set of securities cases in Delaware courts where markets are likely to have focused their attention. Second, the LATE interpretation of the instrumental variables estimate is restricted in terms of external validity. Here, only cases where there is enough controversy to allow judicial biographical characteristics to matter are going to be the subject of the study. These cases may very well be the difficult decisions that set

[73]See, e.g. Priest and Klein (1984); Eisenberg (1990).

new precedent, and the sorts of cases in which judges, like Judge Richard Posner or Justice Stephen Breyer, interested in empirical consequences of decisions, seek guidance (Posner 1998; Breyer 2006).

Two additional remarks are useful. First, it need not be the case that being a Democratic appointee causes the judge to decide differently. Perhaps being a Democratic appointee is simply correlated with other omitted characteristics of the judge that determine decision-making. Even if we are sure that being a Democratic appointee causes differential decision-making, we need to know why that is the case, whether it is due to formative life experiences or subsequent professional experiences. Perhaps Democratic appointees make different decisions because litigants tailor their oral arguments to the judge. In the framework of the Rubin causal model, random assignment of Democratic appointees who make different decisions than Republican appointees, for whatever reasons, is sufficient to estimate a causal relationship of law on outcomes.

5.3 Identification Strategy

The OLS model in equation (2.1) can remain biased because it fails to address reverse causality and omitted variable bias. While the law can drive political backlash, popular policy preferences can also lead to changes in state legislation or more litigation to invalidate existing policies. Moreover, abortion decisions may be correlated with appellate precedents in other legal areas such as the death penalty. If other legal areas also influence policy preferences, then our estimates may be biased upward, since they fail to account for the omitted effects of the other laws. As a solution, we employ an instrumental variables strategy whose random variation arises where the percentages of abortion laws that are pro-choice vary randomly across each circuit and year. We exploit the facts that (1) judges are randomly assigned to three-judge panels for each case and (2) Democratic appointees are more likely to vote liberally in abortion cases.

5.3.1 Correlation Between Judicial Biography and Voting

For the first stage in our two-stage least squares estimation, we use the fact that judges' personal attributes can be correlated with their voting behavior in appellate cases, which translates to panel vote outcomes, and therefore, changes in circuit-level abortion law.[74] Prior research has documented that since the 1970s, federal appellate judges appointed by a Democratic president are more likely to vote pro-choice in an abortion rights case, while Republican appointees favor pro-life decisions.[75] We replicate this finding in our data and present these first-stage results in Table 2.1. Abortion can be a prominent issue in elections and in party identification. A common explanation for why Democratic appointees vote pro-choice is that ideology drives judicial voting, with political party predicting the judge's ideology. Note that the mechanism does not affect the validity of our empirical strategy.

5.3.2 Two-Stage Least Squares Estimation

Figure 2.4 roughly depicts the intuition for our 2SLS identification strategy, in which we exploit the random variation that arises from using the actual deviations from the expected probability of a circuit-year having judges who were Democratic appointees. The flatter line is the expected number of Democratic appointees on a panel. The jagged line is the actual number of Democratic appointees on a panel. (The figure displays the average values across all circuits.) Circuit-years receiving an unexpectedly high proportion of Democratic appointees on their panels receive an unexpectedly higher proportion of pro-choice abortion decisions. Each actual spike above the expected probability of getting a Democratic judge corresponds to the circuit-year randomly receiving a "treatment" of more pro-choice abortion decisions. Thus, changes in people's policy preferences can be attributed to the "treatment" of pro-choice appellate laws. Figure 2.4 suggests the first stage equation:

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[74] Boyd, Epstein, and Martin (2010); Chang and Schoar (2008); Ellman, Sunstein, Schkade (2004).

[75] Sunstein, Schkade, Ellman, and Sawicki (2006).

Table 2.1. First Stage: Relationship Between Pro-Choice Abortion Decisions and Democratic Appointees on Appellate Panels, 1971-2004

	Outcome: Pro-Choice							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democratic Appointee	0.180**	0.172**	0.203*	0.166	0.316+	0.313	0.430*	0.587**
	(0.0397)	(0.0303)	(0.0658)	(0.12)	(0.161)	(0.22)	(0.153)	(0.17)
N	429	429	143	143	117	117	14609	14609
R-sq	0.033	0.257	0.036	0.365	0.027	0.522	0.059	0.701
F statistic	20.531	32.476	9.507	1.896	3.841	2.014	7.878	11.858
Pro-Choice measure	Judge Vote	Judge Vote	Panel Vote	Panel Vote	Percentage	Percentage	Percentage	Percentage
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Analysis level	Judge	Judge	Panel	Panel	Circuit-year	Circuit-year	Circuit-year individual	Circuit-year individual

Notes: Heteroskedasticity-robust standard errors are in parentheses. Standard errors are clustered at the circuit level. Pro-Choice is defined as the judge voting pro-choice (Columns 1-2), the panel voting pro-choice (Columns 3-4), or the percentage of abortion cases that are pro-choice in the circuit and year (Columns 5-8). Democratic Appointee Variable is an indicator for whether the judge was a Democratic appointee at the judge level (Columns 1, 2), or whether the panel has majority Democratic appointees at the panel level (Columns 3-4), or the difference between the Actual and Expected number of Democrats assigned per seat (Columns 5-8). Controls include circuit fixed-effects, year fixed-effects. Columns 2 and 4 include the probability of an appellate panel being assigned 1+ or 2+ Democratic appointees, respectively. Columns 6 and 8 include circuit-specific time trends. Columns 1-6 use appellate judge and abortion case data. The sample in Columns 7-8 uses appellate data merged with GSS respondents. + Significant at 10%; * Significant at 5%; ** Significant at 1%.

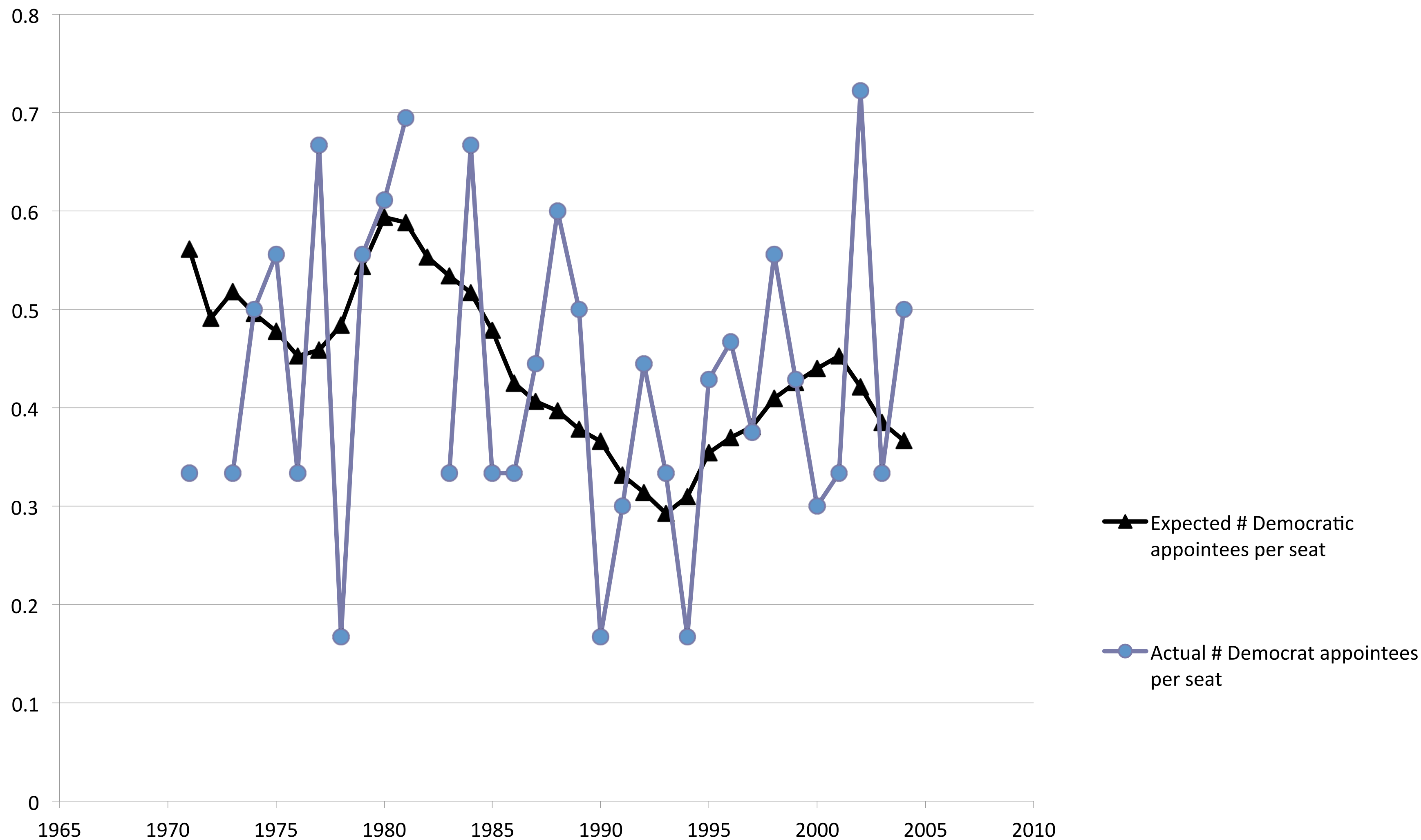

Figure 2.4: Judicial Composition and Random Assignment, 1971-2004

$$Law_{ct} = \gamma_0 + \gamma_1 Treatment_{ct} + \gamma_2 \mathbf{1}[M_{ct} > 0] + \gamma_3 C_c + \gamma_4 T_t + \gamma_5 C_c * Year + \gamma_6 W_{ct} + \gamma_7 X_{ict} + \eta_{ict} \tag{2.2}$$

where Law_{ct} is defined as the percentage of abortion decisions that are liberal, conditional on there being any abortion decisions in that circuit and year. The "Treatment" group ($Treatment_{ct} = 1$) comprises people in a circuit who experience an unexpectedly higher percentage of pro-choice abortion decisions due to an unexpectedly higher number of Democratic appointees being assigned to the panels. The "Control" group ($Treatment_{ct} = 0$) comprises people in a circuit who experience an unexpectedly lower percentage of abortion decisions that are pro-choice. Formally, $Treatment_{ct} = \mathbf{1}\ [(N_{ct}/M_{ct} > \mathrm{E}(N_{ct}/M_{ct})]$, where N is the expected number of Democrats assigned to all abortion cases in that circuit-year and M is the number of abortion cases in that circuit year. N/M is the expected number of Democratic appointees in any given case. The effect of abortion law on policy preferences is the difference in Y_{ict} for $Treatment_{ct} = 1$ or 0, divided by the difference in Law_{ct} for $Treatment_{ct} = 1$ or 0.

For more statistical power in our main IV specifications, we employ the entire excess proportion of cases with a Democratic appointee as a continuous instrumental variable. That is, we estimate in our first stage:

$$Law_{ct} = \gamma_0 + \gamma_1 Z_{ct} + \gamma_2 \mathbf{1}[M_{ct} > 0] + \gamma_3 C_c + \gamma_4 T_t + \gamma_5 C_c * Year + \gamma_6 W_{ct} + \gamma_7 X_{ict} + \eta_{ict} \tag{2.3}$$

where our instrument Z_{ct} is the difference between the actual proportion of cases with Democratic appointees and the expected proportion of cases with a Democratic appointee. We redefine $Treatment_{ct} = N_{ct}/M_{ct}$ - $\mathrm{E}(N_{ct}/M_{ct})$. The moment condition for causal inference is $\mathrm{E}[(N_{ct}/M_{ct}$ - $\mathrm{E}(N_{ct}/M_{ct}))\ \varepsilon_{ict}] = 0$.

This framework in (3) may be the cleanest in terms of identification, where the i.i.d.

condition $E(Z_{ct}\varepsilon_{ict}) = 0$ must be satisfied. However, it is entirely possible that people may be more responsive to the number of pro-choice decisions rather than the percentage of cases. We show estimates from a version of (3) that uses the number of pro-choice decisions instead of percentage of pro-choice decisions as well as the OLS model of (1) in our results. Multiplying the moment condition for (3) by M_{ct} results in $\mathbf{E}[(N_{ct}$ - $E(N_{ct}))\ \epsilon_{ict}] = 0$. We now define $Treatment_{ct} = N_{ct}$ - $E(N_{ct})$ and in equations (2.1) and (2.2), let Law_{ct} be the number of pro-choice abortion cases. As a check for possible omitted variables[76] in excluding M_{ct}, we use Law_{ct} as measured with the number of liberal (pro-choice) decisions and, as a check, the number of conservative (pro-life) decisions.

6 Results

6.1 First Stage Estimates

Table 2.1 documents the relationship between pro-choice abortion appellate decisions and the random assignment of Democratic appointees using our dataset of cases from 1971 to 2004. Columns 1 and 2 show the relationship at the judge level, where we regress an individual judge's vote on an indicator for Democratic appointment, clustering the standard errors by circuit; Column 2 controls for circuit and year fixed effects and the expected probability of a case being assigned a Democratic appointee in each circuit-year. A Democratic appointee is 17.2% more likely to vote pro-choice than a Republican appointee (Column 2). Further, our unreported tabulations show that appellate panels assigned two or more Democratic appointees vote pro-choice 71% of the time, compared with 51% for panels with two or more Republican appointees. These correlations are consistent with those reported in existing literature, such as Sunstein et al. (2006). Columns 3 and 4 show the relationship at the case level, with and without regression controls. Randomly assigning a panel to have a majority

[76]The omitted variables that are associated with M_{ct}, pro-choice decisions, and outcome Y_{ict} may also be associated with the number of pro-life decisions.

of Democratic appointees is predictive of a pro-choice decision, though the estimate is noisier when including circuit, year, and judicial pool controls. The relationship at the circuit-year level is shown in the next columns. Columns 7 and 8 show the relationship after merging with individual-level data from the GSS. Circuit-years with unexpectedly higher proportions of judges assigned to abortion cases who are Democrats predict a higher proportions of abortion decisions that are pro-choice. The F-statistic of joint significance for the instrument defined as the deviation between the actual and expected percentage of judges being Democratic appointees is 11.86 in the merged sample (Column 8).

6.2 Main Results

Table 2.2 shows preliminary results for abortion attitudes. Ordinary least squares estimates of the effect of abortion law, measured as the proportion of judicial abortion decisions that are liberal (pro-choice), show small and statistically insignificant effects on the general population's views about when abortions should be legal (Column 1). The first row displays a summary of the abortion attitudes, the average of the number of non-missing survey responses per individual. Columns 2-4 show estimates exploiting the random assignment of Democratic judges for exogenous variation in appellate abortion decisions. These IV estimates suggest that appellate abortion decisions have a causal impact on people's views on abortion legality. The summary index, being the average of all abortion responses per individual, is positive and statistically significant at the 10% level. It suggests an overall conservative (pro-life) response to more pro-choice decisions. In particular, an unexpectedly higher percentage of pro-choice decisions causes people to be more likely to express pro-life attitudes, believing that abortion should be illegal for women who choose abortion for family size reasons or because they want to remain single; these estimates are statistically significant at the 5% level. Column 3 shows that the causal effect of the number of pro-choice decision also increases the likelihood of conservative responses to prohibit abortion if the woman seeks it for reasons of family size, her own endangered health, family income, or preferring to remain single. For example, an additional, exogenous pro-choice decision makes people 8.9% more likely to oppose allowing abortions for married women who do not want any more children. With a population mean of 44% and standard deviation 50% for this survey question (Table B), one abortion decision can lead to an economically sizable shift in abortion attitudes. Finally, we verify that the effect of an extra exogenous pro-life decision is opposite in sign from the effect of an extra exogenous pro-choice decision (Column 4).

Table 2.3 presents the effect of abortion decisions on individuals' political self-identification, on a spectrum ranging from strong Democrat to strong Republican. Following an increase in exogenous percentage of pro-choice appellate decisions, people are 5.3% more likely to identify as strong Republicans (Columns 2) and a similar magnitude are less likely to identify as an Independent, near Democrat. These findings are consistent with the hypothesis that abortion laws may shift preferences among some individuals so that they change their political association. It is also possible that political parties may adjust their agendas based on abortion issues to attract supporters. In other words, these results can be construed as "backlash" among the population, or alternatively, as evidence that judicial abortion policy affects the strategies of political parties.

Next, we explore whether the main results can be explained by spurious correlations between pre-existing public opinion and abortion decisions (Tables 2.4 and 2.5). Figure 2.5 shows the event study graph of the coefficient on law for the abortion index and the different specifications of the questionnaire, along with 90% confidence interval bounds. Across all specifications, there is an increase at date 0 of pro-life attitudes, in relation to an increase in pro-choice decisions. Subsequently, the attitude turns from backlash to expressive, and after year 4, law appears to have no effect. This is also borne out in the tables.

The OLS specifications show that current year appellate abortion decisions are not correlated with public opinions on abortion from two years ago (Table 2.4, Column 1). Similarly, current abortion decisions are not correlated with the political association from two years ago (Table 2.5, Column 1). We choose a two year-window because the filing of

Table 2.2. The Effect of Appellate Abortion Law on Abortion Attitudes

	(1)	(2)	(3)	(4)	N
Model	*OLS*	*IV*	*IV*	*IV*	
Abortion Law Measure	*Percentage Pro-Choice*	*Percentage Pro-Choice*	*Number of Pro-Choice*	*Number of Pro-Life*	
Outcome Variables					
Abortion Attitude Summary Index	0.0154	0.1040+	0.0593+	-0.1010+	9585
	(0.0154)	(0.0585)	(0.0310)	(0.0609)	
It should NOT be possible for a woman to obtain a legal abortion if:					
There is strong chance of serious defect in the baby	0.0151	0.0394	0.0335	-0.0574	9292
	(0.0146)	(0.0517)	(0.0241)	(0.0541)	
She is married and she does not want any more children	0.0248	0.1675*	0.0885*	-0.1517+	9262
	(0.0247)	(0.0767)	(0.0447)	(0.0830)	
The woman's own health is seriously endangered by the pregnancy	0.0096	0.0711+	0.0419*	-0.0720+	9323
	(0.0104)	(0.0384)	(0.0195)	(0.0413)	
The family has a very low income and cannot afford any more children	0.0156	0.1105+	0.0686*	-0.1173+	9225
	(0.0163)	(0.0648)	(0.0325)	(0.0685)	
She became pregnant as a result of rape	0.0187+	0.0414	0.0217	-0.0373	9256
	(0.0101)	(0.0387)	(0.0218)	(0.0359)	
She is not married and does not want to marry the man	0.0281	0.1780*	0.0964*	-0.1650+	9257
	(0.0253)	(0.0856)	(0.0453)	(0.0906)	
The woman wants the abortion for any reason	-0.0020	0.2300	0.0797	-0.2138	7939
	(0.0245)	(0.1707)	(0.0567)	(0.1891)	

Notes: Heteroskedasticity-robust standard errors are in parentheses. Standard errors are clustered at the circuit level. Regressions control for age and gender and include circuit fixed effects, state fixed effects, year fixed effects, circuit-specific time trends. Column 2 uses as an instrument the difference between the Actual and Expected number of Democrats assigned per seat. Columns 3 and 4 use as an instrument the the difference between the Actual and Expected number of Democrats assigned per abortion panel. + Significant at 10%; * Significant at 5%; ** Significant at 1%.

Table 2.3. The Effect of Appellate Abortion Law on Political Association

	(1)	(2)	(3)	(4)	N
Model	*OLS*	*IV*	*IV*	*IV*	
Abortion Law Measure	*Percentage Pro-Choice*	*Percentage Pro-Choice*	*Number of Pro-Choice*	*Number of Pro-Life*	
Outcome Variables					
Generally speaking, do you usually think of yourself as a Republican, Democrat, Independent, or what?					
Strong Democrat	-0.00590	0.00271	-0.00197	0.00312	14552
	(0.00813)	(0.0141)	(0.00825)	(0.0128)	
Not a Strong Democrat	-0.00500	-0.00106	0.00557	-0.00881	14552
	(0.00579)	(0.0279)	(0.0145)	(0.0231)	
Independent, Near Democrat	-0.00795	-0.0533*	-0.0354*	0.0560+	14552
	(0.00703)	(0.0221)	(0.0129)	(0.0291)	
Independent	0.00405	-0.0533+	-0.0264	0.0417+	14552
	(0.00964)	(0.0249)	(0.0149)	(0.0211)	
Independent, Near Republican	-0.00170	-0.0171	-0.0165	0.0262	14552
	(0.00660)	(0.0185)	(0.0127)	(0.0181)	
Not a Strong Republican	-0.00271	0.0628	0.0427+	-0.0675+	14552
	(0.0105)	(0.0361)	(0.0227)	(0.0360)	
Strong Republican	0.0195**	0.0535*	0.0288*	-0.0456+	14552
	(0.00331)	(0.0192)	(0.0116)	(0.0212)	

Notes: Heteroskedasticity-robust standard errors are in parentheses. Standard errors are clustered at the circuit level. Regressions control for age and gender and include circuit fixed effects, state fixed effects, year fixed effects, circuit-specific time trends. Column 2 uses as an instrument the difference between the Actual and Expected number of Democrats assigned per seat. Columns 3 and 4 use as an instrument the the difference between the Actual and Expected number of Democrats assigned per abortion panel. + Significant at 10%; * Significant at 5%; ** Significant at 1%.

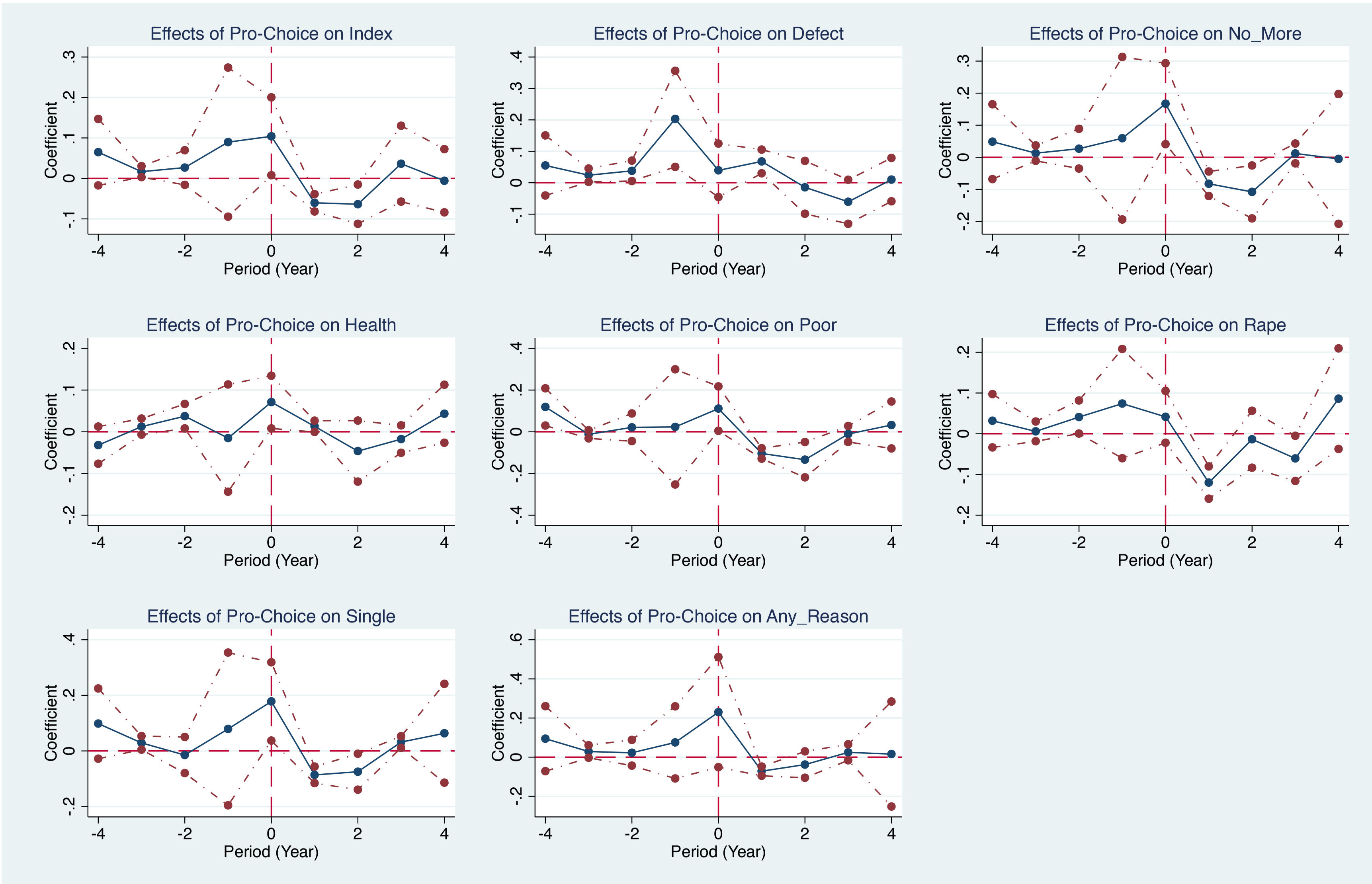

Figure 2.5: Event Study of Pro-Choice Decisions

Table 2.4. The Effect of Appellate Abortion Law Two Years Subsequent to This Year's Abortion Attitudes

	(1)	(2)	(3)	(4)	N
Model	*OLS*	*IV*	*IV*	*IV*	
Abortion Law Measure	*Percentage Pro-Choice*	*Percentage Pro-Choice*	*Number of Pro-Choice*	*Number of Pro-Life*	
Outcome Variables					
Abortion Attitude Summary Index	0.0182	0.0268	0.0136	-0.0247	10362
	(0.0105)	(0.0260)	(0.0193)	(0.0286)	
It should NOT be possible for a woman to obtain a legal abortion if:					
There is strong chance of serious defect in the baby	0.0123	0.0378+	0.0215	-0.0392	10036
	(0.0120)	(0.0197)	(0.0191)	(0.0239)	
She is married and she does not want any more children	0.0228	0.0267	0.0137	-0.0250	10016
	(0.0212)	(0.0376)	(0.0246)	(0.0388)	
The woman's own health is seriously endangered by the pregnancy	-0.00519	0.0373*	0.0208	-0.0380*	10097
	(0.0106)	(0.0178)	(0.0143)	(0.0185)	
The family has a very low income and cannot afford any more children	0.0117	0.0212	0.0109	-0.0198	9993
	(0.0129)	(0.0403)	(0.0295)	(0.0483)	
She became pregnant as a result of rape	0.00971	0.0409+	0.0222	-0.0408	10001
	(0.0138)	(0.0247)	(0.0157)	(0.0261)	
She is not married and does not want to marry the man	0.0312+	-0.0148	-0.0116	0.0211	9997
	(0.0159)	(0.0395)	(0.0221)	(0.0453)	
The woman wants the abortion for any reason	0.0351+	0.0223	0.0109	-0.0204	9273
	(0.0169)	(0.0400)	(0.0259)	(0.0427)	

Notes: Heteroskedasticity-robust standard errors are in parentheses. Standard errors are clustered at the circuit level. Regressions control for age and gender and include circuit fixed effects, state fixed effects, year fixed effects, circuit-specific time trends. Column 2 uses as an instrument the difference between the Actual and Expected number of Democrats assigned per seat. Columns 3 and 4 use as an instrument the the difference between the Actual and Expected number of Democrats assigned per abortion panel. + Significant at 10%; * Significant at 5%; ** Significant at 1%.

Table 2.5. The Effect of Appellate Abortion Law Two Years Subsequent to This Year's Political Association

	(1)	(2)	(3)	(4)	N
Model	*OLS*	*IV*	*IV*	*IV*	
Abortion Law Measure	*Percentage Pro-Choice*	*Percentage Pro-Choice*	*Number of Pro-Choice*	*Number of Pro-Life*	
Outcome Variables					
Generally speaking, do you usually think of yourself as a Republican, Democrat, Independent, or what?					
Strong Democrat	0.00910	-0.0475	-0.0180	0.0359	14940
	(0.00871)	(0.0301)	(0.0161)	(0.0306)	
Not a Strong Democrat	-0.00564	-0.00244	-0.00130	0.00235	14940
	(0.0109)	(0.0323)	(0.0157)	(0.0314)	
Independent, Near Democrat	0.00389	-0.00478	0.00430	-0.00863	14940
	(0.00722)	(0.0254)	(0.0132)	(0.0271)	
Independent	0.0134	0.0637**	0.0275+	-0.0550**	14940
	(0.00961)	(0.0163)	(0.0150)	(0.0175)	
Independent, Near Republican	-0.0175*	-0.0345	-0.0102	0.0207	14940
	(0.00692)	(0.0241)	(0.0153)	(0.0257)	
Not a Strong Republican	0.00112	-0.00314	-0.0125	0.0251	14940
	(0.00804)	(0.0285)	(0.0161)	(0.0344)	
Strong Republican	-0.0000414	0.0371	0.0160	-0.0317	14940
	(0.00986)	(0.0258)	(0.0137)	(0.0255)	

Notes: Heteroskedasticity-robust standard errors are in parentheses. Standard errors are clustered at the circuit level. Regressions control for age and gender and include circuit fixed effects, state fixed effects, year fixed effects, circuit-specific time trends. Column 2 uses as an instrument the difference between the Actual and Expected number of Democrats assigned per seat. Columns 3 and 4 use as an instrument the the difference between the Actual and Expected number of Democrats assigned per abortion panel. + Significant at 10%; * Significant at 5%; ** Significant at 1%.

abortion cases at the appellate courts can be salient and appellate decisions can take up to a year to resolve, even after the judges are revealed to the parties. In addition, few of the IV estimates show positive, though not significant relationships between current abortion decisions and the previous year's abortion attitudes or political association. This can also be seen in the event graph, and could be due to the timing of the law. By restricting to a two-year window, the results affirm there is no spurious "causal" effect. Tables 2.6 and 2.7 show a similar exercise with estimates of the relationship between current appellate abortion decisions and public opinion from four years before.[77] The IV estimates are not statistically significant in most specifications (Columns 2 through 4), and the handful that appear statistically significant are to be expected from running a hundred regressions testing for spurious correlations.

Do abortion attitudes respond to appellate decisions that simultaneously occur in other legal areas? Our policy experiment based on the random assignment of judges can also create exogenous changes in legal areas other than abortion. In Table 2.8, we implement falsification exercises where we explore the effects of appellate decisions from the legal areas of First Amendment commercial speech. This area is also politically controversial, like abortion rights law, but it is not directly linked with abortion ideology. Judges' political biographies correlate strongly with their voting behaviors on these issues, so we also instrument for the law using the unexpected deviation between the number of Democratic judges on the panel and the expected number of Democratic judges on the panel in that legal category. We find that First Amendment commercial speech decisions do not affect abortion attitudes. This result suggests that the relationship between appellate abortion decisions and abortion attitudes is real.[78]

[77] The three-year forward estimates show some statistically significant coefficients. However, these coefficients are not robust to the exclusion of circuit-specific time trends, while the main results and other placebo tests are.

[78] We acknowledge that other highly politically sensitive areas of law, especially those that directly relate to women's rights (such as affirmative action) or those that play a prominent role in partisan platforms may also influence abortion attitudes and/or party identification.

Table 2.6. The Effect of Appellate Abortion Law Four Years Subsequent to This Year's Abortion Attitudes

	(1)	(2)	(3)	(4)	N
Model	*OLS*	*IV*	*IV*	*IV*	
Abortion Law Measure	*Percentage Pro-Choice*	*Percentage Pro-Choice*	*Number of Pro-Choice*	*Number of Pro-Life*	
Outcome Variables					
Abortion Attitude Summary Index	-0.00153	0.0648	0.0632	-0.0662	11844
	(0.0105)	(0.0499)	(0.0654)	(0.0660)	
It should NOT be possible for a woman to obtain a legal abortion if:					
There is strong chance of serious defect in the baby	-0.0151+	0.0549	0.0612	-0.0645	11487
	(0.00797)	(0.0581)	(0.0712)	(0.0778)	
She is married and she does not want any more children	0.000829	0.0486	0.0425	-0.0444	11425
	(0.0171)	(0.0709)	(0.0868)	(0.0868)	
The woman's own health is seriously endangered by the pregnancy	-0.00403	-0.0321	-0.0363	0.0379	11526
	(0.00412)	(0.0271)	(0.0272)	(0.0359)	
The family has a very low income and cannot afford any more children	0.0104	0.119*	0.119	-0.124	11447
	(0.0161)	(0.0543)	(0.0795)	(0.0791)	
She became pregnant as a result of rape	-0.00638	0.0317	0.0325	-0.0340	11414
	(0.00640)	(0.0398)	(0.0518)	(0.0507)	
She is not married and does not want to marry the man	-0.00142	0.0983	0.0949	-0.0993	15171
	(0.0170)	(0.0768)	(0.0939)	(0.0981)	
The woman wants the abortion for any reason	0.0125	0.0943	0.0751	-0.0842	10140
	(0.0203)	(0.101)	(0.105)	(0.121)	

Notes: Heteroskedasticity-robust standard errors are in parentheses. Standard errors are clustered at the circuit level. Regressions control for age and gender and include circuit fixed effects, state fixed effects, year fixed effects, circuit-specific time trends. Column 2 uses as an instrument the difference between the Actual and Expected number of Democrats assigned per seat. Columns 3 and 4 use as an instrument the the difference between the Actual and Expected number of Democrats assigned per abortion panel. + Significant at 10%; * Significant at 5%; ** Significant at 1%.

Table 2.7. The Effect of Appellate Abortion Law Four Years Subsequent to This Year's Political Association

	(1)	(2)	(3)	(4)	N
Model	*OLS*	*IV*	*IV*	*IV*	
Abortion Law Measure	*Percentage Pro-Choice*	*Percentage Pro-Choice*	*Number of Pro-Choice*	*Number of Pro-Life*	
Outcome Variables					
Generally speaking, do you usually think of yourself as a Republican, Democrat, Independent, or what?					
Strong Democrat	0.00459	0.00203	0.0113	-0.0116	15171
	(0.0119)	(0.0342)	(0.0346)	(0.0358)	
Not a Strong Democrat	-0.00772	0.0214	0.0247	-0.0254	15171
	(0.00731)	(0.0232)	(0.0255)	(0.0263)	
Independent, Near Democrat	-0.0120	-0.0240	-0.0346+	0.0356	15171
	(0.00770)	(0.0202)	(0.0208)	(0.0224)	
Independent	-0.00871	-0.0460*	-0.0395+	0.0407+	15171
	(0.00802)	(0.0205)	(0.0213)	(0.0233)	
Independent, Near Republican	-0.0000725	0.0438+	0.0448	-0.0461+	15171
	(0.00779)	(0.0230)	(0.0277)	(0.0270)	
Not a Strong Republican	0.0205*	0.0185	0.0147	-0.0151	15171
	(0.00777)	(0.0178)	(0.0177)	(0.0187)	
Strong Republican	-0.000420	0.000602	-0.00332	0.00342	15171
	(0.00696)	(0.0180)	(0.0193)	(0.0198)	

Notes: Heteroskedasticity-robust standard errors are in parentheses. Standard errors are clustered at the circuit level. Regressions control for age and gender and include circuit fixed effects, state fixed effects, year fixed effects, circuit-specific time trends. Column 2 uses as an instrument the difference between the Actual and Expected number of Democrats assigned per seat. Columns 3 and 4 use as an instrument the the difference between the Actual and Expected number of Democrats assigned per abortion panel. + Significant at 10%; * Significant at 5%; ** Significant at 1%.

Table 2.8. The Effect Other Laws on This Year's Abortion Attitudes

Model *First Amendment Law Measure*	*IV* *Percentage* *Liberal*	*IV* *Number of* *Liberal*	*IV* *Number of* *Conservative*	N
(holding a government regulation banning commercial free speech as constitutional)				
Abortion Attitude Summary Index	0.0164	0.0124	-0.0212	
	(0.0342)	(0.0200)	(0.0705)	7450
It should NOT be possible for a woman to obtain a legal abortion if:				
There is strong chance of serious defect in the baby	0.0162	0.0124	-0.0325	7243
	(0.0352)	(0.0193)	(0.0707)	
She is married and she does not want any more children	0.00483	0.00274	0.00579	7200
	(0.0450)	(0.0283)	(0.0805)	
The woman's own health is seriously endangered by the pregnancy	-0.00312	0.000422	0.0161	7256
	(0.0270)	(0.0175)	(0.0525)	
The family has a very low income and cannot afford any more children	0.0260	0.0265	-0.0437	7188
	(0.0376)	(0.0233)	(0.0975)	
She became pregnant as a result of rape	0.0364	0.0256	-0.0701	7205
	(0.0546)	(0.0272)	(0.118)	
She is not married and does not want to marry the man	0.0352	0.0204	-0.0419	7190
	(0.0385)	(0.0228)	(0.0807)	
The woman wants the abortion for any reason	-0.0192	-0.0179	0.0657	7178
	(0.0416)	(0.0300)	(0.0815)	

Notes: Heteroskedasticity-robust standard errors are in parentheses. The abortion index is an average of the non-missing values of the seven abortion attitudes reported in Table 2-4. Standard errors are clustered at the circuit level. Regressions control for age and gender and include circuit fixed effects, state fixed effects, year fixed effects, circuit-specific time trends. Column 2 uses as an instrument the difference between the Actual and Expected number of Democrats assigned per seat. Columns 3 and 4 use as an instrument the the difference between the Actual and Expected number of Democrats assigned per abortion panel. + Significant at 10%; * Significant at 5%; ** Significant at 1%.

How long does backlash to abortion policy persist over time? In Table 2.9, we explore the longer run effects of appellate abortion decisions. We find evidence that two years after an exogenous increase in pro-choice abortion decisions, people are more likely to voice pro-choice attitudes overall. Four years after an exogenous increase in pro-choice abortion decisions, people are more likely to identify as an Independent, near Democrat. The effect can also be seen in the event study graph in Figure 2.5. Thus, the results may suggest that backlash effects dissipate quickly after a policy decision.

7 Priming Experiment

This study recruits workers through a labor market intermediary (LMI), namely, Amazon Mechanical Turk. The LMI is designed to recruit a large number of workers in a short amount of time. Through an interface provided by the LMI, registered users perform tasks posted by buyers for money. The tasks are generally simple for humans, yet difficult for computers to perform. Common tasks include captioning photographs, extracting data from scanned documents, and transcribing audio clips. The LMI also allows a researcher to implement randomization, although randomization is not inherent to the LMI. Although most buyers post tasks directly on the LMI website, they are also able to host tasks on an external site. We use this external hosting method: we post a single placeholder task containing a description of the work at the LMI and a link for workers to follow if they want to participate. The subjects are then randomized, via stratification in the order in which they arrived at the job, to one of several treatment conditions. Treatment is not revealed at this early state. All workers see identical instructions.

We ask workers to transcribe paragraphs from a Tagalog translation of Adam Smith's *The Wealth of Nations* as well as English paragraphs of dictionary definitions. This task is sufficiently tedious that no one is likely to do it "for fun," and it is sufficiently simple that

Table 2.9. The Effect of Abortion Laws on Future Years' Abortion Attitudes and Political Association

Model	*IV*	*IV*	*IV*	N
Outcomes 2 years later	*Percentage Pro-Choice*	*Number Pro-Choice*	*Number Pro-Life*	
Abortion Index	-0.0637*	-0.0414**	0.0873*	9939
	(0.0295)	(0.0150)	(0.0444)	
Strong Democrat	-0.0237	-0.0185	0.0359	14929
	(0.0319)	(0.0198)	(0.0397)	
Not a Strong Democrat	0.0157	0.0116	-0.0226	14929
	(0.0221)	(0.0176)	(0.0285)	
Independent, Near Democrat	-0.0241	-0.0229	0.0446	14929
	(0.0288)	(0.0242)	(0.0414)	
Independent	0.0515	0.0343	-0.0668	14929
	(0.0369)	(0.0243)	(0.0450)	
Independent, Near Republican	0.000403	0.000120	-0.000235	14929
	(0.0247)	(0.0157)	(0.0305)	
Not a Strong Republican	0.0274	0.0267	-0.0520	14929
	(0.0407)	(0.0240)	(0.0541)	
Strong Republican	-0.0412+	-0.0287+	0.0559+	14929
	(0.0212)	(0.0166)	(0.0296)	
Outcomes 4 years later	*Percentage Pro-Choice*	*Number Pro-Choice*	*Number Pro-Life*	
Abortion Index	-0.00583	0.00175	-0.00411	8324
	(0.0475)	(0.0259)	(0.0615)	
Strong Democrat	0.0281	0.0145	-0.0304	11990
	(0.0291)	(0.0175)	(0.0370)	
Not a Strong Democrat	-0.00168	0.00305	-0.00637	11990
	(0.0271)	(0.0164)	(0.0358)	
Independent, Near Democrat	0.0531*	0.0250**	-0.0523+	11990
	(0.0253)	(0.00932)	(0.0277)	
Independent	-0.0283	-0.0275**	0.0574+	11990
	(0.0283)	(0.00957)	(0.0344)	
Independent, Near Republican	-0.0385	-0.0176	0.0368	11990
	(0.0249)	(0.0110)	(0.0302)	
Not a Strong Republican	0.000620	0.0130	-0.0273	11990
	(0.0290)	(0.0122)	(0.0345)	
Strong Republican	-0.0109	-0.00711	0.0149	11990
	(0.0182)	(0.00873)	(0.0225)	

Notes: Heteroskedasticity-robust standard errors are in parentheses. The abortion index is an average of the non-missing values of the seven abortion attitudes reported in Tables 2-4. Standard errors are clustered at the circuit level. Regressions control for age and gender and include circuit fixed effects, state fixed effects, year fixed effects, circuit-specific time trends. Column 2 uses as an instrument the difference between the Actual and Expected number of Democrats assigned per seat. Columns 3 and 4 use as an instrument the the difference between the Actual and Expected number of Democrats assigned per abortion panel. + Significant at 10%; * Significant at 5%; ** Significant at 1%.

Table 2.10: The Effect of Exposure to Liberal Abortion Decisions on Abortion Attitudes

Model	*IV*	N
Outcome Variables		
Abortion Attitude Summary Index	0.0262	345
	(0.0203)	
It should NOT be possible for a woman to obtain a legal abortion if:		
There is strong chance of serious defect in the baby	-0.00464	345
	(0.0252)	
She is married and she does not want any more children	0.0305	345
	(0.0324)	
The woman's own health is seriously endangered by the pregnancy	-0.0135	345
	(0.0174)	
The family has a very low income and cannot afford any more children	0.0576*	345
	(0.0327)	
She became pregnant as a result of rape	0.0129	345
	(0.0220)	
She is not married and does not want to marry the man	0.0323	345
	(0.0329)	
The woman wants the abortion for any reason	0.0686**	345
	(0.0326)	

Notes: Standard errors are in parentheses. Gender, age, log error rates of the data transcription are controls. + Significant at 10%; * Significant at 5%; ** Significant at 1%.

all market participants can do the task.[79] Because subjects are unaware of an on-going experiment, differential attrition may arise at the time treatment is revealed (Reips 2001). We minimize attrition through a commitment mechanism. In all treatment conditions, workers face an identical "lock-in" task in order to minimize differential attrition before the treatment is revealed. The following are the treatments in our experiment:

1 of 3 Lock-in Tasks: Kaya sa isip o diwa na tayo ay sa mga ito, excites ilang mga antas ng parehong damdamin, sa proporsyon ng kasiglahan o dulness ng kuru-kuro.Ang labis na kung saan sila magbuntis sa kahirapan ng mga wretches nakakaapekto sa partikular na bahagi sa kanilang mga sarili ng higit pa sa anumang iba pang; dahil sa takot na arises mula sa kathang isip nila kung ano ang kani-kanilang mga sarili ay magtiis, kung sila ay talagang ang wretches kanino sila ay naghahanap sa, at kung sa partikular na bahagi sa kanilang mga sarili ay talagang apektado sa parehong miserable paraan. Ang tunay na puwersa ng mga kuru-kuro na ito ay sapat na, sa kanilang mga masasaktin frame, upang gumawa ng na galis o hindi mapalagay damdam complained ng.

Treatment 1 (Conservative Abortion Decision): The Casey ruling upheld the right of states to regulate abortions. The legislators had passed a law that restricted abortion by, among other things, requiring a mandatory waiting period, state-written counseling, parental consent and husband notification. The Court of Appeals upheld every restriction except one. Abortion, they said, was no longer a fundamental constitutional right, but rather a "limited fundamental right." This "right," in other words, could be limited by any law a legislature passed and a court thought was "reasonable."

Treatment 2 (Conservative Abortion Decision): The court upheld a law, considered the most restrictive in the nation, that required women to consult with a doctor

[79] Time and money are the most cited reasons for participation in Mechanical Turk (http://behind-the-enemy-lines.blogspot.com/2008/03/mechanical-turk-demographics.html). Some workers do it out of need. A disabled former United States Army linguist became a Turk Worker for various reasons and in nine months he made four thousand dollars (New York Times, March 25, 2007). Some drop out of college to pursue a full time career with these disaggregated labor markets (Web Worker Daily, October 16, 2008, Interview with oDesk CEO). For more information about the motivation and demographics of Mechanical Turk workers, see, e.g. Paolacci et al. (2010).

face-to-face at least 24 hours before getting an abortion, except in certain cases of rape and incest. The law required doctors to provide specific information about the procedure, risks, alternatives and social service programs, and hand out a booklet containing pictures of developing fetuses. Furthermore, the material doctors distribute will be developed by the state Department of Health and Social Services.

Treatment 3 (Liberal Abortion Decision): The court reviewed a Massachusetts law requiring parental consent before abortions can be performed on minor girls. The court struck down a part of the law that required any woman seeking an abortion to wait 24 hours after signing an informed consent form before having the abortion procedure. The court also struck down the part of the law that required the consent form to contain a description of the fetus.

Treatment 4 (Liberal Abortion Decision): Seven Missouri laws regulating abortion were challenged in a class action lawsuit. The court declared all seven statutes unconstitutional, including a requirement that physicians perform certain medical tests when there was reason to believe a fetus had reached at least 20 weeks of gestational age. These tests, which included assessments of fetal weight and lung maturity, were designed to determine the viability of an unborn child. The statute's indicated that "[t]he life of each human being begins at conception" was also struck down.

Treatment 5 (Control): The focus of art music was characterized by exploration of new rhythms, styles, and sounds. Jazz evolved and became a significant genre of music over the course of the 20th century, and during the second half of that century, rock music did the same. Jazz is an American musical art form that originated in the beginning of the 20th century in African American communities in the Southern United States from a confluence of African and European music traditions. The style's West African pedigree is evident in its use of blue notes, improvisation, polyrhythms, syncopation, and the swung note. From its early development until the present, jazz has also incorporated music from 19th and 20th century American popular music. Jazz has, from its early 20th century inception, spawned a variety of subgenres.

Since all workers will face at most one abortion-related decision, for the specification, we do not need to control for the presence of the case, but instead treat the pro-choice decision as $1, 0, -1$ when they face Liberal Abortion Decision, Control Group, and Conservative Abortion Decision, respectively. Out of a sample of 345 data entry workers, when exposed to Liberal Abortion Decisions (or not exposed to conservative decisions), workers become more conservative on two dimensions of abortion attitudes: whether it should NOT be possible to have a legal abortion if the family has very low income (liberal decisions increase this percentage by 6% points) and cannot afford any more children, and whether the woman wants abortion for any reason (liberal decisions increase this percentage by 7% points). These effects are statistically significant at the 10% and 5% level, respectively, and are similar in magnitude to the estimates in the population sample. Table 2.10 displays the effects controlling for gender, age, and log error rates. The effects are robust to the exclusion of these controls or the inclusion of additional controls, such as dummy indicators for India and the US.

8 Conclusion

Despite a large literature on backlash, there has been little formal theoretical or causal empirical work on the economics of backlash. In this paper, we take a first step at assessing the significance of the question of whether policy decisions affect policy preferences. We present a theoretical framework for understanding why laws can have expressive effects or backlash effects. Using a uniquely assembled dataset and an identification strategy that exploits the random variation connected to appellate decision-making, our study estimates the effect of abortion decisions on political preferences. Democratic appointee judges favor pro-choice abortion decisions. The random assignment of these judges increases the likelihood of pro-choice outcomes. Public opinion subsequently becomes less favorable toward abortion legality, and conservative political party identification becomes more pronounced. This effect is reversed over time, as laws are characterized by expressive effects.

Chapter 3

Physician Publications and Pharmaceutical Company Payments

1 Introduction

Pharmaceutical company payments to physicians have always been a topic of considerable discussion. Recent debate has focused on full disclosure and transparency, and there are considerable efforts to reveal all the payments pharmaceutical companies make to doctors, large or small. The rationale for full disclosure and transparency is that even small payments may bias a doctor's decision-making process, prescribing patterns, and, ultimately, affect health outcomes.

The American Medical Association (1998) and the Pharmaceutical Research and Manufacturers of America (2009) both recommend a maximum gift of $100 to physicians.[80][81] As these guidelines are not strictly enforced, several states have enacted laws requiring full disclosure of all payments from pharmaceutical companies to physicians. There has also been action taken at the national level. The Patient Protection and Affordable Care Act (2010) makes annual disclosure mandatory for all pharmaceutical payments greater than $10

[80] http://www.ama-assn.org/ama/pub/physician-resources/medical-ethics/code-medical-ethics/opinion8061.page

[81] http://www.phrma.org/sites/default/files/pdf/phrma_marketing_code_2008-1.pdf

starting in 2014.

This paper analyzes payment information from the production side, by looking at the factors determining the payment amounts pharmaceutical companies give to physicians and the effect of disclosure on payment levels. The pharmaceutical companies pay physicians to promote company products, and the effectiveness of such promotions depends on physicians' influence. I analyze payments from 12 pharmaceutical companies, comprising roughly 42% of all payments to physicians, to construct a list of all physicians who have been paid between 2009-2011. Using the Web of Science database, I divided the set of all paid physicians into those who have published in medical journals, and those who have not. Using publication history as a proxy for influence (or type), I find that prior research is a strong predictor of future payments, and is robust to alternative measures of research quantity. Most physicians in my sample, almost 80%, are paid by one pharmaceutical company and for one type of payment. The probability of being paid by more than one company almost doubles for published doctors. They are also paid larger amounts.

I take advantage of several states having sunshine laws during the payment years to analyze the causal effect of publications on payments. The disclosure of payments in my dataset is mostly made possible as a result of legal settlements with pharmaceutical companies and the US Department of Justice. For several states, however, disclosure laws were already in place during my payment sample period, and the pharmaceutical companies were required to publish all payments to the physicians residing in those states. The magnitude of the payment could be affected by whether or not such payments become public information, and the effect might vary based on the influence of physicians. The reputation effect suggests that payments as a function of publication should increase in states with existing disclosure laws. Prior research has shown that disclosure laws can lower the average amount of payments, but larger payments may actually increase.[82] I find similar results when looking at the effect of disclosure laws on the magnitude of publication on payments. Published physicians

[82] Chen et al. (2013).

command higher payments in states with existing disclosure laws. This conforms with the reputation story of higher payments serving as compensation for making such information public.

Among published physicians, I also look at which physicians the pharmaceutical companies target within research fields. I find that pharmaceutical company payments are spread over networks of researchers rather than individual specialists in a field. I look at alternative measures of networks, such as coauthorship and citation links. For both specifications, one paid physician in a network increases the likelihood that others will also be paid by the same pharmaceutical company and for the same category. Since physicians working on the same research and being paid by the same company are more likely to be aware of others' payments (although perhaps, not the magnitude of the payments), this transparency effect is consistent with higher disclosure leading to higher payment result.

The rest of the paper is organized as follows. Section 2 covers the literature review. Section 3 presents the model. Section 4 discusses the main data sources and presents preliminary findings. Section 5 presents the main results of the paper and conducts robustness checks. Section 6 concludes.

2 Literature Review

Pharmaceutical companies' promotional spending has been steadily growing in the past several decades. By some estimates, it has reached $57.5 billion with most of it going directly as payments to physicians. To curtail excessive courting of doctors, both the American Medical Association (1998) and the Pharmaceutical Research and Manufacturers of America (2009) have suggested a limit of $100 for gifts from pharmaceutical companies to physicians.[83][84] Some authors, such as Katz et al. (2010), have gone as far as advocating complete elimina-

[83] http://www.ama-assn.org/ama/pub/physician-resources/medical-ethics/code-medical-ethics/opinion8061.page

[84] http://www.phrma.org/sites/default/files/108/phrma_marketing_code_2008.pdf

tion of any type of gifts from pharmaceutical companies to physicians. They argue that even small gifts can generate adverse effects and bias the incentives of physicians.

To address this effect, three states have passed strong disclosure laws, or "sunshine laws," where pharmaceutical companies are required to report payments made to doctors. Minnesota was the first state to pass such a law in January of 1997; it stipulated that payments from pharmaceutical companies to healthcare providers to be reported and be made available to the general public. Vermont and Massachusetts followed suit in June, 2002 and July, 2009, respectively. California, the District of Columbia, Maine, and West Virginia also now mandate reporting of payments by drug companies to healthcare providers, though such reports are not usually available to the public and are thus considered to be weaker disclosure laws. In addition, there is a pending bill in Ohio, and on a national scale, a federal bill that would require public disclosure of all payments from drug manufacturers to physicians (Patient Protection and Affordable Care Act (2010), Chen et al. (2013)).

The literature on the influence of payments on physicians and the effect of disclosure laws is mixed. Loewenstein et al. (2011) argue that disclosures in general may not be an effective remedy for conflicts of interest and could have unintended consequences. Most of the prior literature on pharmaceutical company payments to physicians takes advantage of the differences in laws between states. Ross et al. (2007) look at the early disclosure laws and reports from Vermont and Minnesota, and find mixed results in their effectiveness. They partly attribute the quality of earlier years' data on their results. Wazana (2000) uses self-reported disclosures to look at the effect of payments on physician behavior. Haayer (1982) and Orlowski and Wateska (1992) find some evidence of gifts to physicians affecting subsequent prescribing behavior. Pham-Kanter et al. (2012) find limited evidence of the effect of payments in West Virginia and Maine. On the other hand, Cain et al. (2005) and Ben-Shahar and Schneider (2010) argue that disclosure laws may have the opposite effect. Chen et al. (2013) look at the effect of disclosure laws on payments to physicians. They find strong disclosure laws reduce the overall amount of payments, but increase larger payments of \$100 or more.

I abstract away from the possible change in incentives and behavior of physicians after payments and instead focus on pharmaceutical companies' optimization of payment structures. Using prior publication count as proxy for physician type, I look at the effect of publications and disclosure on payments. Prior research has generally omitted the pharmaceutical company's objective function. Understanding which physicians the drug companies target helps explain how conflicts of interest come about and what the possible solutions may be.

3 Model

I present a simple model that illustrates the main intuition of the effects of publication and disclosure law on payments. The pharmaceutical companies want to target physicians to promote company products. However, they do not know the extent of the influence a given physician has, and how effective she will be in promoting the products. Prior publication serves as a signal for influence the physicians have over their peers and in medical practice. In equilibrium, pharmaceutical companies will target publishing physicians more heavily than unpublished ones. Since, on average, the pharmaceutical companies earn higher revenues from published physicians, payments to those physicians will be larger. This is the first result from the model. Next, I look at the effect of disclosure laws on payments. Disclosure laws act as a negative cost of entering into a contract with a pharmaceutical company, which is equivalent to increasing the value of the outside option for physicians. The second prediction of the model is that the increase in the outside option will result in higher payments to publishing physicians.

Suppose a physician has type θ that measures the effectiveness as a candidate to promote pharmaceutical company products. The overall output also depends on effort e, in a way that the project succeeds with probability $p(e)$ and fails with probability $1 - p(e)$. I assume the standard assumptions that the payoff function is concave in e, meaning $p' > 0$, and $p'' < 0$. When the project is successful, the payoff is θ, and 0 when it is not. The overall payoff is then given by:

$$Y = \theta p(e) \tag{3.1}$$

The distribution of θ depends on whether the physician has published, $F_p(\theta)$, or not, $F_n(\theta)$. I assume that $F_p(\theta)$ stochastically dominates $F_n(\theta)$, in the sense that having (relevant) prior publications will make the physician a more effective candidate for the pharmaceutical company:

$$F_p(\theta) \leq F_n(\theta) \tag{3.2}$$

Exerting effort e costs the physician $c(e)$ with standard assumption of convex costs: $c'(e) > 0$, and $c''(e) > 0$. The physician has outside option of $\bar{u}$. If the payment from the pharmaceutical company to the doctor is w, then the payoff structure needs to satisfy:

$$w - c(e) \geq \bar{u} \tag{3.3}$$

Moreover, I specify that transfers cannot go the opposite way: $w \geq 0$.

The pharmaceutical company's objective, for a given physician type, is to maximize the payoff function:

$$\max_{w}\{\theta p(e) - w\} \tag{3.4}$$

subject to,

$$e \in \arg\max_{\tilde{e}}\{w - c(\tilde{e})\}$$

$$w - c(e) \geq \bar{u}$$

If there is no informational asymmetry, the First Best solution will involve maximizing the entire surplus:

$$e^* \in \arg\max_{\tilde{e}}\{\theta p(e) - c(\tilde{e})\} \tag{3.5}$$

with the solution for w being:

$$w = c(e^*) + \bar{u} \tag{3.6}$$

The First Order Condition (FOC) will be:

$$\theta p'(e^*) = c'(e^*)$$

or

$$e^* = e^*(\theta) \tag{3.7}$$

where $e^{*'} > 0$. Thus, the payoff to the pharmaceutical company will be: $\theta p\,(e^*) - c\,(e^*) - \bar{u}$. Suppose θ_0 is the cutoff for non-negative profits:

$$\theta_0 p\,(e\,(\theta_0)) - c\,(e\,(\theta_0)) - \bar{u} = 0 \tag{3.8}$$

Then, the proportion of doctors who get paid will be $1 - F\,(\theta_0)$, and the average wage will be:

$$E\,(w) = \int_{\theta>\theta_0} [c\,(e^*\,(\theta)) + \bar{u}]dF = \int_{\theta>\theta_0} c\,(e^*\,(\theta))\,dF + (1 - F\,(\theta_0))\,\bar{u} \tag{3.9}$$

Since F_p stochastically dominates F_n it follows that

$$E_p\,(w) \geq E_n\,(w) \tag{3.10}$$

Thus, the model predicts that publishing physicians are being paid more than non-publishing ones.

To consider the effect of disclosure, I assume that such a law increases the cost of each physician associating with the pharmaceutical company. Alternatively, it increases their outside utility, $\bar{u}$. The new participation constraint becomes

$$w = c(e^*) + \bar{u} + \alpha \tag{3.11}$$

where α is the added cost of disclosure. The new setup does not change the optimal First Order Condition, but will raise the cutoff value, θ_0':

$$\theta_0' p\left(e\left(\theta_0'\right)\right) - c\left(e\left(\theta_0'\right)\right) = \bar{u} + \alpha \tag{3.12}$$

Since, for a given effort, average payment to physicians increases, lower payments and lower efforts are no longer counted. The average payment to physicians will then increase, and the published physicians will be paid more under disclosure.

4 Data

4.1 Physician Payments

The two main data sources are payments to physicians and the list of doctor publications. The physician payment information comes from the Propublica dataset.[85] It is a publicly available dataset of pharmaceutical company payments to physicians, aggregated from available information on individual company websites. The pharmaceutical companies disclosed such payments and made them available online mainly as a result of legal settlements with the US Department of Justice;[86] one or two firms did so voluntarily. For some voluntary disclosures, for instance Allergen, the information was later removed from the company website.[87] According to Propublica, the collected data includes all disclosed payments of pharmaceutical companies to physicians for the purpose of promoting pharmaceutical company products and does not include payments for speaking at medical education courses, or as part of principal investigator funding.[88] The data not only lists the type and payment to

[85] http://projects.propublica.org/docdollars/companies

[86] See, for instance: http://www.justice.gov/usao/pae/News/2009/jan/lillysignedsettlementagreement.pdf, http://www.justice.gov/opa/pr/2008/September/08-civ-860.html, http://www.justice.gov/usao/pae/Pharma-Device/astrazeneca_settlementagreement.pdf.

[87] http://projects.propublica.org/docdollars/companies

[88] http://www.propublica.org/article/about-our-pharma-data

the physician by the respective company, but also specifies the date of the payments, as well as the physician's address, if available. The detailed address information is provided by the physicians themselves, and in a small fraction of cases incorrect information, like "Anytown," or "Any Street" is provided.

The payments come from 12 pharmaceutical companies. They span from the third quarter of 2009 to the second quarter of 2011 and account for roughly 42% of all the pharmaceutical payments to physicians. They are highly unevenly distributed among categories and companies. Small payments dominate the list, the most popular category being meals. The biggest contributor, by a large margin, is Pfizer, accounting for over 68% of all the payments to physicians. The variation in payment information among different companies stems from the time individual legal settlements took place. As disclosure requirements become binding over time, there are indications that future updates are gradually becoming more balanced. The data contains payments directed to 316,622 physicians amounting to more than $316 million.

The payment categories for doctors are classified as consulting/advisory, speaking/honoraria, research/clinical trial, meals, travel/lodging, items, other, or combinations of those seven. Despite the large data size, the vast majority are a one-time payment to a physician. In the dataset, the physician is usually paid by only one company and under one category. This setup provides an intuitive measure of the relationship between a company and a physician, and one can easily interpret the results as a form of link between the two.

4.2 Publication Count

I next look at the data structure of the publications. Taking a list of all publications from the Web of Science, restricted to medical journals, I then match physician names to publications with the same author name. In order to limit the number of false positives, I drop all common names from the list of physicians. The procedure is similar to the method used in previous research in the literature (Jacob and Lefgren 2011, and Li 2012). For example, I drop all

Table 3.1. The Set of Occurences for Uncommon Names

Frequency of Occurences	*Unique Names*	*Total Names*
1	66,100	66,100
2	16,663	33,326
3	6,352	19,056
4	3,184	12,736
5	1,918	9,590
6	1,241	7,446
7	887	6,209
8	734	5,872
9	516	4,644
		164,979

This table shows the result of removing common names for matching physicians to authors of medical journals. The resulting distribution shows the frequency of all remaining last names and the number of matching doctors.

last names that appear more than 10 times in the list. As anecdotal evidence, the most common last names in my data are the same that Jacob and Lefgren (2011) encountered in their dataset: Miller, Smith, and Johnson. Among frequently occurring names, I also have non-Anglo-Saxon last names, such as Patel, Nguyen, and Wu, which have not appeared before. Eliminating physicians with common names cuts the dataset by about 60%. This is on par with the sample size cut that Jacob and Lefgren (2011) experienced (around 55%). I am left with 164,979 physicians with uncommon names, who are paid by pharmaceutical companies. As Table 3.1 illustrates, 40% of the last names appear only once in the dataset, and 60% appear only once or twice. In the dataset, the physicians are identified not just by last name, but also by first name, middle name (or initial) and, possibly, geographic location. Therefore, the same last name is not sufficient to indicate the same person in the sample.

Further, Table 3.2 illustrates that the number of payments from the pharmaceutical companies ranges from one to eight. The number of categories from which doctors are paid also ranges from one to eight. For example, I have four doctors who are paid by eight different pharmaceutical companies, but only one is paid across eight categories. The vast majority of the payments per physician are from one company (80%), and for one category (84%). The summary statistics in Table 3.3 show that the mean payment is roughly $1,885. The distribution, however, is very right-skewed with the maximum payment being over $429,000. The table also provides a breakdown of the payments by pharmaceutical company and across categories. As pointed out earlier, the vast majority of payments (83%) are for meals. However, other categories such as combination, items, and speaking are also important components of payments to physicians.

Among the paying pharmaceutical companies, the largest amount comes from Pfizer, accounting for about 69% of all payments. Other companies, such as Cephalon, Eli Lilly, and Allergen also make payment contributions to physicians, but not at the same level as Pfizer. (Or at least their detailed payment information has not been made fully public). The remaining companies are not significant players in the payment market. Such a disparity, especially when compared to their market share, is explained by the way that the data is constructed and depends on when the companies were forced to disclose their payments and make such information public. There is evidence that over time the disclosed payment information is more balanced.

I look at the payments of pharmaceutical companies to physicians based on their type, as defined by their publication information. The number of physicians who have ever published is 13,295 or roughly 8% of the possible 164,979 doctors in the dataset. Physicians who published are on average paid higher than non-published ones: a mean of $4,584 versus $1,884.[89] The distribution of payments also shows that the publishing physicians are more likely to be paid by multiple companies and for multiple categories. When looking at the frequency of each category, the published physicians are paid more in categories that could be relevant in targeting researchers: consulting, research, speaking, and travel. They are paid similar shares in categories typically not considered specific to research: items and meals. This can serve as further evidence that the pharmaceutical companies target publishing

[89]However, the maximum payment is lower for the published physicians than it is for unpublished ones.

Table 3.2. Number of Payments by Company or Category

All Physicians			Publishing Physicians		
Number of Pharma Co.	*Frequency*	*Percentage*	*Number of Pharma Co.*	*Frequency*	*Percentage*
1	131,671	79.81%	1	8,180	61.53%
2	24,709	14.98%	2	3,449	25.94%
3	6,796	4.12%	3	1,254	9.43%
4	1,365	0.83%	4	309	2.32%
5	326	0.20%	5	71	0.53%
6	94	0.06%	6	26	0.20%
7	14	0.01%	7	5	0.04%
8	4	0.00%	8	1	0.01%
Total	164,979	100.00%	Total	13,295	100.00%
Number of Categories	*Frequency*	*Percentage*	*Number of Categories*	*Frequency*	*Percentage*
1	138,874	84.18%	1	9,944	74.80%
2	19,459	11.79%	2	2,162	16.26%
3	4,232	2.57%	3	665	5.00%
4	1,789	1.08%	4	350	2.63%
5	513	0.31%	5	135	1.02%
6	99	0.06%	6	33	0.25%
7	12	0.01%	7	5	0.04%
8	1	0.00%	8	1	0.01%
Total	164,979	100.00%	Total	13,295	100.00%

This table shows the distribution of payments by number of pharmaceutical companies or number of categories, for all physicians and for publishing physicians only.

Table 3.3. Frequency of Payments by Company or Type of Category

All Physicians		Publishing Physicians	
Pharma Co.	*Percentage*	*Pharma Co.*	*Percentage*
Allergan	13.07%	Allergan	11.37%
AstraZeneca	0.80%	AstraZeneca	1.52%
Cephalon	18.73%	Cephalon	30.80%
EMDSerono	2.05%	EMDSerono	3.82%
EliLilly	15.79%	EliLilly	26.48%
GSK	2.64%	GSK	6.66%
JJ	0.66%	JJ	1.47%
Merck	0.78%	Merck	2.04%
Novartis	0.81%	Novartis	1.53%
Pfizer	68.82%	Pfizer	65.04%
Valeant	2.58%	Valeant	4.18%
ViiV	0.12%	ViiV	0.26%
Category Type	*Frequency*	*Category Type*	*Frequency*
Combination	13.07%	Combination	11.37%
Consulting	2.42%	Consulting	7.27%
Items	10.21%	Items	12.13%
Meals	83.55%	Meals	83.83%
Other	0.75%	Other	1.58%
Research	1.25%	Research	3.10%
Speaking	6.70%	Speaking	12.47%
Travel	3.81%	Travel	8.00%
Payment		*Payment*	
Mean	$1,884.79	Mean	$4,584.14
St Dev	$10,808.69	St Dev	$18,205.02
Min	$1	Min	$1
Max	$429,328	Max	$327,103

This table shows the frequency of payments for each physician by pharmaceutical companies and type of categories. The results are reported for all physicians and for publishing physicians only.

physicians for their research expertise, and publication measures can be used as a proxy for effectiveness. Publishing physicians will, therefore, command higher fees for their services. The underlying story of the overall sample – physician payment have a large mode of one payment for one category – still holds true for published physicians: The majority (61%) of physicians are paid by one company, and an even bigger majority (75%) of the physicians are paid under only one category.

On the publication side, the set of 13,295 physicians accounts for 90,122 published papers. I restrict the earliest publication date to 1990 to ensure that the correct people are matched to their publications and that the published research is scientifically relevant during the payment period of 2009-2011. In addition, the Web of Science extends only to 2009, meaning that all physician publications occurred before the payments in my sample. This is important because one might think of the direction of causality going the opposite way, with payments inducing more publications.

5 Results

5.1 Relationship Between Payment and Publication

Does physician type, defined as having prior publications, lead to higher payments from pharmaceutical companies? I analyze this effect by looking at various specifications of publication measures. The simplest specification, derived from the model, is an indicator variable that is 1 if the physician has a prior publication and 0 otherwise. As a robustness check, I also look at the total publication count, the total citation count, and also the log of publication and citation counts. Table 3.4 indicates that in all specifications of publication measures there is a strong positive relationship between publication and citation. Having a publication accounts for $1,180 more in payments. Increasing the publication count by 1% adds an additional $500 in payments. When the dependent variable is citation (or log-citation) count, year fixed effects become necessary, as older years will have fewer citations simply due to truncated data. The Web of Science database on publications and citations ends in 2009, and papers that are published earlier will have, on average, more citations than later publications. The year fixed effects in the regression will account for the negative bias on citations over time. In all specifications, heteroskedasticity-robust standard errors are included with the point estimates.

The results indicate a positive, and economically and statistically significant relationship between past publication and subsequent higher payments for physicians. The baseline OLS results, however, may be problematic due to omitted variable bias that is correlated with publication count and subsequent payment amount. I account for the possible endogeneity in two ways. First, I restrict the sample by looking at only the most established publishing physicians, who I define as those having a publication prior to 2001. By looking at the cutoff sample only, I try to account for the quality of physician effectiveness that is not correlated with the error term. Looking at physicians who have published earlier will presumably isolate the most established researchers and also lessen any time-persistent omitted variable effects that are correlated with prior publications and future payments. Table 3.5 shows that the positive relationship between payment and publication still holds when accounting for earlier years only.

The second way to account for possible endogeneity between publication and payments is to look at variations that affect payment levels, but not the link from publications (or physician types) to payments. State-level variation in disclosure laws provides such a natural setup. By dividing the physicians into two groups – those who reside in states with existing strong disclosure laws, and those who do not, I can isolate an exogenous variation of payments to physicians that is orthogonal to physician's publication record. Table 3.6 presents the results of regressing payment on publishing measures when controlling for existing disclosure laws. The publication effect on payments remains positive and significant. The interaction term is also positive and significant in all specifications, consistent with the model of higher disclosure leading to higher payments.

Table 3.4. The Relationship Between Publication and Subsequent Payments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Payment	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*
Publ. Indicator	2020.8***	1178.2***							
	(115.0)	(108.5)							
Publ. Count			13.25***	7.293**					
			(3.525)	(2.764)					
Citation Count					1.004***	0.622***			
					(0.195)	(0.179)			
log(Publ. Count)							904.5***	509.3***	
							(61.18)	(58.40)	
log(Cit. Count)									457.9***
									(28.49)
Fixed Effects		Yes		Yes		Yes		Yes	
N	153612	153612	153612	153612	153612	153612	153612	153612	153612
R-sq	0.005	0.127	0.000	0.125	0.001	0.125	0.003	0.126	0.004

Heteroskedasticity robust standard errors are in parentheses. The dependent variable is the amount of payments the physician received fro
pharmaceutical companies. + Significant at 10%; * Significant at 5%; ** Significant at 1%; *** Significant at 0.1%.

Table 3.5. The Effect of Strong Disclosure Law on Pre-2001 Publication and Subsequent Payments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pre-2001 Levels	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*
Publ. Indicator	1995.3***	1135.3***								
	(134.8)	(126.8)								
Publ. Count			35.12***	20.13**						
			(8.876)	(6.926)						
Citation Count					1.194***	0.716**				
					(0.235)	(0.223)				
log(Publ. Count)							1127.3***	643.8***		
							(88.72)	(84.25)		
log(Cit. Count)									447.3***	250.4***
									(32.49)	(30.76)
Fixed Effects		Yes		Yes		Yes		Yes		Yes
N	153612	153612	153612	153612	153612	153612	153612	153612	153612	153612
R-sq	0.004	0.127	0.000	0.126	0.001	0.126	0.003	0.126	0.003	0.126

Heteroskedasticity robust standard errors are in parentheses. The dependent variable is the amount of payments the physician received from the pharmaceutical companies. + Significant at 10%; * Significant at 5%; ** Significant at 1%; *** Significant at 0.1%.

Table 3.6. The Effect of Strong Disclosure Laws on Publication and Subsequent Payments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Payment	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*	*Payment*
Publ. Indicator	923.6***	692.9***								
	(164.2)	(170.1)								
Publ. Count			13.26***	7.263*						
			(3.684)	(2.883)						
Citation Count					0.985***	0.612***				
					(0.195)	(0.179)				
log(Publ. Count)							897.9***	505.8***		
							(61.18)	(58.41)		
log(Cit. Count)									454.6***	258.6***
									(28.50)	(27.11)
Strong Discl. Interaction	1254.8***	670.9***	12.595***	6.725***	1.2381***	0.6601***	1204.3***	643.5***	1188.5***	634.9***
	(190.8)	(178.9)	(1.909)	(1.788)	(0.1909)	(0.179)	(190.4)	(178.9)	(190.4)	(178.9)
Fixed Effects		Yes		Yes		Yes		Yes		Yes
N	153612	153612	153612	153612	153612	153612	153612	153612	153612	153612
R-sq	0.001	0.125	0.001	0.125	0.001	0.125	0.004	0.126	0.005	0.126

Heteroskedasticity robust standard errors are in parentheses. The dependent variable is the amount of payments the physician received from the pharmaceutical companies. + Significant at 10%; * Significant at 5%; ** Significant at 1%; *** Significant at 0.1%.

5.2 Relationship of Payments Between Authors

If the pharmaceutical companies target established physicians through publication measures, how do they select among two published physicians who are conducting similar research? If two coauthors are equally likely to be chosen by the pharmaceutical company, would the company target only one, or try to a create network of authors with similar research interests? I analyze whether coauthorship increases or decreases the likelihood of being paid by the same pharmaceutical company and for the same category. I limit the dataset only to those articles with a coauthor paid by the pharmaceutical company in the 2009-2011 timespan.

Figures 3.1 and 3.2 show the time trend of author counts from 1990 to 2009. As Figure 3.2 shows, over time there is a general trend of increased number of authors per paper. For the unmatched sample, which includes both common and uncommon names, the average number of authors per paper steadily grows from 4.6 in 1990 to over 7.5 by the end of the sample in 2009. Figure 3.1 restricts the list of coauthors paid by pharmaceutical companies. The restricted sample of coauthors with uncommon names also shows an overall positive trend that starts at 1.14 and grows to 1.17 by 2009.

Trimming the dataset to only coauthored papers significantly reduces the number of publications. I look at the probability of being paid by the same pharmaceutical company or for the same category, conditional on coauthorship. To do this, I use logit and probit specifications where the dependent variable is 1 if the author and coauthor are paid by the same company or for the same category. As a robustness check, I also weight each observation by the number of times an author appears in the data, so that the author-coauthor link and the coauthor-author link are not counted twice and would not artificially increase the sample size. Tables 3.7 and 3.8 indicate that for the major pharmaceutical companies, there is a significant positive relationship between a coauthor and an author being paid by the same company. There is also a significant positive relationship in the likelihood of being paid for the same type of category. The results are not statistically significant for the pharmaceutical companies that have a smaller share of payments to physicians.

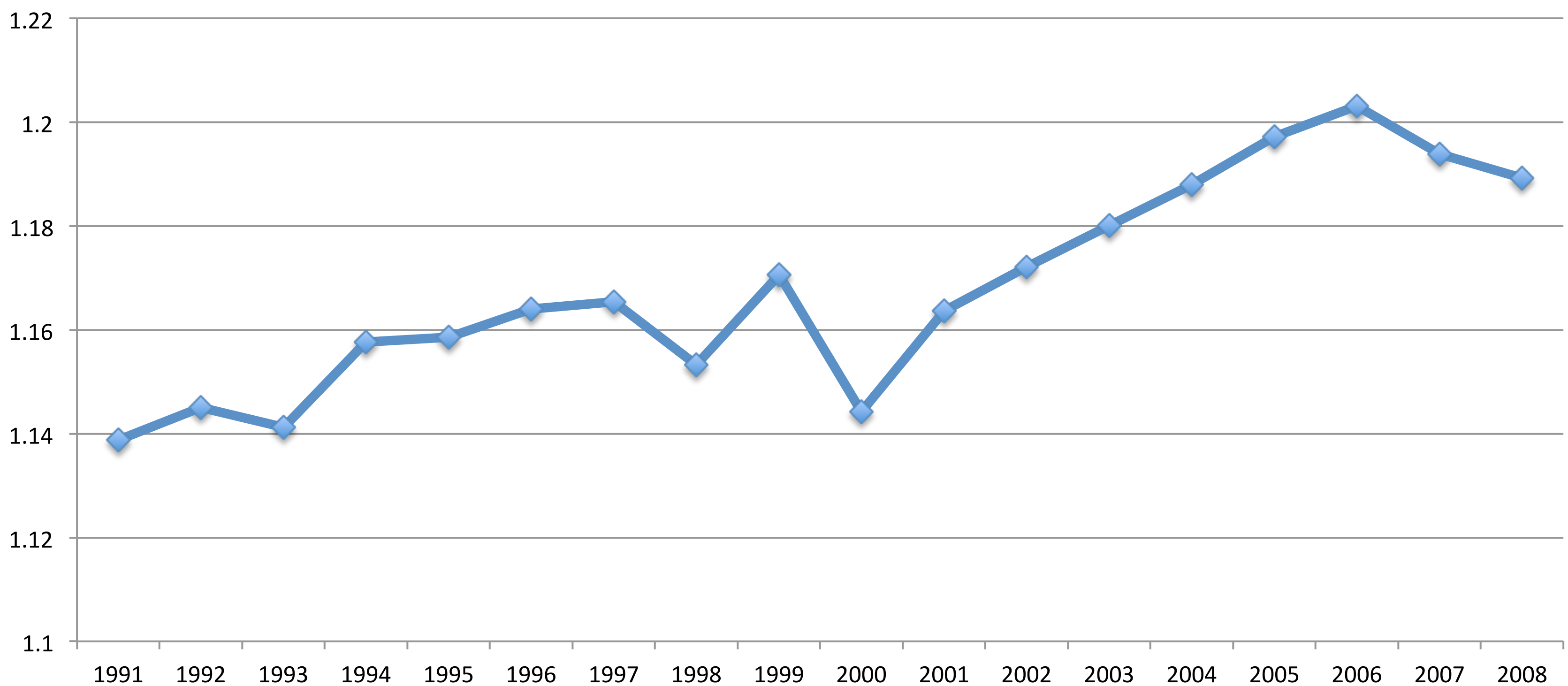

Figure 3.1: Average Number of Paid Coauthors

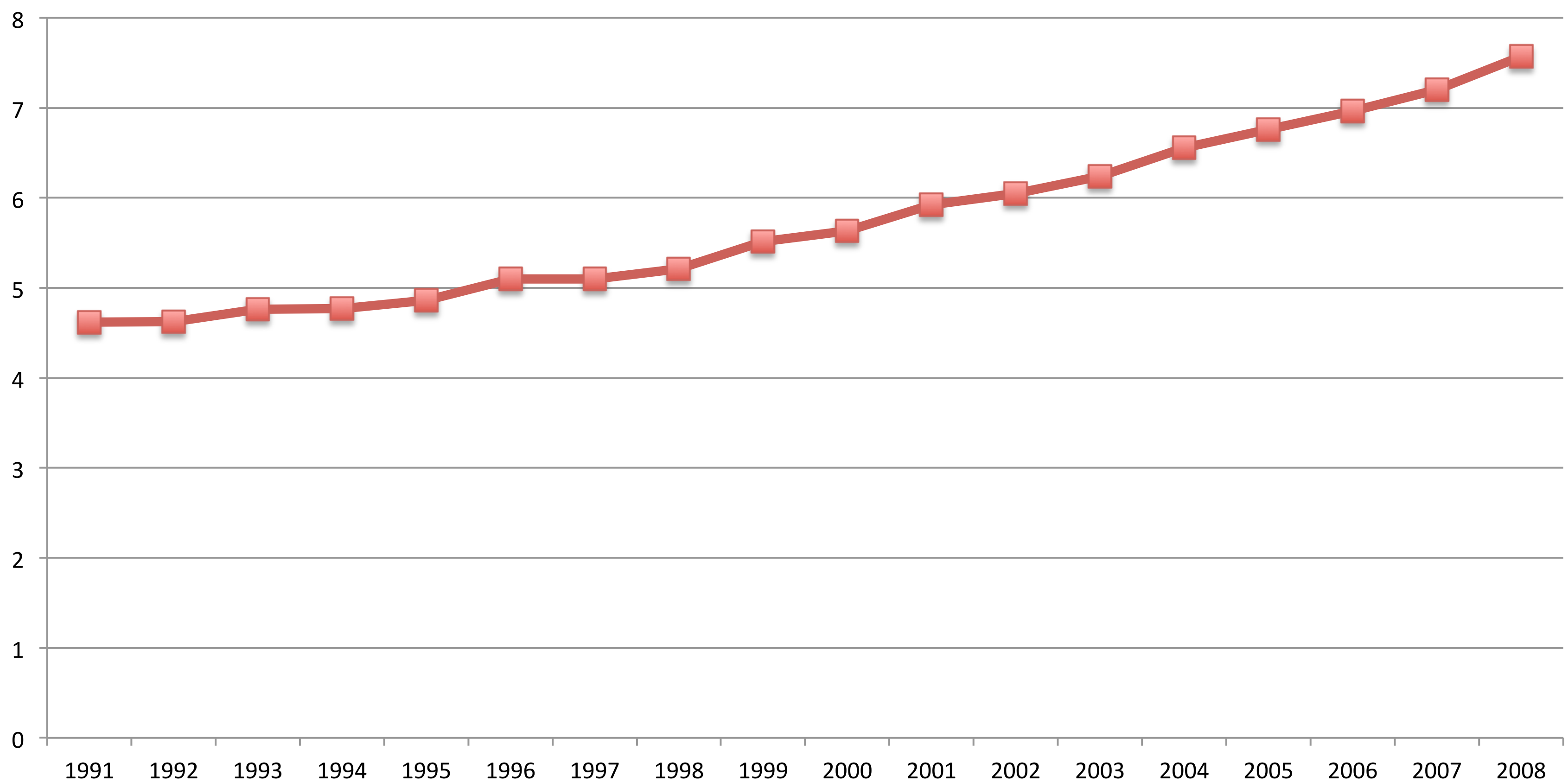

Figure 3.2: Average Author Count for All Publications

Table 3.7. The Relationship of Coauthorship on Payment Types Across Companies

	(1)	(2)	(3)	(4)	(5)	(6)
	Logit	*Logit*	*Logit*	*Probit*	*Probit*	*Probit*
Allergan	0.783***	0.538***	0.768***	0.380***	0.250***	0.374***
	(0.0587)	(0.0648)	(0.0588)	(0.0329)	(0.0383)	(0.0329)
AstraZeneca	-0.346**	-0.364**	-0.686***	-0.157*	-0.178*	-0.341***
	(0.132)	(0.136)	(0.142)	(0.0750)	(0.0748)	(0.0804)
Cephalon	1.018***	0.994***	1.006***	0.543***	0.544***	0.536***
	(0.0390)	(0.0420)	(0.0391)	(0.0213)	(0.0232)	(0.0214)
EMDSerono	-0.219*	-0.124	-0.229*	-0.145**	-0.0835+	-0.150**
	(0.0972)	(0.0854)	(0.0971)	(0.0557)	(0.0495)	(0.0557)
EliLilly	1.000***	0.911***	0.959***	0.531***	0.497***	0.511***
	(0.0402)	(0.0431)	(0.0405)	(0.0220)	(0.0240)	(0.0222)
GSK	-0.00948	0.192**	-0.202**	-0.0200	0.112**	-0.129**
	(0.0712)	(0.0671)	(0.0768)	(0.0415)	(0.0392)	(0.0443)
JJ	0.319*	0.359**	0.0806	0.199*	0.218**	0.0463
	(0.156)	(0.133)	(0.169)	(0.0915)	(0.0795)	(0.0986)
Merck	0.0558	0.0363	-0.116	0.0234	0.00724	-0.0626
	(0.114)	(0.116)	(0.117)	(0.0664)	(0.0663)	(0.0679)
Novartis	-0.133	-0.0377	-0.265	-0.103	-0.0417	-0.171+
	(0.182)	(0.178)	(0.186)	(0.102)	(0.100)	(0.104)
Pfizer	2.221***	2.217***	2.199***	1.300***	1.315***	1.287***
	(0.0361)	(0.0378)	(0.0363)	(0.0203)	(0.0214)	(0.0204)
Valeant	-0.745***	-0.738***	-0.713***	-0.479***	-0.464***	-0.460***
	(0.0835)	(0.0779)	(0.0829)	(0.0488)	(0.0451)	(0.0488)
ViiV	-1.112***	-1.182***	-0.950***	-0.698***	-0.737***	-0.582***
	(0.265)	(0.191)	(0.270)	(0.154)	(0.115)	(0.157)
Avg Payment			0.0113***			0.00607***
(in 000's)			(0.00165)			(0.000850)
Citation Count			-0.0674**			-0.0406**
(in 00's)			(0.0247)			(0.0145)
Const	-1.272***	-1.380***	-1.240***	-0.708***	-0.792***	-0.689***
	(0.0389)	(0.0412)	(0.0398)	(0.0218)	(0.0235)	(0.0223)
Weights		Yes	Yes		Yes	Yes
N	23178	23178	23178	23178	23178	23178

Standard errors are in parentheses. The dependent variable is 1 if the coauthors are paid by the same pharmaceutical company. + Significant at 10%; * Significant at 5%; ** Significant at 1%; *** Significant at 0.1%.

Table 3.8. The Relationship of Coauthorship on Payment Types Across Categories

	(1)	(2)	(3)	(4)	(5)	(6)
	Logit	*Logit*	*Logit*	*Probit*	*Probit*	*Probit*
Combination	0.930***	0.795***	0.936***	0.416***	0.349***	0.418***
	(0.0873)	(0.0921)	(0.0881)	(0.0451)	(0.0456)	(0.0454)
Consulting	0.405***	0.563***	0.455***	0.137**	0.218***	0.160**
	(0.0934)	(0.107)	(0.0987)	(0.0509)	(0.0576)	(0.0531)
Items	0.854***	0.883***	0.858***	0.359***	0.370***	0.361***
	(0.0838)	(0.0982)	(0.0841)	(0.0409)	(0.0468)	(0.0410)
Meals	3.749***	3.883***	3.760***	2.034***	2.136***	2.038***
	(0.0759)	(0.0893)	(0.0770)	(0.0375)	(0.0420)	(0.0380)
Other	1.176***	1.333***	1.175***	0.355***	0.438**	0.358***
	(0.210)	(0.298)	(0.210)	(0.108)	(0.163)	(0.107)
Research	0.894***	0.900***	0.983***	0.445***	0.453***	0.487***
	(0.125)	(0.121)	(0.136)	(0.0686)	(0.0693)	(0.0732)
Speaking	1.149***	0.948***	1.185***	0.524***	0.428***	0.539***
	(0.0835)	(0.0939)	(0.0905)	(0.0418)	(0.0458)	(0.0452)
Travel	-0.0791	0.0185	-0.0407	-0.0674	-0.0282	-0.0506
	(0.118)	(0.131)	(0.122)	(0.0606)	(0.0649)	(0.0622)
Avg Payment			-0.00330			-0.00138
(in 000's)			(0.00254)			(0.00117)
Citation Count			-0.102***			-0.0588***
(in 00's)			(0.0302)			(0.0159)
Const	14.27	22.83*	19.93*	-0.654***	-0.810***	10.10*
	(9.400)	(9.713)	(9.579)	(0.0387)	(0.0438)	(5.004)
Weights		Yes	Yes		Yes	Yes
N	23178	23178	23178	23178	23178	23178

Standard errors are in parentheses. The dependent variable is 1 if coauthors are paid for the same category. + Significant at 10%; * Significant at 5%; ** Significant at 1%; *** Significant at 0.1%.

This indicates that instead of targeting one or two people who are "experts" in their respective fields, the pharmaceutical companies are targeting networks of researchers. They are also paying them for similar services. The result is true for seemingly complementary events, such as meals and travel, where, for example, one event can bring many people with the same interests together. But it is also true for seemingly substitutable categories, such as consulting and speaking. The results indicate that the pharmaceutical companies are interested in networks of physicians and seem to be expansive even in categories where coauthors could act as competitors. To give a concrete example, only 3% of the physicians in the coauthor sample were paid by Merck in the research category; and given that they have been paid, the probability that their coauthor will also be paid by Merck for research jumps to 10%, and the difference is statistically significant.

Moreover, if coauthors are more likely to be paid by the same pharmaceutical company, and for the same category, they are also more likely to be aware of others in a research field who are also being paid (though perhaps not the magnitude of the payments). This may serve as a quasi-disclosure effect within the research field. Since coauthors are, on average, paid more than single authors, such a finding conforms with the same disclosure effect on payments resulting in larger payments for coauthors. This may even induce more inclusion of other researchers into the payment network. Though, additional data is needed to fully establish the latter result.

5.3 Relationship of Payments Between Citing and Cited Authors

As a robustness check for payment networks, I expand the research field definition beyond coauthorship by looking at the effect of prior citation links on subsequent payments by the same pharmaceutical company and for the same category. I link researchers who cite each other but have previously not been coauthors, and look at the effect of such links on payments. The result of coauthorship also holds true for cited and citing authors. Tables 3.9 and 3.10 show that physicians working in the same type of research, who cite each other but have previously not been coauthors, are more likely to be paid by the same pharmaceutical company and for the same category of payments.

6 Conclusion

I look at how pharmaceutical companies determine the payments they give to physicians to promote their products. Merging the Propublica dataset that reports roughly 42% of all payments to doctors from 2009 to 2011, with the Web of Science journal database, I identify payments to physicians along with their publication history. Using publication measure as a proxy for physician effectiveness, I find that physicians with more publications are paid higher amounts, on average. The result conforms to model predictions and is robust to alternative specifications of publication measures.

To account for possible endogeneity, I use the variation in state laws on disclosure: Certain states have strong laws mandating publication of all payments to physicians, while others do not. Looking at the effect of disclosure laws on payments to publishing physicians, I find a positive effect of disclosure law on payments for publishing physicians consistent with the model. The result, also borne out by the model through increasing physician's outside option, is statistically and economically significant. The specification is also robust to accounting for earlier years of publications only.

If the pharmaceutical companies target publishing physicians, how do they choose a particular doctor in a given research field or choose between two coauthors? I find strong network effects of pharmaceutical companies targeting entire research fields versus only one or two specialists. Coauthors are more likely to be paid by the same company and for the same category. I find a similar effect for networks comprised of citation links who have never previously been coauthors. The citing and the cited authors are also more likely to be paid by the same company and for the same category. The effect is significant for large pharmaceutical payers in the dataset. The network effect is consistent with the disclosure of

Table 3.9. The Relationship of Citations and Payment Types Across Companies

	(1)	(2)	(3)	(4)	(5)	(6)
	Logit	*Logit*	*Logit*	*Probit*	*Probit*	*Probit*
Allergan	0.288***	0.283***	0.286***	0.168***	0.165***	0.165***
	(0.0422)	(0.0490)	(0.0423)	(0.0243)	(0.0287)	(0.0287)
AstraZeneca	0.0158	0.199+	-0.0439	0.0123	0.120+	0.0775
	(0.0790)	(0.108)	(0.0821)	(0.0454)	(0.0620)	(0.0650)
Cephalon	0.832***	0.839***	0.834***	0.478***	0.482***	0.484***
	(0.0249)	(0.0330)	(0.0249)	(0.0140)	(0.0186)	(0.0186)
EMDSerono	-0.00169	-0.182*	-0.00753	-0.00391	-0.109*	-0.109*
	(0.0713)	(0.0808)	(0.0715)	(0.0414)	(0.0474)	(0.0475)
EliLilly	0.721***	0.648***	0.724***	0.413***	0.372***	0.368***
	(0.0260)	(0.0344)	(0.0262)	(0.0146)	(0.0194)	(0.0194)
GSK	-0.0553	0.0383	-0.0828*	-0.0302	0.0235	0.00182
	(0.0372)	(0.0477)	(0.0389)	(0.0216)	(0.0279)	(0.0292)
JJ	0.0463	-0.0923	0.0238	0.0224	-0.0629	-0.0759
	(0.0914)	(0.111)	(0.0922)	(0.0544)	(0.0667)	(0.0665)
Merck	0.302***	0.115	0.278***	0.174***	0.0633	0.0420
	(0.0580)	(0.0768)	(0.0588)	(0.0338)	(0.0448)	(0.0452)
Novartis	0.0880	0.0934	0.0711	0.0438	0.0509	0.0337
	(0.0923)	(0.105)	(0.0928)	(0.0529)	(0.0613)	(0.0621)
Pfizer	2.531***	2.466***	2.530***	1.533***	1.495***	1.494***
	(0.0248)	(0.0316)	(0.0250)	(0.0142)	(0.0182)	(0.0183)
Valeant	-0.152*	-0.190*	-0.147*	-0.0935*	-0.116*	-0.111*
	(0.0669)	(0.0790)	(0.0670)	(0.0386)	(0.0458)	(0.0457)
ViiV	-0.0267	-0.157	-0.0236	-0.0247	-0.106	-0.107
	(0.173)	(0.216)	(0.174)	(0.102)	(0.134)	(0.135)
Avg Payment			0.00134*			0.00106*
(in 000's)			(0.000563)			(0.000472)
Citation Count			-0.00108**			-0.00137**
(in 00's)			(0.000403)			(0.000461)
Const	-1.801***	-1.753***	-1.801***	-1.080***	-1.053***	-1.053***
	(0.0267)	(0.0335)	(0.0268)	(0.0153)	(0.0194)	(0.0194)
Weights		Yes	Yes		Yes	Yes
N	52138	52138	52138	52138	52138	52138

Standard errors are in parentheses. The dependent variable is 1 if citing and cited authors are paid by the same pharmaceutical company. + Significant at 10%; * Significant at 5%; ** Significant at 1%; *** Significant at 0.1%.

Table 3.10. The Relationship of Citations and Payment Types Across Categories

	(1)	(2)	(3)	(4)	(5)	(6)
	Logit	*Logit*	*Logit*	*Probit*	*Probit*	*Probit*
Combination	0.123***	0.169***	0.180***	0.0649**	0.0929***	0.0986***
	(0.0373)	(0.0455)	(0.0457)	(0.0227)	(0.0279)	(0.0279)
Consulting	0.647***	0.688***	0.757***	0.382***	0.406***	0.445***
	(0.0366)	(0.0519)	(0.0535)	(0.0217)	(0.0303)	(0.0311)
Items	0.738***	0.766***	0.761***	0.435***	0.455***	0.451***
	(0.0321)	(0.0414)	(0.0417)	(0.0188)	(0.0241)	(0.0242)
Meals	1.681***	1.730***	1.757***	1.030***	1.060***	1.075***
	(0.0292)	(0.0383)	(0.0388)	(0.0174)	(0.0227)	(0.0230)
Other	0.129	0.0126	0.0115	0.0795	0.00850	0.00860
	(0.0842)	(0.129)	(0.128)	(0.0495)	(0.0733)	(0.0732)
Research	0.601***	0.634***	0.753***	0.348***	0.369***	0.438***
	(0.0531)	(0.0733)	(0.0760)	(0.0311)	(0.0426)	(0.0442)
Speaking	0.174***	0.175***	0.268***	0.102***	0.102***	0.156***
	(0.0277)	(0.0356)	(0.0380)	(0.0167)	(0.0215)	(0.0230)
Travel	-0.0474	-0.0789	0.0119	-0.0285	-0.0440	0.00752
	(0.0400)	(0.0556)	(0.0585)	(0.0239)	(0.0328)	(0.0343)
Avg Payment			-0.00500***			-0.00293***
(in 000's)			(0.000764)			(0.000461)
Citation Count			0.00371**			0.00208***
(in 00's)			(0.00125)			(0.000596)
Const	-1.225***	-1.311***	-1.341***	-0.744***	-0.798***	-0.814***
	(0.0301)	(0.0397)	(0.0404)	(0.0179)	(0.0235)	(0.0238)
Weights		Yes	Yes		Yes	Yes
N	52138	52138	52138	52138	52138	52138

Standard errors are in parentheses. The dependent variable is 1 if citing and cited authors are paid for the same category. + Significant at 10%; * Significant at 5%; ** Significant at 1%; *** Significant at 0.1%.

being paid within a research field leading to higher payments. However, more data is needed to establish the link.

As a next step in analyzing network effects, one might look at the subsequent publications of physicians who were paid by the same pharmaceutical company, but who had not collaborated previously. Both the probability of such a collaboration taking place and the quality of research will be important outcome variables to consider. Large collaborations may indicate pharmaceutical companies serving as research hubs beyond the usual university and hospital networks. The quality of research will show the impact of such hubs on the overall advancement of science. Overall, they will both measure the possible effect that pharmaceutical companies' payments have on research, beyond promoting their own products.

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Appendix Table A. Federal Statutes and Doctrinal Developments in Abortion Rights Law

Statute or Legal Decision	**Year**	**Statutory Provision or Doctrinal holding**	**Regulation challenged**
Roe v. Wade, 410 U.S. 113	1973	The Court recognized the right to choose to have an abortion as part of a broader constitutional right of privacy. States may proscribe abortion only in the third trimester, with an exception for the mother's health.	Texas statute
Doe v. Bolton, 410 U.S. 179	1973	The Court overturned provisions requiring that abortion be performed in an accredited hospital, approved by a hospital committee, and that three physicians confirm that an abortion should be performed.	Georgia statute
Hyde Amendment	1976	Federal provision (amendment to Title XIX of the Social Security Act) prohibited states from receiving federal Medicaid funding for abortions, except when the pregnancy jeopardized the mother's life or the pregnancy was the result of rape or incest.	Federal statute
Maher v. Roe, 432 U.S. 464	1977	The Court upheld a state policy that refused to provide Medicaid funding for non-therapeutic abortions, allowing funding only for "medically necessary" first-trimester abortions.	Connecticut statute
Beal v. Doe, 432 U.S. 438	1977	The Court held that Title XIX of the Social Security Act does not require states to fund elective or non-therapeutic first-trimester abortions to receive Medicaid funding.	Federal statute
Harris v. McRae, 448 U.S. 297	1980	The Court upheld the Hyde Amendment.	Federal statute

Appendix Table A. (Continued)

Planned Parenthood of Southeastern Pennsylvania v. Casey, 505 U.S. 833	1992	The Court upheld statutory provision requiring parental notification for minors seeking an abortion, certain reporting requirements for abortion providers, and an "informed consent" provision requiring abortion providers to inform women of the age of the fetus and health risks of abortion and childbirth 24 hours before the procedure. The Court overturned the provision requiring that their husbands be notified when married women seek an abortion and rejected the trimester framework of *Roe* in favor of a viability inquiry more in line with medical advances.	Pennsylvania statute
Freedom of Access to Clinic Entrances Act, 18 U.S.C. § 248	1994	Federal statute made it a crime to injure, intimidate, or interfere with persons seeking to obtain or provide reproductive health services or to intentionally damage or destroy property of a reproductive health care facility.	Federal statute
Schenck v. Pro-Choice Network of Western New York, 519 U.S. 357	1997	The Court upheld "fixed buffer zones" around abortion clinics that prohibit protestors from demonstrating while invalidating "floating buffer zones" around moving persons and cars.	Injunction
Stenberg v. Carhart, 530 U.S. 914	2000	The Court overturned a ban on the "partial-birth" abortion, a specific and unusual method of second-trimester abortion. Because the statute's language broadly encompassed the standard second-trimester abortion procedure as well as this variant, the statute imposed an undue burden on a woman's right to choose. The statute also lacked an exception for the mother's health.	Nebraska statute
Partial Birth Abortion Ban Act	2003	This statute prohibited the "partial birth" abortion.	Federal statute
Gonzales v. Carhart, 550 U.S. 124	2007	The Court upheld the federal Partial Birth Abortion Ban Act of 2003, whose wording was sufficiently narrow.	Federal statute

Appendix Table B. Summary Statistics

Judicial Pool Characteristics for Abortion (1971-2004)	Mean	St Dev	Min	Max	N
Number of Judges	16.835	7.212	3	48	408
Docket Size	3209.19	2135.45	353	12151	408
Probability of Panel Being Assigned Exactly One Democratic Appointee	0.411	0.094	0	0.54895	408
Probability of Panel Being Assigned At Least Two Democratic Appointees	0.413	0.203	0	1	408
Number of Abortion Panels	0.35	0.605	0	3	408
Number of Abortion Panels Having Exactly One Democratic Appointee	0.191	0.463	0	3	408
Number of Abortion Panels Having At Least Two Democratic Appointees	0.125	0.373	0	2	408
Number of Pro-Choice Appellate Abortion Decisions	0.203	0.476	0	3	408
Difference between expected and realized proportion of democrats on panels	0.553	0.471	0	1	117
GSS Respondents					
Age	45.276	17.498	18	89	14409
Male	0.563	0.496	0	1	14466
Should it be possible for a woman to obtain a legal abortion if:					
there is strong chance of serious defect in the baby?	0.8	0.4	0	1	9,189
she is married and she does not want any more children?	0.44	0.5	0	1	9,160
the woman's own health is seriously endangered by the pregnancy?	0.9	0.3	0	1	9,216
the family has a very low income and cannot afford any more children?	0.47	0.5	0	1	9,122
she became pregnant as a result of rape?	0.82	0.38	0	1	9,154
she is not married and does not want to marry the man?	0.44	0.5	0	1	9,159
the woman wants it for any reason?	0.4	0.49	0	1	7,969
Political Party Affiliation:					
Strong Democrat	0.15	0.36	0	1	14,370
Democrat, but not a strong Democrat	0.21	0.41	0	1	14,370
Independent, near Democrat	0.12	0.33	0	1	14,370
Independent	0.15	0.36	0	1	14,370
Independent, near Republican	0.09	0.28	0	1	14,370
Republican, but not a strong Republican	0.17	0.38	0	1	14,370
Strong Republican	0.1	0.3	0	1	14,370